Anatomy and Culture: A New Feminist Methodology

by

Amy Butchart

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ABSTRACT

ANATOMY AND CULTURE: A NEW FEMINIST METHODOLOGY

Amy Butchart
University of Guelph, 2014

Advisor: Professor Maya Goldenberg

In this dissertation I propose, and defend, a new form of feminist empiricism in the philosophy of science—especially the philosophy of medicine. This feminist empiricism, which is a modified form of Longino’s critical contextual empiricism, is focused on the development of a methodological account of how feminists concerned with theorizing the body should engage with those studying the body in the life sciences. I take the eating disordered body as my object of study, arguing that any sufficient descriptions of anorexia and bulimia nervosa presuppose a great deal of dialogue between feminists working on the body from a largely cultural perspective and scientists working on the body based on the rigors of their various fields. I develop and describe criteria that guide such dialogue, pointing out some commitments for the feminist who wishes to enact meaningful change within epistemic communities that have typically been wrought with systematic inequalities. I then relate this discussion to current views in feminist philosophy regarding the social role of science in knowledge-production and the new role that scientifically informed feminists can play in said knowledge-production, arguing for a novel account of how feminist criticism, informed by a rich politics of the body, might make for a more inclusive and comprehensive study of the body that is, nonetheless, palatable to those working within the life sciences.
For Nick and Imogen.
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Introduction: The challenge of theorizing bodies

This dissertation is about feminist understandings of the body, especially the diseased body, and the role that feminists play in theorizing the body in a community of specialists—many of them scientists working in biology, neuroscience, neurology, physiology, and medicine. I’ve chosen to focus on the body suffering from eating disorders—in particular anorexia nervosa and bulimia nervosa. Anorexia and/or bulimia provide an excellent *prima facie* example of what I call in Chapter One a “complex disorder”. Briefly, complex disorders are those that have an etiology that makes it unclear whether we are dealing with a disorder that is mainly psycho-social in nature, or one that is based on the mechanics of the body. Complex disorders lack a complete description without recourse to the sorts of socio-cultural studies of the body associated with philosophy, sociology,¹ cultural studies, and anthropology *and* the natural sciences tasked with studying them. The prospects for understanding complex disorders are thus tied intimately with the prospects for some coordination between those working in non-scientific parts of the academy and those working in the sciences. The prospects can appear quite dim.

The main thesis of this work is that complex disorders can be understood only after scientific and non-scientific efforts have been coordinated—only after the feminists working in the arts and social sciences have engaged in a dialogue with scientific professionals. This dialogue hopes to produce a synoptic vision, which—while it can

¹ Of course, how one classifies the disciplines between humanities, the social sciences, and the harder sciences is itself an important issue, though one that cannot be addressed in this dissertation. The sociology I address is mostly representative of social constructivist approaches, and thus I treat it as of a piece with such studies in the humanities. Such divisions are, by their very nature, always a bit blurred, but such blurriness is of no immediate consequence to the present study.
easily degenerate into a myopic scientific vision or a myopic constructivist vision—

brings into focus for a time a unified theoretical framework through which the body can

be conceived and reconceived. The purpose of this essay is to isolate a methodology for
doing feminism that makes possible such a synoptic vision, and explains how it can be
most fruitfully realized. I will argue that what we need is a dialogue between feminist
theorizers of the body and scientific practitioners, and that feminists are in the unique and
promising position (within academic and other institutions) to initiate this dialogue.

What makes this task such a difficult one is the often hostile and antagonistic
relationship between feminist theorists and those working in the sciences. Feminist
philosophers studying the body have tended to theorize the body as the product of a
complex web of social, cultural and political discourses. The move to thinking about the
body in political, valuational, symbolic and discursive terms was in part motivated by an
historical move away from reductive scientific understandings of the body. The move
away from the sciences was a strategic one for feminists, allowing them to pursue studies
on the feminine body in spite of various androcentric, sexist and racist assumptions
buttressing practices in the sciences—especially the life sciences that have spoken with
the utmost epistemic authority about women’s bodies. Feminists have long realized that
this putative claim of neutrality masks androcentric assumptions and practices, and also
works to make any criticisms to the contrary seem unreasonable and misguided. The only
claims about the body that could count as knowledge-claims are scientific claims. Only
other neutral scientists can critique the assumptions of the life sciences, but they have
little reason to do so.
This reductivist view of biology, which as we shall see, reduces women’s bodies, their behavior and their psychology to a biological imperative for reproduction and child-rearing, effectively closed off any hope for a feminist engagement with the sciences, unless a woman was willing to toe the party line and engage in reductive and scientistic accounts of other women’s bodies. A pre-requisite for being taken seriously as a scientist is a rejection of one’s political and liberatory aims. Science, on this biological reductivist account, is not a tool for political activism; since feminist work is always political, it seems to be merely a definitional concern that there could be no feminist science.²

By the middle of the last century feminists had to come to terms with this stark separation of science and values, and that meant that they needed to find an alternative basis for their liberatory feminism. As feminist theorizing progressed, it took advantage of theoretical frameworks that were intentionally developed as alternatives to European Enlightenment thought and its naturalist underpinnings. Such frameworks included phenomenology, existentialism, Marxism, Freudian and Lacanian psychoanalysis, structuralism, and postmodernism. The gulf between (naturalist) science and (non-naturalist) feminism now seems to be architectonically entrenched.

While some feminists have challenged a reductionist view of science without rejecting the broadly empiricist architechnology that is taken to be central to the sciences, in particular, Helen Longino and other feminist empiricists (some of whom will be discussed in Chapter Four and the Conclusion of this dissertation), feminist philosophy,

² The reader may recognize a shift in the preceding paragraph regarding the use of the word “science”. In the first few pages, we used the plural “sciences”, but here shift to the singular, global noun. Use of the singular noun throughout this dissertation is done for the purposes of grammatical simplicity, and should not be taken as an indication that science is monolithic. Where the distinction matters—when there is a discussion of particular branches of sciences, e.g. neuroscience, medicine, anatomy, etc.—this grammatical convention will be set aside. Other places, where scientific practice, scientific theory, or the institutions of science are specifically under discussion, will also see this convention set aside.
as a historical matter of fact, has largely been pursued from a position diametrically opposed to the sciences, quite often with the assumption that the sciences are an obstacle to a proper understanding of the body outside of a white, heterosexual, male norm. The body on this view is conceptualized as the product of masculinist, racist, classist, ableist, cis-gender, and heteronormative institutional frameworks, and ways of thinking that leave the gendered, marked body as outside, as “Othered”. This conception of woman finds its roots in the phenomenological and existentialist writings of Simone de Beauvoir. To that work we now turn.

I. Background: The feminist turn away from science

The influence of de Beauvoir’s work, especially *The Second Sex*, cannot be understated in the feminist canon. De Beauvoir offers important insights into the dangers of reducing woman to her biology—the dangers of defining woman as a womb. To define woman in this way is to relegate her to her biological sex. De Beauvoir claims, “[w]oman has ovaries and a uterus; such are the particular conditions that lock her in her subjectivity.” Woman, even in her own subjectivity (“consciousness”) becomes nothing more than the sum of her uniquely female parts. She is understood as biologically different from man, but more than that, this difference is significant—she is recognized, and recognizes herself, as inferior.

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3 It would be useful to have a definition of “body” for such discussions. However, much of this dissertation regards the contentious nature of such a definition. Those from the life sciences would like to provide a biological definition of the body; those from the humanities tend to prefer a definition of the body that takes into account the various ways in which bodies are socially constructed through the use of binary categories within systems of signification (hence, “marked” bodies are bodies that are significant within such systems), usually sexed, gendered, and raced. Chapter One offers a more sustained discussion of these thorny issues, and why they matter to this project.

4 De Beauvoir (2009), p. 5.
Defining woman in this way works to perpetuate the myth that a woman’s nature is wholly located in her biology. De Beauvoir critiques this “biology as destiny” myth, arguing that it works to maintain women’s subordinate status relative to men. It also works to structure the relationship between men and women. She argues, “[a] man is in his right by virtue of being man; it is the woman who is in the wrong.”\(^5\) This oversimplified relationship between woman and man is one where woman is necessarily subordinate, only understood after we have defined man as the universal human being. She can never be separate from man—can never be autonomous. This means that she can never fully express her freedom and individuality, because she is only granted the status of the Other. She is object, while man is subject.\(^6\) He is the oppressor and she is the oppressed. De Beauvoir explains, “He is the subject; he is the Absolute. She is the Other.”\(^7\) This creation of the Other means that she is always objectified in the meeting of subjectivities, and thus is not free to move about the world as a self-governed and choosing subject interacting with other subjects. Her Otherness penetrates her consciousness itself.

De Beauvoir’s critique of biological reductivism exposes it as of a piece with other tools of masculinist domination dressed in the garb of the Enlightenment values of impartiality, rationality, the disinterested pursuit of knowledge, political neutrality, etc. These Enlightenment values give the sciences, including biology, their unprecedented authority, but are, nonetheless, chimerical, or at least very misunderstood. The category woman is not tied exclusively to biology (appearances to the contrary), but is tied also to other systems of oppression—though biology and the other related sciences offer a

\(^5\) Ibid, p. 5.
\(^6\) Ibid, p. 6.
\(^7\) Ibid, p. 6.
justificatory basis for political oppression.\(^8\) Because de Beauvoir is able to isolate the systematic oppression of women in a moment long before the existence of modern science, and because biology (as a practiced discipline) is a construct of masculinist and Eurocentric oppression, *woman* is best understood as socially constructed category that is as natural as the biological sciences that speak of its essential nature.

This is all perfectly consistent with the view that to be a woman requires that criteria outside of biology be met. There is a phenomenological truth that, prior to de Beauvoir, had not been conceptualized or expressed. The *nature* of woman is a biological fact, but a woman is not automatically feminine (in the way that she ought to be) because of her sex-organs. “So not every female human being is necessarily a woman; she must take part in this mysterious and endangered reality known as femininity.”\(^9\) Reducing woman to her biological markers fails to recognize the numerous other conditions that exist outside of biology that must be met in order for one to be properly considered a woman. We must distinguish sex from gender. Biological sex is incapable of completely determining gender—“one is not born a woman, rather one becomes a woman.” The internalization of Otherness in phenomenology was always supposed to cover up this distinction between sex and gender. Uncovering the ways in which it has been internalized and propagated has been the purview of feminist theorizing since de Beauvoir.

These themes about otherness remain in feminist philosophy, even after de Beauvoir’s particular brand of phenomenological existentialism was widely rejected. It seems to be part and parcel of most forms of post-structural and postmodern feminism.

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\(^8\) De Beauvoir is concerned with the important role of biology in perpetuating the identification of female sex with social views about women’s psychology and behavior.

\(^9\) De Beauvoir (2009), p.3.
With the postmodern turn\textsuperscript{10}, especially in the work of feminists like Cixous and Butler, otherness remains, and remains as the product of the sometimes subtle and sometimes overt forms of oppression that are manifest at the most fundamental level of the self. This the postmodern feminist shares with de Beauvoir, even if the social and cultural forces at play are even more minutely dissected, and even if the notion of what constitutes the self is significantly changed—a self that is socially constructed, a site of reproduction of gendered norms, with a body that is docile and malleable. What has changed most significantly in postmodern feminism is the possibility of creating an identity from a position of otherness. This is not to say that the construction of identities is a free for all, and that selves can be created at will; rather, in adherence to social norms for the creation of selves there is always some possibility to create a self that pushes the boundaries of such norms and that challenges the coherence of the very system of norms that govern the creation of identity. Otherness can be a prison as it was for de Beauvoir, but it can also be a platform from which new identities and new possibilities can be explored.

Cixous studies otherness as a product of androcentric and anthropocentric rule-governed discourse, which excludes women as “unclean”. Otherness is defined by whatever falls outside of the (European) male norm, including women, racial minorities, animals, and forms of expression that are not in accordance with a clear logic or purpose—where the law, science, history and religious moral injunctions are paradigmatic of such a logic. Cixous’ purpose is to carve out a place for expressions of

\textsuperscript{10} The postmodern turn occurred in the 60s and 70s starting mainly with the work of Foucault and his rejection of certain modern theses common to thinkers from Hegel to Marx regarding the stability and identifiability of a pre-social self. This work, in particular the work of Marx and later Marxists, was intellectually dominant in Continental Philosophy broadly speaking, but especially among feminists post-de Beauvoir.
the feminine that are beyond this law, with its religious and scientific authority that at first seems so universal and so totalizing as to demand complete adherence.

Cixous highlights not only the destructive nature of drawing sharp distinctions between what is part of the world and what is excluded from it, which she calls immonde. This is perhaps the most encompassing binary imaginable within phallogocentric logic. She also points to the possibilities for agency that accompany such exclusions or exiles.¹¹ Cixous seems to be telling us that for some of those who are unclean—who are defined by their exclusion—such exclusion is destructive. For others, those who “transform their exile into a country”, however, this exclusion allows them to create an expressive space to accommodate their alterity. They create identities that are beyond the organizing norms of phallogocentrism—including biology and its important role in categorizing identities and justifying the norms that regulate those identities.

Butler outlines a similar story concerning sex and gender. She attends to the possibility of blurring boundaries and “playing” with gender. Her central notion of “performativity” amounts to the repetitive reproduction of discursive norms within regulative frameworks that sanction certain modes of being, but not others, in such a way that it is thought that some metaphysical substance must underlie discursive norms. In Butler’s conceptual framework, the eventual manifestation of new genders emerges from this play. For Butler, “[g]ender is the repeated stylization of the body, a set of repeated acts within a highly rigid regulatory frame that congeal over time to produce the

¹¹ Cixous says,
So in the same line of substitutions you find: Jews, women, niggers, birds, poets, etc., all of them excluded and exiled. Exile is an uncomfortable situation, though it is also a magical situation. I am not making light of the experience of exile. But we can endure it differently. Some exiles die of rage, some transform their exile into a country. (Cixous (1981), p. 120)
appearance of substance, of a natural sort of being.”12 From this reproduction of gender norms we develop a “metaphysics of substance”, a binary ontology of male and female. But these sites of reproduction, once exposed as reproductions rather than as natural, provide us with a framework for challenging said norms. Butler frames the opportunity as such:

That the power regimes of heterosexism and phallogocentrism seek to augment themselves through a constant repetition of their logic, their metaphysic, and their naturalized ontologies does not imply that repetition ought to be stopped—as if it could be. If repetition is bound to persist as the mechanism of the cultural reproduction of identities then the crucial question emerges: What kind of subversive repetition might call into question the regulatory practice of identity itself?13

For Butler, those who fail to find fit in the binary categorizations offered by this abstracted metaphysics will surely be forced into extremely difficult situations. But from these positions arise the possibility of producing something new through an act of reproduction—for reproduction logically entails production.

So for Butler, as for Cixous, those who are exiled (who are left out of the conceptual boundaries offered by heteronormativity and androcentric—and anthropocentric!—law) are not without the materials they need to produce new relational selves. The main point of departure for Cixous and Butler is that Butler’s philosophy is less reclusive, and there is no way we can carve out a separate place for l’écriture feminine (the term Cixous reserves for feminine modes of creative expression outside of masculinist logic). For Butler, the very efficacy of the performances of subversive genders presupposes a framework within which all are subject to the prevailing discourses of the metaphysics of substance—what Butler calls the “masculinist economy

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12 J. Butler (1999), p. 43-44.
13 Ibid, p. 42.
of gender”. One is not able to recuse herself to be in complete exile from the phallogocentric logic governing the production of her gender. For Butler, the creation of a new gendered identity is necessarily embedded and related to existing gender norms. As Butler tells us, it is not so much the law which is at issue, rather power, and power in its full Foucauldian sense, including, “the productive (inadvertently generative) functions of differential relations.”

Thus, for Butler, The sexuality that emerges within the matrix of power relations is not a simple replication or copy of the law itself, a uniform repetition of a masculinist economy of identity. The productions swerve from their original purposes and inadvertently mobilize possibilities of ‘subjects’ that do not merely exceed the bounds of cultural intelligibility, but effectively expand the boundaries of what is, in fact, culturally intelligible.

For Butler, as for Cixous, activity from abnormal positions is unquestionably the source of real agency. For Butler, however, this activity—this performance—must be done from within frameworks of intelligibility which already exist, though the hope is that they will be altered as a consequence of such (re)productions.

There is a rich history in feminist studies of the body to conceive of the body as a locus of socio-cultural forces, usually within a semiological, psychoanalytic, phenomenological or discursive framework, rather than as a biological, physiological and neurological system. As a consequence, feminist studies of the body, post-de Beauvoir—in particular, postmodern studies of the body—have tended to only take the biological sciences seriously as a point of criticism. Biology, on this view, becomes just one entrenched part of the phallocentrism that functions as an alibi for the oppression of women and other marginalized bodies. The biological sciences are not considered a

potential ally that could be utilized for the amelioration of women and utilized to challenge existing binary and reductive understandings of gendered bodies and gendered selves.

II. The question of a feminist science

The feminist approaches to studying women’s bodies outlined above, and the theoretical frameworks on which they rest, are laudable. Feminists have achieved a kind of theoretical autonomy, seeking a different basis for an understanding of gendered bodies and gendered selves that is independent from, and critical of, a totalizing view of biology as destiny. Nonetheless, this autonomy comes at a cost. By breaking ties with the biological sciences, feminist studies of the body after de Beauvoir become marginalized vis-à-vis the dominating authority of the sciences. Their autonomy is unassailable, but accompanied by a kind of academic and intellectual stigma. Because of the decisive break with the socio-political institutions of the life sciences, and scientific frameworks for generating knowledge, there is slight chance that the feminist account of the ideational or symbolic or discursive body will be taken seriously (by scientists qua scientists), and little opportunity for feminism (done from such a perspective) to bring about change in those socio-political institutions and frameworks.

This is not to say that postmodern accounts of the body have not had a lasting effect on the academy, and certainly not to say that their critiques are not (very often) warranted. It is rather to say that feminists who work from such a theoretical framework (or one of a constellation of related frameworks inspired by de Beauvoir) are not in a
position to have a dialogue with the sciences that might bring about anything but reactionary rhetoric from the sciences, if any response at all.

The inability for postmodern feminists to engage in meaningful dialogue with the sciences is owed to more than just an unreflective scientism and bias amongst scientists, though some scientists have a deserved reputation for scientistic ignorance of what is being done in the social sciences and humanities—especially feminist research. Failed dialogue is also, I will argue, a symptom of a fundamental error by some feminist theorists, who have failed to properly situate their conception of an ideational, symbolic, or discursive body alongside a material, visceral, and physical body. In other words, postmodern accounts of the body too often neglect the physiological and neurological realities of the body. I begin from the premise that there is much at stake for the feminist who chooses to critically engage with the material body—that is, the bleeding body, the neurological body, the sick body, the healthy body, etc.

I am also committed to a second premise, perhaps more contentious: that at least some feminists must study the material body using the sciences dedicated to its study, including biology, physiology, neuroscience, and so forth. While the details of what it means to study the body scientifically must wait for Chapter Four, a brief remark is in order here. Firstly, there is no demand placed on individual feminists to learn the sciences that might impact their discussions of the body. There may be a demand for individual feminists to take heed of existing research that has such an impact, but this is orthogonal

16 Scientism is a word used in motley ways by some philosophers to refer to a collection of concerns, the most prominent amongst them being a) the concern that the sciences claim an authority over domains for which they were not developed, for example: religion, aesthetics or ethics; b) the rather strong claim made by some scientists and philosophers of science that knowledge is only possible through scientific experiment. This isn’t a common view, though it is often how some philosophers might critically characterize science. This characterization of the sciences is not one being pursued in this dissertation.
to the current discussion. What is required is that some feminists become well acquainted with the sciences that may, or already do, speak of the eating disordered body. Thus, it is a claim about an *epistemic community*, not (first and foremost) individual feminists. The amount of scientific knowledge expected from individuals within this epistemic community can range widely, though some will have to have enough expertise regarding the science of the body that is commensurate with their counterparts in the sciences themselves. Others will have to possess a minimal expertise, usually regarding the strategic reading of scientific publications that impact their research. Details of this kind of scientific knowledge will be discussed in Chapter Three. Still other feminists will be required to have little or no knowledge of the sciences.

Secondly, so long as the divisions of labour are clear, there is still room for feminist theorizing about the body that does not concern itself with current work in the life sciences regarding healthy bodies and eating disordered bodies. However, I make a strong claim nonetheless: that such work is most beneficial to the political aims of feminism when produced within an epistemic community that also engages directly and indirectly with the life sciences. Obviously, this epistemic community is better off if its members, taken as a whole, possess fairly esoteric scientific knowledge. However, as we shall see in Chapter Four and the Conclusion, this kind of sophisticated scientific expertise is a *goal* of the methodological prescriptions I introduce; this dissertation is making claims about where we can go as an epistemic community, much less than claims about where we are today. That being said, I will make reference to important work being done by feminist philosophers of science whom are already well-acquainted with the ins and outs of the sciences they study, showing how such acquaintance is covariant with
new and exciting avenues for research. It will thus become clear what the benefits are for an epistemic community that adopts the methodological prescriptions I develop in Chapter Three.

As we have already seen, given a long history of essentialist and Othering representations of marginalized bodies, it is certainly reasonable for the feminist to be hesitant to see the sciences as an ally, or even a reliable tool for her liberatory purposes. Scientific understandings of the body have been guilty of the sort of biological reductionism that de Beauvoir and others have rightly criticized. Moreover, feminists hesitating to adopt a more scientific view of the body are right to notice that there is a tension between liberatory feminism and the socio-political forces within scientific institutions. I will show that although this view is reasonable, it is ultimately ill-advised, establishing at the level of theory a deep mistrust of the sciences that precludes dialogue with scientists about the body. This sort of contrarian position relative to the sciences is particularly troublesome because it elevates problems in the current androcentrist practice of science (and even androcentric assumptions at the base of much scientific theory) to inalterable features of science. This tactic serves to isolate liberatory feminist theory and practice from harmful discourses that come from the sciences, but the consequences of which mean that congress between feminism and science is jeopardized. It is this congress that offers the best hope for a feminist transformation of scientists and scientific practice, working to make scientists and scientific practice (a) more responsive to political concerns of marginalized people, and (b) socially responsible.

I argue that the implications for taking this as our starting point as feminist philosophers of the body are three-fold. Firstly, such an approach opens up a previously
under-explored site for political engagement. Secondly, it facilitates dialogue between feminist philosophers of the body and those studying the body from a scientific perspective. Engaging in dialogue with scientists helps feminists learn about the body as it is studied in anatomy, biology, genetics, neuroscience, cognitive science, and higher-order psychology. Thirdly, such dialogical congress between feminists and scientists has positive effects for the production of knowledge. Science that is more reflective about its constitutive values, and the contextual values of its practitioners and stakeholders, is in a better place to get and interpret more data. This leads to more objective science.

Dialogical interactions with scientists are the best way for the feminist to unveil the systematic role of political and valuational dimensions that operate ineliminably in scientific theory and scientific practice. Indeed, the overarching objective of the work is to (a) provide a fuller understanding of eating disorders and the eating disordered body, but also (b) to show how feminist input can improve scientific theorizing and practice, i.e. make it more objective, and (c) to engage science as a tool, rather than an obstacle, in facilitating socio-political change.

Using the eating disordered body as our object of study, I illustrate the importance of moving beyond the isolationist and reactionary position typical of much feminist studies of the body since de Beauvoir. The body on my view remains a site for political engagement, but such engagement, once we open up our theoretical and methodological frameworks, can more effectively challenge non-feminist, scientific characterizations of the body. Such challenges have invaluable import when it comes to developing a more holistic understanding of the body suffering from anorexia nervosa and bulimia nervosa.
III. Overview of chapters

In Chapter One I lay the groundwork for this study. I begin by outlining two minimal criteria of adequacy that the feminist philosopher studying the body must meet in order for her critical work to be practicable and ameliorative. Firstly, I argue that she must take a critical and engaged approach to studies of the body in the medical sciences. Secondly, I argue that it is not enough to talk about the sciences, scientific practices, and scientific institutions; rather, she (or, rather, at least some feminists) must also engage directly with the empirical sciences in her studies. How much engagement is required is a topic for discussion in Chapter One.

I then show how scientists and social constructivists have found themselves at a conceptual impasse regarding the study of complex disorders. It is on this issue that we begin to see the gulf that exists between these two camps, and the obvious problems that arise when it comes to facilitating communication across this gulf. I argue that feminists are in an ideal position to reorient studies of the body, especially the eating disordered body, and that a methodological, rather than a substantive approach, is best to begin bridging this gulf. I call this approach a form of critical empiricism because it takes seriously a broadly empiricist approach to the sciences, but one that is not divorced from the political and liberatory aims central to feminism.

In Chapter Two I turn to a more focused and sustained philosophical analysis of the eating disordered body. In particular, I outline a phenomenologically oriented embodiment theory as one alternative to studying the body that is able to negotiate some of the tensions between constructivist and scientific accounts. We see that
phenomenologically oriented embodiment theory offers a methodological approach that recasts some of the issues we encountered in Chapter One.

One important facet of this approach involves taking seriously first-person accounts. On the one hand, such accounts are useful because they offer us insight into the way an individual suffering from an eating disorder moves through her world, often challenging some of the practices, assumptions, and narratives we find in either the social constructivist or the scientific camp. On the other hand, careful analysis of such accounts tells us that caution must be exercised when we’re dealing with this kind of data. In particular, it becomes clear that first-person accounts must be interpreted in light of naturalist concerns related to the physiological happenings in the body, and the social constructivist’s concerns related to the social, cultural and political context of the body. It is a mistake to think that the data provided by first-person accounts is more “authentic” or reliable than the data one can cull from scientific and constructivist accounts.

I explore Daniel Dennett’s account of heterophenomelogy as one way of properly contextualizing and interpreting the data we get from first-person accounts. For Dennett, interpreting first-person accounts can only be done from within an interpretivist framework. The practitioner interpreting the data, who may also be the practitioner collecting the first-person reports from a subject, must assume that the subject has beliefs that are very much like the practitioners, that the subject is rational like the practitioner, but also that the subject can be mistaken, or misguided in her understanding of her illness. Thus, first-person accounts are not to be taken as authoritative, and certainly are not to be taken to report on some mysterious content available only to the subject herself. While Dennett offers us an interesting way to navigate this sort of data, his methodology is
intended to be a minimalist and neutral one. A central feature of this dissertation (and feminist work, in general) is that studies of the body, especially the body suffering from complex, and gendered disorders, are not politically neutral. In the end, I turn to Elizabeth Wilson’s embodiment theory. Wilson outlines what she calls “gut feminism”—which is essentially an approach directed at bringing biological studies of the body into congress with the political and cultural body. I argue that she proposes the sort of framework required for reorienting studies of the body—that is, she offers a way to rethink the eating disordered body that engages with the body as a biological entity without abandoning our political motivations and goals.

In Chapter Three I employ the form of critical empiricism I develop in this dissertation to engage with current neuroimaging techniques used to study the eating disordered brain and illustrate how feminists might meaningfully engage with scientific data. I argue that feminists have an important role to play in the wider medical community concerning the way the eating disordered body is conceptualized, but that their voices have yet to be properly heard. My methodology involves satisfying two criteria. Firstly, feminists must communicate with those studying the body from a scientific approach, including adopting a scientific vocabulary for the purposes of such communication. Secondly, feminists must critically engage with scientific data, remembering that scientific data always require interpretation, and such interpretive models can reflect the political ideals of feminists—for example, models of the data can propose non-binary sex or gender categories for interpretation, replacing binary sex categories with a spectral account of sex. Responsible science starts with making explicit one’s proposals for modeling or interpreting the data, always being careful to highlight
possible assumptions about the body that influence said proposals. So long as this process of explication is followed, it is open to criticism and revision—the hallmarks of good science versus junk science.

I engage with recent work involving PET scans to gain insight into the workings of the eating disordered brain. I argue that engaging with recent work in neuroimaging gives the feminist the resources she needs to develop not only a more holistic and robust understanding of the body, including a richer vocabulary for talking about the body, but also a wider audience for her work, including her political agenda(s). Moreover, I argue that adopting the methodological approach I outline does not require the feminist to have expert scientific knowledge; rather, it requires a commitment to adopting a pluralist account of the body. This sort of approach, while it demands that she engage in scientific studies of the body, does not demand that she accept these studies uncritically, nor does it demand that she talk about the body with scientists using only a scientific vocabulary.

In Chapter Four I situate my work and its significance firmly within the larger community of work in feminist philosophy of science. My critical empiricism has many affinities with other forms of feminist empiricism, especially the work of Longino (2002), as regards the basic commitment to an empiricist architectonic for the sciences. However, it also attempts to maintain some of the commitments of feminist standpoint theory and feminist postmodernism—especially the view that the dialogue I hope to foster between feminist theorists and scientists is not an equal exchange, but that equality is not a prerequisite for such dialogue (as Longino has argued), but a product of communicative dynamics. If “tempered equality” is a precondition for “uptake”, then it’s unlikely that science will ever cease to be a bastion for androcentrism and other forms of
epistemological tyranny. Tempered equality would serve to put feminists in a position to engage in meaningful dialogue with scientists and medical practitioners, but it does not exist in practice—and that is the core of the problem. In order to foster real change in the science via congress between feminists and scientists, feminists must assume an unequal position in dialogue. The motivation to do so is, I hope, clear: to help develop the sciences in such a way that the sciences start to suit their own political ends. Only once a feminist has adopted a scientific vocabulary and become familiar with a scientific way of thinking (though neither completely, nor uncritically) can she engage in productive and transformative discourse with the sciences. Should those working in the sciences choose not to listen or refuse to take her criticism seriously, then they are not only failing to engage in properly rational scientific study, they are also impoverishing scientific knowledge.
I. Introduction

In the introduction to this chapter I want to show how feminism since the 1950s has been tied to a rejection of positivistic and scientistic practices and institutions. This is for good reason. Firstly, in scientific institutions there has historically been a gender divide. These institutions don’t shun women so much as relegate them to the lower echelons of science: lab technicians, nurses, data collectors, etc. Men, on the other hand, occupy positions of power: designers of experiments, health professionals, and lead authors on scientific papers. Secondly, the scientific community has not been hospitable to women and their concerns, perhaps because of the gender divide just cited.

Women and their concerns have found themselves on the margins of the scientific community, which has resulted in a critical stance, quite often directly scientific institutions (and even against prevalent perceptions of the scientific method as “value neutral”), which forms a thread through much of the canon of feminist philosophy and feminist studies more generally. Of course there have always been some feminist theorists (i.e. feminist standpoint theorists and feminist empiricists) who have taken the sciences more seriously. But, even such projects are defined in part by a skepticism of current scientific practice—the difference being that such feminists hold out an optimism for the future course of the sciences, should we ever find a way to be more inclusive of women’s voices.
It seems that any feminist study of the sciences must adopt a critical attitude, and must think of science as a series of institutions in need of much reform.\textsuperscript{17} We can even think of this kind of critical awareness as being a minimal criterion of adequacy for the sorts of feminist projects we will be looking at in this dissertation: feminist studies of the body. Failure to talk about the sciences, and talk about them critically, should cause us to question the seriousness of any comprehensive feminist proposal for a study of the body.

What we will propose here is one further criterion of adequacy for a feminist study of the sciences. Feminists not only need to talk critically \textit{about} the sciences, they need to also talk critically \textit{to} the sciences and scientists.

\textbf{II. Complex disorders: What they are, and why they’re interesting}

The body has long been a focal point for a number of different and often disparate studies. Many of these studies have focused on the disordered and diseased body as it manifests physical and psychic phenomena. The disciplines used to study such phenomena include biology, sociology, anthropology, psychology, philosophy, neuroscience, epidemiology and medicine, to name only a few. Given the multiple studies of the body, some overlapping, some mutually inconsistent, it’s unsurprising that radically different vocabularies and methodologies have been developed around a common subject matter. However, such disparate studies make it nearly impossible to speak about the body, especially the diseased body, without running into certain

\textsuperscript{17} This includes even the “harder” sciences like mathematics and physics that are often thought immune to such critical examination and calls for reform. When thought of from the institutional perspective, they are social practices working within broader institutional frameworks with limited resources, and there are consequences for how those resources are distributed. Similarly, even if we assume that scientific method is, in the abstract, sound (though this need not be assumed), its implementation is always institutional, offering the opportunity for scientific methods to inherit the problems of the institutions.
methodological problems. This chapter will address the most difficult of these problems in an attempt to show that disciplinary tension is a positive, not a negative, feature of studies of the body. In order to be in such a position, I must first address some definitional matters, which will hopefully make clearer the sorts of disciplinary confrontations and contestations to be explored in later chapters.

As the section heading indicates, our study is concerned with a special class of disorders, here called “complex disorders.” This concept is perhaps more at home in genetics, where a complex disorder is one that has genetic as well as environmental factors as its cause. Often referred to as multifactorial or polygenic disorders, complex disorders require reference to environment or lifestyle, not merely Mendelian genetic inheritance, in an explanation of origin and progress of the disease.\(^{18}\) This definition will not be adopted in the current study, though a related precising definition will be. This precising definition broadens the definition in two ways. Firstly, we will not limit our attention to disorders with a specific genetic component in their origin. Our definition will deal with all physiologically manifested disorders, genetic or not. Secondly, we will be utilizing an expansive notion of “environment.” In this study we will explore the social and cultural dimensions of disease. Thus, we will assume that they can be contributing factors to disease. Here is a cursory first attempt at a definition:

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\text{Complex disorders} = \text{disorders which have both a medical-scientific profile and a} \]

\(^{18}\) For a nice overview of complex disorders see R. Nussbaum (2004).
cultural profile. Such disorders are defined by the fact that they involve physiological and socio-cultural phenomena.¹⁹

Other characteristics of complex disorders that we choose not to make part of the definition are nonetheless central to the concept. For example, such disorders have a complex etiology, by which is meant that they cannot be adequately explained (i.e. causally explained) without recourse to a range of socio-cultural and medico-scientific data. This means that the causes of complex disorders are almost always contested and contestable, and they change as our best socio-cultural and scientific discourse change. Also, complex disorders almost always have traceable genealogies. That is, they are disorders which can rarely be understood without being placed within some historical context, including the differential discourses and scientific technologies available throughout the historical study of the disease.²⁰ This is not merely the mundane claim that disorders aren’t fully understood without technological and disciplinary advancement. It is the stronger claim that these sorts of disorders qua disorders are historically and socially situated.

One may ask why the definition of complex disorders does not include the notions of complex etiology and traceable genealogy. There are two reasons. The first is pragmatic. The more complicated the definition, the more difficult it is to apply. The second is logical. Because definitions are so easily offered (in philosophy and elsewhere), having them do too much of the “heavy lifting” seems at least a bit dishonest. The less

¹⁹ By “profile” we mean some recognized constellation of properties that allow a particular scientific or non-scientific discipline the conceptual resources to identify an object of study.
²⁰ “Complex etiology” is a notion that can be thought of as synchronic: complex disorders must be put into social and cultural context. “Genealogical tracing” is, by definition, diachronic: complex disorders are better understood when we examine previous attempts to grasp their significance.
content we put into the definition, the more we have to actually prove by going beyond
the definition with philosophical argument and empirical evidence. This study attempts to
show the merits of such an approach, and does not want to settle the bulk of the issues
beforehand. More precisely, we do not include the concept of complex etiology in our
definition because it may turn out that a cause can be isolated using some future medical
technology, and that recourse to complex social relationships play very little to no role in
our etiological understandings. Because this is possible, we can’t include such a criterion
in our definition. Likewise, some complex disorders do not have a traceable genealogy,
though this is rare. A good example here might be the HIV/AIDS virus. Because the
disease initially spread within a fairly isolated, but politically and culturally marked
group in the United States and Europe, it was certainly understood only through socio-
cultural discourses that accompanied the early medico-scientific data. However, by virtue
of being a “new” disease, there was no genealogical tracing to be offered. (Unless one
wants to preclude the study of a putatively complex disorder like HIV/AIDS, one needs
to be careful with definitions.)

Complex disorders are, by their very nature, partly identified by a lack of
consensus concerning what they are, but representative examples include: depression,
mania, anxiety, anorexia, bulimia and eating disorders not otherwise specified. One of the
reasons why such disorders are considered “complex” is because they have characteristics
that need to be studied using tools we find in a variety of different disciplines. Perhaps
we should think of all diseases as being better understood if we utilize a plurality of

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21 We should also pause to note that the notion of genealogical tracings isn’t new to this project; it finds it
roots in Foucault. In Foucault’s *Madness and Civilization* (1988) he offers a study on madness where he
shows that only through the development of certain discourses and technologies did madness become
something to be treated. Previously, madness was relegated to the religious domain.
interdisciplinary tools. In this way, complex disorders would exist on a spectrum. Even the common cold, which is defined quite easily by modern virology, is better understood from a plurality of perspectives. For example, we can better understand the trajectory of the spread of the disease when we know something about practices of hygiene in a population, or even methods of greeting. What constitutes a complex disorder is not, therefore, merely the benefit of studying such diseases using multiple tools. (It is perhaps always best to use a plurality of such tools.) Complex disorders form one extreme on the spectrum of diseases because there is a recognition that no one discipline is able to offer identity conditions that are respected by others working in other disciplines. When a sociologist speaks about the spread of the common cold in a socio-cultural context, she is taking the reference of her discourse (the word “common cold”) to be fixed by some other discipline, e.g. virology. Complex disorders are unique in that the fixing of reference, and who gets to do it, are contested. Such diseases are widely regarded as having social, genetic, psychological, physiological, and neurological factors built into their very identifying conditions.

As an example: the medico-scientific profile of manic depression involves explanations concerning the etiology of mania, including its neurochemical or genetic determinants. This profile is also comprised of physical manifestations and various symptoms that present with the disease. The cultural profile of manic depression, by contrast, involves the various discourses employed to articulate the disorder and its effects, such as discourses culturally specific discourses about the possible benefits of manic and depressive behaviours in high-stress and high-performance jobs that require periods of intense focus, or other cultural discourses that allow for the classification of
manic or depressive behaviour as a mental illness, and the subsequent stigmatization of those who suffer the illness. These discourses, and the various understandings of manic depression they endorse, are continually changing and evolving which means that they are socially and culturally located/locatable. They also focus on aspects of the disease that may seem foreign to a medical researcher, such as the “meaning” of the disease and its cultural origins. We have already discussed HIV/AIDS as an example, but a more thorough examination of manic depression, and the work of Emily Martin on that subject, might help illuminate the point.

In her book, *Bipolar Expeditions*, Emily Martin claims that while the biological determinants of manic depression and treatments for those living with it are important, they are not the whole story. She argues, “…the reality of manic depression lies in more than whatever biological traits accompany it. The ‘reality’ of manic depression lies in the cultural contexts that give particular meanings to its oscillations and multiplicities.” Martin offers a cultural examination of manic depression. She contends that cultural expressions of the “irrational” highs and lows of manic depression are bound up with certain current cultural practices and endeavors – e.g. the current corporate world seems to favour CEOs that can embody “[m]ania’s interstitial position between mood (floating, changeable feelings in the psyche) and motivation (organized, goal-directed behaviour)…” Many of the behaviours of the successful CEO are going to be similar in pattern to those of the serotonin imbalanced manic-depressive according to this particular narrative. Such revaluations of manic-depressive behaviour, Martin contends, can serve to change the nature of the disorder for those living under its description.

23 Ibid, p. 234.
We identify disorders such as manic depression as “complex” because they are deeply rooted in particular social and cultural contexts. Beyond the physiological and biological profiles of such disorders, there remain issues concerning what these disorders actually mean, and how they gain significance within a particular culture. Moreover, there are often questions of whether or not a particular disorder or pathology can even be classified as such. For example, there are some individuals who insist that eating disorders are not pathologies at all; rather, they are better understood as ways of life, and, as we have just seen, manic-depressive behaviour can likewise be normalized in such ways that it is not seen as a disorder. In short: complex disorders demand that we recognize the complicated and somewhat precarious interaction between the body and the various circumstances in which it finds itself.

III. The feminist question

The question with which we will be concerned in this chapter is this: Can feminists begin to address the essentially interdisciplinary character of complex disorders? Before we offer an answer to this question, it is important to note that the feminist will likely only care about a subset of complex disorders, namely those complex disorders that have a gendered profile. This can mean two things. In the first sense, a disorder is gendered if it affects men and women differently. In this way, heart disease and diabetes might be gendered, for the origin of such diseases, their progress in the patient, and their treatment differ based on biological sex. In fact, it is difficult to think of a disorder or disease that is not gendered in this broad way. For this reason, I will pass over the issue of “gendered disorders” in the broad sense of the term as fairly uninteresting. However, a more
interesting notion of a gendered disorder remains. This kind of disorder is one that afflict women disproportionately to men, without any clear medical reason as to why. Exemplars of such disorders are anorexia nervosa and bulimia nervosa; most of their sufferers are women, and feminists should find this of immediate concern, whether they study the disease medico-scientifically or culturally.

Another reason why feminists should be concerned with eating disorders as complex disorders is this: of the various ways one can study the diseased body, there is currently a bifurcation in approaches to research. On the one hand, feminists working in the sciences, especially mainstream biology and neuroscience, have tended to think of the critical questions non-scientific feminists ask as largely downstream from their work, such as the use of culturally meaningful metaphors used to describe sick and healthy bodies/minds. Feminists working in the humanities and social sciences hold that such critical questions ought to be at the heart of all studies of the body; the notion that there is a way of engaging in studies of the body within “normal science,” where researchers suspend their critical feminist perspectives, is misguided—feminism is always critical. This current bifurcation is an indication that any comprehensive study of the eating disordered body is going to require both medical and socio-cultural axes of assessment. The further question, as we proceed, is a methodological one: how do we get those who study the body as a scientific (e.g. biological or medical) object to speak with those who study it as a confluence of socio-cultural forces?

Issues regarding the interface of medical science and feminist theorizing about the body (in largely non-medical vocabulary) will have to wait for later chapters in this dissertation. This chapter is meant to lay the groundwork for those more complicated
issues, and is largely classificatory. Firstly, we seek a definition of the eating disordered body, with an especial focus on definitions for anorexia nervosa and bulimia nervosa. This focus on eating disorders will also further illuminate why complex disorders are so complex. Next, we will examine, in the broadest strokes, two approaches to studying the eating disordered body: the naturalistic\textsuperscript{24} approach, which comprises a family of views with certain scientific commitments, largely found in the sciences of the body, and the social constructivist approach, which describes a family of views that attempt non-scientific explanations of the diseased body. It will become apparent by the end of this chapter just how much these two approaches are at odds with one another, and how this tension poses a problem for a comprehensive approach to studying eating disorders—one that utilizes the tools of natural science \textit{and} socio-cultural approaches to the study of the diseased body.

\textbf{IV. Classifying the eating disordered body}

The scope of my investigation is limited to a specific class of complex pathologies, namely anorexia and bulimia. While eating disorders are by their very nature difficult to define and categorize, we nonetheless require at least a working definition. A comprehensive tracing of all of the various characterizations employed in clinical and non-clinical literature on anorexia and bulimia would be a task beyond the limits of this project, and, so, will not be carried out here. Instead, I will begin with the most relied-upon definitions of anorexia and bulimia (based on their DSM-IV diagnostic criteria) before moving on to an alternative method of defining these pathologies. As we shall see,\textsuperscript{24}

\textsuperscript{24} As will be clear with the substantive discussions to come, “naturalist” (and its grammatical cognates) is not meant to denote a particular kind of philosophical naturalism; rather, it is being used as interchangeable with “scientific” and its cognates.
the DSM-IV diagnostic criteria, by their very nature, define the disease by its symptomatic effects. While there is a great deal of disagreement about the DSM-IV criteria, they broadly capture the medical profile of the disease in that anyone who studies these diseases is on board with at least a significant part of the DSM-IV criteria. More serious disagreements arise between naturalists and social constructivists regarding a causal account of anorexia and bulimia. The discrepancies between causal (or etiological) definitions of the disease are striking, radically divergent, and at times contradictory.

**IV.i. Medico-scientific diagnostic criteria: The DSM-IV**

The fourth edition of the American Psychiatric Association’s Diagnostic and Statistical Manual of Mental Disorders (hereafter DSM-IV) and the tenth edition of the International Classification of Disease (ICD-10) offer what are taken to be authoritative diagnostic criteria for both anorexia and bulimia, utilized by those working in psychology, psychiatry, and medicine. Because I am only selectively surveying various definitions of anorexia and bulimia, the only classification system I will make reference to here is the DSM-IV. According to the DSM-IV “[t]he Eating Disorders are characterized by severe disturbances in eating behavior.”25 A diagnosis of anorexia is present when the following criteria are met: (a) an individual refuses to maintain a proper body weight, or does not gain weight during a period of growth; (b) an individual has an intense fear of gaining weight; (c) an individual has a distorted perception of his or her body; (d) amenorrhoea in women. Anorexia is further divided into two-subtypes: a restricting type and a binge-purge type. Those diagnosed with the restricting type do not regularly engage in purging behaviour while those with the binge-purge type do regularly engage in this kind of

behaviour. The second sub-type is generally understood as more closely related to bulimia.

According to the criteria outlined in the DSM-IV, bulimia is present when the following conditions are met: (a) episodes of binge eating that happen repeatedly (binges must occur within a set amount of time, and involve a feeling of losing control of eating during that period; (b) following-up episodes of binge eating with inappropriate compensatory behaviour, e.g. vomiting, laxatives, fasting, exercising etc., to avoid weight gain; (c) episodes of binging followed by inappropriate compensatory behaviour happen at minimum twice per week for three months; (d) body shape and weight have undue influence on self evaluation; (e) behaviours must happen outside of periods of anorexia. Like anorexia, bulimia can be further sub-divided. The purging type classifies individuals that regularly rely on self-induced vomiting or misuse of laxatives, diuretics, etc. The nonpurging type classifies individuals that do not regularly engage in these behaviours, engaging instead in other compensatory behaviours such as exercise, or fasting, for example.

**IV.ii. Thinking through the medico-scientific characterization**

The classifications for eating disorders in the DSM-IV include the criteria outlined above, as well as noting other features of the disorder, for example, association with other disorders and medical conditions, features relating to age, gender, course of the disease, prevalence and family relations. These features are mentioned, however, not well explained or explored. For example, there is no mention of the gendered nature of either anorexia or bulimia, apart from the report that cases of eating disorders are
overwhelmingly found in female patients (although cases of male sufferers of anorexia and bulimia are increasing) and the inclusion of the gendered symptom of amenorrhoea in the case of anorexia. As this example illustrates, the prevailing medical characterization of anorexia and bulimia found in the DSM-IV is clearly limited in certain respects, if we are, as I am, interested in capturing both the social profile of this disease, as well as the medical-scientific profile. In other words: the established diagnostic criteria in the DSM-IV are incomplete, and, thus, cannot provide us with a robust enough characterization of either anorexia or bulimia to reflect the complexity of this pathology.

Outlining the diagnostic criteria and classifications of anorexia and bulimia as separate disorders, the DSM-IV draws our attention to some similarities and differences between the two disorders. It also categorizes a third type of eating disorder known as Eating Disorders Not Otherwise Specified which acts as an alternative diagnosis for those who suffer from an eating disorder but fail to meet all of the diagnostic criteria for anorexia or bulimia. The fact that both anorexia and bulimia are most prevalent in females in industrialized countries, involve pre-occupation with body weight or size and that some studies show there to be familial pattern of the disease among first-degree biological relatives, are examples of similarities. Whereas, the fact that the first criterion of diagnosing anorexia is that the patient refuses to maintain a normal body weight while those diagnosed with bulimia are generally maintaining a normal weight, or differing physiological consequences of the disease, are examples of differences. Drawing such distinctions is important for certain purposes, e.g. those wishing to investigate genetic

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contributors to the development of eating disorders will likely find such a distinction important.

Characterizing anorexia and bulimia based on such a rigidly defined and incomplete set of criteria poses a number of problems for both the theorist and the medical professional. Collier and Treasure remind us that diagnosing eating disorders can be difficult, since individuals often oscillate between anorexia and bulimia. What furthers this difficulty is the fact that risk factors between the two are often similar. This leads us to question the precision with which the DSM-IV criteria can individuate between the two diseases without a great deal of judgment on behalf of individual practitioners entering into the fray. This is one particularly interesting problem posed by a focus on anorexia and bulimia, and eating disorders not otherwise specified, because the criteria of each disease very rarely manifest in a uniform way without a great deal of cross-over. However, the indeterminacy of eating disorders is only one part of what we mean by their being “complex.” For conceptual clarity we can call such complexity “symptomatic complexity”: we very rarely see the effects of anorexia, bulimia and eating disorders not otherwise specified without some question arising as to just what disease it is we are seeing.

Another kind of complexity, call it “causal complexity” or “etiological complexity,” is more interesting. The common cold shows no such complexity, and by using it as an example, we can highlight what we mean. A proximal cause of the common cold is the cold virus. Something interesting could be said of its distal causes, for example, colds may spread more quickly in a society with contact greeting practices, or a society that calls for increased interaction amongst its members, but nothing interesting

can be said about the social or cultural profile of the proximal cause. All rational persons in a properly educated society will agree on the scientific-medical characterization of the proximal cause, and likely, scientists would agree with the sociological or otherwise social-scientific account of the proximal cause. For such a disease we see a kind of coordination of intellectual explanation: scientists explain the unproblematically medico-scientific account of the disease and sociologists and cultural theorists discuss the unproblematically socio-cultural account of the disease. There is no significant disciplinary conflict.

Diseases which are etiologically complex are quite different. This kind of complexity is defined by the fact that the proximal cause of the disease is contested. Normally, such diseases are studied as the product of very different forces, and here there is a competition between two kinds of study broadly.

**IV.iii. Naturalism and social constructivism**

Speaking in the broadest possible terms there are two categories of disciplinary study, each with a very different perspective on the nature of the ultimate cause of complex disorders. The first of these families, as we have already seen, is naturalism. Naturalism is the view that illness can be given a completely biological, or biochemical description, and that the causes of illness are discernable by our best science. This sort of approach is interested in only dealing with causal explanations that make reference to the terms of science exclusively, and the further methodological commitment that all claims about the cause of disease be verifiable by some accepted scientific method. While there is a tendency for the naturalist to give a nod to environmental factors, such as socio-cultural
and gendered influences that contribute to the development and/or onset of eating
disorders, the naturalist’s investigation into the social or cultural profile of the disease
stops here. Alongside the complicated biological, genetic or neurological vocabularies
employed in such studies, we notice a lack of sophistication when it comes to integrating
cultural theory.

One example of such naturalist commitments is found in a study on bulimia
nervosa presented by Cynthia Bulik et al. This study asserts that bulimia nervosa is
strongly familial, and that additive genetic effects contribute to the familiarity observed
when studying families with at least two biological relatives that are affected by bulimia
to the DSM-IV criteria. Studies cited by Bulik et al. (including Kendler et al. 1991, Bulik et al. 1999, Wade et al. 1999, and Kortegaard et al. 2001) have estimated the heritability of bulimia to be 54-83%. The study attempts to
isolate in the genome a locus for the disease. It establishes a genome-wide significant
linkage between sample families with bulimia and families who show an especial
elevation in vomiting behaviour.

The particularities of the research are not our concern here. The study is
mentioned only because it exhibits several characteristics of mainstream scientific studies
of eating disorders. (1) It starts off with a gesture towards socio-cultural studies, but
nothing more: “Although eating disorders have been considered to be largely
sociocultural in origin, a substantial body of literature has now shown that BN (as well as
the related eating disorders anorexia nervosa [AN] and eating disorder not otherwise
specified [ED-NOS]) is strongly familial… and that this familiality is due largely to the
...additive effects of genes...”

(2) It attempts to give a precise scientific locus for the disorder (the study finds a susceptibility locus for bulimia nervosa on chromosome 10p and a suggestive linkage on chromosome 14q). (3) It avails itself only of the DSM-IV criteria for the diseases. (4) It avails itself of methods conducive to such a scientific study, such as genome scanning via blood tests, statistical analysis regarding the frequency of vomiting, and subject reports regarding their medical history. Also, (5) the findings of this report are correlated with other reports showing significant linkage between chromosome 10p and obesity, alcoholism, schizophrenia, bi-polar disorder, and type I diabetes in females. Further, it is implied that these findings show that the putative socio-cultural origins of eating disorders are incorrectly identifying the cause of the disease.

The second family of disciplinary study is social constructivism. Social constructivism is the view that we cannot understand a number of illnesses (or their causes) without situating them in a cultural milieu. This view can be cashed out in a number of different ways, from the rather broad notion that disease cannot be understood without some socio-cultural context, to the idea that disease is a construction with a particular historical lineage (as in the case of Foucault’s genealogies or archaeologies), or much stronger claims about the symbolic significance of disease, which can only be discerned by an understanding of the social and cultural dimensions within which signs get their significance.

Social constructivisms of the first kind are not particularly interesting, or at all contentious. I wish to draw our attention here to a representative example of the second variety of social constructivist views. In her book, *The Social Construction of Anorexia*

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Nervosa, Julie Hepworth argues, “…the dominant psychiatric definition of anorexia nervosa is socially constructed through discourse or, in other words, regularly occurring systems of language.”³⁰ She explores the way anorexia emerged as an object of medical science in the nineteenth century, and moreover, the ways in which explanations of anorexia remained consistent with the way women, science and medicine were already understood.³¹ This application of Foucault’s genealogical approach is informative for the way it is able to contextualize disease and show the ways in which its study, and medical mobilization against it, are historical, evolving within a collection of disciplinary discourses and institutional power.

On this view a disease cannot be understood apart from the social and linguistic practices that make it into, and sustain it as, an object of medical science. We should notice that such views make a very strong claim. The methods of psychology, including the representations employed therein, and the notion of truth it utilizes, are explanatorily insufficient. The claim is not that there is no such thing as scientific truth, or scientific method, only that both of these notions are historically situated. They have a genesis and a development which they themselves are unable to study.

This view has become entrenched as a key tenet of social constructivism. The tools and concepts that science uses are not universally applicable. Here we can see a conflict with naturalism, which, we can recall, is the view that all explanations are scientific explanations. Social constructivists tell us that science can’t be the whole story and scientists, at least naturalistic ones, claim that any explanation worth giving is a scientific one. As we shall see in the next section below, however, this conflict is not the

consequence of dogmatism on each side, but the product of two very natural ways of understanding the tension just surveyed.\footnote{32}{In the last decade and a half social constructivists who study the body have tended to adopt one or another framework in embodiment theory. I will discuss embodiment theory in more detail in the next chapter, especially through Drew Leder’s adaptation of Merleau-Ponty’s embodiment theory. For feminists, hybrid views, i.e. ones that amalgamate a discursive approach like Butler’s with an embodiment theory such as Merleau-Ponty’s, have become very popular. Some examples of this kind of approach include, Dorothea Olkowski (2012); Dorothea Olkowski and Gail Weiss (eds.) (2006), especially contributions in this piece from Judith Butler, Beata Stawarska and Johanna Oksala; and Vicky Kirby (1997). What we might consider classical social constructivist have remained, see especially Emily Martin (2007) surveyed above.}

\textit{V. Naturalism and social constructivism: A seemingly insurmountable gap}

I will argue that the tension between the naturalist and the social constructivist when it comes to characterizing eating disorders, as well as other complex disorders, is a naturally arising one. It is reasonable to think, as the naturalist does, that if an object of study is important for science, then there is no reason why it shouldn’t be studied scientifically. Problems arise however when such a view manifests in \textit{scientism}. The concepts of biology, for example, are not going to prove the best tools for a biologist to investigate the rich and complex \textit{history} of anorexia. To engage in such a study will require engaging with theories and vocabularies we find in, for example, sociology.

It is also natural and reasonable, based on very similar reasons, for social constructivists to think the way they do. Their inquiries into the objects of science often require investigating the various questions about which working scientists publish, or present at conferences. Social constructivists however also engage with historical examples of a more egregious scientism, such as biological narratives employed to motivate policies against gay marriage, or institutions and practices that marginalize
women, racial minorities, and in general, people who don’t fit well into the abstract categories that science often relies on for pragmatic purposes.

This dichotomous way of theorizing leaves us at a conceptual impasse. Naturalists, because of the restricted ontology over which their explanations range, fail to see the social profile of diseases (or underestimate the significance of such a profile), and social constructivists, because they limit their focus to the various gendered, social and medico-scientific discourses shaping disease, never actually engage with the physical body. Because feminists share some of the same concerns with the limitations of science and the prospect of scientism, especially when it comes to issues like biological determinism, they’ve largely adopted the approaches of the social constructivists, a methodological commitment which has dominated feminist discourse since the 1990s.

**V.i. A feminist concern about the gap**

Feminists have remained relatively silent on the gap we find between the naturalist and the social constructivist camps. By this we mean that the gap itself has not been a topic of much feminist theorizing in the arts and social sciences, though feminists working in the academy tend to reproduce the gap by adopting social constructivist perspectives when studying the body. Three reasons can be given for constructivism’s dominance in feminist studies. Firstly, as per our introductory remarks at the beginning of this chapter, feminists concerned with health have long defined their work as a way to illuminate and meliorate the mistakes in traditional medical practices and institutions. Feminists are critical, so their focus is on those parts of scientific theory, institutions or practices that have lent themselves to political oppression and recapitulation of damaging narratives about
women’s bodies. For example, early characterizations of anorexia in the nineteenth century characterized the disorder as closely related to hysteria in that it reflected what was considered irrational, deviant, and abnormal female behaviour.\textsuperscript{33} Because feminism is a critical philosophy, it is not surprising that it would be very wary of basing itself upon the instrumental rationality\textsuperscript{34} associated with the sciences; keeping feminist studies at a distance from this kind of instrumental rationality has, in some way, defined most of the feminist literature on eating disorders.

Secondly, feminist critiques of the sciences have often been tied to political goals. Feminists have sought to reform the law and governmental policy regarding health care concerns, or, more broadly, the socio-cultural context in which such questions begin to make sense. Because of the interlocutors adopted, much feminist work is politico-juridical and normative in character, not scientific.

Thirdly, the methodological turn towards social constructivism allowed for the body to be reconceived as the product of forces much better known to the feminist critic. Rather than viewing the body as skin, flesh, blood and bone, the body can be seen as a complex semiological system of significance produced seamlessly by interwoven discourse. Biology (and its cousin disciplines) became just one more discourse, but more often than not, one that had to be overcome. In short: we find that it is built into feminist methodology to be wary of a scientific approach to, what are assumed to be, problems better dealt with by cultural theory, philosophy, sociology, etc.

\textsuperscript{33} Ibid, p. 29.
\textsuperscript{34} I use the term “instrumental rationality” with its typical meaning in Critical Social Theory. Instrumental rationality involves reasoning towards one’s goals once those goals have been established. There is also “critical rationality”, or the rational deliberation regarding one’s goals.
VI. Bridge Building: a methodological rather than a substantive solution

So far we’ve begun to navigate the multifaceted nature of complex disorders, in particular eating disorders. Complex disorders—understood as those disorders which manifest in both physiological and socio-cultural phenomena—are particularly interesting to investigate because they highlight a number of philosophical problems that have been of importance to feminists (and others) engaging in studies of the body. Narrowing the focus of the present study to a special class of complex disorders, namely eating disorders, has allowed us to begin to see the way differing disciplinary vocabularies and ontological commitments come to bear on our (mis)understanding of the disorders. We have begun to illustrate the current terrain on which feminist philosophers interested in studying complex disorders must tread.

Let us now return to our previous question, which asked whether or not feminists could begin to address the essentially interdisciplinary character of complex disorders. I argue that although the inhospitable nature of the terrain of the debate does seem to pose some obstacles for the feminist theorist, it also opens up new possibilities for developing more holistic and accessible theoretical frameworks for studying eating disorders. Opening up this conceptual space, however, is not a matter of choosing to favour either a naturalist or a constructivist approach, and neither is it a matter of trying to combine the two ecumenically, as if an easy synthesis is in the offering. Instead, a reorientation of the way we do studies of the body, especially the eating disordered body, is required. Such a reorientation requires most importantly that we focus on bridge building so that our views might be heard, and perhaps influence, the wider medical and scientific communities.
Feminists studying eating disorders and the eating disordered body need to speak to (and with) those working in the empirical sciences.

The strategy I am proposing offers a methodological rather than a substantive solution to the problem. The integrated methodology I am developing is a form of critical empiricism. This methodology requires that two desiderata be satisfied. Firstly, we need to engage directly with scientists and the institutions of science. Such a move requires, as I have stated above, that feminists learn to communicate with the sciences using the terms of the sciences. One may ask if this desideratum yields too much already to the naturalist. Such a response is confused, however, if it assumes that the vocabulary of the scientists is adopted along with the naturalistic assumptions of the scientists who normally use such language. The vocabularies and models of the sciences can be critically adopted without assuming the further, and erroneous, idea that such vocabularies are the Trojan Horse within which scientism dwells. This brings us to our second desideratum: we need to ensure that we don’t take the data offered by empirical science as already (and unproblematically) interpreted.

Satisfying these two desiderata has a number of positive consequences. Firstly, the feminist is in a position to communicate with the very people whose practices she wishes to critically assess. Secondly, her mastery (or at least familiarity) with the ways of the empirical sciences will provide her critiques with a legitimacy or authenticity often perceived to be lacking in feminist philosophy of science. Most significantly, though, a satisfaction of the two desiderata will put the feminist into a very interesting position. Because of her knowledge of how to read and interpret the data, she can introduce a nuanced form of empiricism that eschews the notion that data are evidence in
themselves\textsuperscript{35} (a notion all too prevalent amongst those working in the sciences) as well as the notion that the procedures of data collection in the sciences (necessarily) preclude any political motivation, assuming the mythical disinterested seeker of knowledge (an antiquated notion of how science operates that is all too common in the feminist literature). She can show the scientist, and her feminist allies, that as long as we are explicit about our goals and expectations, there is no reason why interpretation (the application of a particular theoretical framework) can’t be politically motivated. For example, a feminist might utilize a theory about gender that eschews binary gender categories and adopts a spectral view of gender when interpreting data regarding, for example, the prevalence of eating disorders. This goes beyond the fairly mundane observation that, say, a genetic account of anorexia nervosa has political or at least socio-medical implications. The feminist can do more than just recognize the impact of the sciences; she can be engaged in the very processes of interpretation that give character to the evidence that has implications for how we view eating disorders (and other complex disorders). The consequences and philosophical significance of this methodological approach will be addressed more fully in Chapter Four; in Chapter Three and Four, I will also offer a more precise account of the kind of scientific knowledge expected of the feminist epistemic community.

\textsuperscript{35} On this view “data” are the uninterpreted results of a scientific study, perhaps the data collected by blinded practitioners unaware of the hypothesis or theory governing the study. Evidence is data interpreted by a theory.
CHAPTER II. THEORIZING THE EATING DISORDERED BODY:
An integrated approach to understanding the self

I. Introduction

In the preceding chapter we explored the multifaceted nature of eating disorders, and the current terrain on which researchers and medical practitioners must tread. Our analysis of natural and cultural characterizations of eating disorders revealed the complexity of such disorders. Moreover, our forays into both the naturalist camp and the social constructivist camp made clear that there are very real tensions between the two ways of characterizing the body, especially when the body is diseased. I argued that these tensions ought not be eliminatively resolved by reducing one approach to the other; rather, these tensions ought to be embraced for their ability to contribute to a more holistic and robust understanding of eating disorders. Feminist research, as I point out, has primarily been conducted from within a social constructivist paradigm, and has remained relatively silent on the “gap” between a naturalist approach and a constructivist one. Failing to properly engage with the natural profile of eating disorders, however, has left feminists with limited access to those working from within a naturalist paradigm. (The same behaviour from the naturalist camp has meant similar paucities in research.) I argued for a methodological solution that requires feminists (and others interested in developing a holistic approach to eating disorders) to engage critically with the sciences. The outlines of this approach were given in the previous chapter, and a more detailed account of the methodology I am working towards will be given in the fourth chapter. However, we should keep in mind the key tenets of this methodological approach that have already been glossed. Firstly, as already stated in this introductory section, the methodological approach I espouse is not a
reductive one. Social constructivists’ work cannot be, and should not be, reduced to, or replaced by, work in biology or neuroscience, nor should the body be eliminated as a site for biological happenings, i.e. reduced to the sort of entity more amenable to social constructivism. Secondly, and just as importantly, the methodological approach favoured in this work is not ecumenical. A full synthesis between the naturalist and social constructivist approaches is neither desired, nor a realistic possibility. If it were, this work would be focused on finding this or that “hybrid” theory, making a methodological approach otiose.

I do not deny that substantial theoretical syntheses have already been made regarding the study of the body in its great complexity. Some accounts of the body already in the philosophical literature have attempted to bridge the gap between the sciences (and their modes of study for the body) on the one hand, and other kinds of analysis that attempt to take into account the subjective character of lived experience. The most prominent theoretical efforts to bridge the naturalist-constructivist divide, phenomenologically oriented embodiment theory, attempts to dispel the view that studies of the body must be done from a purely “mechanical” or scientific understanding of the body. Embodiment theory involves drawing directly on the “lived” experiences of the eating disorder sufferer, and is relied upon as a way of accessing first-person accounts. Therefore, embodiment does not directly give us answers to the questions asked in the last chapter, but it does offer us a template for how, and how not, to go about methodologically coordinating radically different research agendas relating to the (diseased) body. With embodiment theory the issues are recast, challenging the discursive practices of both the naturalist and the social constructivist by incorporating the way the
disease is understood and experienced by the sufferer. Such data, embodiment theorists argue, is indispensible for those wanting to negotiate such complex disorders. Moreover, taking the experiences of the sufferer as relevant and important allows for the sufferer to have agency in a way that is precluded by both the naturalist and the social constructivist. Incorporating first-person accounts and experiences gives us access into the way an individual with an eating disorder moves through her world, sometimes challenging the very narratives and discursive practices that the constructivist or the naturalist takes as constitutive of her disorder. It is primarily for this reason that we explore embodiment theory here.

Embodiment also provides for us a negative example of how we should “read” first-person reports of eating disorders. Because the lived experience of the subject is so significant for embodiment theorists, they must offer an account (an embodied phenomenological account) of how such expressions or assertions are to be given meaning. Normally, the meaning given to such narratives is the one associated with the private contents of the subjects’ experiences. But problems abound for this kind of subjectivist “interpretation” (or lack thereof) of the felt experiences of subjects. The un-interpreted first-person reports in phenomenologically oriented embodiment theory are fundamentally similar to those found in autoethnography (the tendency of ethnographers to take eating disorder narratives at face value), which makes little sense in light of what cultural theorists have said about the disease, and also does an injustice to the scientific method, especially where the subjective character of experience runs contrary to all of the scientific data. One can agree with the embodiment theorist (and the ethnographer) that these narratives are of utmost importance, but only when we recognize how they are to be
understood and how they relate to a scientific understanding of the body, and this requires, as it will turn out, both cultural theoretic and naturalist sensitivities.

This means that although first person reports must be given attention, so too must the biological data we cull from the profile of an eating disorder patient. These data may fail to correspond as we may suspect they should; we must be cognizant that such reports may, in some instances, run contrary to the physiological happenings in the body. If the patient denies that she has an eating disorder at all, claiming rather that she chooses to live her life and manage her diet and exercise regimes in a certain way (perhaps referring to these regimes as constituting a particular way of life), then we must weigh the report against the physiological realities of her body and entertain the possibility that she has internalized certain cultural narratives about thinness, and is unable to recognize their influence on her body and self.

In this chapter, I will begin by surveying a general Cartesian-mechanistic account of the body and explain how it influences our understanding of the medical sciences. Next, I will examine embodied accounts of the body in the medical sciences, and how they problematize the Cartesian-mechanist approach. I will then trace the way the body has been theorized in these two competing frameworks in an effort to highlight the weaknesses with both the Cartesian account of the body-as-machine and the embodied view proposed by Drew Leder, which he calls (following Heidegger) the ‘lived body.’ \(^{36}\) I will argue that while Leder’s lived body is successful in solving some of the most immediate problems with a Cartesian/medical approach to the study of the body it remains an unsatisfactory framework. The embodied subject we get from Leder’s ‘lived body’ approach is, problematically, unable to account for the cultural markings that enter

into the constitution of identity. Moreover, the assumption that the first person “reports” or “lived experiences” of the embodied subjects provide more conclusive, or reliable data, than that which we find in cultural studies or naturalistic studies of the body is problematic, and must be carefully avoided by anyone pursuing this line of inquiry.

This assumption is also at the heart of the “autophenomenological” or “autoethnographical” approach to the study of eating disorders (even when such an approach makes no specific commitments to an embodiment thesis). I will not argue against the importance of such narratives, only the assumption about the authoritative access of the subject to her own phenomenological states. What I offer is a cultural critique of both embodiment theory and autophenomenology. When we take uninterpreted first person reports as authoritative and argue that one cannot be misguided or misinformed about her experiences we are simply reiterating a form of Cartesianism. To avoid an error of this sort, I argue that cultural discourse must accompany personal narrative. Here I will draw on Paula Saukko’s successful inclusion of first person narratives as an example of how such accounts might be included in a theoretical approach. I will draw on Saukko’s work to argue that while an autoethnographical approach (or, as we shall see amounts to the same thing, an autophenomenological approach) is able to make visible the experiences of those suffering from eating disorders by calling various cultural and medical narratives into question, taking first person reports of experiences with eating disorders as authoritative continues to provide us with an incomplete picture. Saukko offers a hybrid cultural-phenomenological account that is successful in arguing that personal experience often runs contrary to cultural narratives, however, such an approach is unable to answer the more important question concerning
the self and its relationship to the body or to cultural norms. I draw on Ian Hacking’s Foucauldian account of the relationship between the self and social norms to illustrate a more robust, non-Cartesian account.

Next, I turn to a more detailed theoretical account of the problems with autophenomenology and outline what I take to be one way of preserving the utility of first person accounts in studies of complex disorders. To this end I draw a distinction between autophenomenology and Daniel Dennett’s account of heterophenomenology. Following Dennett, I argue that taking first person accounts as evidence is important to any proper methodology which wants to study the self, but first person reports must be properly interpreted.

Lastly, I will argue, following Elizabeth Wilson, that there is a way to reorient studies of the body such that we can include both the psyche and soma in our accounts in a way that does not deny the commitments of either practicing scientists or cultural theorists. Wilson is also able to maintain political commitments necessary for feminist work on the body that Dennett’s heterophenomenological framework is silent upon. In short: I contend that Wilson’s framework to the study of the body is better equipped to theorize the eating disordered body than phenomenologically oriented embodiment theory, autophenomenology, and heterophenomenology.

II. The body in medical discourse: The Cartesian and the “lived” body

In this section I will address two different paradigms for understanding the body in medical discourse: the Cartesian/medical body, and the ‘lived body.’ I will begin with some introductory remarks to show the stark differences between these two paradigms
before turning to more nuanced and detailed discussions of both in subsections II.i and II.ii.

Phenomenologically oriented embodiment theorists, following Merleau-Ponty, argue that the mind and body—self and extension in the world—are intimately intertwined. They challenge Descartes’ characterization of the body as machine—as substantively and dichotomously separable from human consciousness. The sciences, especially the medical sciences, are still often conducted within this Cartesian paradigm.

An alternative model to the Cartesian paradigm, the ‘lived body,’ has been developed to destabilize this paradigm. The ‘lived body’ model moves beyond dualistic terms that describe the mind, soul or consciousness as always in opposition to the body and bodily events. The ‘lived body’ is in marked contrast with the Cartesian mechanist view in the sense that one’s behaviour is not best described in purely mechanical terms. It is also in marked contrast with the other side of the Cartesian dualist coin: the purely phenomenal and subjective view of the self as soul/mind/consciousness, with at least some mental states being completely separable from embodiment. The ‘lived body’ is an attempt to avoid reducing the self to one of these Cartesian extremes, and to recognize that the body, while it admits of purely mechanical description, is best understood as an integrated intentional system with bodily and appetitive urges towards the sorts of intentional objects which populate the subject’s experience. Furthermore, experience (and mentality in general) as intimately intertwined with the body, its makeup, and its movements in the world.

37 Ibid, p. 24. As Drew Leder explains in his piece, “A Tale of Two Bodies,” “…the lived body is an ‘intending’ entity…. bound up with, and directed toward, an experienced world. It is a being in relationship to that which is other: other people, other things, an environment. Moreover, in a significant sense, the lived body helps to constitute this world-as experienced.” (p. 25)
I want to survey both the Cartesian body and the lived body in the next two subsections. It is my contention that both views are unsatisfactory, and that they do not exhaust the frameworks within which we may view the medical body. These surveys will provide us with an account of the shortcomings of both views.

II.i. The Cartesian body and Leder’s critique

Since Descartes, the modern medical sciences have largely drawn a sharp distinction between mind and body. This binary and hierarchical division structures current medical discourses and practices. This distinction serves several purposes: firstly, it provides the medical practitioner a domain over which her expertise may be exercised with authority, viz. the body. Secondly, the bifurcation simplifies medical practices, for a mindless body is fully ready to be studied by the scientific method and cured by completely mechanical (or mechanical-chemical, etc.) means. It is, properly speaking, a scientific object: the object of the medical sciences. Thirdly, it allows us to integrate discussions of the body with discussions of other matters in scientific inquiry. By this I mean to say that a mindless body produces no particular difficulties for a unified science of all physical things, beginning with our most primitive physics and ending with specialized scientific studies of a higher-order. In other words, a mindless medical body is one that is reducible, at least in principle, to neural, chemical or even physical analysis.

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38 Descartes first makes the distinction between mind/soul and body Parts IV and V of his Discourse on Method (1637), but the distinction gets its most clear expression in the Second and Sixth Meditations of his Meditations on First Philosophy, where bodies are defined by their essence (extension in space), and minds by their essence (thought, or the act of thinking).

39 Of course, one can arrive at a mechanical understanding of the body even if they don’t assume dualism by assuming a purely materialistic understanding of mind. However, this was not the way that the mechanical body became prominent in the tradition of modern natural science. It was not a Hobbesian, but rather a Cartesian route we took to mechanism.
The Cartesian body emerges out of a mechanistic worldview. This view is one of many mechanistic views of nature and the body that were prevalent in the seventeenth century. On such a view, nature is but extension in space of entities (atoms, corpuscles, or what have you) which enter into mechanical processes. From the Ancients to the Medieval period, concepts of natural philosophy were dominated by the Aristotelian idea that the world is comprised of hylomorphic and continuous elemental wholes. This means that when we encounter an object with an essential nature, for example a plant or a human being or a rock, there is no smallest bits which are (a) indivisible, and (b) do not contain the constituent element from which the whole is composed. In other words: nothing like the modern idea of the atom or the corpuscle is countenanced in the Aristotelian tradition.

The seventeenth century brands of mechanical philosophy, turning their back on Scholastic accounts of nature in the pursuit of a lawful (as opposed to merely normative) description of physical processes (from the small and local to the celestial), became the dominant paradigm in western thought, with deep influence on the way the body is regarded in modern anatomy and medicine. In fact, the development of anatomy in the modern period cannot be divorced from its mechanistic assumptions. The body is the sum of its various parts with each part working independently, engaging in the process that it is suited for and which is necessary for the proper functioning of the body as a whole. Against the backdrop of this context, the body works like a tool. The body is nothing but dumb, inanimate matter obeying mechanistic principles, or, when animated by a soul, the whims of a conscious, non-mechanical ego, entirely different in substance from the body.

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40 See Aristotle’s *Physics*, 194b23-24. This was the dominant view, with the exception of ancient atomists such as Democritus, Epicurus, and Lucretius, from ancient Greece to the birth of modern mechanism.
In fact, Descartes makes no distinction between material systems operating by mechanical law and (non-ensouled\textsuperscript{41}) living systems. Thus, for Descartes, the study of the body, the study of anatomy, was on a par with the study of other mechanical phenomena, e.g. the study of planetary movement as explained by vortices in celestial fluid. An integrated physical picture had been proposed, and the body took no special place in such an account. Thus, medical science, as a product of its being a mechanical, anatomical study, has established itself as a science through the neglect of any non-mechanistic attributions to the body – e.g. the sorts of characteristics given to the body in phenomenologically-oriented embodiment theory.

Leder describes the impact of the Cartesian paradigm on modern medical practice:

“Modern medicine is profoundly Cartesian in spirit. As such, this notion of embodiment, one in which the non-living takes primacy over the living, has extensively shaped medical theory and practice.”\textsuperscript{42} An understanding of the body that operates under this pretense underscores our treatment and management of health and disease in a number of important ways.

Beginning in the eighteenth century, this view fostered an increase in pathologization and classification of disease.\textsuperscript{43} That is, the body became a vehicle for knowledge: an anomalous piece of matter ready at hand for exploring and dominating. Science works its magic best on this kind of subject, and the medical sciences had to follow suit. Leder explains that this kind of characterization of the body is even more pronounced in the context of nineteenth century technologies. “Such technologies as the

\textsuperscript{41} Descartes held that ensouled living systems would have one tell-tale mechanical sign that set them apart from non-ensouled systems: langauge. (See Part V, Discourse on Method.)

\textsuperscript{42} D. Leder (1992), p. 21.

\textsuperscript{43} Ibid, p. 21.
stethoscope, the blood test, the X-ray, allow a kind of dissection of the living body, analyzing it into its component parts, exposing what life ordinarily conceals.”⁴⁴ In essence, modern medicine relegates the patient to the status of a passive corpse. In this framework the body is entirely free of context. Leder argues that this “machine-model” of medicine necessarily results in “mechanistic forms of treatment.”⁴⁵ Ultimately, modern medicine has figured out how to successfully treat the body as a machine.

Leder maintains that the most commonly noted consequence of this treatment involves a failure to consider psycho-social factors, most notably the patient’s own experiences of her body through time – her own subjectivity. He argues:

Not as widely recognized are the metaphysical roots of this neglect. Insofar as the body is modeled upon a lifeless machine, the role of subjective experience in determining one’s health history will tend to be overlooked.⁴⁶

In short: the machine model fails to consider the role of subjective experience in illness.

Leder explains that this pronounced distinction between mind and body, between subjectivity and objectivity, “demands an almost schizophrenic shift between, at one moment, examining the machine-body, and at the next, acknowledging the person to whom it belongs.”⁴⁷ This must be the case, for the bifurcation has structured medical practice in such a way that it can only recognize as medical the unlived body. All attributions of personhood require a shift from this medical perspective.⁴⁸

⁴⁴ Ibid, p. 22.
⁴⁵ Ibid, p. 22.
⁴⁶ Ibid, p. 23.
⁴⁷ Ibid, p. 24
⁴⁸ One can see this shift between body-talk and mind-talk more clearly if one looks at how philosophers have attempted to maintain the mind/body distinction under the moniker “non-reductive physicalism”. These views are quite common, but I survey here only one. Donald Davidson’s “anomalous monism” holds that mind and body are identified, but that the sciences do not, actually can not, avail themselves of the predicates of the “psychological vocabulary.” Likewise, when we engage in folk-psychological discourse, i.e. when we talk about the mind qua mind, we can do so only from the holistic conceptual realm of beliefs, desires and intentions. To offer a physical explanation of a mental event would be utterly mystifying. We
Leder’s point is that medical bodies are integrated in ways that cannot be accommodated by the binary Cartesian model. When one is in pain, or one is ill, it is not simply a body that is mechanically malfunctioning. In fact, from the mechanical point of view, it is hard to see how one could even develop a standard of “proper function,” since all function is equally mechanical (i.e. equally obeys the laws of mechanics). That we say a patient is ill presupposes some non-mechanical reference-frame, presupposes a subjective experience. Without it, we would have no way of individuating between differing mechanical states of affairs in a way that mapped onto categories of health or illness. If we restrict ourselves to a purely mechanical understanding of the world, including medical bodies, then it is hard to see how one could even manage to distinguish in the mechanical phenomena objects or aggregations that are “healthy” or “sick,” since neither term refers to mechanical criteria. Given that these observations are correct, the Cartesian model points to its own limitations and shortcomings. As to how the model may be enriched or replaced is still an open question, but we will, in the following section, examine Leder’s proposal.

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must choose one conceptual category or the other, or, at best, switch between them for various practices. See Davidson, “Mental Events” in his Essays on Actions and Events.

49 Notice that this does not presuppose that all illness is experienced, only that we need a framework of subjective experiences from which we can classify physical systems as healthy or unhealthy, well or ill. With a machine one has no trouble detecting functional versus broken-down. Likewise, on a mechanistic view of the body sick and healthy are easily identifiable states, once we have some non-mechanistic framework through which we understand mechanisms. From the point of view of mechanics itself, no systems are sick or healthy. They are merely functioning or not functioning. From a medico-mechanical framework we are able to exploit mechanics to tell us something about the underlying mechanical differences between healthy and sick, but only because medicine offers us a frame of reference from which it even begins to make sense that some mechanical processes are sick and some are healthy. So, sick and healthy are not even concepts of a mechanical philosophy. Within medicine, healthy or unhealthy, well or un-well, are clearly delineable only when they make reference to an agent’s subjectivity. It may be the case that Alice has cancer, but has no subjective experience of cancer. This is acceptable. What is not acceptable is a classification of matter as cancerous without the presupposition that somewhere, at sometime, there was a particular illness that fit these physical characteristics that caused a particular experiential state to be undergone. We need subjective experience insofar as it, and it alone, provides the conceptual framework within which medicine can carve up the world into healthy and unhealthy bits.
II.ii. The “lived” body

Reforming medicine requires changing the conceptual framework within which medical practice occurs. Leder shows us that uprooting Cartesianism is no easy matter. It is deeply entrenched in the way medical institutions operate. The lived body, according to Leder, provides us with a viable alternative. While we shall see that Leder’s phenomenology addresses many of the straightforward problems plaguing the Cartesian analysis just surveyed, we shall see that it too suffers insuperable difficulties as a framework for the study of the medical body.

Ultimately, in this phenomenological framework, the body takes on a new role. “The body is not simply a thing in the world, but an intentional entity which gives rise to a world.” This understanding posits both material and ideational manifestations of the body. The self is not reducible to either one or the other. According to Leder, “[t]he existential account does not replace the biological account, but rather places it within a broader perspective.” The body is rather a product of an entanglement between our being in the world physically, and our subjective interactions within this world. Leder writes: “Just as our physical structure lays the groundwork for our mode of being-in-the-world, so our interactions with this world fold back to reshape our body in ways conducive to health or illness. A medicine of the lived body dwells in this intertwining.”

Notice that the Cartesian hierarchical primacy of the mechanical body has not been replaced by a hierarchical primacy of the subjective (which would, in itself, be a Cartesian conception). Leder’s phenomenological framework for the body is telling us that we can make no sense of such a mind/body distinction, not that medicine (or perhaps

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50 D. Leder (1992), p. 27.
51 Ibid, p. 29.
52 Ibid, p. 29.
Descartes himself has its priorities wrong. There is simply no easy way to draw the bifurcating line between mind and body. Thus, Leder argues that we can take the tools of the medical science, and, through them, “allow the body to speak forth its illness in terms of biological mechanisms and disruptions,” but only if we accompany such mechanical tests with the patient’s story. Thus, the body is not free of context. To understand an illness is to understand not only the mechanical state of the body at a specific time, but how that state fits with an overall biographical narrative of the patient, including “her habits of exercise, diet and substance use, the state of her job or marriage, her emotional proclivities, her modes of dealing with stress.” Leder isn’t denying the possible efficacy of traditional diagnostics and treatments; rather, he is arguing that the Cartesian picture does not explain their efficacy. “To attend to the lived body is not to forsake the tools and learning that Cartesian medicine has provided. It is merely to refuse to grant this mechanical wisdom the status of ruling paradigm.” What Leder is arguing for is what he calls an “existential dimension” to our understanding of the body, one that is “no less important than its physiology.”

So how do we explain the ubiquity of the Cartesian model in the medical sciences? In The Absent Body, Leder makes the case that our bodies, in many ways, are often “absent” when it comes to the way we experience the world. We move through the world without much awareness of our own bodies, and bodily processes (especially those internal processes that we have limited access to), until something breaks down, or ceases to function properly. We tend to only become aware of our bodies, our bodies only

53 Ibid, p. 29.
54 Ibid, p. 29.
55 Ibid, p. 31. (Italicized in original.)
56 Ibid, p. 29.
become “present” to us, when there is something functioning abnormally. Leder argues that Cartesian metaphysics of (in principle) separable body components and mind components is a product of the phenomenological “receding” or “retraction” of the body in consciousness. The Cartesian picture of the body as a tool makes sense because the body seems to play such a role. Like a regularly used tool, the body’s operations seem second nature to us, and for that reason we are rarely forced into a phenomenological awareness of our bodies, much in the same way that we take a pencil for granted until the tip breaks. This is not to say that the body is truly separable from the conscious mind, rather that it is so integral as to become for all intents and purposes phenomenologically invisible. This naturally leads to a downplaying of the epistemological and existential role of the body as a sort of transparent medium, all but unnecessary for the gaining of knowledge. For Descartes himself, of course, the body – its sensorium in particular – posed an immediate obstacle to knowledge that needed to be overcome and subordinated to reason. However, this naïve view does not hold up to scrutiny. As we have already seen, the entanglement of body and mind that gives us the illusion of a Cartesian metaphysics of disembodiment (and the illusion of an unlived, purely corporeal body) forces us to reject this naïve metaphysics in favour of an integrated view. Embodiment theory tells us that the body is not only useful for our negotiations in the world, it grounds us in the world, and without it, subjectivity would be utterly impossible—the Cartesian ego being an untenable and impoverished subjectivity. In short, what makes the Cartesian picture so attractive is exactly what makes it incorrect. We get a Cartesian metaphysics in the course of offering an only incomplete and improper analysis of the relation between
self and world. It is a product of the fallacious inference from apparent disembodiment to disembodiment.

Leder’s treatment of the apparent absence of the body in immediate awareness not only tries to avoid this simple Cartesian mistake, but also tries to give us a more complete and accurate description of how the embodied self negotiates the world. Broadly speaking, Leder’s view is very close to Merleau-Ponty’s, and it is also a supplement of what is missing in Merleau-Ponty’s original analysis. Leder extends Merleau-Ponty’s notion of “flesh” (an embodied extension of consciousness into the world that is also world constituting) to the new notion of “flesh and blood.” Leder thinks such an extension gives us not only an account of how the exosomatic body (the skin and surface musculature) is imperative to consciousness, but also how the viscera, the interior organs and tissue, factor into embodiment. Whereas the flesh can be forgotten, can recede from awareness, the interior, the viscus, actually resists phenomenological appearance.\(^{57}\) This is because we cannot have a conscious perception of such viscera as internal organs, blood, or bone, for example. Visceral processes are simply beyond our personal control in a way that our flesh is not.

But Ingrid Richardson and Carly Harper argue that Leder’s inclusion of the interior is not sufficient. Leder has given us a phenomenology of the interior, but one that exposes an inherent masculine bias based on visibility. Richardson and Harper argue that

\[\ldots\text{women’s bodies regularly transgress Leder’s conceptualisation of the perpetually contained and unrevealed visceral interior: when menstruating, for example, our visceral depths “come to the surface” of our corporeality; when pregnant, inner bodily changes and foetal movements are frequently both focal and visible.}\]^{58}\]

\(^{57}\) D. Leder (1990), p. 54-55.
While the authors are correct to raise this concern, they are wrong to think that this problem is insurmountable for Leder. Leder’s neglect of the female awareness of interiority would only be problematic if he was unable to manipulate the view to accommodate such experiences. While Richardson and Harper are right to say that Leder’s model as it stands “describes a specifically masculine embodiment” it is quite reasonable to think his views can be extended to accommodate menstruation, pregnancy, and menopause. He need only to argue that the viscera of women tend not to resist in the same way as the viscera of men, or simply add to his analysis an account of instances of masculine awareness of viscera, such as hernias, ejaculate, and prostate enlargement. In other words, Leder can either accept the particularities of female bodies and modify his view to account for them, or more plausibly, modify the view to include a new class of phenomena that do not fit neatly into the exosomatic/endosomatic dichotomy, but are surely not indicative of a masculinist framework.

My own criticism of Leder’s theory of body is quite different from Richardson and Harper’s. It is my contention that Leder has more or less accurately described the ways in which body and mind, interior and exterior, enter into an embodied view of the medical subject. However, I do not think that such a view is sufficient. I will argue in what follows that embodiment studies are a necessary component to a proper understanding of the medical subject, but that a view that stops at embodiment misses the mark. The entanglement of mind and body is of utmost importance to our study, however, we may also seek other entanglements, like those that exist between an embodied subject and a culturally marked subject. While it is the case that a mechanical understanding of the body is insufficient because it does not capture subjectivity, it is also
the case that an embodied subject is likewise insufficient because it does not reflect the cultural markings of the body that many take to be constitutive of identity, in fact, a presupposition for the very possibility of subjectivity.

My cultural critique will also be launched against autophenomenology because taking the first person account of the subject in isolation from the cultural context and web of power relations in which she finds herself also gives us an incomplete account. Paula Saukko offers us an insightful approach to addressing this further entanglement in her reflective approach to the cultural and embodied narratives we have thus far explored in our attempt to outline a more comprehensive account of the eating disordered body. It is to Saukko and the autophenomenological approach that we now turn.

**III. Autophenomenology: A way of capturing a multiplicity of voices**

This general move away from Cartesian mechanism (or body-as-physically-deterministic-system) is perhaps most clearly exemplified by current autophenomenological or autoethnographical studies. We are using autophenomenology and autoethnography as more or less synonymous. There is no essential variation between the two terms other than differing disciplinary vocabularies. Both rely on the use of first person accounts in the study of a particular phenomenon, state of affairs or state of being.

Autophenomenology is perhaps best known in the philosophical literature as a continuation of the Cartesian idea that one’s phenomenal states are authoritatively and subjectively known by an act of introspection. The subject is the only one equipped to offer first person accounts of occurrent experience and any knowledge available through

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59 The term autophenomenology is used in philosophical circles, whereas anthropologists often employ the term autoethnography.
introspection is of a special and very private kind. For most in the Cartesian tradition, such knowledge, if construed as the product of direct awareness and introspection, is the only sort of certain knowledge that we can have. This Cartesian model of experiential knowledge is, of course, also a product of Cartesian epistemological foundationalism: all certain knowledge is a product of ideas immediately given to the mind and all other knowledge (even if just probable) gains its epistemic status based on rational links with the immediately given certainty of a sub-class of ideas.

I propose to use autophenomenology in a slightly different way, removed somewhat (though, as we shall see, only somewhat) from the Cartesian roots that anchored philosophical autophenomenology from Descartes to Husserl. A pure phenomenology (comprised solely of first person reports) is very likely a philosophical impossibility. The Husserlian practice of epoché (bracketing off all non-phenomenal content to describe the experience as it is in itself) is not a significant part of any current research program. However, autophenomenology, in a less philosophically rigorous sense, is still extremely popular, especially when studying diseases or aspects of diseases which are thought to rely heavily on the mindset and perceived reality of the subject. Of course anorexia and bulimia are two key examples of diseases that cannot be, as we have already seen, studied without understanding the experiences of those who suffer. Autophenomenology in this sense takes first person narratives of the subject’s naturally interpreted understanding of her own experience (as opposed to the uninterpreted, or bracketed, pure experience of phenomenology). However, much of the Cartesian tradition still holds sway. These reports are considered privileged, the product of introspection/awareness, and are assumed to be authoritatively known. When patients
express or report on how their diseases “feel” or “seem” to them, the question of their accuracy is taken as already answered by the method itself. The patient cannot be thought mistaken about the experiential impact of his or her disease. We can call such a methodology a “Cartesian legacy,” and acknowledge that autophenomenology is still, though not in the strictest sense, Cartesian.

Saukko presents us with an example of an autophenomenological account, namely one that is personal and anecdotal, in her book *The Anorexic Self*. Through this narrative she is able to convey the deeply varied nature of women’s experiences with eating disorders. She is able to provide what other constructivist accounts (e.g. Bordo) are unable to provide, namely a characterization of eating disorders that is inclusive – allowing for a multiplicity of voices from the grass roots. The question remains as to how useful such personal narratives may be, and how they may be brought to bear in a structured way (even if only under an idealized conception of such a program).

From one perspective, call it the “phenomenological assumption,” the first person narratives of anorectics and bulimics are not only indispensible for researchers (and often cathartic and therapeutic for the patients themselves), but also definitive. From another perspective, one often taken by feminist cultural theorists, call it the “cultural assumption,” such narratives must be taken with a grain of salt. They may be useful for research (and may certainly still be cathartic and therapeutic for patients), but their use is often restricted to a special “reading” which belies certain underlying cultural factors at work in shaping the self (without such shaping being known, at least transparently and instrospectively, to the patient herself). Some cultural theorists, working in the tradition of post-modernism (e.g. Butler, Bordo), challenge the very notion of the stable and
coherent self supposed by phenomenologists – a kind of substantive essential self that can be trusted for producing truths about itself, rather than recapitulating the myths that mark it.\textsuperscript{60} The tension here may seem stark. Where the “phenomenological assumption” rests on a Cartesian footing (even where it tries to move beyond Cartesian dualisms), the cultural assumption eschews the very tenets of that Cartesian faith: the transparency of ideas, the nature of introspection and awareness, the authoritative perspective of the subject, and the more or less stable self that underlies these notions.

What we see in Saukko is an attempt to mediate this tension by favouring the phenomenological over the cultural assumption, though the cultural assumption still plays an active role in her methodology – part of what Saukko calls, following Carol Rambo Ronai, a “layered account”. Saukko explains, “… I have layered discourses that diagnose anorexia and my own experience of being the object of those discourses. The aim of such layered accounts is to set the discourses interrogated into motion.”\textsuperscript{61} Speaking from her own experiences as someone with a history of anorexia, Saukko argues for the necessity of the inclusion of such narratives, highlighting the limitations of cultural theoretic discourses that often leave her “intellectually troubled and personally insulted.”\textsuperscript{62} She is particularly troubled by theorists such as Bordo who assert that anorectics have “disavowed their gender in a bid to starve their bodies of its womanly shape”\textsuperscript{63} because of the operation of omnipresent myth. The critique offered here is against the cultural assumption as a methodological starting point. Cultural theory cannot understand anorexia (or presumably other eating disorders) without accepting the inclusion of

\textsuperscript{60} See J. Butler (1999) and (1993); Bordo (1993); Bartky (1988); Fraser (1989); Nicholson (1990).
\textsuperscript{61} P. Saukko (2008), p. 33.
\textsuperscript{62} Ibid, p. 1.
\textsuperscript{63} Ibid, p. 3.
personal narratives, but such an inclusion must grant a level of autonomy and authority to
the self that cultural theory often hopes to destabilize and problematize through critique.
In other words, the “layered account” places its bets with the phenomenological
assumption, though at times those bets are hedged by remarks about the usefulness of
cultural theory as a tool for showing anorectics that they are resisting cultural myth even
when myth has had an undeniable effect on them (and, arguably, as a necessary tool for
organizing these narratives into something more than just isolated expressions of personal
meaning). Saukko’s is therefore a hybrid phenomenological and cultural account, but one
that tries to make real the experiences of anorectics against (and then hopefully within)
cultural theory – the same cultural theory which at once can give political, social and
ethical weight to such expressions but which often, almost paradoxically, ignores
personal experience in overly normative claims about the meaning of anorexia and its
impact on the self/body.

What we find with Saukko, as I have already outlined above, is a brand of
hybridization between taking the autophenomenological accounts of subjects with eating
disorders seriously, while at the same time situating such accounts in a particular socio-
cultural context. This approach primarily involves recognizing that the personal stories
articulated by anorectics or bulimics will not always map neatly onto the discourses
expounded by cultural theory. The way we negotiate between the two, Saukko suggests,
is through critically interrogating both the personal narratives and cultural discourses.
Drawing from postmodern figures, Saukko proposes that we “…continuously interrogate
all discourses that suggest to us what we should be or what the world should be.”64 In
short: this hybrid account to the study of eating disorders is normative and

64 Ibid, p. 113.
methodological in character. Saukko isn’t attempting to describe the nature of the self or how it comes to be inscribed with an identity; rather, she is laying out an approach that isolates a viable political position that can be taken by those who have an eating disorder whose experiences are not as cultural theory tells them they should be.

Adopting Saukko’s conceptual framework however doesn’t answer the more primary question that this project is concerned with. Recall that our purpose is to find effective ways for speaking about the body such that a number of differing practitioners and theorists of radically different disciplines may find an object about which to dialogue. Saukko’s normative framework gives us one way of mediating between personal experience and cultural theory, and certainly the body has much to do with both the experiences of the subject and her cultural background, but we have not established anything about the nature of the self regarding its relationship to the body or to cultural norms. Her layered account more or less presumes at one level (or layer) there exists a self not dissimilar to the Cartesian self assumed in pure phenomenology. At another level (or layer) lies the grand cultural narratives that are thought to make intelligible those bodies and bodily practices that are acceptable or unacceptable, sick or healthy, odd or normal. Yet another level (or layer) is that of medical intervention, though Saukko does not give as much attention to this level. Presumably here we find an ontology of Cartesian mechanical bodies. This talk of layers (the use of the metaphor of layering) is already indicative of a separation of spheres, each with its own separate and assumed ontology. Of course, these spheres can come into congress (as they do when patients use their own experience to critique and destabilize this or that cultural account of their disease), but the stable ontologies in each sphere are still disparate, and not challenged.
From the perspective of our inquiry, the ontology of the body should not be so easily found, and should not so perfectly match one’s research goals (only to be entirely rejected by some other practitioner working in some other field that is at least putatively studying the same sort of thing). The self isn’t easily isolated as something like a Cartesian ego when we engage in introspection, then jettisoned and given a mere mechanical proxy when dealing with medicine, a proxy which is then largely ignored when thinking of the body as a discursive locus in cultural analysis. The methodological problem we have is to discern how the body is, not how it is assumed to be relative to this or that discipline, as well as to discern how the body is the product of all of these disciplines at all times.

There are similarities between Saukko’s view and other Foucauldian attempts to describe the complex relationship between personal experience, the body and social norms which do not succumb to this kind of ontological compartmentalization. Ian Hacking offers us one such example, which he calls the “looping effect” based on antecedent work in sociology done on “labeling theory”. In an attempt to understand the proper ontology of persons, especially persons who occupy contested identity categories, Hacking proposes that human beings are natural kinds (and therefore fall under the purview of the natural sciences), but that they are a particular subset of all things that are natural kinds. “Human kinds,” as he calls them, are natural kinds that can be cognizant of their categorization and change their behaviour (or at least their attitudes toward categorizations) as a result. This process is called “the looping effect of human kinds.”

For Hacking, human kinds are formulated under the same model of causality as all other natural kinds. However, unlike other natural kinds, human kinds are sensitive to (and responsible for) specific organization and categorization techniques for sub-classes
of the kind. Human kinds are “kinds about which we would like to have systematic, general, and accurate knowledge; classifications that could be used to formulate general truths about people.” Hacking argues that we want to have this type of knowledge about these kinds of people in order to predict how the people in these groups will respond to attempts to help them modify their behaviour. That is, classifications of human kinds not only allow us to predict human behaviour, they also serve to shape it in some ways. Such is the effect on individuals who are shaped by being put into this or that class or category, often altering their own behaviours because of it.

Hacking accordingly contends that the fundamental difference between non-human natural kinds and human kinds is that classes of human kinds are “laden with values.” They have intrinsic moral worth, and are representative of things people want to be or do not want to be. That is, they are different than the brute facts about other natural kinds which we also categorize, but which cannot negotiate at all their categorization. Accordingly, as a category of human kind develops and changes, so do the boundaries of the set, and descriptions of those in it. This change can be drastic, and intrusive enough to change even our historical record and understanding of the past. As Hacking describes it, “one way in which some human kinds differ from some kinds of thing is that classifying people works on people, changes them, and can even change their past. The process does not stop there. The people of a kind are themselves changed.” Therefore, the way that people are classified accordingly alters not only the ways that they conceive of themselves, but also their behaviour and interactions with others.

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66 Ibid, p. 360
Hacking uses the term ‘looping’ because the process is thought to generate feedback effects that work to alter the category at hand, with the most rapidly looping effects arising from categories with strong moral connotations. This type of categorization and effect is ubiquitous to all categorizations of humans, but the effect is best noticed in marginalized (or “abnormal”) groups, such as prostitutes or gay men carrying the AIDS virus.

Hacking’s view has a certain ontological economy which is lost in Saukko’s view. We humans are what we are no matter the object of study that finds us, be that introspection, cultural analysis, medicine, biology, psychology, criminology, sociology, etc. Nonetheless, Hacking can still establish the intricate ways in which various discourses (such as those employed by the disciplines just listed) can mold us into the intelligible identities that can be countenanced in the current matrix of intelligibility. He’s able to capture the way these discourses operate on the self without presuming the self to be anything special or particular. All that is required is the very minimal and unsurprising assumption that human beings are able to engage in self-reflection – which, we may presume, nobody in any discipline would find contentious. No other tricky metaphysics of the self need be mentioned.

Saukko says much about Foucault’s postmodern conception of the relationship between the self and society (and how the self should negotiate cultural narratives and powerful discourses), but she is asking the wrong sorts of questions (from the perspective of our analysis). We need first a descriptive account of how the self develops (how it becomes a recognizable identity) within these discourses, and only then can we ask how these discourses go on to shape and refine the self in accordance with those discourses.
Let us make clear that Saukko need not think that the ontology in each sphere be a simple one. She draws on Deleuze and Guattari’s view that society and persons are not related via a relationship of cause and effect, rather “tenuous vibrations,” as well as the related notion that persons, relationships, and social formations are “assemblages,” formed from heterogenous and unstable elements.  

However, none of these overtures allays the concern that her hybrid methodology reifies an introspective self that shares in the Cartesian legacy, based on the conceptual commitments which are presupposed by the inclusion of personal narratives as relevant and fairly definitive data.

Her talk of tenuous vibrations and assemblages goes some way to establishing what is so complex about the relationship between introspective self, body, scientific discourse, and society/culture, but such talk fails to give an account of how such discourses may be unified in the ways we propose. This should be no surprise. As someone with one foot in the phenomenological sphere, and the other in the cultural theory sphere, Saukko is concerned with establishing a conceptual framework in which critique of cultural myths based on personal experience can make sense. Our purposes are quite different, and, as we shall see below, we are not committed to the methodological framework that Saukko, given her desiderata, must utilize: namely the central role she gives to autphenomenological or autoethnographical reports.

IV. Problems with autophenomenology

We might ask: is it necessary that we take all autophenomenology seriously? If the grouping of experiences, from which we can extrapolate an ethnographical account, reveals that a constant reiteration of cultural images pertaining to women’s bodies has

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69 P. Saukko (2008), pp. 112-113.
causal correlation with eating disorders, even if an individual narrative destabilizes this, why take the individual narrative seriously considering that person could be confused, and in fact fall under the causal generalizations that the cultural theory is positing? Isn’t it more likely that such an account, in spite of vehement claims to the contrary by individual anorectics, is part of this cultural phenomenon? We seem to have so much evidence telling us that there is an unmistakable correlation between cultural images of women and eating disorders. A simple weighing of all of this evidence against single experiential narratives will most likely lead to our rejection of the autoethnographical data. It is hard to justify a different response to the empirical situation at hand.

I argue that an autoethnographical approach simply cannot provide us with the sufficiently integrated framework for addressing complex pathologies we seek. The assumption of the non-mechanical lived body has been turned too far away from naturalism. The turn toward autophenomenological data has privileged first person experience as the only valid data for study, where this has a prima facie implausibility. One particularly problematic form of such data involves narratives by anorectics who claim that their eating disorders are, in fact, not pathologies at all; rather, they are implicated in a “way of life.” As the naturalist shows us, concrete processes that take place in the lived body have a significance that goes beyond the experiences of the subject. Similarly, as the constructivist reminds us, cultural influences may be utterly opaque to someone with a disease like anorexia nervosa; one need not know that they are recapitulating gendered norms of appearance to be doing so.

Embodiment theorists, however, are not part of a homogenous group. While the methodological approaches employed by all phenomenologically oriented embodiment
theorists are similar, namely they focus on the lived experience of the subject, there remains much variation in terms of their willingness to weigh such accounts against socio-cultural, physiological, neurological, or biological data. As we will see, once we take seriously personal narratives as they relate to such data, we are left with a more robust articulation of both the medical and social profile of complex disorders, such as anorexia and bulimia. However, at this point, we are discussing not an autophenomenological account, but a heterophenomenological account – the former taking as authoritative the experiences of the subject, the latter taking such experiences as useful, and perhaps even essential, though never yielding to the subject authority over the meaning (or accuracy) of her experiences. In this section I will preserve the usefulness of narrative approaches to the study of complex disorders by distinguishing between autophenomenological interpretations of first person reports and the heterophenomenological study of such reports. Secondly, I will explore Daniel Dennett’s account of heterophenomenology to show that first person reports of mental states are only seemingly “first person” reports. As we will see, Dennett argues that all such reports require a rich interpretivist semantic framework in order to be made sensical, and, as such, employ a full catalogue of data which is necessary to attribute meaning to one’s utterances about her mental states.

Autophenomenology isn’t itself a methodology; rather, it is a semantic framework for attributing meaning to first person reports. As such, it needs to be clearly distinguished from narrative approaches which are not concerned with weighing first person data, or narratives against a backdrop for interpretation that call such reports into question in light of cultural, scientific, or medico-scientific data. I hold that there is
nothing wrong with narrative approaches per se. My problem is not with first person accounts, or how they might feature as evidence in studies of anorexia and bulimia; rather, my problem is with autophenomenology as a semantic framework for the interpretation of first person claims.

It is an autophenomenological assumption that first person claims have their normal meaning as a result of the authority that the person expressing them is thought to have over her immediate mental states. The problem is therefore with this semantic interpretation of first person narratives, not first person narratives. I would argue that first person narratives, if given a less narrow semantic interpretation, are central to any methodological approach to studying anorexia and bulimia. Other approaches might be: behaviouristic, medico-scientific, cognitive, or cultural. As I have already mentioned above, I will be pursuing in this section something different from all of these approaches, namely, a heterophenomenological interpretation where first person access is still respected but not absolute in terms of establishing the meaning of first person reports.

V. Heterophenomenology: Making scientific sense of first person narratives

Daniel Dennett presents us with a way to take first person points of view seriously, while at the same time maintaining a commitment to the scientific method. He calls this method of investigation heterophenomenology. Heterophenomenology takes phenomenological data to be greatly important. In fact, the primary means of collecting data is phenomenological. Yet, the heterophenomenologist does not give a phenomenological

account free reign. In fact, the subject could be mistaken, even about her own phenomenology. As Dennett explains, a proper heterophenomenological study begins with the assumption that scientific study of the mind imposes upon us an interpretivist semantic framework: “basically, you have to take the vocal sounds emanating from the subjects’ mouths (and your own mouth) and interpret them!” The process of interpretation, Dennett contends, allows the investigator to become familiar with “what the subject believes to be true about his or her conscious experience.” The key, for Dennett, is to not miss out on any significant data for the study of the mind. It has often been thought that this requires two very different modes of study: study of the subject’s brain (and body) on the one hand, and study of the subject’s subjective experience on the other. The study of the brain poses no particular problems for natural science, but the study of the subjective life of experience requires an entirely different, perhaps unscientific method. Dennett disagrees. Dennett offers the following “scale of methods of scientific investigation” to make clear the distinction of different levels of active participation on behalf of minded subjects:

- experiments conducted on anaesthetized animals;
- experiments conducted on awake animals;
- experiments on human subjects conducted in “beviorese” – subjects are treated as much as possible like laboratory rats, trained to criterion with the use of small rewards, with minimal briefing and debriefing, etc.;
- experiments in which human subjects collaborate with experimenters – making suggestions, interacting verbally, telling what it is like.

These levels are meant to indicate the different ways in which one can study the body from various different perspectives. Using Dennett’s language of stances, the first level, studies of anaesthetized animals, can be done from what Dennett calls the design stance,
treated the animal as a functioning mechanism. The third level, experiments conducted on human subjects in behaviourese, when viewed in accordance with strict Skinnerian behaviourism, also reduces the subject to a machine, this time as a machine capable of responses to stimuli. All of the other levels presuppose what Dennett calls the intentional stance: the assumption that the subject is an intentional system with wants, desires, and beliefs about certain objects of states of affair. The last level, where collaboration between subject and experimenter occurs, is a special instance of the intentional stance, one in which the intentions of the subject are used to provide a semantic interpretation of a particular kind of stimulus response, namely meaningful speech. As Dennett notes, only the last of these experimental methods holds out much hope of success. But many have made the mistake of thinking about the last group of experiments as requiring a first person (or perhaps second person) methodology, “but in fact it is still a third-person methodology if conducted properly. It is heterophenomenological.”

For example, third person perspective is needed for the wealth of data required to make a scientific judgment about the mind, including the utterances of the subject (which are not mere sounds, but speech acts), other event like experimental button presses, what we may know about the structure and activity of the brain, and assumptions about the subject’s environment.

When we interpret utterances (sounds) as speech acts, when we attribute to a subject rationality and mentality, we do a lot: we treat her as having beliefs and other propositional attitudes, and other sorts of intentionality. We realize that she has access to a phenomenological world different from the world, but we should also realize that this world is not a construct to which she has a particular kind of authority: it is as much a construction of the experimenter (the interpreter) who must posit certain beliefs and other

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74 Ibid, p. 20.
attitudes based on the subject’s bodily movements, her utterances, her aversion or attraction to various events in her environment, etc. The “subjective” realm of consciousness should therefore not be viewed as something beyond the scope of science, much less should it be viewed as a privileged realm about which we can know nothing save what can be gleaned from a subject’s first person reports. This is to assume a naïve semantics of such reports, and to ignore the rich catalogue of information that must be in place for such utterances to have meaning in the first place. All of this data comes to us at once — if it didn’t, the subject might as well not be engaged in intentional speech acts at all. What comes out of her mouth may as well be “belches and moans”\textsuperscript{75}.

By taking the accounts of the subject as important data we are able to come to understand the intimate features of her world, but not by assuming her to have some kind of direct awareness of an internal Cartesian theatre whose stage displays immediate objects of a purely subjective sort. We become privy to the world as she experiences it, privy to her—in Dennett’s words—heterophenomenological world. The details of this world are integral to the study of human consciousness, mental states and beliefs.

But, notice that unlike the autophenomenologist we haven’t given priority to the subjective world presented by the subject. Rather, first person accounts must be measured against any and all available data. All of this data together provides us with the kind of interpretivist framework for understanding utterances as speech acts which Dennett rightly holds to be the necessary precondition for any scientific study of the mind. Dennett reminds us, “[h]eterophenomenology is the beginning of a science of consciousness, not the end. It is the organization of the data, a catalogue of what must be

\textsuperscript{75} Ibid, p. 20.
What Dennett is showing is that the narrative approach to the study of eating disorders (the asking and answering of questions, the eliciting and telling of stories, the processes of communication and meaning-making) are all intimately bound up with other forms of data, be they determinations of behaviour, reactions to certain experimental cues, the attribution of certain psychological states (paradigmatic amongst them beliefs), or even more precise studies of the brain and physio-anatomical facts. All narrative studies presuppose, to some degree, this rich data set; they do not exist in contradistinction to such scientific studies. Further, the heterophenomenological approach demystifies the workings of subjectivity. The self is not some unseen commodity, incapable of being grasped by the natural sciences, nor does it necessitate a strict bifurcation between the hard sciences, the life sciences, and the social sciences. This entails that all studies of the mind utilize the sciences, but it also informs us that the sciences cannot ignore subjects—that data sets cannot be considered complete if they only avail themselves of behaviour, brain imaging, or any other methodology which attempts to remove the communicative dynamics which give rise to those patterns we recognize as mental and intentional.

But here, as we have seen, we go well beyond the “lived body” of Leder. The “lived body” is an attempt to show how embodiment affects phenomenology. But, it is not just our embodiment that matters, but also the communicative milieu in which we make attributions of belief and desire, attributions of intentional consciousness—a view of the subject as embodied, culturally located, bound to a complicated socio-physical environment, and subject to a complex network of disciplinary studies, including neuroscience, psychology (both folk and scientific), language and linguistics, cultural sciences, and evolutionary biology.

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theory, feminist theory, and countless others. In short, heterophenomenology preserves our central methodological desideratum: all studies of the subject call into being a rich and multidisciplinary framework that straddles both naturalism and autophenomenology. Most importantly, Dennett reminds us that all of this data is required at the very outset of our inquiry. It is all presupposed in the very semantic framework which help us give sense to (or make sense of) first person narratives. Of course Dennett does not draw our attention to the cultural, social and political aspects of the attributions of intentionality required to interpret first person reports. His program, as we shall see in the next section, is a minimal program. He wants to know what the bare minimum assumptions are to understand an interlocutor’s utterances as meaningful speech. In the next section we will see how Elizabeth Wilson adds an explication of these cultural, social and political factors while preserving something very much like the minimal commitments of intentional study introduced by Dennett.

**VI. Elizabeth Wilson’s embodiment theory**

Elizabeth Wilson also offers an integrated approach to the study of the body and the self. Though her position differs from Dennett in that it does not specifically reference any interpretivist concerns (or semantical concerns more generally), she does argue that the sciences must be brought back into feminist studies of the body without losing the political dimension feminists and other cultural theorists are primarily concerned with in their research. What Wilson offers us is a framework that maintains the minimal scientific requirements introduced by Dennett while extending such a framework to incorporate the added criteria for the study of the body demanded by a feminist program.
In this section I will begin by outlining Wilson’s critique of feminist theorists of the body, namely, their failure to properly engage with the biological body. Wilson, through Ferenczi, offers a specific approach to, and understanding of, the biological body. Wilson suggests we adopt what she calls “gut feminism” as feminist theorists studying the body. She argues that “gut feminism” presents us with a way of innovatively bringing the biological body back into feminist and cultural studies of the body. Next, I will argue that through Wilson we are able to establish a properly integrated picture of the body which is better equipped to include the concerns of the phenomenologist, the autophenomenologist, and the feminist theorist for whom the body is also a political realm. This picture, as I will argue, is better able to accommodate a feminist project than the heterophenomenological method espoused by Dennett.

Wilson argues that although the body is a site of interest for feminist and cultural theorists, their interest in the body is rarely biological. She argues that feminist theorists have “naturalized anti-essentialism.” Failure, or downright refusal, to engage with biological and neurological details of the body proves debilitating for feminists interested in engaging politically with the sciences. Meaningful interaction with the sciences requires engaging with the data and discourses of science, even where the data appears to be outside of the scope of a critical feminist lens. This is necessary, according to Wilson, if feminist and cultural theorists critiquing biological and scientific structures want to avoid recapitulating ‘anti-essentialist’, ‘anti-scientific’ discourses with which we are all too familiar.

79 Ibid, p. 15.
Wilson traces the birth of this tendency by feminist theorists to prefer to engage with the ideational in relation to the body rather than the biological back to Freud’s preference for this kind of explanation. Feud’s work on hysteria, in particular his transition from a preference for neurological modes of analysis to psychological modes of analysis for understanding organic and hysterical paralyses inspired this shift in feminist work on the body. While organic paralyses occur because of an “underlying biological lesion”, hysterical paralyses are not influenced directly by “the facts of anatomy”. That is, they are not influenced by the nervous system. Hysteria, then, interrupts the regular, everyday functioning of the body without reference to a logic of anatomy and how the biological body is put together. Wilson explains,

[t]his early neurological work, in conjunction with psychotherapeutic treatment of neurotic patients, laid the foundation for Freud’s account of conversion hysteria. He claimed that conversion hysteria is the transformation of psychic conflict into somatic symptoms.

In conversion hysteria, then, the symptoms undergone by the body are not governed by the dictates of biology; rather, they seem to follow a different logic altogether. The cultural, the symbolic and the ideational realm are the motivating forces for transformation of the body. According to Wilson, “[t]he important conceptual point for Freud is that in hysteria the ‘material substratum’ (i.e. cortex) is undamaged, but ideas

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81 Of course, I attributed the feminist move away from the biological body to a methodological and conceptual concern in de Beauvoir work, which focused on her brand of phenomenological existentialism. As I said in the introduction, feminists preserved de Beauvoir’s mistrust of biological accounts of the body even after they adopted alternative theoretical frameworks to replace phenomenology. Wilson is picking up here on the feminist use of Freudian psychoanalysis that was popular in the 1980’s. Feminists in the 1990’s and 2000’s went on to replace Freudian psychoanalysis with Lacanian psychoanalysis, especially when Lacan is given a poststructuralist and postmodern reading. For a concise, but very thorough account of the use of psychoanalytic theory in feminism, especially in the work of Irigaray, Kristeva, Kofman, Clement, and Cixous, see Emily Zakin’s Stanford Encyclopedia Entry “Psychoanalytic Feminism” (2011).
83 Ibid, p. 67.
84 Ibid, p. 68.
about the body have undergone some kind of alteration.”

This conceptual shift, from thinking of the body as influenced by purely anatomical and biological happenings to thinking about “somatic events” also as the product of cultural and symbolic forces, Wilson argues, had an enormous influence on feminist work on the body in the 1980s and 1990s.

Wilson argues that the problem with feminist tendency to favour ideational explanation over biological explanation, however important it has been to the development of contemporary feminist embodiment theory, has left feminists without sufficiently robust accounts of the body. She claims, “[m]ost troublingly, it seems that the very sophistication of feminist accounts of embodiment has been brokered through a repudiation of biological data.”

Such an approach has undoubtedly left feminists without any avenues for aligning with the biological sciences. Biological data about the happenings in the body are considered too reductive and dismissed. Dismissing biological accounts outright and “naturalizing an anti-essentialist” approach to studies of the body, has, as Wilson contends, and as I will echo, left feminists both unable to engage with the sciences and without the explanatory power necessary to theorize about the body. Such limitations have, consequently, restricted both their intellectual and political scope.

Wilson draws on the example of the bulimic body to illustrate the importance of maintaining a commitment to engaging biological data in our feminist analysis of the body. To do this she follows Sándor Ferenczi, the Hungarian psychoanalyst who was part of Freud’s inner circle. Ferenczi argued for a more biological understanding of hysterical conversion and trauma. This focus in the biological over the ideational set him apart from

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85 Ibid, p. 68.
86 Ibid, p. 68.
87 Ibid, p. 70.
Freud and caused an inevitable schism between the two. Because of his break with Freud his biologized account of hysterical conversion has not been well received in the psychoanalytic community. Wilson asks the Ferenczian question: “[w]hat does a hysterical conversion tell us, not only about the psyche but about the character of biological substrate?”

Seeking to explain episodes of hysterical conversion in biological terms allows for the possibility of the kind of robust explanation Wilson contends is missing in feminist embodiment theory. That is, such an approach allows for the psyche and the soma to be included in our studies of the body.

Wilson proposes a robust biological account of the bulimic body, which once pathological, engages in behaviour indicative of the disease by biological transformations of organs and systems of organs: in this case the digestive system. Wilson refers to this account (based heavily on the localization of bulimic symptoms in the gastrointestinal system) as “gut feminism.” Gut feminism, Wilson explains, is “a feminism that is able to think innovatively and organically at the same time.”

In short, what Wilson means by this is that there is no a priori hierarchy, or even demarcation, between psyche and soma. This is a reaction against the clear division that prevailed in psychology from Descartes to Freud.

The fundamental idea is that organic subsystems of the individual, and the sort of electro-chemical communication we see between organs, accounts for the behaviour of the organism as a whole to which we misattribute centrally coordinated, even attentive, thought processes. These subsystems are capable of what the psychologist Ferenczi referred to as “materializations,” what Wilson refers to as “organic thought.”

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89 Ibid, p. 86.
consequence of such a view is that exactly how these systems relate, how they sometimes identify with each other, and how they manifest into persons, is of immediate interest. In instances of severe psychological disturbance the organic substrate begins to react. The organ itself begins to think. Wilson explains,

> the gut is sometimes angry, sometimes depressed, sometimes acutely self-destructive; under the stress of severe dieting, these inclinations come to dominate the gut’s responsivity to the world. At these moments any radical distinction between stomach and mood, between vomiting and rage is artificial.\(^{90}\)

It’s not that there is no distinguishing the psyche from the soma; rather, it’s that such a distinction is not an “a priori, fundamental demarcation.”\(^{91}\) Such an approach to thinking about the body is able to provide us with the integrated picture that we are looking for. The body is viewed not essentially as biological, cultural, or cognitive, as if any one view of the body is reducible to the other; rather the body becomes only intelligible as an integrated system, or collection of subsystems, that often require piecemeal examination.

The approach to theorizing the body we get from Wilson is one that is more suited to addressing the concerns of not only the feminist theorist, but the phenomenologist and the autophenomenologist, as well. This is because such an approach, as we have seen, maintains a commitment to taking the political realm of the body seriously. On this view, the eating disordered body is not just the product of social or cultural markings; rather, it is the product of social and cultural interactions with the biological cum psychological (itself a complex interaction between localized biological states and lived experience).

What we get from Wilson is a more robust framework for theorizing about the body. The key difference between Wilson and Dennett is the extent to which Wilson engages with the body politically. For Dennett, the heterophenomenological method is

\(^{90}\) Ibid, p. 84.

\(^{91}\) Ibid, p. 84.
intended to be “neutral.” In other words: heterophenomenology is quite simply the minimal requirement for doing a proper scientific study of the mind. In fact, Dennett claims that it’s precisely this fact about his method (that it’s both minimal and straightforward/uncontestable) that has brought about confusion by his critics. Dennett claims, “[m]ost of the method is so obvious and uncontroversial that some scientists are baffled that I would even call it a method.”92 The claim is that the method is so neutral, so taken for granted in properly conducted scientific study on human consciousness, that many would never think to express it. But for this very reason, one can add to this minimal scientific framework and still, by Dennett’s innocent and uncontroversial standards, be engaging in respectable scientific work.

Wilson’s methodology is both scientific and political; in fact, her gut feminism is set up in such a way that the physical realization of the body (especially the diseased body) has a necessary political dimension to it. This in no way robs it of its scientific character. Her integrated program is, as Pablo Schyfter puts it in the title of his review of *Psychosomatic,* “the sexual politics of neurons.”93 Wilson is arguing for the interactions between neuroscience, psychology, biology, and feminism as key concerns for feminists doing work on the body.94 And, moreover, the idea that once we are careful to think through such connections, work on the body is already always situated within the political realm. This is because the genesis of the eating disorder (the purely necessary cause) is found in cultural and social discourse. Politics will thus have a say, or at least ought to have a say, in why this should not be the case. As ammunition for such normative claims, we must nevertheless look towards the body and the atavistic

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94 Ibid, 352.
neurological effects of such social and cultural causes if we are to understand just how deleterious they can be. Politics tells us that these insidious images of the body are wrong; “gut feminism” tells us to what extent the body begins to be diseased, even when these social and cultural cues are no longer operant—or better yet, how they can remain operant once they become biological. Such integration of politics and the sciences has the benefit of better characterizing the body as an agent itself in a complex world. According to Schyfter: “Specifically, [Wilson] proposes the use of neurology and biology in feminism as a step in rejuvenating the discipline and discovering new directions for research.”95 In short, feminists engaging in studies of the body need to be forming alliances with scientific studies by engaging with such studies critically.

Our study hopes to adopt this kind of general Wilsonian integration of proper scientific method and political engagement. Just because we seek to be scientifically relevant does not mean that we must reject our political motivations and goals. The positivistic idea of disinterested science no longer holds the kind of interest it once did. We fully acknowledge that both the motivations and the details of such a project will reflect (and inform) our political goals but argue against the view that this political dimension of our study makes it unscientific. Thus, the desire to discuss the scientific (or naturalist) basis of disease is not akin to some positivistic hope to unify science, or reduce all studies of the body to some apolitical scientific realm of objects. On the contrary, it is to bring the political (and the social and the cultural) into the sciences—injecting the relevant desiderata, modes of critique, and methods of analyses which should never have been removed from the scientific worldview, but which have been missing since the Cartesian conception of the body as machine.

Of course, one could maintain that science merely describes the world as it is and how it works, but this kind of descriptionism—which was last defended by Ernst Mach and the logical empiricists—seems highly implausible given the expectation that science do more than just describe, and answer the “why” questions. Science has long been expected to offer explanations, and to answer at least some “why” questions. An explanation introduces a whole host of politically, socially, and culturally loaded evaluative judgments about the relative weighting of evidence, the choice of theories, what questions are salient and relevant, and even the role of seemingly innocent judgments about theoretical simplicity and determination of empirical adequacy. Some of these issues are taken up in more detail in Chapter Four.
CHAPTER III. NEUROIMAGING AND THE EATING DISORDERED BRAIN

I. Introduction

In this chapter, I lay out some prescriptions for how a feminist might attempt to engage with the scientific data presented in contemporary studies of the brain via neuroimaging techniques. I examine neuroimaging studies of the eating disordered brain in order to demonstrate my critical empiricist methodology. This exercise notably does not require full proficiency into the technical minutiae of scientific practice, nor does it require ignoring the deep suspicion that feminists might have towards biology’s historic role in promoting sexist ideas about women. This endeavor is intended to be an expression of my methodology – the consequences and philosophical significance of which I will outline in greater detail in the next chapter and the conclusion. I begin with a brief review of my methodology. Next I survey an important part of the landscape of neurobiological study on eating disorders. Specifically, I focus on neuroimaging technologies that permit the scientist to study the relationship between neuroscience and behaviour in patients with, or having recovered from, eating disorders. I then move on to describe some recent research regarding disturbances in serotonin and dopamine activity in patients suffering from anorexia nervosa and bulimia nervosa, with fairly specific reference to what this research has to tell us as feminists—in particular, why feminists should be concerned at all with the functioning of the nervous system as it relates to behaviours of restriction and binging and purging.96

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96 This focus is not meant to detract from other levels of analysis or other foci other than neurotransmitters. It is likely the case that feminists will also want to concern themselves with cognitive neuroscience, or even...
The focus I place here on neuroimaging (and, \textit{a fortiori}, neuroscience) is not meant to be exhaustive of my methodological approach, rather merely an example of how feminists might engage with scientific work on the body that is informative for their research. The life sciences, like all sciences, approach their purported subject matter from various perspectives or levels. One can study the eating disordered body from chemical, biological, neuroscientific, or psychological perspective, each with its own view of the body. The body is, as I have argued, the object of study of non-scientific disciplines as well. A more comprehensive examination of how these levels relate to one another, with their complicated ontologies, is beyond this work, though we did address some of these issues in Chapter Two and will speak to issues regarding ontology and interpretation of data in Section V later in this chapter. In spite of a lack of a purely ontological criterion for “levels” of scientific investigation, it is fairly unproblematic to assume that there are level distinctions in the sciences, and that feminists (as an epistemic community) have to make sure not to view science as a monolith. Feminists who adopt a methodology based on dialogue with the sciences adopt the responsibility of making clear what parts of what sciences matter to their particular studies.

The primary purpose for this chapter is to lay out some prescriptions for how a feminist may attempt to engage with the scientific data presented in contemporary studies of the brain via neuroimaging techniques. These prescriptions are meant to guide the feminist—who is not necessarily a trained expert in this or that field— to be able to negotiate scientific publications. Perhaps more importantly, they are meant to indicate how such a feminist (with minimal knowledge of the field—say a graduate student)
should view her position in an epistemic community of feminist researchers, some of whom have spent a lot of time and resources developing an expertise in their relevant fields. I thus make two claims. Firstly, that feminists working on topics related to, or even adjacent to, the sciences are encouraged in their early careers to get a minimal understanding of those sciences. Further, I recognize that this is not sufficient for effective dialogue between feminists and their scientific counterparts. Some feminists, especially those of senior rank in academic departments, will need to gain further expertise. I will examine how these prescriptions might lead to increased dialogue between the sciences and feminists, or at least an increased uptake of key neuroscientific findings by feminists doing work on eating disorders (and, by extension, other complex disorders).

II. Two requirements for critical empiricism—Or, how to get the scientists listening

The approach I am developing in this project calls for a reorientation of the way we as feminists study the body, in particular, the eating disordered body. I have already (in Chapter One) provided a detailed overview of the obstacles faced by feminist theorists studying the body. I demonstrated that the terrain of the debate is bumpy for the feminist theorist due to significant differences both in terms of vocabulary and ontological commitments between feminists and others working on a common object of study.

Feminists, especially those working within a social constructivist framework, and others, especially those working within a naturalist framework, find themselves at odds, unable to communicate across disciplinary boundaries. As a consequence of this inhospitable, and often outright hostile, setting, however, the feminist theorist finds herself in a unique
position. She is able to move beyond the naturalist/constructivist dichotomy and open up new conceptual space in order to develop more holistic and accessible theoretical frameworks. Feminists studying the eating disordered body, as I have already argued, need to foster effective communication and build bridges with the wider medical and scientific communities.

How such bridges may actually be built remains to be seen. The solution I propose, which I call a form of critical empiricism, is methodological rather than substantive, for just this reason—questions about how scientists and feminists will actually communicate with one another cannot be asserted with any confidence a priori. What we shall say in this chapter is best thought of as normative, and subservient to one overriding question: *If feminists wish to have a voice in how the sciences are done, then what is the best way to have that voice heard?* There is no commitment here to whether or not the scientist will listen, only a commitment to the claim that a necessary (though perhaps not sufficient) condition for getting the scientist to listen to critical and feminist concerns is through coming to speak the scientific language—to understand the scientist’s methods, procedures, and ways of inference (from the data) so that they may be critically assessed. Firstly, then, I want to speak briefly to two requirements that must be met in order to successfully integrate the form of critical empiricism I am developing in this project. The first involves communication between the feminist and the naturalist. The second requires the recognition that the scientific data always require interpretation. It is up to the feminist theorist studying the body to engage with the scientific data, offering a critical voice to studies taking place in the sciences. Satisfying these two criteria also gives the feminist researcher a wider audience for her critical work – moving beyond the
very narrow (and often miniscule) academic audiences typical of social sciences and humanities research. Likewise, the scientist working in the name of good science is responsible for following these same two criteria.

Later in this chapter, I look at the recent use of neuroimaging technology (specifically, PET) as applied to the eating disordered brain; offering a reading of this work can be seen as a case study of sorts, illustrating how communication may work in practice. Specifically, the survey of this research shows that the level of understanding necessary in order to meaningfully engage with scientific study is minimal, in spite of often opaque references to procedures and methods, and even more challenging use of esoteric terminology. My main assertion is this: one need not know the science as well as the scientist does, but one must know something of the scientist’s ways of addressing the relevant issues, and (a) the promising uses of this research when applied back to more traditional forms of critical study in the social sciences and humanities, and (b) the new possibilities for critical discussion once feminists expand their audience to include naturalists/scientists.

It is important to clarify that the requirements of (a) and (b) are off-shoots of my approach. These “off-shoots” need to be better explained, and their relevance and importance to the feminist agenda better situated. The first we might call the “correspondence off-shoot”, which involves correspondence between cultural discourse and neuropsychological discourse. More precisely, correspondence simply is the kind of communication between critical voices in the social sciences and humanities and practitioners of the sciences—the coming together for purposes of discussion and mutual reflection on a common object of study. This correspondence helps ensure that the
feminist can get a better holistic understanding of what is happening to the eating disordered body. For example, current neurobiological research shows that childhood personality traits, such as perfectionism or obsessionality, contribute to a vulnerability to developing an eating disorder in early adolescence, when cultural stresses and age-related changes set in (usually with puberty). There has been shown to be a “disphoria reducing character to dietary restraint”\(^{97}\) in such individuals. This facilitates what has been called a “vicious circle” of anxiety and disphoric mood, perpetuating the illness at the neurobiological level. Severe dietary restraint, far from being a mere consequence of a cultural cause, is itself part of a neurobiological cascade effect—the eating disordered body, in a sense, maintains itself through its insidious transformations of the body at the most basic levels. Not eating *improves* the disphoric mood while at the same time causing neurobiological changes, perpetuating the disease and making it a chronic, and all too often deadly, condition. By reversing the normal psychological effects of food intake, the eating disordered body is forever altered, at this point regardless of the cultural *milieu* in which it finds itself.

This research and an understanding of epigenetics gives the cultural theorist some insight into why cultural cues will affect some bodies, and not others; perhaps more significantly, it also allows the cultural theorist a further means of describing the perpetual and transformative nature of eating disorders, why it is that certain cultural cues “mark the body” in such perpetuating and seemingly inalterable ways, and with a deeper analysis of why such marking is more than merely symbolic, but discursive—helping her to make her claim that discursive practices really do *make* bodies, for the feminist is in a position to show how some bits of culture have more than a symbolic reality. The eating

\(^{97}\) W. Kaye (2009), p. 1310.
disordered body is not merely marked and signified with cultural significance: it is the manifestation of signification.

The second off-shoot we might call the “critical off-shoot”, which allows for increased reception of feminist points of view in the sciences through better communication and understanding. This second off-shoot is familiar, going back to traditional problems in the feminist philosophy of science. The “critical off-shoot” is perhaps best thought of as an extension on the work of feminist empiricist Helen Longino. Longino’s criticism of science is similar to the one we pursue here, because it does not argue that science requires any major architectonic changes in order to be palatable or useful to feminism. What is required is merely what we have been arguing for: the inclusion of women and other marginalized voices into preexisting procedures. For Longino, this means maintaining the empiricist commitment to theory selection and development, data collection, interpretation, and inference from the evidence; all that is required is that women be involved, and be encouraged to study those topics that interest them, increasing the objectivity of science as a social practice. Empirical science is not masculinist in its nature, says Longino, just in its current institutionalized practices.\footnote{See Longino (2005), “Can there be a feminist science?”, (1992) “Taking Gender Seriously in the Philosophy of Science”. See also G. Einstein’s discussion in her (2012) “Situated Neuroscience: Exploring Biologies of Diversity”.}

I am arguing for something very much like this kind of model. There are differences, however. Firstly, I see no reason why there cannot be methodological progress in the underlying empirical standards the more that feminists engage in the practice of science. Scientific institutions, scientific theories, and scientific practices will surely be improved if women (and other marginalized voices) engage in empirical study, but so too may be the empirical methods themselves. Secondly, the dialogue I urge

between scientists and feminists means that on my model, feminists have to be engaged
with the sciences, not necessarily engaged in science. Feminists can make science more
open to their ideas through a critical understanding of science without being (or even
becoming) scientists. However, these nascent differences should not diminish the
importance of the similarities between Longino’s project and this one—similarities that
go some way to further explicating why this project is also a form of empiricism.

III. Pluralism and questioning the biology is destiny thesis: What the sciences are
saying about the brain and the body and why this is interesting for feminist research
Understanding neurobiological disturbances in sufferers of eating disorders is thought to
offer invaluable information concerning the pathogenesis of the disorder, and, as a
consequence, the potential to help medical professionals provide more effective
management and treatment of the disorders. This consequence of neurobiological study
on the eating disordered brain, however, is not particularly contentious. Given the
methodological direction of this present work, we must ask a further question: What can
such studies add to feminist research of the body? In other words: Why should feminists
engage with neuroimaging data? What can we get out of studying the body in these
ways?

I am arguing that it is important not only for health-care professionals with duties
of care, but also for other researchers, specifically feminists of a non-naturalist sort. This
does not mean that the feminist theorist needs to go out and try to be a neurobiologist, for
example. Rather, it is about the possibilities that exist for the feminist when she engages
with the scientific data. Studying the body in this way is fruitful because it ensures (a) a
more comprehensive account of the body, as well as vocabulary for talking/writing about the body; (b) feminists find themselves in a larger community of interlocutors, and thus with a wider audience; (c) feminists are in a better position to bring to the “scientific table” a better understanding of the ways in which values, including political values, operate ineliminatively in scientific practice. Thus, furthering science by bringing to light the role of political values, and making clear how scientific practice should identify and eliminate, or fruitfully utilize, existing biases. The scientific data could potentially be (and I will argue, is) instrumental in helping the feminist theorist re-think the body.

What I am ultimately advocating for is a pluralist approach to studying the body. This means that the feminist theorist, while she needs to engage directly with the naturalist camp, need not necessarily think about or talk about the body in a naturalistic way. Fundamentally, then, I am asking the feminist to reconsider an oversimplification that is a keystone of much postmodern and post-structuralist accounts of the body—the “biology is destiny” and “biology is oppression” narratives. This is not to call into question the historical validity of such narratives. When it comes to marginalized identities, particularly discussions about gender and race, biology has often been a force for continued oppression of groups through binary divisions between male and female, heterosexual and homosexual, black and white, and has all too often been inculcated in regimes of normalization that entrench the norms that make such binary divisions sensical and natural. The veracity of these narratives is, we can hold, historically unassailable. However, a far more contentious thesis has not been adequately established

99 The biology is destiny thesis comes from Simone de Beauvoir in The Second Sex, Part I: Destiny, Ch. 1: The Data of Biology. In her words: “Woman? Very simple, say those who like simple answers: she is a womb, an ovary: she is a female: this word is enough to define her. From a man’s mouth, the epithet “female” sounds like an insult…” p. 21. The idea has also been explored by Evelyn Reed in the context of Marxist philosophy, see her (1971) “Is Biology Woman’s Destiny?”
by postmodern and post-structural critical science studies. The thesis that this is a
*necessary* function of biology, reflective of a deep heterosexism and racism that
permeates the very institutions that comprise the biological sciences, including
universities, hospitals, clinics, scholarly journals, and even pop-cultural ideas about
medicine and health. I want to argue that this thesis overextends itself, and that it has, by
and large, offered feminists (and others working on marginalized voices) a way of
ignoring how biology generates its most central claims, especially with regard to the
eating disordered brain and body. In short, the “biology is destiny” and “biology is
oppression” narratives, while possessing more than a kernel of truth, have too often been
used to set aside a plethora of issues that feminists working on the body ought to be able,
willing, and responsible to address—as an alibi for not knowing about the bodies of
which they speak.

Some of these issues will be taken up in more detail in the next section. The main
methodological redirection of this work is meant to ensure that feminists no longer have
such an alibi, and can no longer ignore the ways in which the sciences produce useful
discourse about the body. Also, the increased communication between scientists other
theorists will no longer allow for an unacceptable behavior coming out of the naturalist
camp—the mere hand-waving niceties and platitudes about “cultural cues” that
accompany the scientific research. “Culture” isn’t merely some amorphous entity that can
collect all of the unknown causes of complex disorders, placing them into a tidy black
box; a deep understanding of cultural conditions is central and necessary to any
understanding of the disease.
**III.1 Relevant neuroscience research on eating disorders for feminists**

Recent imaging technologies such as functional Magnetic Resonance Imaging (fMRI), Positron Emission Tomography (PET), and Single Photon Emission Computed Tomography (SPECT) enable scientists to study the brain in greater detail than ever before.\(^{100}\) Imaging data revealing abnormalities in the structure or functioning of the brain are, in turn, thought to offer new ways of understanding the puzzling symptoms and abnormal behavior of those suffering from EDs. I examine some recent and important work done under the direction of Walter Kaye at the Eating Disorders Center for Treatment and Research at the University of California, San Diego.

In a 2004 review article in *CNS Spectrums*, Frank et al., including Kaye, explain that “[b]rain imaging techniques now give us the opportunity to assess regional brain activity and neuroreceptor function in vivo in humans, and, therefore, may help us understand how neuronal circuits are related to behavior and pathophysiology.”\(^{101}\) Studies in other mental disorders such as anxiety offer a model for current research, since individuals suffering from eating disorders have comorbid disturbances in mood and anxiety.\(^{102}\) Of course, a complete study of all of these techniques would be far beyond the scope of this current project. Moreover, a rehearsal of the various different technologies and their findings is not necessary here. What I am interested in doing in this chapter is engaging with one line of neurobiological inquiry, giving it philosophical analysis from a feminist perspective. In particular, I want to focus on imaging data assessing the 5-HT (serotonin) and DA (dopamine) system functioning in eating disorders.\(^{103}\) Frank and

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100 G. Frank et al. (2004), p. 540.
101 Ibid, p. 539.
103 W. Kaye et al. (2009a), pp. 575-578.
Kaye explain that the 5-HT system is very complex, far too complex to be accurately understood or thoroughly studied in humans. Despite this complexity, however, Frank and Kaye articulate the importance of studying this system. They argue that positron emission tomography (PET) brain imaging enables scientists to better comprehend “neurotransmitter activity and dynamic relationships to behavior.”\(^{104}\) Enhancing our knowledge through such imaging studies is useful for providing “new insights into targets for more effective treatment”\(^{105}\) of eating disorders. For example, data culled from experiments comparing ill and/or recovered individuals with eating disorders to control women is thought to offer invaluable insight into promising new pharmaceutical treatments for anorexia and bulimia.

Of course this focus on treatment may not be directly pertinent to feminist studies of the body, but such studies also offer us a great deal of insight into how those individuals “recovering” from anorexia and bulimia are forever changed (at a neurobiological level) by their having the disease. More importantly, many studies point to the ways in which the disease physically perpetuates itself regardless of the social-cultural causal factors, which may tell the feminist a great deal about how dangerous the disease itself is—not just the cultural context in which the disease arises and worsens. The most recent imaging research also supports more fine grained distinctions in risk groups based on genetic vulnerabilities and a feminist might want to take care to note this data so that she can begin to make sense of why some bodies are anorexic or bulimic in a given cultural context and why some bodies aren’t.

\(^{105}\) Ibid, p. 727.
There has been a great deal of interest in studying the role that the neurotransmitter 5-HT may play in AN and BN. In a 2009 review article in *Nature Reviews: Neuroscience*, Kaye et al. explain that this interest is predicated on the fact that there is a great deal of evidence to suggest that “this neurotransmitter system could play a part in symptoms such as enhanced satiety, impulse control and mood” in AN. Moreover, according to Kaye et al., “there is much evidence of abnormal functional activity of the 5-HT system in subjects with AN.” There is also evidence of disturbed activity in subjects with BN. This is interesting because there are significant differences between the behaviors of subjects with AN and subjects with BN, yet disturbances to the functioning of the 5-HT system remain common to both. I would contend that this commonality is interesting because the relation between AN and BN is not clearly established by the behavioural data, and therefore, may likely not be clearly related in socio-cultural studies. Moreover, as a consequence such studies may teach us something about their *prima facie* assumption that the two diseases have something to do with one another (in spite of their very different behavioural profiles) if one merely looks to the science. This is just one example of the kind of fruitful result that can be achieved by feminists who engage with the science.

According to Kaye et al. the alterations of the brain and peripheral organs in individuals suffering from AN raise the question of whether such alterations are the cause or consequence of the disorder. Disturbances that persist after an individual has recovered suggest that such disturbances may have been present prior to illness. Like disturbances

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to the 5-HT system, disturbances of DA (dopamine) function are thought to contribute to symptoms of AN and BN, and there is evidence to suggest that altered DA function also persists after recovery.\textsuperscript{110} Determining that abnormalities were preexisting makes for a better, more comprehensive understanding of the neurobiology of eating disorders. Moreover, more fully understanding the neurobiology facilitates the development of better, more effective treatments for AN and BN.\textsuperscript{111}

We have already noted, that neurobiological alterations persist after recovery from AN or BN. But, what has not been made clear is whether disturbances in 5-HT function after recovery are leftover scars from the illness, or whether they were present in childhood before the onset of the disorder.\textsuperscript{112} Kaye et al. maintain that the consensus over the last 10-15 years has been that individuals who go on to develop anorexia nervosa share certain traits in childhood. Vikas Duvvuri and Walter Kaye argue,

Regardless of subtype, individuals with AN are characterized by marked perfectionism, harm avoidance, low novelty seeking, conformity, and obsessionality. Most of these clinical features appear in childhood, before the onset of weight loss and tend to persist long after weight recovery. This pattern of onset and persistence of clinical features argues against the notion that they are merely epiphenomena of acute malnutrition or disordered eating behavior.\textsuperscript{113}

Duvvuri and Kaye explain the pathophysiology of AN. They suggest neurobiological alterations be divided into two separate categories: those that existed before the onset of AN, i.e. genetically determined traits and those that occurred as a result of malnutrition and starvation, i.e. “state alterations”. Premorbid trait alterations contribute to the vulnerability to develop AN, while the state alterations that are a consequence of

\textsuperscript{110} W. Kaye (2008), p. 125.  
\textsuperscript{111} U. Bailer and W. Kaye (2010), p. 60.  
\textsuperscript{112} W. Kaye et al. (2007).  
\textsuperscript{113} V. Duvvuri and W. Kaye, (2009), p. 456.
starvation and emaciation help sustain the illness.\textsuperscript{114} Kaye, in a 2009 Editorial in the *American Journal of Psychiatry*, makes the following claim about the course of both AN and BN. He claims, “genetically determined temperament and personality traits contribute to a vulnerability to developing anorexia nervosa and bulimia nervosa during adolescence in females.”\textsuperscript{115} These trait alterations are exacerbated by state alterations, which may also work to speed up and intensify the eating disorder.

Unlike many other disturbances in neurotransmitters in subjects with eating disorders, studies have shown that the abnormal functioning of the 5-HT system persists even after recovery. Kaye et al. argue,

In fact, brain imaging studies consistently show that, when compared with healthy subjects, individuals with or having recovered from eating disorders have elevated and diminished binding potential for postsynaptic 5-HT 1a receptors and 5-HT 2a receptors, respectively. Studies of individuals with or having recovered from AN tend to produce similar findings, supporting the notion that there are trait-related alterations of 5-HT function in AN.\textsuperscript{116}

Such evidence is considered important to studies of EDs because it lends support to the hypothesis that there are trait alterations in 5-HT functioning which contribute to the development of the disorder. Moreover, these studies further support research on the relationship between 5-HT alterations and emotional disturbances, such as anxiety, that frequently accompany eating disorders.\textsuperscript{117}

Neurobiological alterations, however, are only one of the necessary ingredients for developing AN. There are several other important contributors that must also be present in order to act on the trait-related vulnerabilities (usually in adolescence) noted above. Kaye et al. explain, “First, puberty-related female gonadal steroids or age-related

\textsuperscript{114} Ibid, p. 457.
\textsuperscript{115} W. Kaye (2009), p. 1310.
\textsuperscript{116} W. Kaye et al. (2009a), p. 575.
changes might exacerbate 5-HT and DA system disregulation. Second, stress and/or cultural and societal pressure might contribute by increasing anxious and obsessional temperament.” This second requirement, which draws our attention to the complex relationship between the socio-cultural context of the individual and neural functioning, we might want to call an “external factor”. Of course, feminists may want to argue that there is nothing “external” about such a factor, and that it is as fundamental as the problems which find their root in 5-HT disregulation. Such matters are not of primary concern—at least not for now. Since I propose a that inter-disciplinary dialogue is essential to even begin to understand such questions about what is “fundamental” and what is of, say, “higher-order”, I am able to relegate these concerns to that very dialogical process. For now, what is fundamental is merely what is methodologically necessary: which is how we can get the dialogue in question off the ground. For that, feminists need to better understand what is happening in the brain.

We find prerequisites which tend towards dialogue in at least two varieties: Firstly, is the common object of study, namely the eating disordered body, however, this is not as promising a place to start as one might think, partially because there may be very real disagreements over the ontological and conceptual commitments one adopts as scientist versus feminist critical scholar. This does not preclude discussion, but it does mean that much of the discussion might degenerate into the different proposals for different ontological frameworks, which may lead to impasse, or at least stagnant discussion. The second route is perhaps more promising. As alluded to above, it comes from a shared observation of the behaviours associated with AN and BN. In a sense, these

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observations of behavior form a neutral (because not explicitly theoretical) platform from which discussion about the behavior (interpretations of the observational data can occur). This means that the feminist can merely concern herself with just as much of the neuroscientific theory as is mobilized by the scientists to explain, or even just hypothesize, about the causes of the abhorrent behavior in question. Here, the most current neuroscience is clear: because AN is associated with 5-HT and DA disregulation, we should expect to see behavior consistent with the disregulation of those systems, and we do. Kaye et al. cite restricted eating behaviours, behavioural inhibition, and “a bias toward anxiety and error prediction” when it comes to disturbances in a 5-HT system, and “altered response to reward” when it comes to disturbances in a DA system.\textsuperscript{119} Also, Kaye et al. note that these personality traits that make an individual more susceptible to developing AN are not always negative. They “…include attention to detail, concern about consequences, and a drive to accomplish and succeed.”\textsuperscript{120} In fact, the authors go on to say that “[i]t is our clinical experience that many individuals who recover from AN do well in life.”\textsuperscript{121} They then speculate that behaviours related to the ability to “plan ahead, control impulses, and avoid harm”\textsuperscript{122} might have been of adaptive value for our ancestors, who would have had to have gone long periods of time without proper food supply.

This is interesting to the feminist because these are personality traits that are fostered in adolescent women. Because these behaviours are socio-culturally prescribed and rewarded, the feminist can explain why eating disordered behavior (the actual restraint from consuming calories and/or binging and purging) is so prevalent within the

\textsuperscript{119} W. Kaye et al. (2009a), p. 581
\textsuperscript{120} Ibid, p. 581.
\textsuperscript{121} Ibid, p. 581.
\textsuperscript{122} Ibid, p. 581.
populations most afflicted by the disease. The feminist who knows just a little bit more about the neuroscience we’ve surveyed above will also be able to say more about why the disease affects only some adolescent women who have the attenuate behavioural traits, such as obsessionality, perfectionism, drive for success, behavioural inhibition, etc. The feminist theorist who thinks about anorexia or bulimia as the expression of a deep-seeded socio-cultural narrative about how women ought to be find an ally, not an enemy in the neuroscience. Most significantly, though, they are able to refer to the role of disregulated 5-HT and DA system functioning to explain what one might consider a somewhat embarrassing blindspot in their theory: a mechanistic explanation of why some young women who are exposed to the same prescriptions and reward structures get these diseases, while other women do not. These considerations change the order of explanation in a number of ways that the feminist should welcome. No longer is the sufferer of an eating disorder the simple product of socio-cultural pressures and unattainable beauty standards. No longer is it possible to characterize her as more weakly willed or more easily influenced by media images, performance expectations, family pressures, or the beauty myth. We treat these diseases as the complex things they are. They are a confluence of multiple factors, some socio-cultural, economic, and psychological, some genetic, neurological and chemical. As Kaye et al. make clear, the scientist must recognize (even if only vaguely) the importance of socio-cultural influences. What has been surveyed here indicates that feminists must leave some role for mechanistic explanations of some factors in the development of these diseases. Compare to Emily Martin regarding the societal rewards for the behaviors that accompany mania and depression (more so the mania part). I don’t want to pursue this in any detail, but it
might be the case that this sort of complex social reward system for the behaviours listed above are similar for a wide range of complex disorders.

**III.ii Feminism, behaviour, and two prescriptions for how to do research**

I must reiterate, even if it seems repetitive, that this recognition of multiple factors is not a retreat for the feminist into a kind of scientism. The scientific theory and evidence is utilized insofar as it can be informative to studies of the disease in the social sciences and the humanities. There is something of a template for how to integrate scientific ideas into social sciences and humanities research in the work of Emily Martin. Her research is on mania and depression, but the affinities are illuminating. I quote her at length here, to bring to the surface how the non-scientist can incorporate scientific findings into her research without doing damage to the autonomy of her home discipline.

Will I be claiming that manic depression is not “real”? Not at all. I will claim that the reality of manic depression lies in more than whatever biological traits may accompany it. The “reality” of manic depression lies in the cultural contexts that give particular meanings to its oscillations and multiplicities. Will I be claiming that people living under the description of manic depression do not need treatment? Not at all. I will claim that whatever suffering attends the condition should be treated by any means possible. But I will also say that manic depression is culturally inflected: its “irrational” heights and depths are entwined in the present-day cultural imagination with economic success and economic failure. This is a central reason, as we will see, why manic depression’s triumphs and failures hold very different kinds of promises and threats for those in powerful social positions compared to those in weak ones.\(^\text{123}\)

The feminist will always be able to say that any complete understanding of complex disorders is inextricably bound with the socio-cultural and economic contexts in which the disease is embedded. But, as Martin shows us, this is not to construe the disease as having a fundamentally social ontology—that we must consider biological and social

\(^{123}\) E. Martin (2007), p. 29.
characteristics of the disease when we think of its reality. Would anorexia and bulimia (not to mention mania and depression) be the same sorts of diseases if put into other social contexts? Likely not. Perhaps they wouldn’t even be considered diseases, just as manic behaviours (and maybe even psychopathy!) amongst the economic elite is not seen as diseased behaviour. But this does nothing to mitigate the significance of the scientific findings, and nothing to mitigate the methodological importance of studying the phenomena from many disciplines. In fact, as we have seen, the underlying science becomes more important as part of an integrated and pluralist account of complex disorders once we recognize its explanatory limitations—but once we also recognize the explanatory limitations of our home disciplines as well.

Martin’s research is not yet a complete template for the kind of integration of feminism and the sciences that I am talking about here. Martin deals with the importance that brain chemistry and genetics play in terms of the development of mania and depression, and subsequent role that such scientific determinants play in the way a patient living under the diagnosis “manic-depressive” experiences her disease. Dealing with the brain science in any real detail, however, is not part of her project. She clarifies, “[c]ultural aspects of knowledge from the brain sciences are not a major focus of this book, largely because this knowledge, though central, did not play a dynamic role in the main settings of my fieldwork.”\(^{124}\) She goes on to explain that the important contribution of genetic and brain determinants was simply assumed by those she engaged with both inside, and those outside of the medical community.\(^ {125}\) Her main focus is on ethnographic findings from interviews and interaction with people who are manic-depressive, with

\(^{124}\) Ibid, p. 11.

\(^{125}\) Ibid, p. 11.
special attention being paid to how the disease is categorized in the DSM and how it is
differently diagnosed across cultures. The hard data of the brain sciences or genetics is
merely referred to in passing, comprising only two pages of the book, much of which is a
justification for why she is not dealing with the hard sciences where others who talk
about mania and depression do.

Nonetheless, Martin’s work is important for two reasons. Firstly, it at least
acknowledges the complexity of a disorder like manic-depression, and acknowledges that
we cannot make sense of such a disease without talking about how scientific and socio-
cultural understandings of the disease are intertwined. Secondly, while she does not get
into the hard data offered by the brain sciences, she is careful to recognize their relevance
for her ethnographic and cultural study, including how they affect one’s experience with
the disease. Knowledge of neurotransmitters and genetics can have both a negative and a
positive effect on those living under the description of the diagnosis. For some, such
information can be deeply troubling, for it alters the way they see themselves and the way
that they see themselves through the eyes of others, i.e. as irrational, abnormal,
unbalanced, erratic, etc. For others, such information can be liberating. Martin uses her
own personal experience as an example when she says, “I often heard from my
psychiatrist that my problems were related to my neurotransmitters, and I always found
this comforting. I took this to mean that my problems were not entirely within my
control.”\footnote{126 Ibid, p. 13.} As important as the recognition of the science behind manic-depression might
be for Martin, we have yet to see an example that goes beyond a form of feminism that
just makes space for brain and genetic determinants—we have yet to see a feminism that
fully engages with the hard data of the sciences. And, it is no surprise that some feminists
might be wary of Martin’s personal anecdote just surveyed, in which inclusion of the sciences (minimal as it may be) seems to come with at least a hint of the biology is destiny paradigm, allowing the agent to become a patient of her own biological predispositions. A more complete template for our proposed form of feminism is found in Elizabeth Wilson, and will be dealt with in Section V below.

For now, we must acknowledge that feminists are right to have two looming worries about why they should engage with the sciences at all, let alone in more robust ways than we have seen with Martin. Firstly, the feminist would be right to question why it is she needs to become proficient in the vocabulary and methods of neuroscience to talk about eating disorders. It is always easy to say that one should know more about one’s area of study, especially when that area of study is a fundamentally interdisciplinary one. But exigencies of tenure and promotion, attempts to publish one’s work, departmental and university service duties, not to mention the seemingly endless flow of information one is expected to know mean that scholars are spread very thin. This is perhaps even more pronounced for feminist scholars who often find themselves spending a great deal of time justifying their research to colleagues and administration in ways that those working in more established branches of academia do not. Where is one to find the time to read neuroscientific studies, let alone the time required to train oneself in understanding what they mean. We thus arrive at our first methodological prescription, one that emphasizes the minimal commitments of the non-scientist when it comes to engaging with the sciences.

Methodological prescription 1a: we feminists must realize that we’re part of epistemic communities. Not all feminists need to acquaint themselves with the minutia of scientific studies. That being said, some feminists do need to have cognitive expertise, and feminists (generally speaking) should be encouraged to
develop a minimal understanding of the sciences related to their areas of study, perhaps taking as their benchmark a familiarity with review articles and the discussion sections included in most scientific publications.

This means that many feminists, especially young scholars, can survey much of the literature that might impact their studies without an onerous time commitment that would make the dialogical relationship I am advocating for here otiose. Of course, this minimal expectation is not sufficient on its own. It only makes sense as a minimal requirement for good research against the backdrop of a collaborative epistemic community in which some feminists (again, likely more senior in academic rank) have an impressive and hard earned cognitive expertise.

Pursuing methodological prescription 1a also has dialectical consequences. It places a demand on the scientists who now have a new audience to craft ever clearer and more informative sections of their papers for the nonscientist, though still academic researcher. Thus, we can formulate the converse to 1a:

Methodological prescription 1b: When scientists have their audience expanded to include non-specialists it behooves them to include in their academic publications a brief overview for the non-specialist.

Of course the existence of methodological prescriptions 1a and 1b also places demands upon the ways in which those in the humanities and social sciences go about constructing their work. We thus propose a second set of methodological prescriptions:

Methodological prescription 2a: Feminists and others working within philosophy and similar disciplines (on objects of study that are also studied in the sciences) should include expanded abstracts (or something akin to a key word section) indicating how their work might be fruitful or interesting to the scientist, perhaps even discussing those domains in which the scientist might apply the relevant ideas and concepts.

Methodological prescription 2b: Rather than making vague and passing references to the role that social and cultural context plays in the assessment of complex
disorders, at least some scientists can have a more robust knowledge of current humanities and social sciences research, as per methodological prescription 2a.

Of course, the latter part of this prescription will probably only be possible after a significant amount of dialogue between the scientist and the nonscientist has already taken place—and as a critical mass of feminists with the hard-earned cognitive expertise forms. There’s no guarantee that this will happen, though some of the feminist philosophers of science I survey in this work, especially in the Conclusion, give us hope that a critical mass of expertise is near. I argue that this is one of the ways bridges can be built between the naturalist and the constructivist camps. And building such bridges is, after all, the point of this project.

Now I want to move on to the second worry the feminist likely has about our proposal to incorporate the best science into feminism. This worry, introduced already, comes from the heritage of the biological sciences to stand in support (if only partial) of systems of white male dominance by portraying such dominance as “natural” and determined by sexual and racial characteristics. Feminists, based on the history of biological narratives, have every right to be wary of the possible misuses and abuses that can enter the fray when scientists begin to talk about topics intimately tied to sexuality, gender, and race.

But, as we have already noted, the feminist and the neurobiologist are not as theoretically “far apart” as they may seem. In fact, they often are puzzled by the same questions, and, as we have seen, they learn from each other how to better formulate these

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127 I don’t want to make the claim that this is the only way that there can be congress between scientific and non-scientific disciplines. There is, of course, congress between philosophy and psychology in neuroethics and also a history of collaboration in philosophy of mind with cognitive and perceptual psychology, work being done in artificial intelligence and work being done on cognitive modeling. These are all worthwhile pursuits, but none seems to make explicit, as does this study, how feminist concerns might be best met by a meeting of scientific and non-scientific disciplines.
questions and how to better come up with answers. This state of affairs can only come about once the necessary condition is met that science be responsible to, and responsive toward, cultural concerns, and when feminists drop the dialogue blocking “biology is destiny” thesis (in its more aggressive, necessary form). Both camps find themselves puzzled over why some people develop eating disorders while others do not. Like the cultural theorist, and the feminist, the scientist also recognizes that there are “external factors” that contribute to an individual developing an eating disorder, even though they may disagree on the degree to which such “external factors” are responsible, or even regarding what constitutes an “external” factor, and why we might want to call it that. Feminists might be able to provide insight into questions concerning the contribution of sociocultural factors to the development of eating disorders. Feminists are also in an ideal position to offer a gender sensitive, critical perspective to scientific study and data. Such a perspective is especially important when the object of study is one that has a gendered history, and social meaning.

Before we can really delve into the matter of establishing communication between cultural theorists and neurobiologists we need to say something of scope. Specifically, we must recognize (without being uncritical about it) that biology’s empirical success and cultural cache make its role in discourse about the body paradigmatic. This recognition is, in itself, possibly upsetting to the feminist who is worried that biology may return to its old ways. Nonetheless, as a practical matter, it is more important for the feminist theorist to be able to say how her work is commensurate with the science, less so than the vice versa. We say “commensurate” rather than “consistent”, because the demand for consistency is both too strong on the one hand and too weak on the other. On the one
hand, it is too much to ask the feminist to be consistent with the sciences and their dictates, since the feminist critic might want to deny certain central claims made by scientists about the value-neutrality of scientific theory, or claims by some scientists regarding knowledge as the product only of scientific experimentation, etc.; consistency with scientific discourse is far too strong a criterion. On the other hand, it is too weak because feminist work that is detached from scientific data altogether is also consistent with said scientific data, but this is exactly what we do not want. What we are striving for, and something that is sorely lacking in much current research, is a kind of *commensurability* between scientist and feminist critic—an inter-communicative paradigm within which critical discussions can take place, ensuring that the main interlocutors are not merely speaking past each other.\(^{128}\) For the feminist, being commensurable with the sciences means (at minimum) properly articulating and justifying problems she may have with the scientific data or research, and thus (at least on occasion) using language the scientist can understand.\(^{129}\) Of course, this is a *first* step. For the practical reasons addressed above, it must surely be the feminist who must make this leap. Nonetheless, the methodology is still a two-way street; the feminist, having taken the leap, then rightly has an expectation that her voice will be heard, and her concerns addressed, by scientists working on eating disorders and other complex diseases. If

\(^{128}\) Of course, I am borrowing Kuhnian language here, but I mean something slightly different by “paradigm”, for in Kuhnian “normal science” there isn’t room for critical reflection, especially regarding anything that might count as a foundational matter. Perhaps a better notion would be Imre Lakatos’ “research programme”, since research programmes do the job of organizing and ordering the sciences, just like paradigms, while also allowing for competing and mutually critical discourses to form.

\(^{129}\) For this reason, Longino’s goal of reaching objectivity via the development of community standards is perhaps too strong. I do not want to suppose that dialogue between feminists and natural scientists will lead to agreement, even about community standards for scientific practice, though it may in some cases. We do not need to constitute objectivity in order to engage in discussion, and we do not need to make objectivity (i.e. agreement about the shared object of study) a goal of discussion. The two concepts, dialogue and agreement, are divorceable. Of course, structured dialogue is arguably the best way of achieving agreement, and also the best way of achieving objectivity.
scientists don’t respect the voices of women under such circumstances, then they are not doing good science, and they can rightly be criticized for their errors, which, using these criteria, can be clearly articulated.

But there is certainly a risk in taking this leap—a level of risk so inherent that the methodological move called for in this essay cannot but help be called a “leap”. Why should the feminist be so ready to incorporate the findings of the sciences? After all, as we have established, it has certainly been a fact, made clear by the historical record of biology in practice, that women have not faired well. Biology has been used as an alibi to naturalize gender traits, traditional gender roles, heteronormative assumptions about sexuality and its place in reproduction, the perceived inferiority of women (in physical as well as cognitive attributes). This is not even to mention underlying assumptions about the secondary status of women vis-à-vis medicine (e.g. the male medical norm in pharmaceutical developments) or other more specific episodes of sexism, heteronormativity, and racism in clinical research, too numerous to include in our study here. What matters for us is this: all such episodes certainly militate against the critical feminist hitching her research to explicitly scientific modes of thinking and doing. The feminist may well wonder why it is we are seemingly providing prescriptions for becoming part of the problem: androcentric science. This worry is rational, and I must have a response to it if the current project is to be taken seriously.

It is important to recognize the pull of the biology is destiny thesis, and the role that it played in directing many forms of recent feminism away from the sciences, and towards culture. Because of the historical veracity of the thesis, feminists were motivated to reflect on the various ways in which biology played a sociocultural role in maintaining
systems of dominance based on sexuality, gender, and race. The feminist found herself between a rock and a hard place if she wished to engage in liberating, or ameliorative theorizing. It was not good enough to simply move away from biological determinism if it was merely to be replaced by a sociocultural determinism. Either way the oppression of women and minorities, and the dominance of men, was either natural or naturalized. No space could be found from which women and other marginalized voices could be adequately represented.

The feminist theorist studying the body has long preferred engaging with the ideational body over the biological or neurological body. In Chapter Two we looked at Elizabeth Wilson’s account of how this tendency of feminists to offer an explanation of the body rooted in this ideational realm is directly related to Freud’s preference for this kind of explanation. This practice, Wilson argues, has lead feminist theorists to naturalize a kind of anti-essentialism in their studies of the body. The ideational self became central to feminist studies. It opened up the conceptual space that was missing from purely biological or neurological accounts. Of course, this self (following Lacan and Foucault) was a socially constructed entity. But, as early as the work of Foucault, society and its institutions and its constitutive discourses, came to be seen as at least somewhat malleable, and certainly in flux. Foucault’s careful archaeologies and genealogies exposed our shifting attitudes, ways of thinking and doing, in relation to madness, sexuality, and criminality. This model of the self was heavily based on a certain strain of poststructuralist feminism perhaps most exemplified by Judith Butler’s work on gender and gender performance.

\[^{130}E. \text{ Wilson (1998), p. 15.}\]
Butler’s work is a rejection of the biology is destiny formulation of the self, and its ontological assumptions. Butler employs the term “the metaphysics of substance”\textsuperscript{131} to describe the idea, stemming back to Aristotle, that certain attributes of the self are not contingent “accidents”, but necessary and essential. The phrase itself comes from Nietzsche, who was the first to critique this long-standing view of the self. Butler adopts Nietzsche’s anti-essentialist view of the self, arguing that the metaphysics of substance is but an illusion produced by the seamless formation of the self through its actions.

Even when gender seems to congeal into the most reified forms, the “congealing” is itself an insistent and insidious practice, sustained and regulated by various social means… Gender is the repeated stylization of the body, a set of repeated acts within a highly rigid regulatory frame that congeal over time to produce the appearance of substance, of a natural sort of being.\textsuperscript{132}

But here the presupposition is the dichotomy between biology is destiny and culturally influenced performativity; between the metaphysics of substance and a view of the self that is more fluid.

\textbf{IV. The Promise of Neuroplasticity}

In this section I want to make two claims. Firstly, neuroplasticity shows us that there is a way to think of our biology in such a way that it does not become our destiny. Secondly, neuroplasticity may in fact have implications for research on eating disorders. Both claims together are promising for feminists.

As long as this dichotomy introduced in the final paragraph of the last section (biology-is-destiny vs. culturally influenced performativity) is taken to be fundamental to feminist theorizing, the move towards a cultural, semiotic and post-structural view of the

\textsuperscript{131} J. Butler (1999), p. 27.
\textsuperscript{132} Ibid, p. 43-44.
body and self seems to be prescribed for the feminist. However, I argue that we have reason to challenge this dichotomy. While the feminist is right to worry that biologically deterministic narratives may do great harm, this worry can be assuaged by examining some of the massive leaps that have taken place in the past two decades in the sciences—changes that began in neuroscience proper in the late 1960s and early 1970s. During this period, the machine metaphor of the brain (as a static system of integrated and highly specialized parts operating deterministically and automatically) came under question. Of course, there had always been dissenting voices in psychology and in brain studies to this canonical view—but it wasn’t until the last 45 years or so that certain phenomena (often regarding recovery from traumatic brain damage) could be credibly cited as “Neuroplasticity”—the brain actually changing its structure.133

These changes occur in a myriad of ways, due to experiential learning (both during and after what brain scientists call “the critical period” of youth), cultural cues, and more significantly via cortical remapping after traumatic brain injury. The mechanism is quite simple. The activities (and attenuate behavior) that one engages in turn on and off the genes in nerve cells, which make proteins that ultimately are able to change the structure of the brain. Thus, plasticity is not limited to early childhood development. The sorts of neuronal connections made in early childhood—connections that were fully accepted by mainstream neuroscience—are not different in kind from what we see in adults, especially in response to new and contrived stimulation, designed to remap the brain so that it can do something that it could not do before. Yes: these

133 For a nice historical overview, see the Preface to N. Doidge, *The Brain that Changes Itself* (2007), especially pp. xvii-xx.
connections occur more naturally and easily in childhood, but adult plasticity shows us the ways the brain can be altered by direct and intense exercise (mental or physical).

A case in point is Paul Bach-y-Rita’s work from the late 1950s and 1960s. Bach-y-Rita was the first contemporary scholar\textsuperscript{134} to recognize the power of plasticity—largely for personal reasons. His father, Pedro Bach-y-Rita (the Italian poet and scholar) suffered a massive cerebral infarction that paralyzed his face and half his body, and left him unable to speak.\textsuperscript{135} Rather than have his father attempt to walk, Paul Bach-y-Rita had his father recapitulate childhood tasks in the hope of having him re-learn skills normally associated with early childhood development. He had his father crawl, wash pots, and do light gardening, and “at the end of a year, his recovery was complete enough for Pedro, now sixty-eight, to start full-time teaching again at City College in New York.”\textsuperscript{136} After his father’s death five years later (due to a heart-attack), Bach-y-Rita had an autopsy conducted on his father. The brain damage was severe. A massive lesion (mostly in the brain stem) never healed, leading Paul Bach-y-Rita to conclude that his father’s brain must have re-organized itself—otherwise, the significant improvement in speech behavior and motion would be entirely inexplicable.

In the late 1960s, Bach-y-Rita also conducted experiments on sensory substitution. He took patients who were congenitally blind and attempted to teach them to “see” using tactile perception. As Doidge notes, “Bach-y-Rita determined that skin and its touch receptors could substitute for a retina, because both the skin and the retina are two-dimensional sheets, covered with sensory receptors, that allow a ‘picture’ to form on

\textsuperscript{134} The concept is mentioned by William James in his (1890), \textit{Principles of Psychology} but there is no significant research based on James’ ideas.
\textsuperscript{135} Doidge (2007), p. 20.
\textsuperscript{136} Ibid, p. 22.
them.”\textsuperscript{137} Using a chair with 400 vibrating points attached to a plate, hooked up to a camera, an “image” was conveyed to the brain via the skin. Patients were able to see shadows, read, discern the relative distances of objects, discern perspective, and even recognize a photograph of the super model Twiggy.\textsuperscript{138} This was further evidence that the brain is able to compensate through structural changes to overcome a limitation if properly stimulated and exercised.

Of course, these are merely some of the first applications of neuroplasticity. More recent research has focused on non-traditional treatments of deficits ranging from dyslexia to senility, from PTSD to OCD. What makes some of these therapeutic breakthroughs so profound is their simplicity. Simple association games can stunt the onset of senility (even reversing its behavioural effects in some cases). Even properly applied psycholanalytic therapy has been shown (using fMRI technology) to reduce the effects of panic disorder in patients suffering from anxiety. More specifically, abnormal activation of the limbic system as a response to threatening stimuli was reduced after psychotherapy.\textsuperscript{139} What is interesting about this is the possibility that such therapies could reduce the need for harsh pharmaceutical drugs; using thoughts instead of drugs certainly limits side-effects.

Initial congress between philosophical and scientific ideas might already have an obvious impact, especially for those who wish to discuss the body, but who have long thought that science leaves no room for talking about the body in nondeterministic and flexible ways. The growing literature on the importance of neuroplasticity provides evidence for the feminist that biology need not be destiny.

\textsuperscript{137} Ibid, p. 16.
\textsuperscript{138} Ibid, pp. 10-11.
\textsuperscript{139} Ibid, pp. 233-234.
In one way of illustrating the plastic capabilities of the brain, or the idea that the brain can change both in its structure and function as a result of what we do with it, Doidge examines the relationship between neuroplasticity and sexual attraction. Specifically, he uses the current Internet porn epidemic as an example that highlights the degree to which sexual tastes can be acquired. He claims that, “[p]ornography, delivered by high-speed Internet connections, satisfies every one of the prerequisites for neuroplastic change.”\textsuperscript{140} He refers to the constantly changing and varied nature of the pornography we find on the Internet as proof that sexual tastes cannot be purely instinctual, innate, and the simple product of evolutionary development. He explains, “…the content of pornography is a \textit{dynamic} phenomenon that perfectly illustrates the progress of an acquired taste.”\textsuperscript{141} What was once considered softcore pornography, i.e. “women in various states of undress”, is now commonplace on mainstream media.\textsuperscript{142} Hardcore pornographic material has gone through, according to Doidge, a similar change. “Hardcore pornography now explores the world of perversion, while softcore is now what hardcore was a few decades ago, explicit sexual intercourse between adults…”\textsuperscript{143} What’s important about this shift, in terms of drawing a connection with neuroplasticity, is twofold. Firstly, the fact that more explicit pornographic material is now part of the mainstream media means that young people with developing (and especially plastic) minds now have more ubiquitous and constant engagement with it, since they don’t have to seek out such material. Secondly, and this is what Doidge is really interested in exploring with this example, is the degree to which “the plastic influence of

\textsuperscript{140} Ibid, p. 102.
\textsuperscript{141} Ibid, p. 102.
\textsuperscript{142} Ibid, p. 103.
\textsuperscript{143} Ibid, p. 102-103.
pornography” can affect, and reshape, the brains of adults.\textsuperscript{144} This observation was confirmed in his own clinical practice, where in the 1990s Doidge treated a number of young men suffering from the same sort of addiction to pornography.

Doidge explains the profile of the young men he saw in his practice in the following way: “Each had acquired a taste for a kind of pornography that, to a greater or lesser degree, troubled or even disgusted him, had a disturbing effect on the pattern of his sexual excitement, and ultimately affected his relationships and sexual potency.”\textsuperscript{145} He goes on to explain that these men who became addicted to porn were generally well adjusted, i.e. not socially withdrawn, in successful relationships, etc. Each of them reported spending increasing amounts of time watching porn on the Internet and masturbating, which eventually led to problems becoming aroused by their partners (who they had previously had no trouble being aroused by).\textsuperscript{146} Moreover, Doidge explains, “[t]heir sexual fantasy lives were increasingly dominated by the scenarios that they had, so to speak, downloaded into their brains, and these new scripts were often more primitive and more violent than their previous sexual fantasies.”\textsuperscript{147} The important thing to note here is that the tastes of the men, or what it took to get them aroused, were changing, and these changes were being carried over into their relationships.\textsuperscript{148} The addiction to porn ends up playing out much like an addiction to anything else: more and more is needed to achieve the same high, the same pleasurable sensation. Thus, like any other addiction, plastic changes were occurring in the brains of the young men Doidge treated.

\textsuperscript{144} Ibid, p. 103.
\textsuperscript{145} Ibid, p. 103.
\textsuperscript{146} Ibid, p. 104.
\textsuperscript{147} Ibid, p. 104.
\textsuperscript{148} Ibid, p. 105.
Their brains were going through changes in the same way one’s brain changes as a consequence of a drug addiction: engaging in the addictive behavior (e.g. watching porn, doing drugs, binging and purging, etc.) activates the “pleasure-giving neurotransmitter” dopamine in the brain.\(^{149}\) Dopamine is associated with reward—it is released when we accomplish something.\(^{150}\) It is also, Doidge claims, implicated in facilitating plastic change. He explains, “[t]he same surge of dopamine that thrills us also consolidates the neuronal connections responsible for the behaviors that led us to accomplish our goal.”\(^{151}\)

Altered function of the DA system is also found in patients with anorexia, possibly contributing to “altered reward and affect, decision-making, and executive control, as well to stereotypic motor activity… and decreased food ingestion…”\(^{152}\) Because it is such an important neurotransmitter, we know that disruptions to this system can have profound effects. Over activity of dopamine receptors in the basal ganglia are significant because of this region’s association with experiential learning and decision-making. Disturbed levels of dopamine associated with restricted eating in AN and binge-purge behavior in BN have atavistic biological effects regardless of the removal of environmental cues. This might allow the feminist to explain why the cultural narratives that may very well have induced AN and BN behavior are so difficult to reverse, but we may also be in a position to apply the principles of neuroplasticity to overcome these seemingly rooted changes in the brain. Once again, there is no current research on the applicability of the principles of neuroplasticity to eating disorders. We are merely

149 Ibid, p. 106.
150 Ibid, p. 106.
speculating here that if the brain can change itself in adulthood so that it leads to diseased behavior, perhaps it can change itself in accordance with those same principles for the better.

We must remember that the scope of this current project is not to assert the usefulness (or not) of any specific scientific framework for studying complex disorders such as AN and BN. Rather, the scope of this project is to encourage the feminist that not all biological research is deterministic, and to encourage the scientist to take seriously slightly more sophisticated accounts of cultural influence, as regards complex disorders and their putative socio-cultural origins. It may be that neuroplasticity is a garden path, but that is not our immediate concern. What is our immediate concern is this: if neuroplasticity turns out to be an instance of barking up the wrong tree, then it will be a team of interdisciplinary scholars who determine it as such. If it is an area of fruitful research, the same interdisciplinary tribunal will make that determination.

**V. Integration: What feminism looks like when it goes scientific**

We have seen in the last two sections why feminists should not shy away from addressing the sciences in their research. In fact, as the last sections shows, feminism might find an ally in some of the most recent research on adult plasticity—both as an explanation for how brains, minds, and behavior change for the bad, and as a promising avenue for understanding how we may treat complex disorders via application of the principles of neuroplasticity. While there has been no significant work on neuroplasticity and eating disorders in particular, the conceptual framework, if it is able to be extended, offers us a number of exciting and novel possibilities for research (not to mention treatment). In this
section, however, I am going to speak more generally to how a feminism that incorporates the sciences might look, and what a feminist philosophical analysis of science looks like.

For an illustration, we will examine Elizabeth Wilson’s engagement with Simon LeVay’s 1991 study on sexual preference, “A difference in hypothalamic structure between heterosexual and homosexual men,” published in Science. Here, Wilson engages in the sort of study my brand of feminism is proposing. Though she is looking at issues of personal identity rather than illness, she offers us a good example of how feminists might fruitfully engage with neuroscientific data.

In his study LeVay looks at the size of the nucleus (a distinct anatomical part of the brain) in (presumed) heterosexual male cadavers, homosexual male cadavers (many of whom died of AIDS, and who were only identified as homosexual by their medical records), and (presumed) heterosexual female cadavers to determine whether there is a difference in size between the homosexual and the heterosexual male nucleus.153 Drawing on previous research that determined a difference in nucleus size between men and women (women’s nuclei are smaller), LeVay concludes that the nucleus is smaller in homosexual men and women, meaning it “is dimorphic in relation to sexual orientation rather than in relation to gender.”154

While it is clear that LeVay’s study is rife with risky data, LeVay, as Wilson points out, is careful to articulate how cautious one must be when interpreting his findings in his original 1991 piece. What LeVay preaches in word, however, is not necessarily followed in deed. Since 1991, LeVay has been “a prolific contributor to the

154 Ibid, p. 53.
further interpretation of the original data,” speculate widely about the biology of sexual orientation in popular venues including talk shows such as *Oprah*, a novel “about the scientific context and cultural implications of neurobiological research on sexuality”, and in legal testimony. Moreover, LeVay is not alone in his continued work with the data. Wilson tells us that there has “been much in the way of further interpretation of his report by academic, scientific, political, legal, and media commentators.” In other words: Wilson is not the first to engage with this scientific study.

According to Wilson, her critical approach to the LeVay study differs significantly from what has come before her. Many have been troubled by LeVay’s use of overly simplistic categories of *homosexual* and *heterosexual* and their presumed dimorphic relation. The case has been made that rather than employing a dimorphic analytical category, LeVay should have used a distributed analytical category in order to better capture the data with one’s model. This is especially the case, it has been argued, when one looks at the way the data is plotted graphically. The graph, if interpreted dimorphically, shows that homosexual male hypothalamuses are, on the average, slightly smaller than heterosexual male hypothalamuses. However, the “clusters” are far from convincing, and the “outlying data” could equally well be taken to show a flaw in the dimorphic interpretation (if not a deeper flaw with the collection of data and experiment itself). In fact, the data plots are such that “cluster” must be used very liberally. One could be forgiven for thinking that the data supports a distributed analytical categorization, perhaps offering evidence for the multifarious possible gender categories that might be posited by a spectral (as opposed to binary) view of male sexual orientation.

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155 Ibid, p. 105 (footnote 3).
156 Ibid, p. 105.
157 Ibid, p. 49.
Wilson warns against such interpretations of the data. Wilson explains that “this analytic choice (dimorphic vs. distributed) obscures a more useful reading of the data”, arguing that it is the “relationship between dimorphic and distributed forms that is the more instructive in LeVay’s study.”

For Wilson, unlike others who’ve engaged with LeVay’s 1991 study, the most important and interesting interpretation lies in figuring out this relationship. In other words: determining how to interpret data that clusters in a dimorphic way, while at the same time offers many “exceptions” that directly conflict with this pattern.

Wilson is not disregarding the problems inherent in dimorphisms. In fact, she claims that she is also critical of dimorphic sexual categorization. She cites Janet Halley’s important criticism of such a practice, which claims that LeVay did more than just employ the words “homosexual” and “heterosexual”; rather, the nature of these categories was ontological. That is, the categories “homosexual” and “heterosexual” constitute the entire range of sexual possibilities. This criticism, that the data should be interpreted in a non-dimorphic way, though important, is incomplete for Wilson. Instead, Wilson argues,

What LeVay’s data show is neither two discretely sexualized nuclei nor an aimless pattern of nucleic volumes. Rather, the data demonstrate a reticulating pattern, a complication of the disseminated (ranging) with the dimorphic (divided). In this reticulating structure neither of these patterns governs the field of neurological possibilities to the exclusion of the other. Instead, the data invite another, more difficult, interpretive challenge: to envisage how dimorphic patterns might relate to, be implicated in, arrest and cleave, but also be partially generative of, more distributed organizations.

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158 Ibid, p. 50-51.
159 Ibid, p. 54.
161 Ibid, p. 56.
Wilson is urging us to look at the data with fresh eyes, asking us to note that the data are not illustrative of a clear distinction between heterosexual and homosexual, nor is it illustrative of clearly debinarized sexualities. Here, the ontology does not come before the data, where one simply adopts the sexual categories one thinks are ontologically primitive, seeing if there is some problem with the imposition of said interpretation on the data. Rather, our ontological commitments (especially if we are empiricists) need to recognize how complex the data are in light of how complex the human brain is, and analyze the propensity of the data to undergird several seemingly different perspectives—each one illuminating in its own right, at least in the sense that each tells us something about the complexity of the systems (viz. brains) giving rise to the data. This is not to say that empiricism prescribes one interpretation over the other—as if one could deduce one’s ontology from one’s empirical data. It is to say, rather, that the complexity of the data should be an excellent indicator that one’s assumed ontology is likely too simple if it is presupposed before hand. In fact, we could probably raise it to the level of a methodological principle, as is common in much contemporary feminist literature, that binaries are to be held in great suspicion. But here, the scientific data is our guide as to what is wrong with overly simplified ontological presuppositions, and a guide to how not to engage in interpretation of the data.

Rather than asking us to contain our analysis to a cultural or psychological framework, Wilson wants us to stay firmly entrenched in the neurological domain itself when we generate an interpretation of the data. She explains that much of the popular commentary on the brain posits the following: “the brain is seen as either one of the most complex structures in the universe, or as an organ delimitable to simple, localized

\[162\] Ibid, p. 59.
constraints.”\textsuperscript{163} Drawing on such a reductive and straightforward theoretical framework, where a clear chasm is drawn between the simplistic and the infinitely complex interpretations of the data, will not yield the most fruitful insights. She explains,

\[i\]f the brain does, in places, coalesce into symmetrical and localized organizations, it does so only within a wider circuitry that reticulates this localizing symmetry. Likewise, these circuits are themselves constituted through an intimate adhesion to localized dimorphisms.\textsuperscript{164}

Thus, we should be careful not to read the data as either inherently dimorphic or inherently distributed. By breaking away from this tendency, Wilson argues we can gain a great deal of insight about the relationship between neurological substrate and sexuality.

In the end, Wilson was not interested in whether sexuality is dimorphic or fluid—at least that is not her primary interest. Engaging with both LeVay’s study, and his commentators, Wilson highlights the way feminists can explore the data, while bringing to it a critical and political agenda. Wilson learned that there is a way for the feminist to proceed with the data, rather than ignoring it, or explaining away its complexity in favour of a received ontology. By ignoring the data, feminists lack a foothold from which critique can be made efficacious. By adopting the rigors of empirical research (as opposed to imposing their favoured ontology) feminists can correct methodological oversimplifications and reveal blind-spots in current research, as well as perhaps discovering new ways to think about gender, the body, and the brain. One example might be putting an end to overly simplistic dichotomous thinking that pits “binary sexuality” against “fluid gender” in all domains in which we talk about sexuality, gender, and desire. Such universal application of this dichotomy has been a staple of third-wave feminism.

While the dichotomy is clearly interesting, it may quickly become a false dichotomy.

\textsuperscript{163} Ibid, p. 60.
\textsuperscript{164} Ibid, p. 61.
when applied to at least some data, as we have seen with Wilson’s interpretation of LeVay.

Cultural theorists often attempt to speak of mental phenomena by avoiding the brain, using terms of culture, economics, or semiotics. This allows social constructivists to reach what they take to be non-biological conclusions about the fluidity of gender and sexual desire. Such conclusions are only non-biological if we assume that biological accounts of the neural bases of such categories are binary, reductive and fixed (or the opposite: completely fluid, spectral, fluctuating, and distributed). Our best neuroscience (not LeVay’s study) tells us otherwise. It is true that the complex neural data that underlie sexual preference can often be simplified by placing them into dimorphic forms, e.g. ‘homosexual’ and ‘heterosexual’ preference. Cultural theorists are right to be wary of this kind of oversimplification of the data. However, their tactic of imposing what seems to be an equally foreign interpretation on the data is unjustified, as is the tendency to not even go this far—the tendency to ignore the data (as opposed to this or that assessment of the data). Quite often, the complex and intricate findings that constitute the empirical research are allies to feminist accounts of the fluidity of sexual preference, but such alliances should be discovered, not imposed, and be open to the exigencies and challenges of empirical data, not immune to scientific method.

As we have just witnessed, there is plenty of room—and a great deal of need—for the feminist and the cultural theorist to engage with the scientific process. Wilson highlights the importance of identifying and questioning assumptions operating in scientific study. For example, she illustrates the danger in thinking that data showing a statistical clustering with some outlying points must always be interpreted as either
dimorphic or distributed. Instead, we might conceptualize such data as both dimorphic
and distributed, complicated and connected. It is up to the feminist and the cultural
theorist to ask questions about what criteria are being relied upon in both scientific
studies and critical analysis of said studies. To this end, in a study about eating disorders,
for example, the feminist theorist might want to inquire about what criteria were used in
determining who would be eligible to participate in the study (e.g. DSM criteria).
Drawing our attention to criteria that might otherwise go unquestioned not only opens up
new theoretical possibilities, it also establishes a potentially dialogical connection
between the feminist and the scientist, adding a level of sophistication to the scientific
study in question.
CHAPTER IV. SCIENCE, VALUES, AND DIALOGUE:
An amendment to Longino’s philosophy of science

I. Introduction:
In the previous chapter we looked at the ways in which feminists might utilize neuroscientific studies to inform their research on eating disorders. It became clear that there is much that the feminist can learn from taking what amounts to little more than an intermediate interest in scientific findings. We saw that there were two outcomes of feminist forays into the neuroscientific literature. Firstly, the feminist puts herself into a position where it is possible to see properties of the diseased body that would have been invisible to her without the modern scientific machinery of imaging technologies and neurochemical and neurobiological theory. Secondly, we saw that there is a strategic benefit for the feminist who engages with the sciences in the terms used by the sciences. Specifically, we saw that this opens up a dialogical space that demands, on pain of scientific rationality and incommunicativity, the scientist pay attention to her. (It is not clear that the scientist is under any obligation to listen to the feminist or the cultural theorist who talks about the body in radically different ways than does current neuroscience.) Strategically, if the feminist takes her goal to be a political one, i.e. placing feminism in a position so that it may have ameliorative effects on how the sciences operate, especially as regards their operation upon gendered selves with complex disorders, then the feminist must function under a methodological principle: one that points to the creation of a dialogical space in which the scientist is obliged to pay attention to the importance of cultural, social and valuational dimensions of diseased bodies, their diagnoses, their characterization, and their treatment.
We also saw that feminists might find an ally as opposed to an enemy in the contemporary neuroscientific literature. Those neuroscientists interested in neuroplasticity have focused on the brain’s adaptive ability to compensate for diminished function (often due to brain damage in specific regions of the brain). This has called into question strong forms of the localization thesis—that certain areas of the brain are solely responsible for certain cognitive, perceptual and motor function. What studies into neuroplasticity have found shows the brain to be a much more flexible entity, able to compensate for localized damage. We can no longer attribute to science writ large the view that identification of the self with the brain and the body is a deterministic, reductive, one-way, and static relationship. The feminist who likes to think of the gendered self as dynamical, fluid and flexible finds allegiance amongst this sub-set of neuroscientific researchers. In short: the ontological status of the mind-brain-body relationship has been problematized by neuroscience itself in ways amenable to integration with critical feminist views of the self.

This chapter is concerned with enabling conditions for uptake of transformative criticism, and also situating this dissertation within the larger community of feminist epistemologists and philosophers of science. Some excursions into this terrain have already occurred, especially in Chapter Three, Section II where I hinted at how my view differs from Longino’s as a form of critical empiricism, and in Section V where I showed my methodological prescriptions to be more aligned with those we find in Elizabeth Wilson’s work. In this chapter, I expand on my treatment of Longino in the current chapter, pointing out the nuances of the similarities and points of departure with her work. (I return to a discussion of Elizabeth Wilson in the Conclusion to this dissertation.)
The purpose of this chapter is three-fold. First, I attempt to explicate a new way for thinking about how political and valuational questions can be made front and centre in scientific research, and the role of feminists in bringing about these changes. This is the key insight from existing feminist work in the philosophy of science over the last 30 years, an insight which I wish to preserve, though I want to divorce it from the general postmodern turn away from Enlightenment values in science that has been advocated by many feminists. I take Sandra Harding’s highly influential text, *The Science Question in Feminism*, as an exemplar of such an approach, if only because it provides such an excellent snap-shot of the state of the field at the turn of the 1980s, when much work in feminist philosophy of science was gaining momentum. Second, I borrow from Longino’s feminist empiricism to show how Longino offers an example for how we might develop a feminist philosophy of science that does not reject the broader empiricist outlook definitive of Enlightenment science, while still allowing for a feminist focus on gender equality in science. Where Harding calls for a post-Enlightenment feminist science, and feminist philosophy of science, Longino is sceptical that we need to overhaul science from the roots up to properly accommodate women and other marginalized people.

Third, I amend Longino’s notion of “tempered equality”, which she proposes as a precondition for uptake. I suggest that this gets the picture backwards. Tempered equality is not a precursor or precondition for uptake, rather uptake is a precondition for tempered equality. In other words, feminist theorists seeking uptake from neuroscientific researchers of eating disorders have the unenviable task of seeking uptake from positions of socio-economic and intellectual marginalization. Nonetheless, I argue that they are
capable of exploiting communicative dynamics in order to have their views received and seriously addressed, by adopting modes of discourse more suited to the scientist than her allies in philosophy departments or cultural and women’s studies programs. Where I depart from Longino is this: For Longino the solution rests in reforming scientific institutions so that a plurality of voices can engage in the traditional empirical pursuit of better theories and better knowledge. I hold out only minimal hope that such political reform at the institutional level will be effective, and share in some of the postmodern skepticism that such a solution risks populating our institutions with women and minorities who are nonetheless compelled to perpetuate the same narrow, allegedly value-neutral conception of the sciences normally associated with dominant, patriarchal, misogynist, capitalist, and racist practices. I will argue that her methodological framework for bringing women into the sciences falls short of explicating the mechanism by which critical feminist voices can be heard by the sciences. In response to these concerns I try to show how my dialogical form of critical empiricism gets us the best of both worlds: the postmodern focus on the centrality of the political as well as a commitment to a more sober empiricist methodology and a more effective understanding of how values (political and otherwise) operate in the sciences. The outlines of my brand of critical empiricism are on offer in Section IV.

II. Harding and the political

Given that this is not a work in the history of the philosophy of science, it might seem slightly incongruous to devote the first section of this chapter to an historical episode in the late 1970s and early 1980s captured in Sandra Harding’s canonical book The Science
**Question in Feminism.** The focus on this episode is important for conceptual reasons, and important for the purposes of situating what can be seen as two forms of feminist philosophy of science. The first is a postmodern critique of Enlightenment science. The second is a more concerted effort to discern the ways in which institutions, current practices, and attitudes might systematically preclude the recognition of alternative voices in the functioning of science.

I will first address the postmodern critique of science, utilizing Harding’s work as an important touchstone, granting that it plays an important role in directing our attention to political questions, and maybe even political foundations, in science. However, I will argue that Harding’s gloss of postmodern feminism betrays a confusion regarding the relationship between feminist critique and science itself, implying a vantage point on science that is impossible to achieve if one adopts the monolithic view of science that many feminists have taken to be their target. After this examination of Harding’s work I will then highlight some of the ways in which Longino’s more measured feminist empiricist critique of scientific institutions acts as a corrective for some of the excesses of Harding’s program. Nonetheless, I will show that Harding’s view needs to be more sensitive to some political problems addressed by postmodern feminists, but more significantly, is in need of slight reform regarding her social norms for a functioning science that is inclusive of transformative critique.

Sandra Harding’s work on the history of science and its interface with philosophical development in *The Science Question in Feminism* has been a touchstone for feminist philosophy of science since its publication in the mid-1980s. The work brings together literature by feminists in the history and philosophy of science, sociology,
and other critical science studies, which she sees as converging upon a central idea: that our culture is a scientific culture, one in which scientific forms of rationality and scientific modes of inquiry have become unassailable and authoritative. Our culture is scientific in another sense as well. Scientific ways of thinking and doing have permeated every sphere of human life, including economics, sociology, history, political economy, anthropology, and other social sciences, not to mention the more “exact” or “hard” sciences, including physics, chemistry, biology, and medicine—the order of this list intentionally progressing from “hardest” to “softest”.

This scientism is in no way problematic in its own right, but because of the unassailable and authoritative standing of the sciences, Harding takes core issue regarding scientific rationality as being beyond criticism. Critiques of science are rare and normally directed towards the periphery, not the core of science. Harding’s aim is to bring together feminist critiques of science without attempt to synthesize away their differences or infelicities. She wants to isolate

important trends in the feminist critiques of science with the aim of identifying tensions and conflicts between them, inadequate concepts informing their analyses, unrecognized obstacles to and gaps in their research programs, and extensions that might transform them into even more powerful tools for the construction of emancipatory meanings and practices.¹⁶⁵

Through this “survey”, the work aims to expose the deeply political nature of scientific thought and practice, thereby exposing the androcentric assumptions that have operated without criticism since the Renaissance—especially the assumption that science operates best when it is objective, impartial, rational, and neutral (i.e. value-free). The centrality of science in the modern West has seen it become an alibi, an aid, and confidante of masculinist society.

At the centre of the androcentric assumptions that structure science is the view that science is a totalizing enterprise. This idea found its roots in the logical positivist Unity of Science thesis, a thesis heavily informed by Wittgenstein’s dictum in the Tractatus that “the world is a totality of facts, not of things.” Because science is taken to be totalizing, it alone expresses these facts, which are all the facts to be expressed. The Unity of Science thesis holds that all meaningful discourse is fundamentally scientific and in principle open to empirical verification. Not only are higher-order sciences (or at least their factual core) reducible to an idealized physics, but all forms of discourse as well, including all talk of human emotions, political, aesthetic, ethical, and religious values. All is reducible to science, or is ultimately meaningless metaphysical nonsense.

While the logical positivists were the first to explicate a totalizing thesis in this Tractarian language, Harding does much work to show that the sentiment runs throughout the empiricist tradition and beyond. What is new in the Vienna Circle’s Unity of Science thesis (Unity, for short) is not the explicit thesis that all of our knowledge is reducible to sensory facts; rather, what is new is the notion that all facts are at bottom scientific, and at bottom reducible to facts of a fundamental science (physics). The sciences exist in a hierarchy with physics at the base. Its concepts and objects are the most foundational, its modes of reasoning the most fundamental. This idea was developed most fully in the logical positivist doctrine of physicalism—originally a thesis of Otto Neurath and then systematically propounded by Rudolf Carnap. According to Harding,

[f]or the Vienna Circle, the sciences formed an ontological and methodological continuum, a hierarchically arranged ordering that placed physics at its pinnacle, followed by the other physical sciences, then the more quantitative and “positive”
social sciences (economics and behaviorist psychology were their models) leading the “softer” and qualitatively focused ones (anthropology, sociology, history). Unity, for Harding’s positivist, is so attractive because of an underlying assumption that belies a commitment to an Enlightenment view of science as central to all intellectual (for the positivist read: scientific) pursuits. For this reason it might be supposed that Unity is suspicious for those seeking emancipation from what they perceive to be Enlightenment scientism.

But is Unity necessarily wed to Enlightenment scientism? Harding isolates, though never fully endorses, another postmodern and feminist form of Unity that survives the turn away from Enlightenment thinking, one that preserves much of the basic structure of the Vienna Circle’s conception of the sciences, including the continuum of sciences, and the hierarchical structure of the sciences. This Unity is not the subject of feminist criticism, but a product of it (according to Harding). On this popular re-ordering, the continuum we get in the traditional Unity thesis is put on its head. Harding notes that, for many feminists, “this thesis is asserted both as a description of what in fact is the case in the sciences and as a prescription of how the sciences should be ordered. It has been and should be moral and political beliefs that direct the development of both the intellectual and social structures of science.” Our research projects, on this view, are to be assessed primarily according to their moral and political motivations. The most worthy scientific pursuits—or those that are most objective—according to the feminists Harding surveys, are those aimed at “eliminating sexist, racist, classist, and culturally coercive understandings of nature and social life.”

166 Ibid, pp. 249-250.
No field of inquiry—be it physics or anthropology—is free from politics, and remembering this is crucial if we want to (a) do good science, and (b) reform androcentric scientific institutions. In fact, the modes of argument and reasoning we find in moral and political discourse (disagreement about values, what course of action should be taken, what matters more: collectives, or individuals? etc.) infuse all sciences—in obvious ways for the “soft” sciences like anthropology and history, but also in the harder exact sciences. In other words, the reversal of classical Unity occurs when feminists make explicit the counter-Enlightenment thesis that objectivity is not value-neutral; objectivity is rather characterized as a recognition of the operation of values within all scientific inquiry.

While feminists put at the foundations of science a discussion of political concerns, feminist Unity is similar in many ways to classical Unity. Feminists, like classical empiricists and their neo-Kantian variants, are concerned with a characterization of objectivity. This is one of the grand Enlightenment projects, and one that was thought settled, at least in large part, by a history including the Copernican Turn, the development of Newtonian physics, galvanic chemistry, and evolutionary biology (not to mention the many ways in which the project is extended to include the prediction and control of human populations via statistics, demography, regimes of hygiene, and politics). We achieve objectivity when we cease to ignore “coercive values” (e.g. racism) and promote “participatory values” (e.g. antiracism) in the sciences. Both of these projects can only proceed when the philosopher and the scientist admit that science cannot be value-neutral.169

169 Someone might think that we shouldn’t abandon efforts to be value-neutral, but this is exactly the sort of effort we should abandon. Science cannot be value-neutral and this is not something to strive towards.
While the feminist *Unity* is not explicitly Harding’s view, it nicely captures the structure of the literature at the time of her writing of the book, and this central feature of emancipatory feminist work structures how she considers the science question in feminism as an essential reformulation of certain central features of Enlightenment thinking of science, including objectivity as part of the political. Thus, without endorsing this tendency Harding finds in the literature, she is excited to endorse a core program that comes out of it—one that proposes daunting tasks, but also exciting opportunities for emancipatory feminism on the cusp of the postmodern concerns of the 1980s. In her words,

This assertion of the priority of moral and political over scientific and epistemological theory and activity makes science and epistemology less important, less central, than they are within the Enlightenment worldview. Here again, feminism makes its own important contribution to postmodernism—in this case, to our understanding that epistemology-centered philosophy—and, we may add, science-centered rationality—are only a three-century episode in the history of Western thinking. \(^{170}\)

And Harding finishes off the book by continuing this line of thought,

When we began theorizing our experiences during the second women’s movement a mere decade and a half ago, we knew our task would be a difficult though exciting one. But I doubt that in our wildest dreams we ever imagined we would have to reinvent both science and theorizing itself in order to make sense of women’s social experience. \(^{171}\)

So the task for the feminist philosopher (or anthropologist, historian, or cultural critic) of science is a large one indeed! Feminism is now in a position to rethink the nature of scientific theorizing. Within a culture that is a fundamentally scientific one, this means the feminist must change the way all theorizing occurs.

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\(^{171}\) Ibid, p. 250.
These are certainly laudable goals if achievable, but I question to what extent they are, and if so, by what means? I argue that Harding’s call to reinvent scientific theorizing goes a step too far. I contend that we do not have to reinvent science; we just have to alter current institutions, practices, and principles so that women have a means by which their voices can be heard. Although I agree with Harding that the political obstacles that face any attempt to increase the critical voices of women and other marginalized peoples find a solution only within politics, I would caution that this does not necessitate a retraction of Enlightenment values, nor even, as I will echo from Longino, much of the basic empiricist methodology of Enlightenment science.

One of the commitments that unites standpoint and postmodern feminism is a focus on the embedded and situated knower—a knower that replaces impartial and rational truth-seeker of Enlightenment science and modern theories of knowledge. The standpoint and postmodern feminist, regardless of their differences, are united in thinking that the problematic assumptions that lie beneath classical theories of knowledge can only be addressed if we cease abstracting the knower from his or her context of knowledge acquisition, or (in the case of many postmodern feminists) knowledge production. To borrow Lorraine Code’s illuminating question, standpoint and postmodern feminists are concerned who is “S” in “S knows p”. Only when we understand something of S’s attributes, her gender, race and economic situation, can we begin to assess her role as a knower within an epistemic community that is itself historically situated.

Offering an account of the situated knower that respects her subjectivity is a necessary corrective for a the discipline of “classical epistemology”—a discipline that has

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for far too long hidden its masculinist assumptions about the ideal knower behind
assertions about the supreme epistemological virtues of detachment and neutrality.
Knowers and knowledge are situated, and we must make sense of the theory of
knowledge without the Enlightenment assumption that knowledge can be abstracted away
from contexts of knowing, including sexist, racist, homophobic, ageist and ableist power
imbalances in our institutions, and even in our very theories.

Implied in such thinking is the assumption that $S$ can occupy an entirely
privileged socio-cultural and epistemological position from which she can generate a
narrative about her subjectivity. This narrative may be incomplete (perhaps it must be
so), and it may very well recapitulate the oppression the $S$ has been subjected to.
Nonetheless, it holds for the epistemic community a valuable insight about $S$’s
interactions with other subjects, institutions, and discourses. (For the postmodernist, the
self that we designate by $S$ might be nothing more than the product of these interactions,
though we need not worry ourselves about such concerns of ontology.) So then, $S$ rightly
becomes a focal point for epistemological critique after being so long forgotten. Such a
picture has the story just about right.

However, strangely, much standpoint and postmodern work that has been done to
show us who $S$ is has left that part of $S$’s cultural context that goes by the name of
“science” reduced wholly to a masculinist set of discourses that are to be confronted,
transformed, or maybe even transcended. Science is seen in a totalizing way; as an alibi
for the broader cultural, social, economic, legal and medical oppression of women and
other marginalized people. Science is rarely seen as a possible ally to the cause, and only
rarely seen as a multifaceted and complex set of institutions, practices, and discourses
that are to be negotiated with rather than monolithically overcome. In other words, “science” has been given nothing like the treatment that S has been given, and we therefore fail to see the plethora of nuanced ways in which women and other marginalized voices have interacted with science in ways that are not always as subjects of oppression, or at least not only as subjects of oppression.

Many feminists\textsuperscript{173} working in the postmodern school have taken much from Foucault, though many seem to have placed little focus on\textsuperscript{174} the intricate ways in which Foucault’s research has shown science to be flexible so that it may serve the very needs and desires of a discursive community—the very needs and desires it, to some extent, constitutes within the individual and community. This is exemplified by Foucault’s treatment of the genealogical move from \textit{ars erotica} to \textit{scientia sexualis} in the late 18\textsuperscript{th} and 19\textsuperscript{th} centuries in Europe. For Foucault, the shift is precipitated by the application of Enlightenment knowledge production through the proliferation of new disciplines to study the body and sexual behavior such as sociology, criminology, sexology, and psychology, and the institutionalization and professionalization of discourses surrounding, and constitutive of, sexuality. For these reasons, Foucault balks at the commonly held assumption that Victorian Europe was a repressive society when it came to discourses of sexuality. Victorian Europe was simply a repressive society that


\textsuperscript{174} The focus of the majority of feminists using Foucault’s work for their own ends regards the development of subjectivity within discursive systems based on relations between power and the body. Some of the feminists listed in the previous footnote also draw our attention to the limitations of Foucault’s canonical work on the formation of subjectivities, choosing to develop a theory of the self that preserves a level of autonomy for which Foucault’s social constructivist theory of the self does not seem to allow for. Thus, it would be a mistake to say that there is one kind of feminist use of Foucault, and an even bigger mistake to assume that feminists who use and adapt Foucault do not do so critically and reflectively.
attempted to hide sexuality—i.e. to prevent through law and norm a discussion of sexuality.

Those who believe that sex was more rigorously elided in the nineteenth century than ever before, through a formidable mechanism of blockage and a deficiency for discourse, can say what they please. There was no deficiency, but rather an excess, a redoubling, too much rather than not enough discourse…

Foucault focuses on the ancient practice of confession about sexuality, and the discursive transformation of confession as it was brought under several different scientific theories and institutions in the 19th century, including hospitals, prisons, and schools. What was sought were “true discourses about sexuality” where there was an inducement, or an incitement to discourse. But what is important about Foucault’s views here is that he was careful not to think of scientific theories, practices, and institutions as merely oppressive. Because they are so intricate in the construction of the self, scientific discourses, by their very nature, suppress while also making intelligible new kinds of sexuality through practices of categorization.

We must therefore abandon the hypothesis that modern industrial societies ushered in an age of increased sexual repression. We have not only witnessed a visible explosion of unorthodox sexualities; but—and this is the important point—a deployment quite different from the law, even if it is locally dependent on procedures of prohibition, has ensured, through a network of interconnecting mechanisms, the proliferation of specific pleasures and the multiplication of disparate sexualities.

In other words, those very practices which, on the surface, give the illusion of a simple, repressive kind of discipline and regularization, are in fact those discourses that allow for the verbal characterization, conceptualization and intelligible construction of all different kinds of sexualities. This is why a full understanding of the self, of the sexualized body, requires an examination of the very institutions, theories, practices, and norms that make

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175 M. Foucault (1990), p. 64.
176 Ibid, p. 49.
up *scientia sexualis*. The analogy for this dissertation is illuminating: we need to understand *scientia nervosa*, the body as it is known through our best sciences, if we are to begin to understand the disease.

Once it is recognized that feminism must interact with science on a dialogical level—once it is realized that feminists must talk to scientists, not at them—it must also be recognized that the feminist places herself in a precarious position of having to actually *listen* to the scientists she’s critiquing. This is the minimum dialogical commitment any interlocutor must accept. If the feminist wants politics in science (which she is right to want) then she must accept the very real possibility that science will have a significant say in politics. The inversion of the classical notion of *Unity* tries to bring in the political while giving themselves the authority to determine what is rational, what is “fruitful of future research” (to borrow a phrase from Kuhn), and what is interesting to the working scientist. While science is recognized as perversely influential it is almost paradoxically prevented from having any say in the values that guide its theorizing and practice. What we must recognize is that critical feminist voices do not get to proclaim how to properly achieve such goals without meaningful negotiation with scientific experts. She can proclaim that such negotiations are currently unbalanced, and ought not to be. She can proclaim that there is a “systematic distortion” in communication (to borrow a phrase from Habermas) that will make her job difficult, and, regarding some topics in some historical contexts, nearly impossible to ameliorate. Nobody said that building bridges between critical feminism was going to be an easy task; certainly it is not something to be done from the armchair. Critical feminists and working scientists are often separated in many a way, as we’ve discussed: in educational background, in
socioeconomic standing, in perceived public prestige, and (more often than not) in gender. It might be easier to conceive of the body on totally new cultural grounds, and it might even be very fruitful for new critical ideas; however, if the critical feminist enters into new dialogue with the idea that the discursive or ideational body is the only meaningful body, then she puts herself in the position of being completely ignored by the scientist who might also be working on a similar object of study, like the eating disordered body, or the otherwise marked body.

The danger of such a practice is, of course, that the critical feminist without properly structured dialogue has little reason to believe her conception of the body is correct or incapable of synthesis with other conceptions of the body. She has become dogmatic in the privilege she affords the cultural and discursive conceptual framework. But this post-Enlightenment (i.e. postmodern) dogmatism is little more defensible than its Enlightenment (i.e. modern) cousin.

**III. Longino, science, and values**

Perhaps the solution resides in a refinement of the Enlightenment project that recognizes the role of a political negotiation of the values that underlie scientific practice, but which nonetheless also maintains an Enlightenment commitment to empirical modes of inquiry and scientific methodology. Longino’s work has long been regarded as the most successful attempt reform the sciences through the political recognition of a multitude of voices, while not denying the Enlightenment claim that science is epistemologically privileged in its ability to constitute objectivity via the use of empirically adequate theories. For this reason Longino’s feminist empiricism is important, because it shows us
one important way of thinking about the relationship between critical-political discourse on the one hand, and scientific institution, practices and theories on the other. As will be clear in the following paragraphs, I agree with much of Longino’s empiricist framework, though I will offer some critical remarks about just how plausible it is for feminists to build a philosophy of science and epistemology based on a notion of tempered equality.

Longino, like Harding, wants to distinguish herself from the possibility of a value-neutral science. While Longino doesn’t isolate the historical genesis of what she calls “masculinist science” done in accordance with “androcentric bias”, it would not be at all inappropriate to think of the problem much in the same way Harding does: as a legacy of Enlightenment assumptions about the separability of values and science. This being said, Longino is worried about what we do after we recognize the error of Enlightenment thinking—after we realize that value-neutral science is impossible. For Longino, how to negotiate this transition to a post-masculinist science is risky. She cautions us against the notion that there can be a “feminist science”. Her motivation in doing so is not to separate science and feminism. Rather, her aim is to show that science is better understood as “practice rather than content, as process rather than product…”

She wants to change the way we think about the relationship between feminism and science so that we focus more on how to *do science as a feminist* and less on distinguishing a feminist science from the mainstream masculinist science. To do the latter parsing is simply to recapitulate the problematic notion that there can be a value-free science, by replacing androcentric and masculinist assumptions with feminist ones.

Longino explains this problematic *replacement approach* to feminist science thusly:

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177 H. Longino (2005), p. 53
Feminists—in and out of science—often condemn masculine bias in the sciences from the vantage point of commitment to a value-free science. Androcentric bias, once identified, can then be seen as a violation of the rules, as “bad” science. Feminist science, by contrast, can eliminate that bias and produce better, good, more true or gender free science.¹⁷⁸

The mistake is two-fold: Firstly, and most importantly for feminist research, this kind of approach does nothing to upset the Enlightenment idea that science, when operating in ways that are ideal (or at least close to the ideal), should be free from values that will interrupt its proper (neutral and objective) function. This approach views feminism as nothing more than a remover of old impediments, and science as value-neutral in the right sort of way. The assumption is still the old one: science ought to be value-neutral. All we must do is remove those pernicious biases that interfere with its normal operation.

Secondly, this approach assumes that the only problems science faces are all of the same level or type. After removing androcentric biases (assuming of course we know what they are, and assuming also that they serve no essential purpose for scientific practice—both risky assumptions, indeed) we might move on to removing racist, ethnocentric, ableist, ageist, and hetero and cis-normative biases that silence and exclude minorities in systematic ways. If we can remove all of these biases, then feminism has done its job. Of course, this assumes there are no deeper (or meta) level concerns about the operation of bias and the role values play in the practice of science. This approach fails to make sense of the role values ought to play, and must play, if science is to function. It is utterly quiet about how values operate in the selection of theory, the development of hypotheses and auxiliary hypotheses, the interpretation of data, the selection of research programs and research questions to guide us, much less those values

¹⁷⁸ Ibid, p. 60.
regarding simplicity, empirical adequacy, and usefulness, which seem to operate at an even deeper level.

Rather than identify feminist philosophy of science with a removal of androcentric bias, Longino thinks there is more to the picture. Like Harding, she thinks that there must be a more important role for political and social values in the practice and understanding of science. Unlike Harding, Longino wants to examine which of those values might be adopted by mainstream scientists once the myth of value-neutral science has been abandoned. Some may be considered more central and constitutive, others more nebulous and socio-cultural. Harding thinks there is something natural about such a distinction, and perhaps something conceptually interesting about a bifurcation between what she calls “constitutive” and “contextual” values. Constitutive values are internal to science and scientific processes. They are “the source of the rules determining what constitutes acceptable scientific practice or scientific method.” Contextual values are those values that come from the social environment where the science is done. Longino explains: “The personal, social, and cultural values, those group or individual preferences about what ought to be, I will call contextual values to indicate that they belong to the social and cultural environment in which science is done.” She argues that while this distinction is conceptually useful, in practice it is extremely difficult to discern which values are constitutive and which are contextual, and both are relied upon for the functioning of science.

For Longino, once we recognize that value-neutrality is no longer a tenable view of functioning science, we are forced to consider the ways in which values, some

seemingly more central to the sciences, others seemingly tangential, all operate in rich and interactive ways. Because values form a non-principled class, and because of their often blurry boundaries and a lack of definitive and explicit criteria on which they could be individuated, no traditional analysis within “theoretical reason” can be offered, at least not one that fails to recognize their blurry and nebulous character. That being said, there is a manner in which we can isolate, even if only imperfectly and imprecisely, those values (or “theoretical virtues”) that underlie scientific practice.

The picture is rather Kuhnian. For Kuhn, questions about the objectivity of science rest ultimately on the adoption of a paradigm within which questions about what counts as a viable research program, what counts as a good scientific hypothesis or question, what counts as empirical evidence for or against a theory, etc., are all contingent on the presence of an accepted *paradigm*. The problem Kuhn had to contend with (the question raised by many of his critics) stems from the way he characterizes the futility of inter-paradigm communicativity and rationality of standards in *Structure of Scientific Revolutions*, or, perhaps more clearly, the epistemological problem brought on by shifts in paradigm, and the question of whether or not there could be anything like epistemological progress as opposed to arbitrary (i.e. non-justified) change. Kuhn’s critics were not without justification for their worries. Talk of scientific change based on the analogy with political revolution, talk of “conversion” of practitioners from one paradigm to another, and talk of different practitioners inhabiting different worlds certainly fuels the idea that Kuhn thought of work done by scientists during times of “crisis science” and “revolution science” as being irrational or arrational in comparison to
the work done by practitioners in “normal science” (i.e. the kind of work done by practitioners within an accepted and well-established paradigm).

In some of Kuhn’s later work he was at pains to clarify his radical characterization of paradigm change in *Structure*. Kuhn maintained that he never meant for his historicized account of science (as a series of punctuated shifts) to be interpreted as entailing a kind of irrational and disorganized “epistemological anarchism”.\(^ {181}\)

Whether this later work is a clearer characterization of his earlier work, or whether it amounts to an about-face inconsistent with what he says in *Structure* is not our concern. What matters for our purposes, and for Longino’s, is the way that Kuhn introduces the ineliminable role of values in theory selection. His later work is an attempt to show that choosing a theory is not an irrational process, though the kind of rationality it exhibits is one that needs to make appeal to practitioner values, not theoretical reason. Kuhn thought of these values as criteria that could be applied by various practitioners when choosing a theory. They included Kuhn’s list of virtues: *accuracy, consistency* (internal and external), *breadth of scope, simplicity*, and *fruitfulness of future research*.\(^ {182}\)

Longino cites this list in her essay, “In Search of Feminist Epistemology”, explaining that these criteria are generally understood to enhance the truth of those theories that are chosen in accordance with them.\(^ {183}\) She also agrees with Kuhn in general outlook. Such a list of criteria does form a basis for evaluative appeals amongst practitioners when they engage in communication and deliberation as regards their reasons for choosing one theory over another. Theory choice becomes a rational

\(^{181}\) This is a term that comes from Feyerabend in his claim that Kuhn was actually too conservative.

\(^{182}\) This list comes from Kuhn’s essay (1977) “Objectivity, Value Judgment, and Theory Choice”. Longino also cites Kuhn’s list in her essay (1994) “In Search of Feminist Epistemology”.

enterprise, but one akin to other value-laden decision procedures we find in non-scientific endeavors. Where she departs with Kuhn is in the details. Longino wants to see if we might come up with a set of theoretical virtues that is palatable to both the feminist scientist and the mainstream scientist. It is not surprising that we find some overlap with Kuhn’s criteria, though also significant and interesting divergence. Here is Longino’s list:\(^{184}\):

1. **Empirical adequacy**: ensuring that the models of the theory capture the observational data (or phenomena).
2. **Novelty**: recognizing that our current scientific theories might be inappropriate to our current and future contexts. We should be willing to violate current (often sexist or racist) assumptions.
3. **Ontological heterogeneity**: there are two ways of thinking about this virtue. The first is to give a prima facie weight to those theories that focus on individual differences within a sample, as opposed to those that are quick to engage in homogenous categorization. The second is to value differences in and of themselves, not seeing differences as an inferior departure from a given standard.
4. **Complexity of relationship**: ensuring that we more highly value those theories that embrace complex and interactive models than those that offer reductive and overly simplistic accounts of causation and control. This is particularly important for questions of gender in institutions as they might be theorized by social scientists.

\(^{184}\) Ibid, p. 476-479.
5. Applicability to current human needs: choosing those theories that are directly concerned with knowledge generation aimed at ameliorating existing social conditions. We should avoid the pull of theories that have no positive social and political import.

6. Diffusion of power: preferring those research programs that are accessible because they “do not require arcane expertise, expensive equipment, or that otherwise limit access to utilization and participation.”

We can tell that these criteria are a good indication of a working science that is acceptable to feminist scientists because these are the sorts of values to which feminist scientists appeal when justifying their projects. These are a representative (though Longino tells us non-exhaustive) list of those theoretical virtues that comprise community standards for responsible scientific practice. What Longino makes explicit, though it is implied in several places by Kuhn in his essay, is that these theoretical virtues reflect a socio-cultural and institutional milieu, and are themselves susceptible to change. Longino is careful to point out that “[t]hey are subject to the limitations noted by Kuhn, i.e., they require further interpretation to be applied in a given research context, they are not simultaneously maximally satisfiable, and they are not subject to hierarchical ordering or algorithmic application.”

The feminist therefore has a means of studying the significance of evaluative and political elements in the sciences, but this is done without replacing an androcentric and masculinist science with a gender-sensitive, feminist science. The issue is not replacement, as if that were really a live option anyway. The real issue, as Longino deftly

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186 Ibid, p. 479.
notes, is that there are ways of thinking about the values that guide scientific activity as acceptable to both mainstream and feminist scientists; or at least Longino has identified the appropriate level at which discussions about the relative merits of various value-systems (and interpretations of them) is to take place. It is emphatically not a discussion that scientists can have without asking meta-questions about the role of values in science, and therefore not a discussion that can occur if the feminist thinks she can do science by merely removing masculine biases.

It should be apparent that I take Longino to be supplying a much more nuanced and useful account of how the political and the evaluative dimensions of science fit into the whole of science than does Harding. Whereas the postmodern feminists Harding seems so swayed by think that a feminist politics can cause us to re-theorize science in a totalizing way, Longino is much more realistic, and a guidepost for the sort of feminism for which I want to advocate. It is only a guidepost, however, and not a template. The main reason for this is Longino’s (at times) too narrow focus on the inclusion of women in the sciences. While other critics of Longino’s have noted that the inclusion of women does not necessarily entail the inclusion of critical feminist voices, the issue I have with Longino is slightly different than that. The concern is this: by focusing on the importance of feminists doing science we run the risk of only including those critical voices that are on the inside of science. Longino’s theoretical virtues glossed above are a prime example of this. Does the non-scientist have the opportunity to propose differing virtues? Does the community at large have a chance to offer its input? It is clear that Longino thinks that there ought to be such opportunity for non-scientists, but her account of non-scientist input is less convincing than her account of how the feminist scientist can affect real
change in the sciences themselves. Further, the view is incomplete from the perspective of our purposes because it lays down little in the way of pragmatic or methodological prescriptions for how the non-scientific, though scientifically engaged and well-educated specialist, is to proceed.

A further worry follows. Central to Longino’s solution to the problem of masculinist science is a commitment to reforming scientific institutions. While this is certainly a necessary part of any project to better scientific practice, I argue that we need to focus on establishing a kind of dialogical context. In short: on my view, it’s not just about reforming the institutions and getting more women into science; it is also about ensuring that feminism is in a position to engage with the sciences, and getting women into science is only a necessary, not sufficient condition, for critical engagement. Longino does agree that feminist science must engage with existing scientific theory and practice if it is to be efficacious. But this isn’t quite what I am calling for. What I am arguing for here is more than just a dialectical interface between feminist science and mainstream or “established” science. It is rather that feminists, including non-scientists, must put themselves into a communicative position from which science must listen. There is no focus here on a pre-analytic category of persons to whom this condition applies, be they scientists or not. The condition applies to all critical feminists who are working in areas of significant overlap with current scientific study. It is a demand on critical feminists broadly speaking. As we have seen, this demand implies an expectation of a minimal scientific literacy on the part of the critical feminist, or at least some subset of critical feminists. It does not demand that a feminist become a scientist (in the full sense of the
term, with a formal education in the sciences, a paid position in a lab or university, etc.) in order to be in a position to make science better.

Longino comes close to recognizing the importance of including non-scientists with her 6th criterion, *diffusion of power*—with its focus on putting to an end “arcane expertise” and research programs that unnecessarily limit participation on financial, institutional, or technological grounds. However, it is unclear if Longino would agree with the particulars of what has been said here based only on her articulation of that theoretical virtue, and unclear whether or not putting an end to arcane expertise means anything like including anthropologists, cultural theorists, communication specialists, or philosophers in the very fabric of scientific-cum-interdisciplinary research.

### IV. The dynamics of deliberation

I have already suggested Longino provides most of what my view requires. I now want to examine our points of difference. In *Science as Social Knowledge* (1990) Longino outlines her position, calling it “contextual empiricism”. In *The Fate of Knowledge* (2002) she further develops this position, renaming it “critical contextual empiricism” in order to “better capture the social dimension” of her view of knowledge-production.\(^{187}\)

On this view knowledge production is a social enterprise. That is, claims must go through a process of transformative criticism prior to their acceptance as knowledge by the scientific community. Because knowledge production is social, and reliant upon criticism from members of a varied community, knowledge production is susceptible to all of the vagary, indeterminacy and procedural inefficiencies that come with the context of hierarchical institutions and practices, not to mention the obstacles created by

expectations about what knowledge is and who ought to possess it. The social aspects of knowledge production might at first seem negative to a feminist. After all, if knowledge production really is done within a community, and done within existing institutions, we inherit all of the biases operating within androcentric (masculinist, ageist, racist) science. On the other hand, the social character of knowledge is promising for the feminist, if for no other reason than it makes the political and evaluative dimensions of social interaction fundamental to epistemology, and science itself.

In order to better understand the view it is perhaps useful to proceed in a piecemeal way with a deconstruction of its title: critical contextual empiricism. It’s a form of empiricism because it requires scientific theories be, at the very least, empirically adequate (i.e. that they offer a characterization of all of the phenomena). It is contextual in that members of the scientific community are socially situated, and science itself is situated amongst other institutions, systems of belief, and is itself socially located and historically contingent. As we just noted in the previous section, it is not only the contextual values that science is conditioned by, but also the constitutive values related to its own perceived aims, which are deeply intertwined with the contextual values of scientific practitioners and those with a vested interest in science, including lay persons and their views about what science is and does. It is critical because knowledge production involves public scrutiny, where “public” can be broadly, or narrowly, construed to include a range of dissenting voices, including (at least) other relevant experts in a field, and possibly the input of non-experts or experts in other fields. As Maya Goldenberg has noted, Longino is saying more than that knowledge is intersubjective; Longino’s project also shows us that values are “inextricable from
evidentiary considerations that science undertakes”, and that “social arbitration becomes central to the justificatory process”. \(^{188}\)

Given that knowledge-production is social, Longino offers four social norms that, if satisfied, characterize an ideal epistemic community. These norms are: venues, uptake, public standards, and tempered equality. On her view, the degree to which the scientific community adheres to these norms dictates the objectivity of knowledge produced.

1. Venues: This norm requires that there be proper venues to present criticism of research. Moreover, these sites should be those same sites where original research takes place, with the product of critical activities getting “the same weight or nearly the same weight as is given to ‘original research.’” \(^{189}\)

2. Uptake: This norm requires that criticism not be merely tolerated; rather criticism must be engaged with and taken seriously. Importantly, uptake requires that the beliefs of the community change in response to the critical discussion occurring between the community and those offering criticism. \(^{190}\)

3. Public standards: This norm requires that there be public standards to which those engaging in both “original research” and criticism must appeal. These standards are used to evaluate research, and to determine the relevance of criticism. Having publicly recognized standards allows research and criticism to be evaluated by those within a community of research, or by those outside of it. \(^{191}\)

4. Tempered equality: This norm requires that critical dialogue engages all relevant perspectives, so that research is exposed to as many perspectives and

\(^{190}\) Ibid, p. 130.
\(^{191}\) Ibid, pp. 130-131.
criticisms as possible. This requirement, however, involves more than just ensuring that a community is open to dialoguing with differing points of view, and criticism from both its members and non-members. Longino adds the following: “it must also take active steps to ensure that alternative points of view are developed enough to be a source of criticism and new perspectives.”

While all of the norms need to be read carefully, this last norm is especially open to misleading interpretations. Longino doesn’t want to assert that everyone has equal authority in all matters. Equality, vis-à-vis this norm, is tempered (i.e. not absolute) after all. If, for example, a point of view offered does not satisfy the requirements established as public standards, then it cannot be granted the same standing as one that does.

Equality in terms of intellectual authority requires that communities recognize that critical discourse requires a diversity of perspectives, and that they recognize that neither social status nor economic status have any say when it comes to determining which perspectives are going to count. Moreover, this social norm requires that all relevant perspectives be represented in every matter. Where this is not the case, legitimate consensus cannot be established. It is only when all of the relevant perspectives are present—regardless of economic or social status—that our hypotheses can be put through the necessary criticism and scrutiny to be considered objective knowledge. Longino offers the example of excluding women and other traditionally marginalized voices from science and scientific education to illustrate that such exclusions (i.e. the failure to include all relevant perspectives) is more than just an unjust

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193 Ibid, p. 132.
194 Ibid, p. 131.
social practice. It’s also a “cognitive failing” on the part of the scientific community. A properly functioning scientific community must not only take into consideration a range of alternative points of view; rather, they must seek out and “cultivate” these dissenting voices. This criterion essentially forces a community to ask the right sorts of questions prior to deciding to include or exclude a particular point of criticism.

Transcending the social norms for social interaction has its consequences. One can compromise or forfeit one’s intellectual authority when he or she does not properly engage with the other norms of social knowledge-production. If, for example, one fails to engage with others in the right way by not fulfilling the Uptake condition—say, by ignoring a response to one’s criticism—then mere repetition of an old criticism (that has been addressed in accordance with accepted Public Standards) is grounds for diminished intellectual authority, no matter what one’s cognitive authority might be. Such matters pose constraints on how knowledge-production proceeds, even if we focus only on how individuals might satisfy (or fail to satisfy) these norms. This is doubly true of tempered equality. After all, we are steeped in a liberal tradition, one that makes talk of equality amongst individuals a fairly straightforward task. But Tempered Equality, as Longino tells us, also applies to communities, groups and subgroups. How does this norm apply to collectives—and does it tell us how we should of the “we”, as Longino puts it? Can we exclude certain groups of people for failing to take up critique, or other groups for failing

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196 Ibid, p. 132.
197 Ibid, p. 133.
198 Ibid, p. 133.
199 Longino makes the distinction between intellectual authority and cognitive authority. “Intellectual authority” denotes broader critical thinking, reasoning, and observation skills, with the ability to engage in analysis and synthesis. One has intellectual authority if one exercises these abilities well to make “cogent comments” about matters in which one is not expert. “Cognitive authority” denotes some specific and specialized knowledge, say the astrophysical knowledge one possesses as an astrophysicist. See Ibid p. 129 and p. 133.
to put an end to their criticism when said criticism has been addressed? In other words, can scientists working on a post-coital contraception drug exclude criticism from pro-life religious groups if those groups continue to trumpet mis-informed worries about abortion? And it isn’t just about exclusion, either. Are there grounds—expressed best by standpoint theorists—for seeking out certain people for having had (or possibly having) interesting perspectives? In other words, can we call it a cognitive failing if our scientific communities do not cultivate marginalized perspectives? “Cultivation” would be something stronger than just offering Venues and observing the norm of Uptake, which are passive preconditions. Cultivating marginalized voices would be an active process, perhaps engaging marginalized communities in an attempt to illicit possibly transformative narratives from those who would otherwise remain silent, no matter what access they were granted to the processes of knowledge-production.

In short: figuring out exactly how to temper equality forces us to make important decisions about who to include or exclude in the community. Longino recognizes that tempered equality forces us to address questions about group membership, and where the boundaries should rightfully be drawn regarding who the relevant voices are in knowledge production. To paraphrase a question from Longino: Does the “we” of the scientific community include only those conducting or criticizing a particular research project, or is it all of those who are/will be affected by it?

It is clear that for Longino these conditions of an ideal epistemic community are intended to facilitate the transformative criticism necessary for instances of genuine knowledge production. The degree to which these norms are satisfied determines how effective a community is in this domain. Longino tells us, “[t]hese norms enable us to
distinguish those social interactions which are productive of knowledge from those which are not. The inclusive, dialogical and democratic ideals of her position are appealing. I do not wish to offer any criticism of the norms as ideals.

I take the real difference between my view and Longino’s to be this: she seems to view tempered equality and venues as mechanisms for bringing about the possibility of uptake, and public standards as a measure, or way that we decide between different ideas that find uptake in a community—or, less substantively, an outline of the procedures to be followed to non-arbitrarily determine relevant intellectual authority in a specific domain. Longino is thus saying that tempered equality is a precondition for effective critical (and contextual) knowledge-production, like uptake, recognized public standards, and venues. I want to argue that this is not the case; I want to position tempered equality as a consequence of genuine uptake rather than, following Longino, as a precursor to transformative dialogue. To do this I must show how effective critical discourse can exist without this norm; in fact, I will do slightly more than this. I will argue that effective critical discourse not only exists (possibly and historically) without the condition of tempered equality being satisfied, but that tempered equality is best regarded as a product of, not precondition for, such critical discourse.

From what has been said already in this dissertation, it is clear that I agree with Longino that knowledge-production is social, and that it requires dialogue amongst those who have very different (actual or perceived) authority, both intellectual and cognitive. I have argued that we need to retain dialogue amongst epistemic agents both within and outside of a community, and that certain social factors will mean that such dialogue is rarely apolitical, or free from distorting power relations, often regarding the disparate

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academic standing of “softer” humanities and social science disciplines in comparison to their “harder”, scientific cousins—from physics to medicine. But contrary to Longino (and communicative theorists like Habermas), equality isn’t a necessary prerequisite for overcoming systematic problems of communication. If it were, it would be unlikely (and maybe even impossible) that communication should ever take place across authoritative divides. Science would remain a bastion of tyranny, as it would have no internal means of taking seriously critique from those who lack the most rigorous credentials for expert knowledge in a field. What is needed if science is to have a way of remedying its own biases is a communicative practice that allows for meaningful dialogue to occur without even the pretense of equality of authority. What we require are at least a few instances of communication that is actually conditioned by the relations of dominance that feminists (especially those like Longino) take to be damaging to dialogue and objectivity. The proposal is that feminists must learn to utilize and manipulate dominant scientific discourses to their own ends. This requires further explanation.

It is important at this juncture to remember the lessons of earlier chapters in this work. What I am proposing is a practical and procedural (dialogical) solution to the problem of inter-theoretical (or, better yet, inter-disciplinary) communication. I want to keep this proposal as practical as possible. The cultural theorist is likely not going to have the same level of cognitive authority as the medical professional when it comes to medical issues, and this may mean that dialogue between scientists and non-scientists about such issues will always fall prey to systematically distorted communication (in Habermas’ language). It may be the case that some of the best input comes from
marginalized voices operating without tempered equality, exploiting communicative and rational principles underlying an imbalanced exchange.

The idea, when this gets cashed out, is that the critical feminist, cultural theorist, etc. realize that the scientist “has to” listen on pains of communicative rationality when the feminist adopts a sufficiently scientific mode of thinking and speaking, and that uptake is achieved in unbalanced power contexts by adopting a kind of dominant form of discourse, but not completely, and not without an eye to how that discourse might be used to subvert several assumptions that normally go unchallenged by those who use the discourse.

One other way of thinking about this that doesn’t place as much emphasis on communicative theory but stays squarely within the bounds of the epistemological framework Longino has set up, is this: there is a minimum threshold of cognitive authority that must be met in order to critically engage with the sciences. In other words: there’s a minimal cognitive authority which is required to have intellectual authority. One doesn’t really have intellectual authority until one has reached a minimum cognitive authority, so that a minimal cognitive authority is the necessary precondition for intellectual authority. You can’t think critically about complex scientific matters if you don’t have minimal expert knowledge and some special skills. These skills might include mathematical skills or numeracy, an understanding of basic scientific methodology, especially regarding peer review processes, basic models of scientific rationality, an understanding about how scientific questions are well formed, what counts as data, and how data can be transformed into evidence for a theory, the importance of blinding,
including experimenter blinding and other nuanced issues regarding experimental design, and so on.

For Longino, uptake is achieved by tempered equality and venues. But those seem like even harder conditions to secure than uptake. What we need is uptake (i.e. at least some critical feminist views being taken seriously, and not merely being tolerated) so that there can be a more than abstract appeal to tempered equality. The motto of this proposal might read: *Uptake first, then tempered equality.* But this requires discovering (or, better yet, creating) strategic opportunities for uptake within systems of power imbalance—and this is where feminists must learn to speak the language, but to do so in accordance with their own political ends.

I am thus proposing an amendment to Longino’s account, one that hopes to make sense of how her social norms relate to one another when applied to the practical concerns of this dissertation. From this practical perspective, her model of discursive interactions is too much of an idealization to make clear exactly how feminists working in very non-ideal contexts might proceed. I am urging that a more practical and likely strategy is to adopt dominant forms of discourse, and re-characterize the condition of tempered equality as a norm to be achieved, not presupposed. I argue that using such a strategy will work to force a kind of uptake that can subsequently put feminist criticism in a position to make effective demands for tempered equality and more open venues, and ultimately better approaching the ideal that Longino puts in place.

The claim I am making is that Longino has something to learn from standpoint and postmodern feminists, though the lesson is perhaps less radical than those feminists might make it seem. The lesson is really one about the strategic use of one’s social
situation as an academic feminist. Academic feminists are in a position to engage in
dialogue with those front line practitioners in the science, as well as the women who are
subjects of science, primarily as patients, the recipients of new drugs, etc. The
fundamental postmodern insight is that those who occupy such positions also have an
opportunity for critique, provided that they take what little time and resources are
afforded the female academic (given the exigencies of departmental service, teaching,
and research obligations) to meet the minimal demands of cognitive authority, or as I’ve
classified the task throughout this dissertation, learning what needs to be learned of
the sciences to effectively engage with the sciences.

There’s an obvious analogy with the work of bell hooks regarding gender and
race, and the role of the black female academic as a bridge between the kind of abstract
theorizing done by predominantly white academics, and the resistance and activism of
feminist and black power movements. hooks thinks of this position as a necessary in-
between, linking theory with praxis in a way that is informative for the academic (who
now realizes she is doing more than speaking about abstract concepts, but rather concrete
and often theoretically messy individuals) and activists (who gain new tools for
conceptualizing their oppression and for devising creative solutions to real world
problems).

I am saying something similar. There have to be feminists working in critical
disciplines in the humanities and the social sciences that accept a position as a critical
intermediary between largely white, male, professional science on the one hand, and the
collection of critical voices in academia and in activism on the other. What is required is

201 hooks makes this argument in a number of places, including her pieces (2004) “Choosing the margin as
a space of radical openness” and (1990) “Postmodern blackness.”
that some feminists have a foot in two worlds. But this requirement is onerous. Feminists have to learn how to speak and understand two languages. They have to learn how to be able to see the world through two views with the eventual goal in mind to bring those two views of seeing the world into ever more fruitful congress. Of course, this comes with a doubling of the workload (that often goes under-appreciated, and certainly under-compensated). That is the plight of double-consciousness. If feminist discourse about the sciences is to be efficacious, some feminists must accept the burden; they must be feminists with a cognitive authority in their home discipline and the relevant science, or, what’s even more onerous, they must be experts in their home discipline, in the relevant sciences, and in policy (if they take up issues at the interface of science and politics, an area of increasing importance in the last decade or so. Some useful models of science-policy study are offered by Heather Douglas (2009) and other philosophers working in the emerging field of socially responsible science.\textsuperscript{202} I will return to this later.

\textit{V. Conclusion:}

In this chapter I have tried to build off of what I take to be a common thread in Harding and Longino and the feminist philosophy of science more broadly, which is a systematic inclusion of political and valuational dimensions of science within a proper account of science. I have explored this common thesis through the canonical works of Sandra Harding and Helen Longino, though there is a great deal of current research studying the role of values in science and socially responsible science. In the conclusion to this dissertation, I will explore some of this literature in the hopes of pulling out a common theme: one of coordination between feminists working on issues related to science and

the sciences themselves. Such a survey, even if brief, will show the merits of critical congress between feminism and science, which I have argued in this work is a necessary precondition for the study of complex disorders that can only properly be understood if seen from a number of different disciplinary perspectives.

We saw in this chapter that a sea-change occurred in feminist philosophy in the late 1970s and early 1980s, one that set the task for contemporary feminist philosophers of science to come to terms with the political nature of scientific practice, and even scientific theory, and to show how science can be changed in ways that make it more amenable to feminist concerns, or any scientific concern that falls outside of a traditionally masculine, heteronormative, and racial program—even if the feminist project is just the negative one of explicating how seemingly neutral and objective sciences can be masculine, heteronormative and racist. What this sea-change did not do for the feminist working on such issues today is establish a program for how to proceed. In some sense, that has been a primary focus of feminist philosophy of science in the last 20 years, though not necessarily one that has been explicated.

This dissertation touches on important questions in philosophy of science and feminist epistemology. I am concerned with how feminists who talk about the body might learn to understand how scientists talk about the body in an endeavor to make their claims and their ways of seeing the world enter into efficacious dialogue with dominant forms of scientific discourse. The end goal is that we have robust and multi-disciplinary understandings of what the body is, and how it might be diseased. This project is a microcosm of the more general claims made in this chapter: claims about scientific knowledge that are foundational for the sort of discussions of the eating disordered body I
pursue in earlier chapters. What we have seen in this chapter is that the importance of multidisciplinary communication generalizes in exciting ways for the philosophy of science and epistemology. Neither philosophical subfield can hope to be effective without coordination with the sciences. What I hope is clear is that the type of coordination required is of a fairly specific sort; it requires that the feminist recognize that the sciences occupy a dominant social and discursive position that structures the feminist’s critique of science in such a way that she must learn to speak the language if she hopes to enact practical, theoretical or meta-theoretical change.
CONCLUSION

I. Road map of terrain covered thus far

This dissertation has been concerned with outlining an approach to studying the body, in particular the eating disordered body, that is able to capture the complex relationship that exists between the physiological body and the cultural discourses that shape our understanding of it.

We started in Chapter One with an outline of the difficulties that arise when we try to accurately capture the eating disordered body. There has been a tendency to treat studies of the biological body and studies of the cultural body as distinct—in fact, the tendency has been to treat these disparate studies as applying to wholly different things: the physiological, anatomical, biological, and neurochemical body of the life sciences on the one hand, and the ideational, symbolic or discursive body referenced in cultural studies, media studies, and certain parts of philosophical discourse (especially postmodern feminism). I argued that the chasm created by disparate theories of the body left the material body, in particular the eating disordered body, largely underexplored in feminist literature. The gulf that exists between naturalist accounts of the body, and social constructivist accounts of the body, limits communication on a common object of study.

Despite good reasons for feminist theorists favouring an ideational body over a material body, I argued that feminist studies of the eating disordered body are enriched when research in the medical sciences is consulted. Furthermore, consulting empirical studies in our critiques of scientific practices and institutions is important if we want our criticisms to carry any weight outside of our disciplinary boundaries. In other words:
critical feminist studies will remain marginalized (at least outside of philosophy and cultural studies departments) unless some feminists within the broader feminist epistemic community engage directly with the sciences regarding scientific studies that make substantive claims about biological sex, gender, and a host of other problematic categorical tools. Such consultation with what the sciences are saying also makes feminist criticisms more accurate—i.e. the feminist is able to speak to specific issues regarding data collection, data interpretation, choice of theoretical framework, exigencies of blinding and other methodological concerns, not to mention criticisms of institutional design. Of course, this call for feminist engagement with and in the sciences places certain demands on the feminist, but allows her to enter into a critical dialogue with scientists from a position that is, arguably, much more difficult to ignore than existing postmodern critiques of science.

In Chapter Two I outlined one way of theorizing about bodies that integrates the biological (mechanical) with the ideational. Phenomenologically oriented embodiment theory facilitates an understanding of the body that takes seriously the subject’s own lived experience alongside her biological and cultural realities. We problematized the tendency to take first-person accounts of phenomenological experience as authoritative, and saw why cultural theorists might be especially wary of trusting such accounts. We then looked at Dennett’s heterophenomenological account of first-person reports as one way of making sense of such reports without granting them the kind of authority they have received in some strains of post-Cartesian embodiment theory. We saw that it might be best to turn our attention away from phenomenologically oriented embodiment theory
toward a heterophenomenological approach that adds a political dimension to Dennett’s minimalist heterphenomenology.

In Chapter Three I more thoroughly outlined my form of critical empiricism in order to more fully understand, and more accurately theorize, the eating disordered body. This methodology requires feminists to satisfy two criteria. Firstly, this approach requires feminists to engage with those studying a common object of study from a medical or scientific perspective. For the purposes of facilitating such communication, developing a familiarity with scientific vocabulary and discourse is necessary. Secondly, while this approach requires feminists to critically engage with scientific data, it involves recognizing that such data does not come already interpreted—and it is her role to show how they can be interpreted in a number of different ways, only some of which are palatable for the feminist in accordance with the political motivations that guide her interpretations, and those critical theoretical frameworks that have been developed by feminist philosophers of science, and other feminists theorizing the body without explicit reference to natural bodies.

I draw on recent work in neuroscience involving the use of PET scans to study the effects of eating disorders. I argue that this sort of engagement provides the feminist with rich, new resources for studying the body. The methodological approach to studying the body I am advocating for does not require the expertise of the neuroscientist or the physician; rather, it requires a commitment to developing a pluralist account of the body, and dialogical commitment to adopt some of the dominant discourse that surrounds the body, which, if we are to be realistic about it, is obviously a scientific discourse—though not one that is immune to strategic feminist transformations from within.
Finally, in Chapter Four I situated this project within the wider field of feminist philosophy of science. I showed some of the affinities of my approach with other forms of critical empiricism, in particular, with the work of Helen Longino. I also pointed out the ways in which my approach aims to maintain some of the commitments we find in feminist postmodernism and feminist standpoint theory, which, with only slight modification to something like Longino’s criteria approach to democratic science, can be preserved.

Most significantly, in Chapter Four I aligned my work most closely with Longino’s contextual empiricism, though I challenge her reliance on “tempered equality” as a precondition for effective communication leading to what she calls “uptake”, and I argued that feminists are in a strategic position to engage in communication with mainstream scientists without the precondition of tempered equality. In fact, I argued that only through systematically distorted communication can feminists achieve uptake by adopting dominant modes of scientific discourse in order to transform them in accordance with the political interests of feminism (so far as the data allow). Only after some of her ideas get uptake within the community can the feminist push for a kind of tempered equality. In other words, tempered equality is a *product* of the uptake of feminist ideas, made acceptable for dominant science. But this means that feminists are basically in the business of transforming their status as second-class intellectual citizens (vis-à-vis the sciences), utilizing dominant forms of scientific discourse to demand the inclusion of their concerns. Feminists are not in the position of achieving a kind of political or communicative equality before the fact.
II. A call for socially responsible science: Two forms of feminist engagement

In what remains of this conclusion, I would like to further situate my form of critical empiricism alongside recent work being done in what could broadly be called socially relevant or socially responsible philosophy of science. I maintain that there are two overlapping, yet conceptually distinct, ways for feminists to be engaged with the sciences, and that only one of these ways has been fully characterized in the literature. The first way for feminism to be engaged with the sciences is by focusing on the political and valuational dimensions of scientific practices and institutions. This kind of critique gives us criteria by which we can examine scientific practice (including theorizing) and scientific institutions. We will have more to say about this kind of feminist engagement with the sciences below.

The second kind of feminist philosophy of science that calls for a deeper understanding of the sciences themselves is the feminist approach I have been arguing for throughout this dissertation. This kind of critique requires that at least some feminists be well-trained and well-versed in scientific practice and scientific research programs, including a working understanding of scientific methodologies and ways of speaking. This form of feminist philosophy of science is fruitful because it allows the feminist to be engaged with the scientist in the scientist’s own terms, and, as we have seen, this places upon the scientist a dialogical commitment (that finds its roots in communicative rationality) to listen to those concerns. Of course, this form of feminist critique, like the first, is based on the presupposition that science is not “value neutral” and that the feminist is not required to leave her political agenda behind. In this way, the common thread that binds both forms of feminist critique is a commitment to exposing and
exploiting the inherently political nature of scientific theorizing and scientific practice in order to reform the sciences. However, the kind of feminist critique I envision, and which I have tried to explicate in this dissertation, recognizes an even broader conception of what is political in the sciences. Before I explicate this broader conception of the political I think it is important to first characterize the common concern with the political nature of science that is characteristic of feminist philosophy of science. While we have seen how previous feminist philosophers of science, especially Harding and Longino, view the political dimensions of science, an overview of work being done on socially responsible (or socially “relevant”) philosophy of science is required. I will provide a brief account of such work and how it dovetails nicely with the kind of feminist critique I have been characterizing and defending.

Because the category of socially relevant philosophy of science is rather large and unruly, I will focus most of my attention on a special issue of *Synthese* in which guest editors Carla Fehr and Kathryn Plaisance introduce us to what they call socially relevant philosophy of science (SRPOS). Although SRPOS is a broad category, capturing diverse research projects, all of these projects are, according to Fehr and Plaisance, “motivated by a concern for public welfare”\textsuperscript{203}, and all forms of SRPOS focus on the relationship of SRPOS to the sciences themselves and the *stakeholders* of those sciences—the sciences that have the greatest ability to ameliorate or aggravate current social issues. Also, much focus is placed by Fehr and Plaisance on the relationship of SRPOS to current mainstream philosophy of science—including how SRPOS can illuminate traditional concerns in mainstream philosophy of science, and be informed and motivated by ideas in mainstream philosophy of science.

\textsuperscript{203} Fehr and Plaisance (2010), p. 307.
For these reasons, it is important to situate the feminist philosophy of science here developed into the SRPOS literature. One obvious analogy between the ideas developed here and SRPOS as surveyed by Fehr and Plaisance is this:

SRPOS is a highly pluralistic endeavor. It includes philosophical engagement with scientific research on socially relevant topics, philosophical activities that attend to the interactions among scientists and various communities that contribute to and are affected by scientific research, as well as philosophy of science disseminated in ways that reach beyond communities of philosophers.\(^{204}\)

Certainly, much of what I have said in this dissertation is but an echo of these concerns that define much current work being done in SRPOS, or, more specifically, the feminist wing of SRPOS. Further, Fehr and Plaisance isolate four themes common to much SRPOS work, specifically the work that represents SRPOS in the *Synthese* volume. These are:

1. Collaboration with scientists.
2. Addressing policy, regulation, and institutional structure.
3. Investigating intercommunity relations.
4. Changing philosophical practice.\(^{205}\)

All four of these themes are alive in my research. In reverse order, I think that existing work on the body in philosophy, generally speaking, and in the medical sciences, raises a number of issues for the feminist philosopher of science, who wants to address complex disorders from a multitude of different perspectives. (The Cartesian body of anatomy and medicine, the lived body of embodiment theory, the heterophenomenological perspective, and the body as a culturally significant object.)

\(^{204}\) Ibid, p. 302.
\(^{205}\) Ibid, pp. 304-307.
My research is also concerned, explicitly, with the relationship between epistemic communities that have previously been considered distinct, or inherently antagonistic towards each other, though my research calls into question both of these assumptions. The epistemic community of the sciences dealing with the body, and the epistemic community of non-academics and their relationship to one another is a central feature of this work. Moreover, this work is motivated by the social welfare of those who suffer from complex disorders, their families, and their communities. While these are topics that have not been elaborated on in this work, that is merely because I hope to get the methodological and conceptual issues on the table first. Needless to say, if complex disorders did not afflict such a broad and gendered community, this work would lack motivation. Sadly, that is not the case.

Regarding the second theme, “addressing policy, regulation, and institutional structure”: I take this to be a central desideratum for any feminist who is focused on changing scientific practice and changing scientific institutions so that they better reflect the communities’ interests that they represent, in particular, women and other marginalized groups. This dissertation presupposes the importance of such work. Without the aim of changing policies, practices and institutions, there would be no aim for feminist philosophy of science, or feminist studies of the body. That being said, many feminists have engaged in esoteric research regarding the body that is unlikely to bring about this sort of change. I take my work to apply criteria for separating such work from work that can have an impact on policy, practices, and institutions. This does not mean that work that is focused on discursive analysis of the body (a touchstone of postmodern feminist philosophy) has no role to play in an epistemic community of feminist
philosophers with certain interests in how the body is studied. It is rather to say that such work has an impact primarily derivatively through its work on socially relevant philosophy of science, and thus, without socially relevant philosophy of science and at least some feminists engaging with the scientists in a direct way, such research would be of only academic interest. I take my research and the research of others interested in socially relevant philosophy of science to be carving out a space within which feminist philosophy, and not just feminist philosophy of science or socially relevant philosophy of science, can become applicable to the very sciences that feminists critique.

Most importantly, I take this dissertation to be an important contribution to the first theme highlighted by Fehr and Plaisance, though I would argue that “collaboration” between non-scientists and scientists, between feminist philosophers of science and practicing scientists, be given a wider reading than is normally supposed in the SRPOS literature. Whereas others who have done much work to create collaboration between philosophers of science and scientists, especially Nancy Tuana and her work on the ethical and social epistemological issues with climate change at the Rock Institute for Science and Values, my research argues for such collaboration to the supplemented by another kind of more critical collaboration. This critical collaboration is done when philosophers of science, particularly feminist philosophers of science, interject themselves into the very processes of scientific production of knowledge. When feminist philosophers of science apply a deep understanding of scientific data collection and interpretation, they can do even more for the reform of androcentric and anthropocentric science than feminist philosophers of science who focus on after the fact conceptual
analysis and criticism about the weighting of evidence, and the unearthing of implicit biases and value-assumptions in research programs.

Of course, all of these projects are of the utmost importance, and others, particularly Douglas, have focused on these issues, but have given much less focus on the role of feminist philosophers of science to engage in the actual sciences that they critique. Feminist philosophers of science have been doing this for a long time. I am merely arguing that feminist philosophers of science have yet to properly characterize this activity, and how it fits into existing philosophy of science, and how it fits or does not fit into traditional philosophy departments. This dissertation can really be regarded as a plea for the importance of this activity, which at least some philosophers of science, especially those with an educational background in biology, medicine, or psychology, are uniquely and ideally positioned to carry out. Without a proper characterization of this activity, the fear is that it will be undervalued by philosophy departments and the discipline as a whole, and will possibly not be pursued by promising graduate students as a research program. Fehr and Plaisance talk about this latter issue more generally by those engaged in SRPOS in section 5.2 in their introduction to the Synthese volume.

III. A deeper engagement and a deeper politics

It is my contention that the form of critical empiricism I have developed in this dissertation compliments work being done in the philosophical study of socially relevant science. Where my work departs from this literature is in its broader characterization of the political: I think there’s a kind of naturalized variant of postmodern feminism that locates politics in the body itself—i.e. gut feminism and the idea that there’s a feminist
way not just of critiquing science, but a feminist way of doing science\textsuperscript{206} once you understand that the political body can be more than just the marked body of former postmodern feminists. Naturalized feminist philosophers of science are, like their postmodern counterparts, committed to a radical politics of the body; unlike postmodernists, naturalized feminist philosophers attempt to divorce the traditional postmodern commitment of a politics of the body from their \textit{identification} of the body with semiological or discursive practices. We immediately see new avenues for research when we adopt a kind of scientific or naturalized politics of the body, and when we put an end to the (unwarranted) postmodern identification of the body with the semiological or discursive body.

This new way of doing feminist philosophy of the body delimits a role for feminism that has hitherto been underexplored, even in the SRPOS literature surveyed by Fehr and Plaisance. There is a place for feminism between the constructive project of socially responsible science, on the one hand, and the \textit{after-the-fact} criticism of scientific findings using competing non-natural frameworks on the other. Feminism can also operate \textit{within} existing institutions (even when they are unjust), and can do more than criticize scientific findings produced by these institutions. Feminists can be part of the process of the generation of scientific findings, and can guide and transform those findings in accordance with their political commitments (in so far as feminists are producing something recognized as science by scientists). Largely, feminist philosophers of science have been focused on critiquing institutions and thus reducing the probability of junk science before it can start, or critiquing the scientific activities and scientific

\textsuperscript{206} This is another way in which my critical empiricism is similar to Longino’s. Longino thinks there isn’t really a feminist science, just a way for feminists to do science and make it better in the process. I agree.
findings in an attempt to correct junk science after the fact. Both of these practices, as important as they are, can be seen as taking place “outside” of the sciences themselves. I want to say that there’s a lot going on in between the before and after. Where other feminists have rightly been focused on the before and after, I think that feminists should also be part of scientific activity. Some feminists have to be engaged in the sciences in the minimal ways described in Chapter One (in outline) and in more detail in Chapter Three, which is why the SRPOS focus on collaboration, especially of the sort advocated by Tuana regarding climate change, is so important.

Let me be clear: feminist philosophers of science have already been urging for this kind of engagement with the sciences, and some have even begun to be so engaged. However, this dissertation is, to my knowledge, the first attempt in the literature to characterize that activity and its philosophical and social importance.

In more precise terms, it is also a first step toward an account of why feminist studies of the body (in particular) can be done in such a way that it preserves the postmodern commitment to a politics that is situated within the body, but now conceived of as a fundamentally natural entity. Politics is situated in the visceral and the neural (the interior), not only in what we might call the exterior (marked bodies, the institutions that surround science and make it possible, or the social norms and practices that govern gendered science). In Chapter Two we saw one example of this with Elizabeth Wilson’s “gut feminism”. Wilson calls for a change in the way we approach feminist analysis of the body. In particular, she argues that feminism can enter into productive alliances with the biological sciences, generating political and biological accounts of the body that conceptualize the relationship between the psychological realm and the biological realm.
of the body differently. Taking the bulimic body as an example, Wilson argues that there is no clear, straightforward, linear distinction to be drawn between the psyche and the soma. Despite considerable research, and consensus that bulimia and mood disorders go hand in hand, there remains no biomedically confirmed etiology for bulimia. There is however a tendency in the literature, Wilson points out, to conceptualize the relationship between bulimia and mood disorders as if one must be primary, and thus be the “trigger” for the development of the disorder, or a particular binge-purge episode. Such a conceptual framework, however, is limiting. She argues,

> There are a number of demarcations that these etiological discussions in the literature seem to force on the reader: depression then binging; satiety or mood; brain not gut. It has been my argument, via Ferenczi, that these Boolean demarcations among organs and between psyche and soma are intelligible only within a conventional (flat) biological economy. It seems to me that the lack of a clear path from one cause to one effect, from one organ to another, or from the psychological realm to the biological, and back again, indicate not a lack of conclusive data but the workings of the biological unconscious made manifest.

When we reorganize the way we think about organic substrate in complex disorders like bulimia, we can extend our possibilities for research and understanding.

> What is novel about the framework I have developed is the centrality of dialogue: this is something we don’t see in either Harding or Wilson. While it has a place in Longino (e.g. her emphasis on transformative criticism), what sets my view apart from hers is my communicative, as opposed to democratic, characterization of successful exchange and uptake. To my knowledge, it is distinct from all other ways of understanding dialogical commitments, even in collaborative research projects as discussed by those interested in SRPOS.

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208 Ibid, p. 83.
For these reasons, this dissertation has covered a lot of ground. Not only does it touch on methodological concerns (as a primary focus), but it also touches on the metaphysics of the body, and the ways in which we come to know the biologically and culturally significant body, introducing concerns in feminist epistemology—from standpoint theory to postmodernism—en route to a defense of a particular brand of feminist empiricism. The project is thus about more than just building bridges between naturalist and socially constructivist studies of the body, but about just how much of the discipline of philosophy is inculcated in any discussion of the body, especially one so politically and metaphysically important as this: the body as a nexus for complex disorders.

Along the way, the main methodological lesson learned has been this: science is not best seen as something to be overcome by feminists (perhaps in something like the de Beauvoirian tradition), but ought to be considered a tool, a resource, and a set of institutions and practices (and persons populating those institutions engaged in those practices) with which we must engage, often employing their vocabulary and their conceptual schemes with an eye to transforming those vocabularies and conceptual schemes—and with them the practices and institutions themselves so that they are more open to new ways of thinking about the body and challenging the assumptions which have become a naturalized part of current functioning science.
BIBLIOGRAPHY


