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Rethorizing Psychiatry: Birth of a Postmodern Clinic

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Preface

After training in psychiatry, I went back to pursue an interdisciplinary PhD in “human sciences.” A key preoccupation of the human sciences is the role of theory in scholarly work, and in the text that follows I use recent theoretical approaches in the human sciences to reconsider the fundamental organizing structures of psychiatric knowledge formation. I have roughly divided the project into a “theory” section and an “applied” section. The first four chapters outline the theories I will be using in the later chapters.

Chapter 1 outlines how the tropes of “atheoretical” and “theory” are being used in psychiatry and the human sciences, critiques psychiatry's atheoretical approach, and explores the possibilities for a retheorized psychiatry along the lines of work in the human sciences. Chapter 2 argues that, although language is central to “theory” and a “retheorized” psychiatry, language is complicated and there are a variety of ways of understanding it. Thus, I consider three perspectives on the sign—referential, relational, and consequential—in order to unpack their implications for a more general theory of representation. My purpose here is to work out an “applied theory of representation” that can be used to evaluate possible psychiatric knowledge and practices.

Chapter 3 moves from a theoretical understanding of language to a theoretical understanding of power. If psychiatric knowledge is always constructed in and through linguistic signs, what serves to fix psychiatric languages into specific historical blocks or knowledge formations? The short answer is power and struggle. Thus, in this chapter, I

use Michel Foucault's theories of discourse and power to articulate an understanding of the relative stability of knowledge formations in spite of their simultaneously and inescapably arbitrary and underdetermined elements. These perspectives on knowledge formations are compared and contrasted with the perspectives of a recent textbook in psychiatry. Rather than a "science of truth," the version I argue for could profitably be called a "politics of truth."

Chapter 4 organizes the theoretical discussion so far around a sustained reflection on modernism and postmodernism as relevant to psychiatry. Three themes of psychiatric modernism are outlined and contrasted with three contrasting themes of psychiatric postmodernism. I argue for the multiple advantages that will follow for psychiatry, or some branch of psychiatry, when it shifts to a more postmodern scaffold. Of course, change brings loss as well as gain. Thus, I also discuss some of the losses that will follow from a postmodern turn.

Chapter 5 is a bridge chapter that explores other recent theoretical scholarship similar to mine applied in the domain of what I call "cultural studies of medicine." I make this shift to medicine for the practical reason that there has been more work in the cultural studies of medicine than there has in cultural studies of psychiatry. However, as an emerging genre, cultural studies of medicine is an ideal domain to introduce new retheorized efforts in psychiatry. The next two chapters do just that, with chapter 6 devoted to the dramatic rise in neuropsychopharmacology in psychiatry, and chapter 7 devoted to the role of *DSM*—psychiatry's dominant diagnostic manual.

The last chapter imagines a possible feminist successor science for psychiatry. After reviewing the main themes of feminist epistemology (and of some like-minded

thinkers), I apply their efforts to a “re-formed” psychiatry. This application is speculative and politically unlikely to occur, but the task for this chapter is not to be practical. The task is to imagine how things might be otherwise. Of course, imagination does not make it so, but it is a step in the process.

Chapter 1: Retheorizing Psychiatry

To put it quite simply, this chapter explores a paradox. Over the past 20 years, psychiatry has rallied itself with great fervor to become a champion of “atheoretical” scientific knowledge, while, over that same period, the human sciences have become “theory mad beyond redemption” (Kreiwirth & Cheetham, 1990, p. 1). After much reflection on this issue, I have come to side with the human sciences and am now advocating “retheorizing” psychiatry along the lines of much work in the human sciences. For this position to make sense, however, I must first outline how the terms “atheoretical” and “theory” are currently being used in psychiatry and the human sciences.

The Rise of Atheoretical Psychiatry

In 1980, when the American Psychiatric Association (APA) published a revised version of their standard diagnostic manual, *Diagnostic and Statistical Manual of Mental Disorders*, 3rd edition (or *DSM-III*), U.S. psychiatry underwent what many are calling a “scientific revolution.” These two events, the publishing of *DSM-III* and the concurrent rise in “scientific psychiatry,” I argue in this chapter, also hail the emergence of “atheoretical psychiatry.” I put “atheoretical” in quotes as a way to bracket off the truth of psychiatry’s atheoretical claim. I’m not exploring in this section whether psychiatry really is atheoretical or not but rather how psychiatry came to understand itself as atheoretical. As this chapter proceeds, I will problematize the very possibility of an atheoretical psychiatry, but here I’m primarily outlining a recent history of psychiatry’s

self-understanding. Gerald Maxmen, in his book *The New Psychiatry*, congratulates psychiatry for its emerging scientific status and sums up nicely the effect of *DSM-III* on “scientific psychiatry” with the following proclamation:

On July 1, 1980, the ascendance of scientific psychiatry became official. For on this day, the APA published a radically different system for psychiatric diagnosis called . . . *DSM-III*. By adopting the scientifically based *DSM-III* as its official system for diagnosis, American psychiatrists broke with a fifty-year tradition of using psychoanalytically based diagnoses. Perhaps more than any other single event, the publication of *DSM-III* demonstrated that American psychiatry had indeed undergone a revolution. (1985, p. 35)

In Maxmen’s historical narrative, the rise of scientific psychiatry and the publication of *DSM-III* are part of the same pattern of changes, or the same “scientific revolution,” through which psychiatry has passed over the last 20 years. Maxmen’s narrative is a tale of enlightenment progress. For Maxmen and the new psychiatry, more science equals more progress. The qualifier “more” is important because it is not simply that the old “psychoanalytic” approaches were not scientific. Indeed, psychoanalysis itself rode on a narrative of scientific progress. Freud was often at pains to point out that psychoanalysis was a “scientific psychology”—which in Freud’s own enlightenment narrative is why psychoanalysis was superior to philosophy or religion. But, for Maxmen and the new psychiatry, psychoanalysis is not scientific enough. Indeed, for Maxmen, psychoanalysis is so close to religion and philosophy that it is only with the *DSM-III* that psychiatry truly effects a scientific revolution.

Maxmen is not alone in marking the turning point toward a new scientific psychiatry with the publication of *DSM-III*. Though he is perhaps unique in his religio-secular fervor (“For on this day, the APA published a radically different system of

psychiatric diagnosis . . .”), other psychiatric commentators are in general agreement that *DSM-III* marks the beginning of the new scientific psychiatry. For example, Robert Spitzer, *DSM-III*’s principal architect, calls the manual a “signal achievement for psychiatry” and “an advance toward the fulfillment of the scientific aspirations of the profession” (Bayer & Spitzer, 1985, p. 187). In chorus with Spitzer, acclaimed psychiatrist Gerald Klerman, speaking at the 1982 APA conference (organized around the theme “Science in the Service of Healing”), asserts:

DSM-III represents a fateful point in the history of the American psychiatric profession. . . . The decision of the APA first to develop *DSM-III* and then to promulgate its use represents a significant reaffirmation on the part of American psychiatry to its medical identity and its commitment to scientific medicine.” (1984, p. 539)

In a similar vein, the latest edition of the APA manual, *DSM-IV*, uses an only slightly more moderate tone to call *DSM-III* a “major advance” that has “greatly facilitated empirical research” (APA, 1994, p. xviii). Clearly the inauguration or, better yet, the coronation of *DSM-III* has been a turning point in the new psychiatry’s self-understanding as a more rigorous science.

Significantly, the new *DSM-III* attempted to bring not only a heightened scientific psychiatry but also an atheoretical or theoretically neutral psychiatry. Indeed, Joseph Margolis argues, in a philosophical review of *DSM-III*, that theory neutrality is the “master theme” of *DSM-III* (1994, p. 106). Margolis does not have a difficult time making the argument. Indeed, the insight that theory neutrality is the master theme of *DSM-III* requires little philosophy. Spitzer himself makes the goal of theory neutrality plain both in his introduction to the *DSM-III* and again in a review explanation of *DSM-*

III's method: “[*DSM-III*] takes an atheoretical approach with respect to etiology”

(Margolis, 1994, p. 106). Spitzer’s justification is as follows:

given the present state of ignorance about etiology, we should avoid including etiological assumptions in the definitions of the various mental disorders, so that people with different theories about etiology can at least agree on the features of the various disorders without having to agree on how those disorders came about.
(Margolis, 1994, p. 106)

It is clear from this quote that a core originating impulse of *DSM-III* was to be theory neutral with respect to etiology. This state of affairs has changed little in recent years. Though the *DSM-III*’s goal of “theory neutrality” has been extensively criticized (see Margolis for an example), the recent publication of the latest *DSM* reproduces this same theme. According to *DSM-IV*’s introduction, the uniqueness of *DSM-III* was that it formally introduced into psychiatry the “important methodological innovation” of a “descriptive approach [to psychiatric diagnosis] that attempts to be *neutral with respect to theories*” (APA, 1994, p. xviii; italics added). It seems that Margolis’s conclusion that theory neutrality is the “master theme” of *DSM-III* captures the rhetoric of the new *DSM-IV* as well.

This is not particularly surprising, and I suspect it will be quite difficult for psychiatry to give up its newfound “atheoretical identifications.” I say this because it is *DSM-III*’s theory neutrality that, according to the “scientific revolution” narrative of the new psychiatry, more than anything else, finally allowed psychiatry to rid itself of prejudice and superstition and thus take its rightful place among the objective sciences. Indeed, the new psychiatry saw the move to an atheoretical scientific *DSM-III* as a move from psychiatric Myth to psychiatric Truth. This will not be an easy identity to shake. Richard Wyatt, former Chief, Adult Psychiatry Branch, Division of Intramural Research,

National Institute of Mental Health and an important contributor to the rise of scientific psychiatry, proudly puts it this way:

Good psychiatry requires careful observations and descriptions, *unvarnished by theory*. This point is demonstrated by the changes made from the second edition of the *Diagnostic and Statistical Manual of Mental Disorders (DSM-II)* to the third edition (*DSM-III*); the latter is an attempt to describe things as they are, but the former often blurred observations and interpretations. *DSM-III* adds objectivity, reliability, and prognostic validity. . . . It uses the minimal level of inference necessary to characterize the disorder. This movement toward clear, unambiguous description of psychiatric syndromes lays an important foundation for correlative and experimental exploration of the psychiatric illnesses. (1985, p. 2018; italics added)

Wyatt interprets “good psychiatry,” clearly the object of psychiatric desire, as psychiatry that operates with the benefits of *DSM-III*’s improved scientific methodology. Good psychiatry, for Wyatt, operates without the distortions of theory and progressively advances toward the “unambiguous description” of psychiatric syndromes and their eventual treatment. For Wyatt, the advance of science in psychiatry leads unquestionably toward advance in psychiatry. What is good for science in psychiatry is good for psychiatry. As a consequence, “bad psychiatry,” for Wyatt, can be understood to be all psychiatry that relies on “blurred” alternatives. Bad psychiatry is based on nonscientific, non-*DSM-III*, approaches. Thus, any approaches to psychiatric problems not based on *DSM-III*, whether they be psychoanalytic, existential, family, social, political, philosophical, pastoral, narrative, or cultural, are simultaneously put out of play. These alternative approaches do not have to be addressed directly on their own merit or even tended to in their specifics. They are simply dismissed through an all-encompassing

charge that, like superstition, they are little more than confused smears of “observations and interpretations.”

Science Studies and the Critiques of Atheoretical Science

Thus, the new psychiatry has come to organize itself around a trope of “atheoretical science.” But is science best understood as atheoretical? Are there other ways to understand how science works? If so, what are the effects and consequences of alternative understandings? I find these questions are rarely posed in the literature on scientific psychiatry. For the new psychiatry, that science is “atheoretical” and that it is the obvious route to “progress” are the founding assumptions on which psychiatry has justified its revolution. When I step outside the psychiatric literature to evaluate and analyze this assumption, however, I find a wealth of scholarly material that would suggest a more complex perspective on science. Indeed, it is difficult to decide where to begin an assessment of the new psychiatry’s “atheoretical science,” because there is literally too much literature complicating this scientific ideal to choose from. As Sharon Traweek explains, over the past 30 years in the interdisciplinary domain of “science studies” there has been “a near avalanche of research on the way communities of scientists, engineers, and physicians make knowledge,” research that critiques the view of science as theory neutral (1993, p. 4).

According to Traweek’s estimates, there are now over 20 academic disciplines studying science, medicine, and technology as a social, rather than an atheoretical, phenomenon. These disciplines include philosophy, anthropology, architecture, art history, business and public administration, cultural studies, economics, education, history, international relations, law, literature, political science, psychology, public

health, religious studies, sociology, and women's studies (Traweek, 1993, p. 4). David Hess, in a recent review of science studies, attempts to sort out and simplify the vast literature on science by dividing science studies into four broad scholarly genres or research traditions: history and philosophy of science, sociology of science, social studies of scientific knowledge, and feminist and cultural studies of science. Hess argues, and I would agree, that, though science studies is not unanimous and is at times quite acrimonious, as a whole it provides a rich "conceptual tool kit" for a more nuanced and complex understanding of the very possibility of an "atheoretical" model for science, technology, or medicine (1997, p. 1).

What have science studies scholars come to understand about science, and how well does their understanding match psychiatry's recently trumpeted atheoretical perspective of science? Traweek attempts to work out an answer to the first part of this question by articulating several widely accepted "findings" of the last 30 years of science studies research (1996, p. 140). Most of these findings are correctives to the "received view" of science as objective and theory neutral. For Traweek, basics of the received view of science include:

- Scientific method identifies and controls all variables in an experiment.
- Scientific knowledge is amassed progressively and cumulatively.
- Scientific reasoning proceeds by deduction and induction; hypotheses are deduced from existing experimental data, and experimental data are tested against hypotheses inductively.
- Scientific research is made objective by eliminating all biases and emotions of the researchers.
- Scientific research is neutral with respect to social, political, economic, ethical, and emotional concerns.
- Scientific research has an internal intellectual logic; there is [also] an external social, political, economic, and cultural context for science that can only affect which scientific ideas are funded or applied.

- Improvements in the quality of human life and the duration of human life during the past 200 years are due primarily to the application of scientific discoveries.
- Technology is applied science.
- Basic research and applied research are easily differentiated.
- There is a significant rate of “social return” on scientific research. (edited from Traweek, 1996, p. 141)

According to Traweek, these received views of science are usually narrated indirectly in the form of what she calls “reverential stories.” These stories include a “list of saints’ (geniuses’) lives, their miracles (discoveries), and holy sites (laboratories) and can usually be found in television documentaries, basic textbooks, and official histories of science” (Traweek, 1996, p. 141). Because psychiatry has recently adopted this very same received view of science, it is perhaps not surprising that the new psychiatry is also rapidly putting together its own reverential story (like the one found in Maxmen’s *The New Psychiatry*) centering around the recent miracle of *DSM-III* and the saints who devoted themselves to its development.

The received views of science, however, have been powerfully challenged by the last 30 years of science studies. From Traweek’s perspective (though like Hess she finds science studies not to be a unified whole), science studies scholars generally agree on basic alternatives to the received view—whether these scholars call themselves “empiricists, nominalists, postmodernists, feminist epistemologists, actor-network theorists, post-Althusser/post-Gramscian Marxists, systems analysts, chaos theorists, discourse analysts, ethnomethodologists, postcolonialists, [or] constructivists” (1996, p. 148). For Traweek, these largely held agreements include:

- There are many practices called “science” by their practitioners, not one such practice; there are many methods called “scientific method” by their practitioners, not one such method. That is, each research subfield has its own distinctive

research practices. Hence, the proper terms are plural: “sciences” and “scientific methods.”

- The forms used in scientific writing have converged and have not varied significantly over the last couple of centuries. For example, all references to the agency of the scientists involved in the research is minimized. The written presentation of findings has become quite stylized and terse; it would be almost impossible to reproduce an experiment based upon the information provided in scientific articles.
- Access to scientific knowledge is highly restricted. That is, there is restricted access to different stages of training; to findings, positions, publications, and conferences—the whole infrastructure of knowledge production and consumption.
- Problem selection is a process highly subject to the available resources.
- Adjudicating which experimental data to take as facts and which theories to take as important is a collective process conducted by those who are tacitly empowered with the authority to participate; it does not include all practicing scientists in a particular field.
- Closure of debates about the status of data and theories is not accomplished with definitive findings as to their truth status, but with a consensus that certain data and/or theories are more useful to more of the practitioners who are entitled to participate in the debate.
- The forms of reasoning conducted in research communities as they interpret the signals from their research equipment recapitulate all the known forms of human reasoning.
- Being conducted and constructed by groups of human beings, scientific, technological, and medical practices and ideas are necessarily social and human. Because those practices and ideas are about the phenomenal world, they often, but not always, also require an engagement with that world. What constitutes a satisfactory engagement with the phenomenal world is necessarily open to debate among the practitioners.
- The definition of science is made by those who are empowered to offer resources for work they consider scientific; for example, the work funded by the NSF, SSRC, NIH, or NIMH is science. (edited from Traweek, 1996, p. 144)

Probably the most succinct and generally agreed upon phrase that encompasses these findings comes from Andrew Pickering: “science as culture and practice” (1995, p. 1). This phrase builds on and fine tunes the more polemical claim of Bruno Latour, “the status of a [scientific] statement depends on later statements” (1987, p. 27). Fewer science studies scholars are comfortable with Latour’s version, however, because it

bumps too closely into the problem of relativism. I will have much more to say about relativism in chapter 2, but for now let me just say that neither Traweek nor Pickering in their summary descriptions of “science as culture and practice” mean to say that anything goes in science. They do very much mean to say, however, that the status of accepted and legitimized scientific knowledges is determined largely by social and cultural phenomena. Of course, not everyone who considers herself or himself a science studies scholar would agree with all of Traweek’s assertions or with Pickering’s summary notion of science as practice and culture. However, as Traweek points out, “most researchers take these statements as a sort of boring baseline of shared knowledge in the field” (1996, p. 144).

The wide “science as practice and culture” agreement among science studies scholars creates something of a dilemma if one wishes to take *DSM-III*’s manifest content literally. Indeed, since these science studies findings are so much at variance with the “atheoretical” received view of science, it is difficult to understand how those who “do science” (like the new psychiatry) and those who “study science” have such divergent opinions about how science works. In other words, if science studies is “right,” why is it that science advocates such as supporters of the new psychiatry, in the face of so much literature which complicates and reconsiders the standard view of science, “have such turgid notions about science, engineering, and medicine, [which are] often spoken with either an ex cathedra voice or a pounding clenched-fist-in-the-face voice?” (Traweek, 1996, p. 145). The answer Traweek offers is that the science practitioners and science advocates lack familiarity with science studies: “To continue in these [received] beliefs [of science] is to signal that one is unfamiliar with the massive body of scholarship that

has undermined them” (p. 141). Traweek’s explanation of scientists’ unfamiliarity with science studies is largely a cultural explanation. After all, different disciplines are in many ways different cultures, and different disciplinary discourses are in many ways culture bound. As a result, it is plausible that science advocates, such as those championing the new psychiatry, just do not know about the science studies research.

Traweek’s unfamiliarity thesis was perhaps more plausible before the recent publication of Paul Gross and Norman Levitt’s *Higher Superstition* (1994). Gross and Levitt’s book, by my interpretation, is a shrill and extended polemic against science studies launched by two staunch conservatives of science. However their work is interpreted, one thing is certain: Gross and Levitt are not unfamiliar with science studies. Indeed, it is because of their great familiarity with the avalanche of research coming from feminists, multiculturalists, and social critics in science studies that they wrote the book. For Gross and Levitt, *Higher Superstition* is a wake-up call to scientists unaware of the dangers posed by this diverse group of “science-bashers” which they place under the general label “science studies.” Gross and Levitt set a shrill tone of “crisis” rhetoric that the popular press has picked up. Indeed, the result has been a series of popular press scare stories warning the public against insurgent “antiscience” movements across academe (Ross, 1996b).¹

¹ Many in the scientific community have been quick to embrace Gross and Levitt’s attack, and this is perhaps best represented in the much published “Sokal” affair. In this incident, Gross and Levitt’s arguments were brought to a wide audience when they inspired the like-minded physicist, Alan Sokal, to empirically prove the ridiculousness of science studies research. Sokal, in an unprecedented and nonconsensual “experiment” on the editors of the journal *Social Text*, submitted an article in which he pretended to critique an overly objective or theory-neutral perspective of physics. When the article was published, he announced that he had been disingenuous all along and that what he passed off as physics in the paper was all wrong. Sokal’s conclusion from his “experiment,” which was also carried pretty much verbatim in the news stories, was that the *Social Text* editors’ inability to pick up his faulty physics (and/or their failure

In the wake of *Higher Superstition*, Traweek's unfamiliarity thesis alone is not enough to explain continued resistance in the halls of science, including psychiatry, to the major agreed upon findings in science studies. In my discussions with psychiatrists, though unfamiliarity may account for some of their resistance to science studies, it is far from the whole picture. Granted, as of now, psychiatrists are not particularly aware of or interested in science studies. Science studies is not, for example, a literature that psychiatrists are discussing or writing about. However, my discussions with psychiatrists suggest that if they were to become aware of the science studies literature, they would not find it persuasive. Rather, most would likely react similarly to Gross, Levitt, and Sokal. They would exclaim some kind of righteous indignation. I make this conclusion based on the reactions that psychiatrists I have talked with have had to the editorials surrounding the Sokal affair. According to my own (undocumented and unsystematic) qualitative analysis, most psychiatrists who are aware of the Sokal affair at all think that Sokal was right and that this "science studies crap is way out of hand." Thus, counter to Traweek's unfamiliarity thesis, it would seem that familiarity with science studies among science advocates does not bring reconsideration of science. Instead, it brings contempt of science studies.

If unfamiliarity is insufficient to explain the resistance to these critical science studies "findings," what else is contributing? Unless we posit that science studies as a group is all-wrong about science (and, to give a sense of the acrimony within science studies, some "realist" philosophers of science go almost this far), one answer to why critical science studies findings are resisted seems to be the way the "science" trope is

to send the article out for review to a qualified physicist) proved that *Social Text*, and science studies in general, is full of pretentious nonsense.

used to struggle for legitimacy and power. In the case of the new psychiatry, by championing a rigorously scientific theory of neutrality, psychiatrists join hands with the other scientists to become what feminist science historian Donna Haraway calls the “modest witnesses” of nature (1997, p. 24). For Haraway, the scientist as modest witness is

the legitimate and authorized ventriloquist for the object world, adding nothing from his mere opinions, from his biasing embodiment. And so he is endowed with the remarkable power to establish the facts. He bears witness: he is objective; he guarantees the clarity and purity of objects. His subjectivity is his objectivity. His narratives have a magical power—they lose all trace of their history as stories, as products of partisan projects, as contestable representations, or as constructed documents in their capacity to define the facts. The narratives become clear mirrors, fully magical mirrors, without once appealing to the transcendental or the magical. (p. 24)

Thus, when the new psychiatrist adopts the posture of modest witness, like the scientist he emulates and imitates, he may claim: “I have nothing to do with the form this knowledge has taken. Nature made me organize it this way.” In reward for accepting a “passive” position with respect to nature, the psychiatric researcher fully expects to inherit the power and authority of science.

To put it another way, through aggressive theory neutrality, psychiatric science joins with science in general to achieve what both Haraway and Traweek have called the magical position of a “culture of no culture” (Haraway, 1997, p. 23). By adamantly denying the theory-laden and culturally contextual dimensions of psychiatric knowledge, scientific psychiatry denies being situated in a culture. When the new “atheoretical psychiatry” presents itself as a culture of no culture, the personal interests and social

biases of psychiatric researchers drop out of the picture of psychiatric knowledge. All that remains is the freestanding Truth of psychiatric research.

Haraway's and Trawick's incisive and evocative interpretations of "modest witness" and "culture of no culture," along with the extensive grounding of their insights in the vast science studies literature, offer compelling arguments for concluding that psychiatry's "atheoretical" interpretation of itself is much more complicated than it is aware or that it acknowledges. However, I don't bring up this telescopic review of science studies to get into a long discussion of the debates and conflicts in the field. I am not interested here in "whether psychiatry is right" or whether "Haraway and Trawick are right." My purpose in reviewing science studies is no more than to drive a wedge in the idea that the new scientific psychiatry is obviously "theory neutral." This wedge gives me room to ask not "who is right" but rather two slightly different questions with regard to the new psychiatry's "atheoretical science": (a) what other way may the role of "theory" in psychiatric knowledge be understood, and (b) what are the politics of the different understandings of "theory" in psychiatry?

This book is largely about the first question. As I have said, I will be arguing that psychiatry let go of its "theory-neutral" ideal in order to explore alternative possible answers to the theory question in psychiatry. However, I do not expect that arguments alone will be persuasive for reworking theory in psychiatry. In other words, I don't see how a book that focuses only on claims for and against theory in psychiatry, no matter how well it is argued, would change a significant number of psychiatric minds. There seem to be too many power issues at stake. Whether "theory" is reworked in psychiatry will depend on more than what versions of theory best capture epistemological and

ontological questions of truth and reality. I will be arguing, therefore, that the theory which is best for “retheorizing psychiatry” is a theory that encompasses not only epistemological and ontological questions of truth and reality but also questions of the politics of truth and reality. Fortunately, I do not have to do this out of whole cloth. There is an extensive literature on theory that already exists across campus in the human sciences. Moreover, fortunately, recent theoretical efforts in the human sciences have already confronted many of the theoretical questions that a retheorized psychiatry faces.

But what is “theory” in the human sciences and how can it help? To answer that question, I suggest that psychiatry break free of its intellectual and disciplinary quarantine and return from across the river, or sometimes simply from across the street, back to the cauldron of productive confusion known collectively as the “main campus.” What psychiatry will discover once it makes this journey is a swirl of activity in a variety of human science disciplines—including English, history, philosophy, communications, anthropology, sociology, and psychology—and in a burgeoning number of multidisciplinary programs—including women’s studies, gay and lesbian studies, Africana studies, disability studies, postcolonial studies, cultural studies, and science studies. Each of these disciplines is deeply engaged in the study of theory, its significance, its blindness, its resistance, and its consequence. Thus, it is crucial for my project of retheorizing psychiatry that throughout the human sciences there already exists for possible psychiatric self-understanding a body of theory deeply engaged not only with questions of reality and truth but also with questions of the politics of truth.

Theory in the Human Sciences

With so much attention to theory across the human sciences, it might seem that it would be easy to give a straightforward definition of “theory.” Unfortunately, from the perspective of simplicity, there is not a universal theory available for retheorizing psychiatry. There is, in other words, *no atheoretical theory*. Theory is not a “natural kind,” by which I mean there is not an a priori theory independent of human activity. Neither is theory linguistically fixed in a stable and reliable relation with terms like “practice,” “fact,” “philosophy,” or “model.” Thus, theory is not found but is always made and constantly remade, and, as such, attempts at formal ahistorical definitions of theory are of little value. Instead of quixotically pursuing a timeless theory, it is better to approach theory by considering the uses to which theory is being put in particular contexts. My focus in this section, having just explored the emergence of the “atheoretical” and “theory neutrality” in psychiatry, will be to explore the emergence of an alternative approach to theory, an approach that has risen to dramatic prominence across the main campus in the human sciences. Kreiswirth and Cheetham sum up the current preoccupation with theory in the human sciences as follows:

However one might look at the humanities and social sciences today, it seems quite clear that the theory wars of the 1970s and 1980s are, for the most part, over and that theory has “triumphed.” There are, to be sure, still skirmishes between those for and those against theory (if one can glibly reduce the rich spectrum of positions to two), disagreements that seem to have moved from the contested arena of literary studies into other disciplinary territories. . . . Yet, whatever one thinks of the diverse debates in the name of or in opposition to this or that theoretical formulation, this or that mode of inquiry, this or that figure, there is little doubt that theory itself has become an obsession of contemporary academics in the humanistic sciences, if not the intellectual community at large. Not only

may we be “theory-mad beyond redemption”—to borrow a phrase of Poe’s—but we may even wonder how desirable such redemption might be, or indeed, how it might be possible to envision it without what we now call theory. (1990, p. 1)

During the same period in which psychiatry has been consolidating itself as “atheoretical,” the humanities and social sciences, and indeed “the intellectual community at large,” have become “theoretical” beyond redemption. In this context, I argue, psychiatry makes a huge mistake to remain obstinately atheoretical and in so doing cuts itself off from dialogue with the broader currents of intellectual thought.

What is the triumphant theory to which Kreiswirth and Cheetham refer? One challenge for making sense of theory in the human sciences is that increasingly “theory” stands alone without delimiting modifiers either before or after. Kreiswirth and Cheetham put it this way, “No longer is the term [“theory”] wedded to antecedent adjectives, as in critical theory, literary theory, or psychoanalytic theory. No longer does it routinely drag behind trailing genitives—of social action, of language, etc. Although we still use such compound formations, an independent and self-sufficient theory . . . has apparently taken over” (1990, pp. 1–2). The advantage of a ubiquitous theory without labels or modifiers in the human sciences is that it makes theory exceedingly difficult to ignore. If theory is ever-present, scholars in the human sciences are less likely to lose their self-reflexivity and imagine that the knowledge they create is “atheoretical” or universally true. The disadvantage of a theory without labels or modifiers is that it is difficult to know what is meant by the term “theory.” In addition, and perhaps even more problematic, an unmodified theory seems to take on an increasingly reified and naturalized status—as if theory is a natural category rather than a historical one. Obviously, in spite of theory’s prominence and seeming self-sufficiency in the human sciences, theory in the human

sciences, like “atheoretical” psychiatry, must be understood as a kind of intellectual practice within a particular historical context.

Thus, to begin the process of clarifying what is meant by “theory” in the human sciences and also to avoid naturalizing the theory I am recommending for psychiatry, I will start by replacing the antecedent labels which theory has gradually lost. In the context of recent work in North American human sciences, “theory” designates a conglomerate of interdisciplinary writings that, though difficult to generalize about, are perhaps best initially labeled with the hybrid phrase “poststructuralist postmodernism.” I borrow this unwieldy phrase from Hans Bertens (1995, p. 6), who also uses it in the context of searching for a way to label recent theory in the human sciences. In addition to being unwieldy, Bertens’s label is unusual because it is much more common to use either the label “poststructuralist” or the label “postmodern” rather than combining them together. It is also a problematic label because both of the terms “poststructuralist” and “postmodernism” are contested and there might be many other terms that could be used (e.g., semiotic, deconstructionist, linguistic, hermeneutic, social constructionist, late modern). In spite of these problems, I agree with Bertens’s general conclusion:

no matter how one would want to draw such lines, in the later 1970s a broad complex of deconstructionist/poststructuralist practices became firmly associated with postmodernism. . . . Nowadays often called “theory”—although it goes against all theory in a more traditional, say Popperian, sense—it has in the course of the 1980s filtered into and affected a large number of disciplines, in which its intellectual premises are usually simply called postmodern or postmodernism. (p. 9)

In other words, by Bertens’s account, recent theory in the human sciences is best understood as an association of poststructuralist and postmodernist writings and themes,

although it is most often given a simplified label of “postmodern” or “postmodernism.”

Since the label “poststructuralist postmodern theory” is so cumbersome, I too will opt for a simplified label. Somewhat (but not totally) arbitrarily, I will settle on “postmodern” or “postmodernism” throughout the book to refer to theory I have in mind for psychiatry.

Before reducing the label to a simplified “postmodern theory” version, however, let me review briefly what these two elements (poststructuralism and postmodernism) in theory represent.

Recent human science theory may be termed “poststructuralist” because that term evokes the intense consciousness of language that is a core feature of current theoretical work. Of course, there are many conceptual tools and disciplinary traditions available to approach and understand language, but in the current scene of North American human sciences, the understanding of language evoked by theory is usually a poststructuralist understanding. Poststructuralism has come to designate approaches to language which began with Saussure, were later developed extensively by Levi-Strauss and other “structuralists,” and were eventually superseded by the “poststructuralist” writings of Lacan, Derrida, and Foucault. The poststructuralist focus on language takes two main themes: (a) a self-reflexive awareness of the role of language in shaping knowledge and practice, and (b) a consistent attempt to chart the effects of power relations in determining language (and therefore knowledge) usage. These poststructuralists’ writings and themes have become so influential in the North American human sciences that their work has often become synonymous with an unlabeled “theory.”

The following examples help make this point more clear. The first example is from a very influential essay by Paul de Man entitled “The Resistance to Theory:”

The advent of *theory* . . . occurs with the introduction of linguistic terminology in the metalanguage about literature. By linguistic terminology is meant a terminology that designates reference prior to designating the referent and takes into account, in the consideration of the world, the referential function of language or, to be somewhat more specific, that considers reference as a function of language and not necessarily as an intuition. . . . Contemporary literary *theory* comes into its own in such events as the application of Saussurian linguistics to literary texts. (1986, p. 8; italics added)

The second example is from J. Hillis Miller's essay "The Triumph of Theory . . .":

By *theory* I mean the displacement in literary studies from a focus on the way meaning is conveyed. Put another way, *theory* is the use of language to talk about language. Put yet another way, *theory* is a focus on referentiality as a problem rather than as something that reliably and unambiguously relates a reader to the "real world" of history, of society, and of people acting within society on the stage of history. (1987, p. 283; italics added).

The last example is from literary and cultural critic Stuart Hall. In his essay "Cultural Studies and Its Theoretical Legacies," Hall argues that a "detour through *theory*" teaches:

the crucial importance of language and of the linguistic metaphor to any study of culture; the expansion of the notion of text and textuality, both as a source of meaning, and as that which escapes and postpones meaning; the recognition of the heterogeneity, of the multiplicity, of meanings, of the struggle to close arbitrarily the infinite semiosis beyond meaning; the acknowledgement of textuality and cultural power, or representation itself, as a site of power and regulation; of the symbolic as a source of identity. These are enormous theoretical advances . . . if you work on culture, or if you've tried to work on some other really important things and find yourself driven back to culture, if culture happens to be what seizes hold of your soul, you have to recognize that you will always be working in an area of displacement. There is always something decentered about the medium of culture, about language, textuality, and signification which always escapes and

evades the attempt to link it, directly and immediately, with other structures.
(1992, pp. 283–284; italics added)

In these three quotations, it is easy to see the poststructuralist, “language to talk about language,” version of theory. In all three of these quotations, “theory” is used to initiate and inspire a linguistic self-reflection for the human sciences. In each quotation, it would work equally well to replace an unmarked “theory” with “poststructuralist theory.” In poststructuralist theory, language is no longer assumed to be a transparent medium available for direct and automatic translation of world to word. In poststructuralist theory, language therefore becomes a concern, a problem, and an object of study in its own right. For literary studies or cultural studies (where these examples originated), poststructuralist theory introduces a moment when it is no longer enough to study literature or cultural artifacts in themselves but becomes necessary to study the mechanism of meaning-making inherent in the very language of literature and cultural artifacts. For this reason, one aspect of “theory” as it is being used in the human sciences can be understood roughly as “poststructuralist theory”—or the study of language and other signifying practices, and, in particular, the way signifying practices are used to make social meaning. Thus, poststructuralist theory in the human sciences recommends that human science scholars, rather than focus exclusively on the content or even the form of a specific literary or cultural object, reconstruct the general system of conventions and linguistic distinctions that enables a specific literary or cultural object to have meaning.

The second label I have marked as relevant for theory is “postmodern.” Theory in the human sciences may be labeled “postmodern” because it represents a break, a rupture, or a discontinuity with modernism. “Postmodern” as a term has been multiply evoked in recent years to refer to a number of breaks with modernism: aesthetic breaks,

architectural breaks, cultural breaks, societal breaks, and philosophical or knowledge breaks. I will be focusing primarily on the last of these. Following Jean-François Lyotard's very influential monograph, *The Postmodern Condition: A Report on Knowledge* (1984), the term "postmodern" was brought into the orbit of poststructuralist theory and came to designate (at least in one of its polysemous usages) a break between modernist forms of knowledge and new postmodernist forms of knowledge. As Lyotard explains:

I will use the term *modern* to designate any science [or knowledge] that legitimates itself with reference to a metadiscourse . . . [of the kind which makes] explicit appeal to some grand narrative, such as the dialectics of Spirit, the hermeneutics of meaning, the emancipation of rational or working subject, or the creation of wealth. . . . Simplifying to the extreme, I define postmodern as incredulity toward metanarratives. . . . To the obsolescence of the metanarrative apparatus of legitimation corresponds most notably, the crisis of metaphysical philosophy and of the university institutions which in the past relied on it. (pp. xxiii–xxiv)

Modernist knowledge grounds itself on a foundation of Truth through Method—like the truth of science through scientific method. Postmodernist knowledge is skeptical and incredulous of the great modernist Truth narratives. In chapter 4 of this book, I will have much more to say about the break between modern scientific knowledge, particularly as it is organized in psychiatry, and the emergence of a postmodern approach to psychiatric knowledge. Here, my goal is not to extensively develop the term, but to demonstrate the way the articulation of a break between modern and postmodern understanding of knowledge has become a predominant organizing theme of recent theory.

Again, some recent quotes from the human sciences will be helpful. For example, the anthropologists George Marcus and Michael Fisher describe the relevance of postmodernism to their efforts in theoretical anthropology this way:

Present conditions of knowledge are defined not so much by what they are as by what they come after. In general discussion within the humanities and social sciences, the present indeed is often characterized as “postparadigm”: postmodernism, poststructuralism, post-Marxism, for example. It is striking that in Jean-François Lyotard’s acute exploration of *The Postmodern Condition: A Report on Knowledge*, he too should cite the contemporary “incredulity towards metanarratives” which previously legitimated the rules of science. He speaks of a “crisis of narratives” with a turn to multiple “language games” that give rise to “institutions in patches.” “Postmodern knowledge,” he says, “is not simply a tool of the authorities, it refines our sensitivity to differences and reinforces our ability to tolerate the incommensurable.” The key feature of this moment, then, is the loosening of the hold over fragmented scholarly communities of either specific totalizing visions or a general paradigmatic style of organizing research. The authority of “grand theory” styles seems suspended for the moment in favor of a close consideration of such issues as contextuality, the meaning of social life to those who enact it, and the explanation of exceptions and indeterminants rather than regularities in phenomena observed—all issues which make problematic what were taken for granted as facts or certainties on which the validity of paradigms had rested. The part of these conditions in which we are most interested is what we call a crisis of representation. (1986, p. 8)

Marcus and Fisher use the tropes of “postmodernism” and the “crisis of representation” (both of which signify for them the break from knowledge as usual) to question modernist scientific approaches to anthropology and to foreground new approaches which have much more room for experimentation and for cultural critique. Though they do not use the term “theory,” native anthropologists would understand what you meant if

you used that term. Indeed, in the halls of the American Anthropological Association conference meetings, an unadorned “theory” is one of the ways anthropologists refer to Marcus and Fisher’s work. This is hardly unique. Repeatedly in the human sciences, postmodernism is equated with “theory” in similar ways.

In other examples, theoretical psychologist Kenneth Gergen refers to postmodernism as ushering in an era of “truth in trouble” (1991, p. 81). Gergen has devoted much of his “theory” career in a series of articles, lectures, and books to advocate for a “postmodern turn” in psychology. Historian Peter Novick pursues a similar tack with his book *That Noble Dream: The Objectivity Question and the American Historical Profession* (1988). He invokes postmodernism this way:

There is no satisfactory term with which to describe the multiple but loosely convergent assaults on received notions of objectivity which swept across the academic world from the 1960s onward. The most common designation is “postmodern.” We are alleged to be living in a “postmodern condition” (Jean-François Lyotard), which encompasses “postmodern politics” (Sheldon Wolin), “postmodern science” (Stephen Toulmin), and a forest of other “posts.” (p. 523)

Philosophy of education and women’s studies professor Linda Nicholson invokes “postmodernism” in a similar manner:

Postmodernists have focused on the growth of science and its widening influence over many spheres of life throughout modernity. They have claimed that in the name of “science,” authority has become exercised in a variety of ways, in the disciplines, the media, popular advice manuals, and so on. By pointing to the element of power in such modern practices, postmodernists have extended the field where power has traditionally been viewed as operating, for example from the state and the economy, to such domains as sexuality and mental health. . . . Therefore, the postmodern critique has come to focus on philosophy and the very idea of a possible theory of knowledge, justice, or beauty. They claim that the

pursuit itself of such theories rests upon the modernist conception of a transcendent reason, a reason able to separate itself from the body and from historical time and place. Postmodernists describe modern ideals of science, justice, and art, as merely modern ideals carrying with them specific political agendas and ultimately unable to legitimize themselves as universals. Thus postmodernists urge us to recognize the highest ideals of modernity in the West as immanent to a specific historical time and geographical region and also associated with certain political baggage. Such baggage includes notions of the supremacy of the West, of the legitimacy of science to tell us how to use and view our bodies, and of the distinction between art and mass culture (1990, p. 4).

Gergen, Novick, and Nicholson, like Marcus and Fisher, all use the notion of postmodernism to invoke a break from modernist approaches to knowledge. In each case, “postmodernism” and “theory” are used interchangeably. Members of each discipline would understand what you meant if you referred to these writers’ efforts either as “postmodernism” or as “theory.” With either term, the point is to convey the notion that key principles of modernism become overturned in postmodernism.

Modernism is not so much supplanted by postmodernism as much as it is problematized. The goal of theory as “postmodernism” is to make modernism visible as a possible “way of life” with a specific set of priorities, rituals, institutions, norms, and expectations and as a way of life that exists among an array of possible alternatives. These alternative ways of life are comparable with one another and modernism is not a hands down winner when compared with other alternatives. Of course, modernism does bring gains along some developmental lines, but modernism does not bring purified progressive gains—rather it brings a series of both gains *and* losses. Sociologist Zygmunt Bauman perhaps most eloquently states this aspect of postmodern theory:

Postmodernity is modernity coming of age: modernity looking at itself at a distance rather than from the inside, making a full inventory of its gains and its losses, psychoanalyzing itself, discovering the intentions it never before spelled out, finding them mutually canceling and incongruous. Postmodernity is modernity coming to terms with its own impossibility; a self-monitoring modernity, one that consciously discards what it was unconsciously doing. (1990, p. 272)

If there is a nonarbitrary aspect of my choice for “postmodern” over “poststructural” as a simplified modifier of “theory,” it is because the resonance of “postmodernism,” which Bauman captures in this quote, is so much in keeping with the spirit of this book. Bauman’s version of postmodernism (his antiutopian emphasis on trade-offs, tough choices, and irreducible conflicts) is the kind of theoretical spirit that I would like to bring to a retheorized psychiatry.

Before concluding this chapter, I would like to discuss a final feature of theory. In addition to theory’s concern with language and with problematizing modernism, theory in the human sciences has also become a form of “postdisciplinary critique.” By “postdisciplinary critique,” I mean to evoke a rising trend in human science writing—what Richard Rorty has called a “new genre” and what Clifford Geertz has called a “blurred genre”—that borrows and intermingles ideas and methods from multiple disciplines to analyze, critique, and ultimately politicize complex disciplinary phenomena not easily reached from within a single disciplinary perspective (Rorty, 1982, p. 66; Geertz, 1973, p. 19). In “theory” as postdisciplinary critique, the self-consciousness that marks theory as poststructuralist and postmodernist reflects back not only on the objects of the human sciences but also on the practices of human sciences as well. As such, a final feature of theory in the human sciences is that previously stable and accepted

disciplinary definitions, categories, and boundaries have become objects of intense debate and controversy.

Thus, the term “theory” has come to signify not only poststructuralist and postmodernist preoccupations but also the new postdisciplinary genre that has resulted from recent theoretical analysis and reflections. For example, Jonathan Culler evokes theory as postdisciplinary critique when he states,

Theory is a genre because of the way its works function. . . . what distinguishes the members of this genre is their ability to function not as demonstrations within the parameters of a discipline but as redescriptions which challenge disciplinary boundaries. The works we allude to as “theory” are those which have had the power to make strange the familiar and to make readers conceive of their own thinking, behavior, and institutions in new ways. (1982, p. 9)

Thus, as a new genre of postdisciplinary critique, theory is a kind of political protest writing against the limits and restrictions of disciplinary knowledge. Critique of disciplinary knowledge as usual in the human sciences is central to this new form of writing. If the goal of theoretical inquiry, as cultural studies scholar Lawrence Grossberg is fond of saying, is to answer Marvin Gaye’s eternal question “What is going on?” then the choice of research and conceptual tools used must depend on the kinds of questions being asked. In addition, the questions being asked must themselves depend on the particular historical context (“What is going on?”) rather than predetermined methodological restraints. To adopt, uncritically, formalized disciplinary methods and practices often places inquiry into a straitjacket (and a quiet room) before it begins. This happens because the very disciplinary methods and practices being used (and the distinctions, priorities, and rituals they inscribe) too often carry with them a heritage of

investments, exclusions, and social effects that theoretical inquiry is attempting to analyze.

Thus, critique of established knowledge formations is key here. As Kreiswirth and Cheetham point out, the signifiers “theory” and “critique” have become an almost ubiquitous co-occurrence in recent academic debates and are used practically interchangeably in conferences, books, institutes, and papers (1990, p. 2). “Theory” as a new critical genre, as a postdisciplinary critique, in addition to being critical of disciplinary boundaries is increasingly critical of historical and ideological domination and oppression as well. In the “Triumph of Theory,” Miller argues that “theory” has become particularly attuned to “history, culture, society, politics, institutions, class and gender conditions, the social context, the material base in the sense of institutionalization, conditions of production, technology, distribution, and consumption of ‘cultural products,’ among other products” (1987, p. 283). As such, postdisciplinary critique is meant not only to understand but also to intervene in these political issues and concerns “beyond” disciplinary boundaries.

Literary theorist, bell hooks, picks up these themes to argue that “theory” has a particular role in explaining current political antagonisms outside, as well as inside, the academy. hooks argues that “radical postmodernism, most powerfully conceptualized as a ‘politics of difference’ should incorporate the voices of the displaced, marginalized, exploited and oppressed” (1990, p. 25). Political theorist, Chantal Mouffe, echoes hooks (and Bauman) in her reflections on the political role of theory:

The challenge to rationalism and humanism does not imply the rejection of modernity but only the crisis of a particular project within modernity, the Enlightenment project of self-foundation. Nor does it imply that we have to

abandon its political project, which is the achievement of equality and freedom for all. In order to pursue and deepen this aspect of the democratic revolution, we must ensure that the democratic project takes account of the full breadth and specificity of the democratic struggles in our times. It is here that the contribution of the so-called postmodern critique comes into its own. (1993, p. 12)

In its many manifestations, theory as a genre of postdisciplinary critique is a kind of rebellion against (and reform of) status quo assumptions about academic work and the relation of academic knowledge to economic and political practices outside the university. Theory as postdisciplinary critique seeks to liberate the human science disciplines from rigid disciplinary boundaries not for the sake of liberation alone, but in the service of political and social liberation outside the academy as well. Harking back to Marx, theory as a genre of postdisciplinary critique seeks to use the human sciences not only to understand the world but also to affect it. In the words of Donna Haraway, “The point is to make a difference in the world, to cast our lot for some ways of life and not others” (1997, p. 36). Or, in another of Haraway’s incantations, “The point is to learn to remember that we might have been otherwise, and might yet be, as a matter of embodied fact” (1997, p. 39). In other words, the goal of theory as postdisciplinary critique is to help reimagine and rearticulate the world in another way.

Summing up this section, I have not provided an essential or foundational definition of “theory” in the human sciences nor have I attempted to discover the “Truth of Theory.” Rather, I have attempted to weave a garland of possible meanings out of the many thematics that have been associated with recent theory in the human sciences. Theory in the human sciences is poststructural; it is postmodern; and it is a kind postdisciplinary critique. This formation of theory provides powerful tools and opportunities to break away from modernist disciplinary practices as usual and to

embrace a much more nuanced understanding of the role of language and power in the shaping of knowledge.

Psychiatric Studies—“Rethorized”

Out of this swirl of “theoretical” activity in the human sciences, my proposal for reinvigorating psychiatric studies emerges and takes shape. A retheorized psychiatry that reengaged itself with the human sciences, indeed a psychiatry that maintained its connections with general intellectual thought, would be a psychiatry that accepted and seriously wrestled with (rather than aggressively attempted to jettison) theory. A retheorized psychiatry would allow itself to be decentered and dislocated from its increasingly settled path. It would address rather than efface the multiple determinations, besides objective Truth, of the currently leading representations of psychiatric knowledge. A retheorized psychiatry would thus, in the words of Edward Said, wrestle with “the fact that a representation is *eo ipso* implicated, intertwined, embedded, interwoven with a great many other things besides the ‘truth,’ which is itself a representation” (1978, p. 272). Once this idea is fully understood, I believe it will seem impossible, at least for some, to go back to the rhetoric of “atheoretical” psychiatry.

The rest of this book will be devoted to working through what retheorizing psychiatry might mean. In the next chapter, I start this project by turning more specifically to the question of language by sifting through theoretical literature on “the sign” for an articulation of an applied theory of representation.

Chapter 2: Working Through the Sign

Recent “theory” literature from the human sciences is invaluable for informing and expanding questions of psychiatric representation. In the last chapter, I discussed how “theory” is at least partially indebted to poststructuralism. Two key concerns of poststructuralism—language and power—will thus be central for a retheorized psychiatry. Starting with language, this chapter will focus on the role of language and linguistic representation in psychiatric knowledge formations. I will develop an applied theory of representation by working through three theories of the sign. Toward the end of the chapter, I will draw out some practical implications of this effort by considering epistemic principles that may be used as criteria for evaluating psychiatric knowledge. In the next chapter, I will turn my attention to the question of power in psychiatric knowledge production.

Psychiatric Knowledge as Linguistic Representation—Developing an Applied Theory of Representation

Psychiatric knowledge is not a direct understanding of people and their problems, though this fact is easily and often repressed (forgotten, ignored, disavowed). Psychiatric knowledge is a representation. Psychiatric knowledge is a set of shared meanings through which psychiatric stakeholders communicate with themselves and others. These shared meanings are constructed in the medium of language (which in this context is broadly understood to include all cultural signifying practices). Psychiatrists and those who understand them must communicate through shared sets of concepts, images, and ideas

which enable them to interpret, feel, and act in the world in roughly similar ways. In other words, these representational codes not only allow communication between psychiatrists and others, they also provide resources for interpreting the world. Psychiatrists, to the degree they are part of the same community, interpret their patients (sometimes called “consumers” or “survivors”), their personal and family relations, and even their colleagues’ thoughts, moods, expressions, dress, behaviors, and bodies in roughly similar ways because they use roughly similar representational resources. When these psychiatric representations move out into the general culture, as they most certainly do, they become significant tools of cultural self-understanding.

Since psychiatric knowledge is a representation and representational language is unavoidable for communication about psychiatric concerns, one of the most important roles for theory in retheorized psychiatric studies is to develop an applied theory of representation. Such a theory would serve as an abstract reflection on how representation works, what are its strengths, and what are its limitations. It would provide a philosophical touchstone for psychiatric controversies, and it would provide an improved perspective on psychiatric knowledge formations. It is important, however, to emphasize the adjective “applied.” Psychiatric studies does not need an authoritarian theory of representation that, similar to scientific method or positivist style philosophy of science, would attempt to provide wholesale and definitive answers regarding (and procedures for determining) what should count as valid psychiatric knowledge (Rouse, 1996, p. 21). Psychiatric studies has already suffered too much from the rather arrogant way in which critics, using science or philosophy as their supertext, have attempted to define good (true) and bad (false) psychiatric knowledge from a narrow and limited position. When

science or philosophy attempts to legitimize knowledge in this manner, it too often becomes little more than a disguised promotion of idiosyncratic and culturally bound value judgments.

This being said, however, an applied theory of representation should not eschew all evaluative concerns, and it should not sidestep the related problems of “anything goes” relativism and “cultural” relativism. An applied philosophy of representation must avoid “anything goes” relativity. At the same time, it must be able to evaluate knowledge in the context of multiple cross-cultural interchanges—where a robust anthropological assumption of equal complexity across cultures and subcultures holds true. My way around the problem of “anything goes” relativity is to develop principles of evaluation that are flexible enough to serve as local and specific evaluative criteria without overly constraining the results to one “true” representational possibility. My way around the problem of “cultural relativity” is to include cultural politics in the process of knowledge formation. Politics requires networking, association, coalition, and so on. For knowledge formations to be accepted, they must have enough backers to form a cultural or subcultural group. Thus, the goal of representational evaluation is not to discover “the one universal Truth.” Rather, the goal is to appreciate and understand the possibilities and trade-offs of alternative ways of organizing the world, which are generally alternative cultural (or sub-cultural) ways of organizing life.

Philosopher Peter Winch eloquently expresses a version of what I have in mind: “Seriously to study another way of life is necessarily to seek to extend our own—not simply to bring the other way within the already existing boundaries of our own” (1977b, p. 176). Since alternative ways of life and alternative representational practices go hand

in hand, Winch can be read just as well with an emphasis on alternative knowledge formations as he is with his own emphasis on alternative ways of life. In other words, an applied theory of representation should help psychiatric studies understand how another individual, another subculture, or another culture may use alternative psychiatric knowledge formations in organizing their way of life; it should not attempt to dictate another's world according to a single standard.

This being said, evaluating knowledge representations from another culture or subculture is particularly difficult because the evaluator not only confronts another way of organizing human meaning but also encounters his or her own ethnocentric biases—the tendency to see other ways of being as inferior ways of being (Crapo, 1990, p. 31). As Winch further suggests,

The concept of learning which is involved in the study of other cultures is closely linked with the concept of wisdom. . . . What we may learn by studying other cultures are not merely possibilities of different ways of doing things, other techniques. More importantly we may learn different possibilities of making sense of human life, different ideas about the possible importance that the carrying out of certain activities may take on for a man, trying to contemplate the sense of his life as a whole. (Winch, 1977b, pp. 182–183)

In short, from Winch's anthropologically inspired philosophy, the world is multifaceted and knowledge is not universal. There are multiple ways of giving meaning to the world, and there are multiple ways of prioritizing human activities and practices. In addition, for Winch, these human meanings and activities exist within a cultural frame. This is key, because Winch recommends that, when we compare specific meanings or activities across cultures or subcultures, we compare not simply specific meaning to specific meaning or activity to activity but way of life to way of life. For example, one way of life

may have a “more efficient” mode of transportation (cars) compared with another (walking). To understand and contrast the relative meanings and importance of these specific phenomena, however, one has to understand and compare the whole way of life that is organized around the different specific phenomena. Cars, therefore, may not be more “efficient” when one adds all the waste of production, consumption, and pollution that goes with cars and with the roads and other infrastructure that supports the way of life of auto transport. Thus, the comparison question is not simply cars versus walking, but rather a way of life organized around cars compared with a way of life organized around walking. This is a much more complicated kind of comparison, and it undermines the certainties of ethnocentric bias.

From these reflections, the tasks of an applied theory of representation emerge: it should avoid single-reality perspectives, ethnocentrism, and “anything goes” relativity; simultaneously, it should supply evaluative criteria for judging particular knowledge formations. To meet these goals, my applied theory of representation must work through a theory of the sign. I say this because representational languages are composed of signs. In other words, languages use elements to stand for or represent thoughts, concepts, ideas, or feelings. Spoken languages use sounds, written languages use words, visual languages use images, fashion languages use clothing, body languages use gestures, and facial languages use arrangements of facial features. All these elements, or signs, are used in the service of communication and performative interaction. In each case, the elements of a language—sounds, words, images, clothing, gestures, or facial features—are used to construct meaning and transmit it. In other words, they signify. They are the vehicles or media that carry meaning because they operate as signs, which stand for or represent

(signify) communicated meanings. Simply put, representation is a function of signs, and, accordingly, a theory of representation must rely on a theory of signs.

Signs, however, are complex. There are a variety of theoretical approaches to them, and each theory of the sign suggests an alternative theory of representation with an alternative metaphysics and an alternative epistemology. In this chapter, I will look at three theories of the sign—Western common sense (referential theory), poststructuralism (relational theory), and pragmatism (consequential theory)—in order to unpack their implications for an applied theory of representation. My goal is to further articulate possibilities for evaluating psychiatric representations and representational practices. Much of my emphasis will be on the poststructural and pragmatic theories of the sign because these perspectives are least developed and least understood in psychiatric communities. Ultimately, my applied theory of representation will include aspects of all three theories of the sign. My emphasis on the poststructural and pragmatic, however, helps me avoid the usual impasse that arises in informal psychiatric conversations and in the formal philosophical and sociological discussions of psychiatric knowledge. This impasse may be articulated as: *either* psychiatric science reflects the real world, *or* psychiatric science is a social construction that (if it reflects anything) reflects only the interests of dominant groups (Bernstein, 1983, p. 2). In the end, I will privilege pragmatic approaches for my suggested philosophy of representation because they seem best able to incorporate, and hold in tension, elements from both sides of this impasse.

Theories of the Sign: Western Common Sense and Analytic Philosophy

Starting with common sense, at least Western common sense, a sign is something that refers to, stands for, or indicates, something in the world. By “common sense” I

mean that this “referential” theory of the sign comes closest to outlining the default, but unarticulated, background assumptions that most Westerners seem to have for how signs work. For example, the sign “tree,” from a referential perspective, stands for, or indicates, a tree. Of course, this “something in the world” can be conceptual as well as material. As such, a sign can stand for an idea, say “freedom,” just as well as it can stand for a thing. But in either case, it is understood that there is “something in the world” which the sign points to that would be there independent of whether humans had a tag for it or not. This Western common sense approach to the sign was taken up by Gottlob Frege at the end of the 19th century and has been further articulated by much work in the analytic philosophy of language. For Frege, as for most adherents of Western common sense, including psychiatric common sense, sentences are composed of words, and the truth conditions of sentences depend on the *reference* relations between words and objects (Frege, 1952). Thus, the truth conditions for a sentence like “Vincent Van Gogh had bipolar disorder,” for example, may be expressed in terms of reference as follows:

- “Vincent Van Gogh had bipolar disorder” is true if and only if
- a) there is some object that “Vincent Van Gogh” designates and
 - b) “bipolar disorder” applies to that object.

There are two distinct reference relations in this sentence: (a) *designation*—holding between the name Vincent Van Gogh and an object; and (b) *application*—holding between the predicate bipolar disorder and many objects, bipolar ones (Devitt & Sterelny, 1993, p. 18). For the sentence to be true, it must refer to a Vincent Van Gogh who was actually Bipolar. This Western common sense theory of the sign, which has been elaborated by analytic philosophy, may be called the *reference theory of the sign*.

Within analytic philosophy, the reference theory of the sign is far from monolithic, and it has been the source of considerable debate (for a discussion see Devitt & Sterelny, 1987). Instead of outlining the details of these controversies, however, I want to focus on the larger implications of referential theories of the sign. These implications are crucial because, as I mentioned, the reference theory of the sign, being Western common sense, is the unofficial working theory of the sign in the psychiatric community. For my purpose in this chapter—working out an applied theory of representation—the details of analytic philosophy of language are not as important as the metaphysical and epistemological implications and commitments that often follow from a reference theory of the sign. I am not attempting to say that these commitments necessarily follow, only that reference theories of the sign tend toward a particular direction. It is possible to develop reference theories of the sign that are consistent with a variety of metaphysical and epistemological positions. What I am attempting to demonstrate in this section, however, is that when the psychiatric community adopts a reference theory of the sign, they are also inclined to adopt (either voluntarily or against their will) a certain theory of the world and of truth.

Starting with metaphysics, the theory of the world most consistent with reference theories is *realism*. In reference theories of the sign, the meaning of the sign is created by something outside the sign, something in the external world. Reference theories attempt to connect the sign directly to the “real world,” which is the world independent of what anyone might say or think about it. Of course, signs may still be arbitrary in a reference theory in the sense that other signs might have been chosen at the time of referential

dubbing. Signs are not arbitrary, however, in the sense that there is any doubt that the thing being named is real.

It follows from this that the epistemology, or theory of truth, most consistent with reference theories is a correspondence theory. In a correspondence theory of truth, the truth of a sign depends on its correspondence with the actual world, the world independent of human representations. There is a direct connection between signs and reality, and this connection determines truth. In a correspondence theory of truth, tradition, authority, intuition, emotions, and desire are largely irrelevant in determining truth—"Don't tell me how you feel about Van Gogh, or what your art classes told you about him, tell me the facts!" What matters in a correspondence theory of truth is whether there is a direct connection or correspondence with the actual world. Thus, the reference theory of the sign implies, and is most consistent with, a metaphysics of realism and an epistemology of correspondence.

But a realist metaphysics and correspondence theory of truth are problematic for an applied theory of representation because they tend to push psychiatry toward authoritarian and universalistic approaches to psychiatric knowledge evaluation. Psychiatry's tendency to accept these authoritarian and universalistic approaches is, from my perspective, the major problem of psychiatry today. What do I mean by this? Psychiatric knowledge artifacts (research papers, conference presentations, committee meeting conversations, journal editorials, pharmaceutical advertisements, drug company declarations, case study portrayals of clinical encounters, etc.) are composed of signs. In a realist metaphysics and a correspondence theory of truth, those signs are assumed to be true to the extent that they correspond to the real world. Therefore, it follows from this

perspective that the task of psychiatric knowledge evaluation is to determine whether the signs of psychiatric knowledge correspond to the real world. From such a perspective, correspondence is all that matters in legitimizing knowledge. Other evaluative aspects of knowledge, such as its coherence with tradition or its potential consequences, are not considered relevant. Accordingly, knowledge that corresponds to the real world is assumed to be necessarily true, which allows wielders of that knowledge to take an authoritarian stand toward non-knowledge holders. Also, it allows knowledge holders to assume they are universally right, regardless of contextual settings and alternative perspectives.

In psychiatry this realist metaphysics and correspondence theory of truth are resulting in an intolerant approach to inquiry that privileges empiricist methods based on the model of the natural sciences. “Scientific psychiatry” excludes philosophical reflection, experiential wisdom, political critique, and literary or imaginative insight. Furthermore, scientific psychiatry’s intolerance of diverse methods of inquiry is compounded by an intolerance of alternative truth perspectives. Because realism and correspondence theories of truth assume (and psychiatry is no exception) that there is only one true world to which language may correspond, then it follows that there can be only one Truth. For psychiatry, this works out to a situation in which “competing perspectives” are just that: competing. In the end, one competitor must win out or be replaced by another singular version. There is no room for multiple truths. More than one perspective may not be simultaneously right. The problem is that whoever is empowered to interpret the supertext of reality inevitably does so from a cultural standpoint. Psychiatric knowledge evaluators so empowered will often limit other possibilities of

knowledge, marginalize unorthodox knowledge, and impinge upon genuine exchange of alternative perspectives. Thus, realism and a correspondence theory of truth are problematic outcomes of referential theories of the sign and, therefore, leave reference theories unappealing for an applied theory of representation.

Theories of the Sign: Saussure and Poststructuralism

Fortunately, reference theories of the sign are not the only theories of the sign available for an applied theory of representation. In the late 19th century, about the same time Frege was developing his referential theory, Ferdinand de Saussure was developing a relational theory of the sign that later became the stimulus for poststructural philosophy. Saussure's theory of the sign focused on the relationship between the signifier (for example, a word) and the signified (which for Saussure is a concept) rather than on the reference between the signifier and the object. For Saussure, the signifier "tree" stands for, or indicates, the concept of a tree (not the object of a tree). Also, the concept of a tree is distinguished from the concept of a bush (or a vine, or a pole, or an oak, or a giraffe . . .) not through referential features but through relational features. As Saussure explains, "The mechanism of language turns entirely on identities and differences. . . . [with no] element of imposition from the outside world" (Saussure, 1972, p. 118). In other words, for Saussure, a language works through internal relations and not through external reference. This is possible, Saussure argues, because language prestructures meaning through a system of differences without positive terms:

A linguistic system is a series of phonetic differences matched with a series of conceptual differences. This matching of a certain number of auditory signals and a similar number of items carved out from the mass of thought gives rise to a

system of values. It is this system which provides the operative bond between phonic and mental elements within each sign. (Saussure, 1972, p. 118)

Thus, for Saussure, language works without reference because speakers use a relational grid of signifiers (rather than reference) to differentiate concepts and therefore to communicate with other speakers who have access to a similar grid of signifiers and concepts.

In constructing his theory of the sign, however, Saussure entangled himself in a contradiction that has resulted in a library of productive (and not so productive) controversy for structuralist and poststructuralist philosophy. Because the applied philosophy of reference I am advocating is largely poststructuralist inspired, I must outline some of the details of this controversy. According to Saussure's theory of the sign, language is a social product in the service of communication. His indubitably brilliant insight was that in language what matters are not speakers connecting with the world but speakers connecting with each other. For Saussure, language is a

fund accumulated by the members of the community through the practice of speech, a grammatical system existing potentially in every brain, or more exactly in the brains of a group of individuals; for the language is never complete in any single individual, but exists perfectly only in the collectivity. (Saussure, 1972, p. 13)

Thus, through a social crystallization within speakers, language creates a community in which "all the individuals [are] linguistically linked . . . in a kind of mean, . . . the same signs linked to the same concepts" (p. 76). As such, language for Saussure is analogous to a "contract agreed [upon] by members of a community," and the goal of the language contract is communication (p. 14).

After Saussure outlined this social constructivist perspective, however, he turned around and, in an attempt to be objective and rigorous, argued that linguistics should be an objective science of language. When he set up the research goal of linguistics (and more broadly of semiology) as “a science which studies the role of signs as part of social life,” Saussure so idealized science as to directly contradict his theory of the sign and language (Saussure, 1972, p. 15). Although, for Saussure, the “language we use is a convention and it makes no difference what exactly the nature of the agreed sign is,” the linguistic science that he founded to study language aims at a truth outside human convention. In no uncertain terms, Saussure describes his linguistic science as capable of reaching unmediated referential Truth: “The aims of linguistics will be . . . to determine the forces operating permanently and universally in all languages, and to formulate general laws which account for all particular linguistic phenomena historically attested” (Saussure, 1972, p. 6). This creates an unwieldy contradiction because the science Saussure has in mind is based on a natural science model, which allows the referential theory of the sign to slip in the back door and effectively cancel the power of his relational theory of the sign.

The obvious problem with Saussure’s idealization of science is that science is also conducted in language. As such, linguistic or semiotic scientists are as caught up in webs of linguistic categories which constrain and enable meaning as the communities of language users they study. Semiology, therefore, to be consistent with Saussure’s own theories of language, cannot be “permanently and universally” true. Rather, it is at best a potentially useful way of organizing the world of human interaction, and it involves gains and losses from other ways of organizing that world. I believe that Saussure’s idealization

of science, more than his actual theory of the sign, is what led to the considerable critiques of his theory by later poststructural theorists.

Poststructuralists expanded Saussure's social constructionist theory of knowledge to include a more radical critique of structuralist semiotic science as well. After the first wave of poststructuralist writings, later poststructural science-studies scholars have taken the next step to include all scientific knowledge. The result is a perspective on language, including scientific language, that understands linguistic constructions as creating a world of human communication without an authoritarian law of reference. Thus, Saussure's linguistic theory, amplified by poststructural critiques, has been as potent as an ethnographic sojourn in a land of strangers (an immersion into the alternative epistemes of a distant historical age, a long literary exposure to a fantasy world, or a vivid but hallucinogenic dream) in waking human sciences theory from the slumber of transparent knowledge.

Indeed, without his idealization of science, Saussure's relational theory of the sign reveals everything that Michael Foucault first experienced in a passage by Jorge Luis Borges:

This passage quotes a certain Chinese encyclopedia in which it is written that animals are divided into: "(a) belonging to the Emperor, (b) embalmed, (c) tame, (d) suckling pigs, (e) sirens, (f) fabulous, (g) stray dogs, (h) included in present classification, (i) frenzied, (j) innumerable, (k) drawn with a very fine camelhair brush, (l) et cetera, (m) having just broken the water pitcher, (n) that from a long way off look like flies." In the wonderment of this taxonomy, the thing we apprehend in one great leap, the thing that, by means of this fable, is demonstrated as the exotic charm of another system of thought, is the limitation of our own, the stark impossibility of thinking that. (Foucault, 1970, p. xv)

Foucault's revelation here, his radical insight, is none other than the stark impossibility of a purely referential theory of language. Saussure's insight is the same. If classificatory schemas work through relation rather than reference, there is an arguable silliness at the core of referential pretensions.¹ Saussure may have comforted some with his enlightenment reasonableness and his idealization of science, but for those who were reassured by his calm, linear, rigorous expository style, for those who never "had a revelation" while reading his theory of the sign, the comfort could not last. If there was any doubt where Saussure's theories were leading, any vestiges left in his theory that kept language still clinging referentially to the world, it was soon to be expunged by the meticulously exhaustive critiques of poststructuralism.

The most explicit poststructuralist critiques of Saussure's theory of the sign came from psychoanalyst Jacques Lacan and philosopher Jacques Derrida. In his early work, Lacan rethought Freudian theory through the frame of Saussure's theory of the sign and, in the process, pushed Saussure's theory to its most nonreferential expression. In Lacan's article "The agency of the letter in the unconscious, or reason since Freud," he argues that "quite contrary to the appearances suggested by the importance often imputed to the role of the index finger pointing to an object" language is the "locus of signifying convention" (Lacan, 1977, pp. 149–150). For Lacan, the subject is the "slave of language" in a way that goes radically beyond a reference theory of language and even "well beyond [Saussure's] discussion concerning the arbitrariness of the sign" (pp. 148–149). Lacan

¹ I will be going into it in much more detail in chapter 6, but let me just remark here that this is the reason the new psychiatry's *Diagnostic and Statistical Manual (DSM)* strikes so many as an absurd document. It places people into categories that are crystallized and internalized in psychiatrists through their professional development and education. When psychiatrists go to the clinics and divide people by these same *DSM* categories, they confirm and reify the very categories from which they started.

interprets Saussure's relational theory of the sign as implying that the signifier and the signified (the sound and the concept) are on the same plane. Unhappy with the implications of this interpretation, Lacan argues that "the S [signifier] and the s [signified] of the Saussurian algorithm are not on the same level, and man only deludes himself when he believes his true place is at their axis, which is nowhere"(p. 166). Nowhere, for Lacan, except in the unconscious background of language that controls human thought by supplying the "ultimate differential elements [from which our concepts are composed] and combining them according to the laws of a closed order" (p. 152). In Lacan's theory of the sign, the signifier rules the signified, and "we are, then, forced to accept the notion of an incessant sliding of the signified under the signifier"(p. 154). As a result, for Lacan, the radical implication of language without reference is not only that the signifier loses its hold on the world but also that the subject loses control of language. The subject thus becomes victim to the "dominance of the letter."

Lacan focuses his critique on Saussure's theory of the sign, but it is quite possible to read Saussure's theory of the sign (without his theory of science) as already radical enough to demonstrate the potential dominance of the letter. Key to Lacan's critique of Saussure is his algorithm for the sign:

$$\text{sign} = S/s \quad (\text{signifier} / \text{signified})$$

By this algorithm, Lacan illustrates the dominant sliding of the signifier over the signified. Lacan's algorithm is inspired by a well-known drawing or "sketch" in Saussure's *Course in General Linguistics* (p. 111) which Lacan describes in this way: "an image resembling the wavy lines of the upper and lower Waters in miniatures from manuscripts of Genesis; a double flux marked by fine streaks of rain, vertical dotted lines

supposedly confining segments of correspondence” (Lacan, 1977, p. 154). In Lacan’s algorithm, he takes Saussure’s sketch and flips it over so that instead of the signified (thought) being over the signifier (sound) the way Saussure has it in his sketch, Lacan has the signifier over (and thus dominating) the signified. In that way, Lacan radicalizes Saussure by suggesting that linguistic meaning is out of control of the subject.

It should be noted that Saussure’s sketch of the “fine streaks of rain,” which Lacan so poetically describes as “confining segments of correspondence,” are not meant by Saussure to suggest correspondence to the world, but rather inseparable coherence between the signified and the signifier. In Saussure’s famous phrase, “A language might be compared to a sheet of paper. Thought is one side of the sheet and sound the reverse side” (Saussure, 1972, p. 111). For Saussure, any necessity of connection between the signified and the signifier is only the necessity of convention, which is “entirely arbitrary” without any “element of imposition from the outside world” and only possible through “social activity” (Saussure, 1972, p. 111). If the connection is truly arbitrary, it does not matter whether the signifier or the signified is “on top,” because there is no power associated with the higher position except the power of social convention, which Saussure clearly acknowledges. Thus, Saussure’s theory of the sign by itself, without his theory of science, is already radical enough to take Lacan where he wants to go—an appreciation of the arbitrary nature of language and the capacity of language to dominate its users. Still, it is clear that Lacan’s early work further expanded and highlighted the nonreferential implications of Saussure’s theory of the sign, and it is these implications which will be most relevant to an applied philosophy of representation.

Jacques Derrida's early work, also inspired by Saussure's theory of the sign, is similar to Lacan's in that he brings out the most nonreferential reading of Saussure. However, unlike Lacan, Derrida focuses his critical reading not on Saussure's theory of the sign but rather on Saussure's idealization of science and his moralizing tone of objectivity. After all, it is only through Saussure's relational theories of the sign that Derrida's impressive oeuvre can so compellingly critique the dominating referential theories that undergird Western foundational thinking—what he calls the Western “metaphysics of presence” and “logocentrism”—the main quarry in Derrida's interventions. For Derrida, Saussure's relational theories of the sign are “indispensable for unsettling the heritage to which they belong, [and, as such] we should be even less prone to renounce them” (Derrida, 1974, p. 14). In *Of Grammatology*, Derrida's most sustained critique of Saussure, Derrida brings out the radical implications of Saussure's relational theory of the sign in order to glimpse what he calls the closure of a “historical-metaphysical epoch.” By focusing on Saussure's science of linguistics, while simultaneously using Saussure's theory of the sign to sustain his own critique, Derrida, perhaps overgeneralizing, locates Saussure within the “Western metaphysics of presence.” Derrida reaches this conclusion not by critiquing Saussure's theory of the sign *per se* but by critiquing Saussure's tone and his treatment of writing in his science of linguistics.

Derrida argues that when Saussure denigrates writing to a secondary status and excludes it from his linguistic science, Saussure undermines his own emphasis on the arbitrary nature of the sign and hides the radical implications of a relational theory of the sign. By leaving out writing, Saussure's emphasis on the arbitrary ends up applying only

to the connection between the concept (the signified) and the signifier but leaves the connection between concept and the object intact as a “natural bond.” Although Derrida’s reading of Saussure makes a compelling argument, it is far from obvious that Saussure means the connection between concepts and the world to be “natural.” Saussure rarely addresses the connection between the concept and the world, because his theory of the sign is a bipartite theory that includes the concept and the signifier only. Saussure does not include the world in his theory and therefore leaves the relationship between language and the world unclear. By my reading, however, except when he is talking about his linguistic science, Saussure implies that there is no connection between language and the outside world. As such, Saussure is far from the “metaphysics of presence” under which Derrida subsumes him. In either case, by focusing on the voice (which is only heard and never seen) and excluding the letter (which is by necessity always seen), Saussure’s linguistics cloaks the signifier in invisibility so that the voice may be experienced as a self-present reference to the world. For Derrida, “this experience of the effacement of the signifier in the voice is not merely one illusion among many—since it is the condition of the very idea of truth. . . . The word is lived as the elementary and indecomposable unity of the signified and the voice, of the concept and a transparent substance of expression” (Derrida, 1974, p. 20). Thus, in Derrida’s reading of Saussure, reference and with it a whole metaphysics of presence (which is none other than the realism metaphysics and the correspondence epistemology discussed above), sneak in the back door of Saussure’s theory of linguistics and overpower his arbitrary theory of the sign through an implied “natural” connection between the concept and the object in the “self-present” voice.

For Derrida, Saussure's idealization of speech as natural presence goes hand-in-hand with his idealization of science. In Derrida's view, both Saussure's idealizations of speech and his idealizations of science overestimate the power of language (including scientific language) to mirror and correspond to the world without mediation. Thus, Derrida proposes "grammatology," an alternative approach to linguistic science that would focus on writing rather than speech and would highlight rather than hide the power of linguistic systems of relation to shape and organize human knowledge. By focusing on writing, Derrida hopes to question and unsettle the hubris of Western logocentrism, which imagines itself to be closer to the Truth of the world than alternative systems of thought. Still, the theory of the sign that Derrida adopts for his grammatology is basically Saussure's. Derrida retains Saussure's relational theory of the sign, Saussure's structuring of meaning through a system of differences, and Saussure's sense that language bonds are arbitrary and conventional. The biggest difference is that, by focusing on writing, Derrida makes it clear that the relational theory of the sign organizes not just the connection between concepts and signifiers but also the connection between concepts and the world.

For my purposes, what is most striking about Lacan's and Derrida's critiques of Saussure, and most important for an applied philosophy of representation, is that they bring out the radical metaphysical and epistemological leanings of Saussure's purely relational theory of the sign. Just as referential theories of the sign tend toward a realistic metaphysics and a correspondence epistemology, relational theories of the sign tend toward idealistic metaphysics and coherence epistemology. An idealistic metaphysics is most consistent with relational theories of the sign because, in relational theories, signs work not by referring to the real world but by connecting and differentiating ideas in

order to allow communication. Metaphysics practiced with a relational theory of the sign, therefore, tends to slide into speculation about our ideas of the world rather than speculation about the world itself. If it slides far enough, ideas become all that is real.

Thus, the epistemology that relational theories of the sign tend toward is a coherence epistemology. In a coherence epistemology, constraint on belief tends to come from consistency with other beliefs rather than correspondence with the world. Truth is checked not by its correspondence to the world but by its relational coherence with other ideas. If I believe that the object outside my window is a tree, I check that belief not by investigating the tree but by investigating whether my belief coheres with other beliefs. Do I believe it to be tall, alive, and made of wood? If so, it cannot be a bush, a pole, or a giraffe—it must be a tree. In a coherence epistemology, direct connection between beliefs and the world is not what determines truth, rather, truth is determined by a reasoned relational comparison of beliefs. If a belief compares favorably with other beliefs, it is supported. Thus, coherence epistemology draws much of its strength from tradition, authority, and politics.

I must add, however, that it is, I believe, a (mis)reading of both Lacan's and Derrida's writings as a whole to accuse either of them of relativism or idealism. The later Lacan (from Seminar XI) is much more preoccupied with the "real" (see Žižek for a discussion of this point) and the later Derrida (for example "White Mythology") is much more attentive to the real (see Christopher Norris for an extended interpretation of Derrida along these lines) (Lacan, 1981; Žižek, 1989; Derrida, 1982; Norris 1997). However, while I'm sympathetic to these later developments in Lacan's and Derrida's thought, for the purposes of my outline of the sign, Lacan's and Derrida's early relational

theories of the sign, I believe, are the fire behind the smoke of many idealistic (mis)interpretations of their work. Lacan and Derrida are often interpreted as radically relativist epistemologists and metaphysicians (for example, see Gross & Levitt, 1994). This (mis)interpretation, I would argue, is fostered by their early efforts to radicalize Saussure's theory of the sign.

But regardless of whether Lacan and Derrida are card-carrying relativists, radically relativist idealism and coherence theories of truth, like radically objectivist realism and correspondence theories of truth, are problematic metatheories for an applied philosophy of representation. Just as realism and correspondence theories of truth set up "reality" as a normative metatext with which to evaluate particular knowledge formations, idealism and coherence theories of truth set up "tradition, authority and politics" in a similar position. The problem with reality as a metatext is universalism; the problem with tradition and authority as a metatext is "anything goes" relativity—at least anything that has a tradition of believers to back it up. With a purely relational theory of the sign, psychiatric knowledge formations are evaluated not by whether they match up with reality but by whether they match up with social norms and political constraints. Imaginary psychiatric knowledge evaluators using a relational theory of the sign would be able to judge particular knowledge formations only on their coherence to other psychiatric knowledge formations and traditions. There would be no way to justify a preference between the two traditions or perspectives. Thus, the problem for an applied philosophy of representation with idealism and with a coherence theory of truth is the problem of relativity. Tradition and authority are set up as the metatext, but which tradition and which authority should be preferred and why? There is no way to determine

which to prefer and no way to argue on grounds other than coherence. In summary, if a representational theory relies on a relational theory of the sign, it avoids the substantialist problems of a reference theory of the sign, but it is left with an equally crippling problem for evaluation—the lack of grounds, other than coherence, for preferring one knowledge tradition over another.

Theories of the Sign: Peirce and Pragmatism

Besides referential and relational theories, there is another option for representational philosophy. In the late 19th century, contemporaneous with Frege and Saussure, the American philosopher and founder of pragmatism, Charles Sanders Peirce, also developed a theory of the sign. Unlike the dyadic theories of Frege and Saussure, however, Peirce's theory is triadic. Peirce saw the sign as a three-way relationship between the concept ("interpretant"), the signifier ("sign"), and the thing ("object"). Any understanding of the sign, for Peirce, must always include perpetual tacking back and forth between all three parts of the sign. Peirce was explicitly dissatisfied with either a dyadic theory of the sign that focused primarily on sign-object reference, or a dyadic theory of the sign that focused primarily on the sign-concept relations. Peirce argued that there could at best be a dyadic theory of the sign that focused only on the object in plant life (i.e., "a sunflower, in turning toward the sun"), but that such a theory was highly implausible for human representation. Regarding a theory of the sign that focused primarily on the concept but conveyed no information about the world, Peirce simply stated that it was "very strange" (Peirce, 1955, p. 100). In human signs, Peirce stressed, "the triadic relation is genuine, that is its three members are bound together by it in a way that does not consist in any complexus of dyadic relations" (p. 100). For Peirce, "all

[human] thought . . . must necessarily be in signs,” and all signs are necessarily connected to both interpretants and objects (Peirce, 1991a, p. 49). The advantage of Peirce’s triadic theory of the sign is that it provides greater flexibility than either a referential or relational theory of the sign because it incorporates insights from both theories. For Peirce, signs have meaning because they are referential, but also, signs have meaning because they are relational.

Both the referential and the relational aspects of Peirce’s theory can be better understood by analyzing Peirce’s distinctions between signs. Peirce classified signs into three categories.

1. The *icon* is a sign that refers to the object through its likeness or similarity to it. For example, a sketch of a tree represents the tree by resembling it.
2. The *index* conveys the object by being affected by it; thus a weathercock is an index of the wind.
3. The *symbol* refers to an object that it designates by a sort of law, by convention, or by habit of connection. Most words, for example, are symbols.

The referential aspects of Peirce’s theory are most obvious in his categories of icon and index. In these categories, reference to the real world, either through resemblance or through effect, connects the sign with the object and the interpretant. By contrast, the relational aspects of Peirce’s theory are more prominent with symbols. Symbols are arbitrary, determined by convention, and work by differentiating concepts from one another. However, and this is key for Peirce, none of the categories entirely works by reference or by relation alone. Icons and indexes are interpreted not only by their reference to objects but also by normative rules of interpretation in a given community. A line with a triangle on top does not really look like a tree, and a

weathercock does not really say anything transparent about the wind. Though icons and indexes work through reference, conventionality is also necessary for meaning. The opposite is true for symbols. Symbols are interpreted, not by conventional relations alone, but also through reference, or what Peirce calls the “ground”: “the sign [including the symbol] stands for something, its object. It stands for that object, not in all respects, but in reference to a sort of idea, which I have sometimes called the ground of the representamen” (Peirce, 1955, p. 99). For Peirce, symbolic representations of the real world are not merely representations but also predictions of future events. As such, symbols can never be determined by our ideas alone but also are determined by reference to our experiences with the real world:

When I say that really to be is different from being represented, I mean that what really is ultimately consists in what shall be forced upon us in experience, that there is an element of brute compulsion in fact and that fact is not a mere question of reasonableness. (Peirce, 1991b, p. 243)

Thus, even in symbols, where Peirce most clearly relies on a coherence epistemology, meaning is partly determined by reference to experience of the world.

If an applied theory of representation adopted Peirce’s theory of the sign, it would inherit a metaphysics I will call “semiotic realism” and an epistemology of pragmatism. Semiotic realism suggests that there is a real world out there that our ideas are “in touch with.” At the same time, however, the specific points of contact between our ideas and the world are determined by the semiotic relations from which our ideas are structured, and the semiotic relations are relative to a given community. In other words, there are many ways to describe the world that will lead to a plenitude of good hours, and there are

many points of grounding consistent with that description.¹ Semiotic realism thus rejects universalism (notions of one truth) and anything goes relativism. Roland Barthes articulates this idea with his phrase “pluri-dimensional order” (Barthes, 1982, p. 465). Language, for Barthes, is too limited to capture the world in total. Yet language does evoke and engage the world. To put this in Peirce’s terminology, language contains both referential and relational elements. Thus, a language does not match up to the world in all of its complexity, but different languages have different consequences, and they effectively create different worlds.

Unfortunately, though I have used Peirce’s theory of the sign to help organize my version of semiotic realism, Peirce’s writings do not support Barthes’s notion of pluri-dimensionality. Thus, the version of Peirce I am using must be considered a modified version. Indeed, in the first of Peirce’s classic articles on pragmatism, “The Fixation of Belief,” he sounds very much the robust (rather than semiotic) realist and argues clearly that no matter what we may believe about the world there can be “only one true conclusion” that is real. (Peirce, 1982a, p. 74) In a later article, “How to Make Our Ideas Clear,” he is more vague about this, and he grounds “truth” and “reality” in the more social constructionist phrase: “The opinion which is fated to be ultimately agreed to by all who investigate” (Peirce, 1982b, p. 97). By this phrase, however, he seems to mean that,

¹ I take the phrase “good hours” from Ralph Waldo Emerson, who Cornel West considers the grandfather of American pragmatism (West, 1989, p 9). In Emerson’s essay, “Experience,” he uses the “good hours” phrase this way: “To finish the moment, to find the journey’s end in every step of the road, to live the greatest number of good hours, is wisdom...[T]he only ballast I know is a respect to the present hour.” (Emerson, 1946, p. 274). By taking this experiential turn, the necessity of organizing the world according to correspondence or coherence is undermined. Neither correspondence or coherence directly address the question of experiential consequence. Does organizing the world in a

if investigation were carried out long enough, the final agreement would be a single truth, not multiple ones.

I see nothing necessary in Peirce's insistence on this. Indeed, if the relational part of Peirce's theory of the sign is modeled along the lines of Saussure's work, I think multiple truths are more consistent. Thus, if investigators are working within differing language practices, they will come up with different linguistic formations to preoccupy them and to organize their life (and their world). Thus, to get around Peirce's version of robust realism and still keep his triadic version of the sign (and avoid a long philosophical argument), I will simply take William James's tack: I will reinterpret Peirce, against the grain of his own intentions, as providing a theory that creates a space for both realism and a plurality of social constructions of the real—in other words, semiotic realism.

For James, there cannot be one truth because all truth is instrumental. Beliefs are more analogous to tools than to copies of reality; like tools, they help us cope with the world and coping is more important for James than corresponding. Thus, James may be understood as a semiotic realist in that he does not deny that there is a world or that the world impinges on our sensations, but even our sensations, for James, depend on our thoughts:

which [sensation] we attend to, note, and make emphatic in our conclusions depends on our own interests; and according as we lay the emphasis here or there, quite different formulations of truth result. We read the same facts differently.

"Waterloo," with the same fixed details, spells a "victory" for the Englishman; for

particular way lead to good hours in Emerson's sense? And, are there not a variety of other ways of organizing the world which would also lead to good hours?

a Frenchman it spells a “defeat.” . . . What we say about reality thus depends on the perspective into which we throw it. (James, 1992, p. 118)

Recent science studies scholarship also works well with a metaphysics of semiotic realism. Indeed, several leading science studies scholars have reached very similar conclusions with regard to science and its relation to the world. Donna Haraway uses the phrase “material-semiotic” to capture her awareness that the “imaginary and the real figure each other in concrete fact.” Thus, much of her scholarship involves “taking the actual and the figural seriously as [co]constitutive of material-semiotic worlds” (Haraway, 1997, p. 2). Andrew Pickering uses the term “mangle” in similar ways. For Pickering, science is a “field of emergent human and material agency reciprocally engaged by means of a dialectic of resistance and accommodation.” (Pickering, 1993, p. 559). Thus, the material and the human are “mutually and emergently productive of one another” (p. 567). Pickering uses mangle as a noun (to refer to existing cominglings) and as a verb (to refer to the process of creating new cominglings). Thus, one could argue that psychiatry, in Pickering’s terminology, is a domain that mangles together new kinds of humans, because the material agency that is mangled, through a process of resistance and accommodation, is composed of humans (or patients).

Finally, science studies scholar Joseph Rouse also echoes the theme of semiotic realism in an expanded notion of “science as practice.” For Rouse, scientific practice is more than a representation of the world; it is also a way of interacting with the world. Scientific practice is a dialectic that reconstructs the world and people’s relationship to that world as it redescribes it. However, scientific practices are not restricted to the laboratory. They move outside the laboratory to become “habitual practices and skills through which people make themselves into competent, reliable participants in a more or

less shared world. Who we are is in significant part who we have made ourselves into through the cultivation of habits of mind and body” (Rouse, 1996, p. 132). If we look at this in relation to the new psychiatry, we see, for example, that clinical drug trials have increasingly moved out of the clinical laboratory into the clinics and are now into our daily lives. We increasingly organize and recognize uncomfortable emotional states around the trope of “neurotransmitter abnormality” or “chemical imbalance.” Here we see the hypothesis of the laboratory becoming the everyday experience for many people and the perceived reality of our lives. In other words, this demonstrates what Haraway meant when she said that the “imaginary and the real figure each other in concrete fact.”

Moving from semiotic realism to pragmatic epistemology, the importance of Peirce’s theory of the sign is that it focuses on the criteria of consequences in belief evaluation. From a pragmatist perspective, knowledge functions as a guide for action and the best knowledge is that which leads to the best consequences. Focusing on consequences, however, does not erase the importance of coherence and correspondence as criteria for knowledge. Pragmatic epistemology incorporates coherence and correspondence because good consequences depend partly on relations with authority and tradition and partly on correspondence with the experience of ideas in action. Unique to pragmatic epistemology is its orientation toward the future: what is best depends on what will happen next, not only on what has happened before. Also, pragmatic epistemology is unique in its incorporation of desire into epistemology. In coherence or correspondence epistemology, truth is irrelevant to desire. In a pragmatic epistemology, however, desired consequence is part of what determines best consequence. By including desire, pragmatic epistemology reconnects beliefs with values and, therefore, connects ethical questions

like “what kind of people do we want to be” with metaphysical and epistemological questions like “how is the best way to understand and organize knowledge about people?”

Three Epistemic Principles as Criteria for Knowledge Evaluation

Thus far I’ve devoted this chapter to working through three theories of the sign. After finding the metaphysics and epistemics of referential and relational theories of the sign problematic, I decided to privilege pragmatic or consequential theories of the sign. This left me with a semiotic realism metaphysics and a pragmatic epistemology which work well for an applied theory of representation because they are consistent with recent work in science studies, and because they support three epistemic principles that can be used for knowledge evaluation. Since Peirce’s theory of the sign includes referential, relational, and consequential components of meaning, it provides tools with which to evaluate specific knowledge formations on their correspondence with the world, on their coherence with linguistic and cultural traditions, and on their consequences with respect to desires and values. Granted, Peirce’s theory of the sign does not support an applied theory of representation that outlines an algorithm for knowledge evaluation, nor does it support definitive conclusions about the best knowledge option. However, Peirce’s triadic theory of the sign does provide epistemic guidelines and principles for evaluative discussions.

For psychiatry, an applied theory of representation would work by providing epistemic principles for psychiatric knowledge evaluations. In other words, any specific psychiatric knowledge can be evaluated according to what I will call the three C’s of knowledge evaluation:

1. the principle of correspondence,
2. the principle of coherence, and
3. the principle of consequence.

Using the three C's, knowledge evaluators will defend their preferences by tacking back and forth between the three principles. Thus, for any potential knowledge possibility, at least three questions could be asked: Does the knowledge in question correspond with the world? Does it cohere with things already believed? Does it lead to good consequences? Of course, evaluators will disagree on which knowledge formations best meet which principles, which principles are the most relevant for a particular knowledge formation, and even on how to interpret the principles for a particular knowledge in question.

Since psychiatric knowledge evaluators will not agree on all of these issues, the result of the three-principle approach will be a multitude of "good" knowledge possibilities. Sometimes a new psychiatric knowledge possibility can convince evaluators it meets the principles better than its predecessors. When this happens, the new knowledge will displace the older ones. Alternatively, and much more likely, a new knowledge formation can at best convince evaluators that there is a need for an additional knowledge that is truer to different principles. In this situation, the new knowledge will merely take its place alongside existing knowledge. An obvious example in psychiatry is the relationship between biopsychiatry and psychoanalysis. The three-principle version of knowledge evaluation would allow both to exist. In contrast, the only way for both to peacefully coexist now is if knowledge producers and evaluators are able to convince folks that the two knowledges are not really at odds with each other and that they can actually be brought together. In other words, they must make biopsychiatry and

psychoanalysis into a unified theory in order to hold on to the “one truth” approach. From my perspective, requiring a single, unified truth is too limiting for the complexities of people and the world.

Thus, the three principles would benefit psychiatric knowledge evaluation primarily by providing “lines of argument.” They would provide loose evaluative criteria without being definitive metanarratives. Thus, they would leave room for several alternative perspectives and alternative knowledge formations using the same three principles. Since different stakeholders in psychiatric knowledge will use the principles in different ways, it is unlikely they will come up with the same conclusions. Indeed, some stakeholders will eschew the three-principle approach altogether and place their emphasis on only one or two of the principles or use methods that the three-principle approach doesn’t consider. As a result, the three principles leaves intact the need to consider and work through in much detail questions of who should decide and what to do in the case of conflict between stakeholders. These issues will be taken up further in the next chapter and particularly in the last chapter. For now, let me contrast the three-principle approach with current psychiatric science. Compared with the three-principle approach, contemporary psychiatric science closes down sustained considerations of who should decide and what to do in the case of conflict. When the single evaluative criteria is correspondence alone, the automatic conclusion for who should decide becomes “scientific experts.” Similarly, the automatic answer to the conflict issue becomes “more science until the conflict is resolved.” This approach suffers because it does not take seriously the need to include a greater diversity of knowledge producers in psychiatry, and it does not take seriously the advantages of multiple psychiatric truths.

I should emphasize that multiple truths, however, are not the same thing as “anything may be true.” Far from an “anything goes” perspective, the three principles provide explicit criteria for evaluating knowledge formations. “Anything goes” or radical relativity might counter that all evaluative methods, even ones supported by nonuniversalist principles like the three C’s, would provide enough loopholes that, in the end, actual knowledge evaluation would be little more than a ruse for power and authority. The three-principle approach, though also concerned with this possibility, would argue instead that making evaluative positions explicit along nonuniversalist lines is the best chance that the “will to power” inherent in all evaluative approaches could be publicly addressed. Because evaluators using the three-principle approach will have to make their principles of evaluation explicit, it is less likely that any particular evaluation will be able to hide its values and interests behind supposedly neutral standards of a substantialist approach or behind unstated standards in an anything goes approach. Similarly, the three principles avoid ethnocentrism, not so much by removing cultural preferences from evaluation but by including cultural preference, and the particular knowledge’s relation to cultural tradition, as an explicit criterion that is included in the evaluation process. Ethnocentrism, the preference for a particular tradition, thus becomes one of the lines of argument from which the evaluation takes place. The result, of course, will be multiple knowledge formations with differing relations to differing traditions. From my perspective, however, this is a major improvement over the current “scientific psychiatry”—a (supposedly) single knowledge formation which, I will argue in chapters 6 and 7, is loaded with unmarked interests and preferences while it is simultaneously

touted to be the “universally best” translation of psychiatric suffering and clinical assistance.

In conclusion, retheorized psychiatric studies needs an applied theory of representation to help with knowledge evaluation. By adopting Peirce’s theory of the sign (modified in a pluri-dimensional direction), applied philosophy of representation tends to all of the goals I outlined earlier. Peirce’s triadic theory of the sign supports clear principles for knowledge evaluation, without falling prey to the problems of universalism, “anything goes” relativism, or ethnocentric biases that have made knowledge evaluation seem so difficult in the past. As a consequence, however, epistemic principles do not provide an algebraic equation for plugging in the variables of a particular knowledge formation and achieving an automatic solution. The result is less certainty of knowledge and more appreciation of knowledge diversity. The evaluative criteria I have outlined are hardly a panacea for psychiatry. For psychiatry to use these principles well will take a finely tuned awareness, a richly cultivated sensitivity, and a much more nuanced capacity with regard to questions of stakeholder inclusion and knowledge conflict. However, this applied theory of representation, working as it does with the three epistemic principles of correspondence, coherence, and consequence, is able to articulate a flexible evaluative method for psychiatric studies that avoids the difficulties and extremes of other approaches.

Chapter 3: Struggling with Practice and Power

The last chapter led me through a thicket of retheorizing psychiatric representation and psychiatric knowledge evaluation. In this chapter, I start with two conclusions that follow from that work:

1. metaphysical—the material-semiotic world that psychiatry attempts to know is pluri-dimensional (there are a variety of ways to classify it which result in a variety of ways to organize life), and
2. epistemological—the most psychiatry can hope for in the way of foundational criteria for knowledge evaluation are loose heuristic guides (the details of which not everyone will agree on) between alternative conceptions of people and the world.

If these conclusions are embraced, psychiatry must accept and reconsider the problem of how to work with and how to understand competing psychiatric knowledge possibilities. Psychiatry is not monolithic in its conceptual formations, and psychiatry has not been consistent historically. Within psychiatry today there are a variety of competing visions about how to understand people and their problems. Though the new psychiatry is dominant, it has many alternatives. In addition, over the past hundred years of psychiatry's existence, there has been considerable flux in the leading approaches to psychiatric problems. How can we understand these differences in psychiatric knowledge formations in the light of pluri-dimensionality?

Or, to come at this from another angle, if alternative psychiatric formulations are not simply understood in either/or terms of true or false (real or myth) how can differing psychiatric formulations be understood? My answer, distilled to its simplest formula, is

that psychiatric knowledge formations are complex interminglings of the three C's (correspondence, coherence, and consequences) that are historically and contingently combined through the forces of power and politics to create particular psychiatric communities. Thus, psychiatric knowledges and practices may be understood as the manifest contents of a latent politics of psychiatric truth. They represent struggles over who should lead the field and how the field should be organized, and these struggles are played out within the eddies and currents of larger social forces. Psychiatric knowledges and practices are the always ambivalent end products of political struggle produced under conditions of conflict. Like other politics, psychiatric knowledge and practice politics involve high stakes. Winners gain immediate power in the world of psychiatry (they are the leaders of psychiatric departments and psychiatric research teams, editors of leading journals, recipients of awards and perks, and collectors of fame, fortune, and prestige in the public eye). In addition, they gain the power to create the environment that all other psychiatric stakeholders are forced to live in. Psychiatric knowledge and practice formations function as a kind of truth grid. Once a psychiatric truth grid is established, any discussion and any struggle for an alternative vision of the psychiatric world has to take place on the playing field of that truth grid. Those working within the grid are assumed to be the legitimate experts of knowledge, and they also have a huge advantage in institutional resources. This is an invaluable prize. Using a sports analogy, one could say that winning a battle over truth is like having constant home-court advantage in addition to having exponentially greater capital resources (or, if you will, a higher salary cap) than anyone else in the league.

In order to bring these ideas into sharper focus, I will devote this chapter to developing two theoretical concepts, “discursive practice” and “power,” in considerable detail. These concepts arise from the work of philosopher and social theorist Michel Foucault, and they provide needed tools for bridging questions of psychiatric representation with questions of psychiatric practices and psychiatric politics. Thus, they expand my discussion of semiotic realism and pragmatic epistemology, and they provide a link between my abstract discussions of language and representation and my more specific discussions in later chapters of the social and political relations of psychiatric knowledges and practices.

Psychiatry as a Discursive Practice

Foucault’s understanding of “discursive practice” overlaps considerably with my concluding metaphysics of “semiotic realism” in the last chapter. Not unlike the pragmatists who inspired my conclusions (although from a very different literature base), Foucault argues against sharp distinctions between representation and the world and between representation and practice. For Foucault, these sharp distinctions must be problematized and reworked. His theoretical structure for reworking these distinctions is organized around the concept of “discursive practice.” This concept is also similar to Haraway’s “material semiotic,” Pickering’s “mangle,” and Rouse’s “science as practice” in that all of these approaches problematize the distinctions mentioned above. Foucault develops these ideas considerably further than other theorists, however, and he gives us a much thicker understanding of how representation could be reworked without isolating it from the world or from life and practice. For that reason, I will spend the first part of this chapter exploring how Foucault’s notion of discursive practice can provide us with a

retheorized understanding of psychiatry as a discursive practice. Although language is key to a retheorized psychiatry, language and representation must not be understood as a transcendent realm. Language and practice are very much intertwined, and Foucault's notion of a discursive practice highlights this perspective.

To introduce and foreground psychiatry as a discursive practice, I must first ask, "What holds psychiatry together?" What do giving advice about masturbation and connecting someone's head to an electric current have in common? Why is talking to voices part of psychiatric classification, while praying to God is not? Why does passing a license exam make a psychiatrist, while knowing passable answers to the same test would not? Why do some psychiatrists conceptualize psychiatric problems in terms of neurotransmitter deficits, some in terms of unconscious psychic conflicts, some in terms of faulty family dynamics, and some in terms of community distress—and yet all are considered psychiatrists? Why do some psychiatrists thematize mental illness as a deviation from a natural norm, while others see it as a deviation from social expectations? I ask again, what holds psychiatry together? In other words, where does the unity, in all the disunity that is psychiatry, come from? Indeed, what is psychiatry?

Though these questions are updated to consider contemporary psychiatry, they are the same kinds of questions that Michel Foucault asks regarding human science disciplines in general. "What, in fact, are medicine, grammar, or political economy?" (Foucault, 1972, p. 31). What is the unity of these disciplines? Foucault's answer, which I will be working through in this section, is that they are "discursive practices" which are held together by "rules of formation." Foucault's understanding of discourse and discursive practice is very much inspired by Saussure's approach to language. Saussure

made a distinction in his analysis of language between *parole* and *langue*. *Parole* is a particular statement or linguistic production, and *langue* is the background distinctions, rules, and expectations that make a statement possible and understandable. Foucault uses “discourse,” “discursive formation,” and “discursive practice” somewhat analogously to the way Saussure uses *langue*. Like Saussure, Foucault is after the background assumptions that allow a communication to function.

However, Foucault’s “discourse” is simultaneously more restricted and more inclusive than Saussure’s *langue*. On the one hand, Foucault’s “discourse” narrows the focus of *langue*. For Saussure, *langue* is the universal, ahistorical background structure of language, whereas, for Foucault, discourse is a local historical and cultural product. Foucault is not trying to understand the way *langue* shapes all speech; he is trying to understand particular background assumptions and constraints that operate in particular linguistic communities. On the other hand, “discursive practice” is more inclusive than “*langue*.” Where Saussure’s emphasis was on language and representation, Foucault expands his focus to include performative practices that involve more than representation. For Foucault, discourse analysis is “a task that consists of not—of no longer—treating discourses as groups of signs (signifying elements referring to contents of representations) but as practices that systematically form the objects of which they speak” (Foucault, 1972, p. 49). In other words, discursive practices involve not only representation but also ways of engaging, interacting, affecting, and being in the world. As such, Foucault’s discourse analysis moves Saussure’s semiotics from a representational idiom of *langue* and *parole* to a performative idiom of “discourse” and “practice.”

Foucault describes his project of discourse analysis this way:

I shall take as my starting-point whatever unities are already given (such as psychopathology, medicine, or political economy). . . . I shall make use of them just long enough to ask myself what unities they form: by what right they can claim a field that specifies them in space and a continuity that individualizes them in time; according to what laws they are formed; against the background of which discursive events they stand out; and whether they are not, in their accepted and quasi-institutional individuality, ultimately the surface effects of more firmly grounded unities. I shall accept the groupings that history suggests only to subject them at once to interrogation; to break up and see whether they can be legitimately reformed: or whether other groupings should be made: to replace them in a more general space which, while dissipating their apparent familiarity, makes it possible to construct a theory of them. (Foucault, 1972, p. 26)

Accordingly, Foucault attempts to describe the conventions that exist in the background of a discourse. These conventions are the preconscious horizon that allows discourse to make sense to its users and that narrows the possibilities open to discourse in a particular way. The project, for Foucault, “is deliberately both historical and critical” (Foucault, 1973, p. xix). It starts with “preexisting forms of continuity,” such as psychiatry, but stays with them only long enough to understand the constructions of rules and justifications that hold them together. Foucault’s goal of discourse analysis is to understand “how it is that one particular statement [or set of statements] appear rather than another” (Foucault, 1972, p. 27). His motivation is to see whether specific discursive formations “can be legitimately reformed.”

In Foucault’s discussion of method, he makes a distinction between positive and negative features of discursive practices. Accordingly, a discourse may be analyzed in terms of what it includes and what it excludes. Foucault refers to the positive (or

productive) features of discourse as the “rules of formation” and the negative (or constraining) features as the “rules of exclusion” (1972, pp. 31, 216). He divided the rules of formation into the formation of

1. objects (what discursive statements are about),
2. concepts (the terms in which the objects are formulated),
3. strategies (the themes and theoretical viewpoints), and
4. enunciative modalities (or authoritative speakers) (1972, pp. 31–79).

These rules of formation work to produce discourse.

In contrast, Foucault also outlines several “rules of exclusion” which limit and constrain discursive possibilities. Not anything can be said in a discourse, and the rules of exclusion mark the boundaries. Foucault’s rules of exclusion include:

1. the prohibition of words,
2. the opposition between reason and folly,
3. the opposition between true and false, and
4. the effects of commentary, authorship, and disciplines (1972, pp. 215–224).

In my discussion of psychiatry as a discursive practice, I will focus on those aspects of Foucault’s discursive analysis that seem particularly relevant to psychiatric discursive practice today. With regard to the rules of formation, I will discuss the productive effects of objects, concepts, and strategies used in psychiatric discourse formation. With regard to the rules of exclusion, I will focus on the constraining effects of distinctions between reason and folly and between true and false. At the end of the chapter, I will also consider the formative effects of enunciative modalities, and the exclusionary effects of commentaries, authorship, and disciplines. From my perspective, these last contributions to discourse formation are better understood after working

through Foucault's analytic of power. For that reason, rather than placing my discussion of these final rules of formation and exclusion in this section, I will place it toward the end of the section on power.

Rules of Formation in Psychiatric Discourse

For examples of Foucault's rules of formation and exclusion from recent psychiatry, I will analyze a leading textbook of the new psychiatry: *Introductory Textbook of Psychiatry*, second edition, by Nancy Andreasen and Donald Black (Andreasen & Black, 1995). Starting with rules of formation, I will outline the *objects*, *concepts*, and *strategies* that make up Andreasen and Black's text. Curiously enough, similar to the way Foucault initiates his discourse analysis, Andreasen and Black also begin their text by asking the basic question, "What is psychiatry?" They are concerned to achieve clarity on this question because they too recognize the tremendous diversity and disunity the term "psychiatry" evokes in popular culture and in clinical practice. The "popular mind," they tell us, connects psychiatry with such diverse phenomena as "Freud's couch, Jack Nicholson receiving electroconvulsive therapy in *One Flew Over the Cuckoo's Nest*, or Dr. Ruth discussing sexual adjustment on television" (p. xi). They also point out that "a typical day's work" for a practicing psychiatrist "may involve [such diverse activities as] prescribing medication to a depressed patient, helping a teenager come to grips with the effect of having an alcoholic parent, and guiding a severely handicapped schizophrenic patient toward receiving needed social services" (p. x)

Thus, like Foucault, Andreasen and Black are concerned about the unity of "psychiatry." What holds such diversity together? Here is Andreasen and Black's solution:

What is psychiatry? It is the branch of medicine that focuses on the diagnosis and treatment of mental illnesses. Some of these illnesses are very serious, such as schizophrenia, Alzheimer's disease, or the various mood disorders. Others may be less serious, but still very significant, such as adjustment disorders or personality disorders. . . . As a discipline within medicine, the primary purposes of psychiatry are to define and recognize illnesses, to identify methods for treating them, and ultimately to develop methods for discovering their causes and implementing preventive measure. (1995, p. xi)

Also, not unlike the way Foucault might begin, Andreasen and Black focus their solution to the question of psychiatric unity on the *objects* of psychiatry. The objects Andreasen and Black point to are “illnesses” and “disorders.” The two terms are used interchangeably throughout their text, and they are the major focus of their text. Indeed, the largest section of the book (45%) is called “Psychiatric Disorders,” and it includes all the “mental illnesses” mentioned in the above quote along with 12 others: delirium, dementia, amnesia, and other cognitive disorders; delusional disorder and other psychotic disorders; anxiety disorders; obsessive-compulsive disorders; somatoform and related disorders; dissociative disorders; alcohol-related disorders; other substance-related disorders; personality disorders; sexual and gender identity disorders; eating disorders; and disorders of impulse control (Andreasen & Black, 1995, pp. v–vi).

So far, Andreasen and Black's text and Foucault's discourse analysis are on the same path. Both begin their discussion of the unity of a discourse, in this case psychiatry, by focusing on the *objects* of that discourse. But after this the two approaches rapidly part company. The first divergence comes with their respective understanding of where these objects of psychiatry arise. For Andreasen and Black the answer is simple: the progress of science, specifically, “neuroscience.” Accordingly, in the first chapter of their text,

titled “History of Psychiatry,” Andreasen and Black narrate a history of psychiatric progress, which they tell us spans from the “Dark Ages” of medieval times, through the “first era of neuroscience” (which they call the dawn of “scientific psychiatry”), to the “development of psychoanalysis” (which they ultimately see as a well-intentioned but badly mistaken wrong turn), and finally to the glorious present culmination in the “second era of neuroscience” (or the new psychiatry) (1995, pp. 3–16). Andreasen and Black conclude from this narrative that once psychiatry got on the track of ever-improved brain science, and returned from its unfortunate psychoanalytic dead end, psychiatry has constantly improved and refined its knowledge of the objects of brain dysfunction. In the last paragraph of the chapter, Andreasen and Black tell us that

psychiatry . . . is therefore a discipline dedicated to the investigation of abnormalities in brain function. . . . the drive of modern psychiatry is to develop a comprehensive understanding of normal brain function at levels that range from mind to molecule, and to determine how aberrations in these normal functions . . . lead to the development of symptoms of mental illness. (p. 16)

Thus, for Andreasen and Black, neuroscientific investigations of the real world of the brain lead directly to the progressive discovery of the objects of psychiatry—the illnesses and disorders of brain function. What holds these objects of psychiatry together is the science that discovered them. What holds the science together is a progressive understanding of the real world facts of human brain pathologies (or what they call psychiatric illness).

By contrast, Foucault argues that, since the objects of a discourse like psychiatry are constantly changing, the regularity of these objects must come from something other than the world itself. For Foucault, psychiatric objects do not “naturally” emerge; they arise in a complex social negotiation that includes social norms (what a given community

will tolerate), professional judgment (expert opinion), and current rules of classification (how well possible additional objects of psychiatry fit in with the existing classificatory schema) (Foucault, 1972, p. 41). Obviously, this does not imply that “the world” has no role in the negotiation, only that the world is a single variable in the process. In the language of sociologist of science Andrew Pickering, the objects of science are “mangled” together through a complex process of accommodation and resistance that goes by the name of “scientific method” (Pickering, 1993, p 144). The “mangle” combines human agency and material agency in a nondeterminate outcome. To see Foucault’s negotiation process (or what Pickering calls “mangling”) in concrete form, consider the change in psychiatric objects between Andreasen and Black’s first edition (1991) and their second edition (1995). The second edition contains several “additional topics” that are not present in the first edition. These include “sleep disorders, impulse control disorders, and violence” (Andreasen & Black, 1995, p. ix). But what regulates the appearance of these new psychiatric objects?

Consistent with Andreasen and Black’s understanding of the other objects in the text, they explain these changes by invoking the recently accelerated “growth and development” of the new psychiatry’s “scientific basis.” After all, the 1990s, Andreasen and Black remind us, were not declared the “decade of the brain” for nothing.

Neuroimaging techniques now give us a direct window on the brain, permitting us to see with our own eyes the underlying physiology of mental activities such as remembering, feeling sadness, or making a decision. The psychiatrist who uses these techniques to map the brain is engaged in a voyage of discovery not unlike that of the early explorers who sought a trade route to India and instead discovered America. . . . The chemical systems of the brain are also being remapped, and the mechanisms of drug action in the in vivo intact brain are being

discovered. We can now visualize how the medications that we prescribe block various classes of receptors in the brain and exert their therapeutic effects. . . . Neuroscience and psychiatry are exploring the last uncharted territory in the human body. It is an incredibly exciting time to work in these fields. (1995, p. viii)

“All this growth in knowledge,” Andreasen and Black claim, “has required” the appearance of the new psychiatric objects listed above. For Foucault, however, this explanation would be inadequate. Clearly, Foucault might argue, violence and impulse control disorders are not discoverable through neuroimaging techniques alone. One cannot see violence or impulse control disorders on a PET scan directly without a series of links between neural images and behaviors and without extensive interpretations of those links. Rather than being forced by the “growth in knowledge” (basically the hand of nature), Andreasen and Black’s rather giddy analogy between psychiatric science and European explorers is perhaps a better clue to the process than their more manifest arguments. Indeed, I find Andreasen and Black’s honest imperialistic excitement about neuroscience expansion a compelling portrayal of the way these new psychiatric objects appeared. Europe did not “discover” and colonize the world simply because of Europe’s growth in scientific capacities. Colonization was a complex interaction and mangle of what Europe could do, what they were allowed to do, and what they believed was in their interests to do. There was nothing inevitable about Europe’s imperialism any more than there was anything inevitable about the addition of violence and impulse control disorders to psychiatric discourse. That it occurred was a mixture of chance, sudden disruptions from the past, struggles between different interest groups, and material possibility.

Similarly, Foucault's discourse analysis would suggest that if Andreasen and Black's textbook now speaks of "violence and impulse control disorders," where only a few years ago it did not, it is because of a spiraling interaction between relations of social insistence and intolerance (bringing violent and impulsive people for psychiatric exam) and professional expert evaluators who finessed the classificatory systems to create a fit. The emergence of new objects in a discourse must be understood in the context of the multiple social relations involved. For Foucault, objects like violence and impulse control disorders emerge from a complex interaction between the authority of medical decision, judicial decision, the family, the hospital, and the prison. "These are the relations that, operating in psychiatric discourse, have made possible the formation of a whole group of various objects" (Foucault, 1972, p. 44). For a new object to be part of a discourse, however, it is not enough for it to emerge from social relations. It must be fit into the discursive grid. In the case of violence and impulse control disorders, Andreasen and Black must finesse the classificatory systems of their first edition by placing "impulse control" problems in the "disorder" category, while placing "violence" in the "special topics" category. Both make it into the discourse, but for "violence" to fit Andreasen and Black must take advantage of a catchall classificatory group ("special topics") to make it work. Thus, from the perspective of discourse analysis, the new psychiatric objects of Andreasen and Black's recent edition, like the old objects in their prior edition, are not necessary outcomes of neuroscience—they are mangled negotiations of psychiatric knowledge production.

The second site of divergence between Andreasen and Black and Foucault's approach is that Andreasen and Black stop questioning the unity of psychiatry once they

have described and explained the objects of psychiatry. Foucault, however, goes on to discuss additional elements of a discursive formation—the *concepts* and *strategies*. Thus, starting with concepts, we can ask: If “mental disorders” are the objects of psychiatry, what are the concepts of psychiatry and from where do they arise? Paradoxically, the same “mental disorders” that constitute the objects of psychiatry are also the major concepts of psychiatry. Let me explain. For Foucault, what makes it possible to delimit a group of concepts “is the way in which these different elements relate to one another. . . . It is this group of relations which constitutes a system of conceptual formation” (Foucault, 1972, p. 60). Concepts, for Foucault, allow the “object” statements of a discourse to be “arranged in particular wholes” (Foucault, 1972, p. 57). These conceptual wholes provide a “set of rules for arranging statements in a series,” and they even “recreate a perceptual process” (Foucault, 1972, p. 60). Thus, not only do concepts organize observation, but the very process of observation itself is concept laden.

In psychiatry, the relation between objects and concepts works in a particularly circular fashion. The underlying conceptual formation that relates the objects of psychiatry (the disorders) together is the notion of disorder itself. From Andreasen and Black’s perspective, psychiatry’s unity comes from being a specialty of medical science that studies mental disorders and their treatment. From this perspective, the similarity of medicine and psychiatry is that both have a similar conceptual model of disorders and treatments. The leading conceptual model of medicine, despite many years of critique, is the model of disease as a discrete entity and treatment as a “magic bullet.” Using this model, medical disorders are understood as circumscribed abnormalities (such as pneumococcal pneumonia) that can be treated with circumscribed interventions

(penicillin). When applied to psychiatry from the perspective of discourse analysis, this creates a situation in which psychiatry's objects and concepts are coconstituted and mutually reinforcing. The disorders of psychiatry—which constitute the unity of the field—are at once both objects (the focus of the field) and concepts (the conceptual schema that holds the objects together).

This spiraling interaction between objects and concepts does not stop here, however. When “disorder” is understood as a concept rather than an object, the concept of disorder (as opposed to the object of disorder) must rely on a different set of objects. Andreasen and Black call the objects that make up the concept of disorders “signs and symptoms.” Signs and symptoms are the objects or elements that, when they are put back together in conceptual wholes, create the disorders. Andreasen and Black devote several pages to detailed definitions, descriptions, and categorizations of “common signs and symptoms.” An outline of their efforts includes the following:

delusions (persecutory delusions, delusions of jealousy, delusions of sin or guilt, grandiose delusions, religious delusions, somatic delusions, ideas and delusions of reference, delusions of being controlled, delusions of mind reading, thought broadcasting/audible thoughts, thought insertion, thought withdrawal),
hallucinations (auditory hallucinations, voices commenting, voices conversing, somatic or tactile hallucinations, olfactory hallucinations, visual hallucinations),
bizarre or disorganized behavior (clothing and behavior, social and sexual behavior, aggressive and agitated behavior, ritualistic or stereotyped behavior),
disorganized speech or positive formal thought disorder (derailment or loose associations, tangentiality, incoherence, word salad or schizophasia, illogicality, circumstantiality, pressure of speech, distractible speech, clanging, catatonic motor behavior {stupor, rigidity, waxy flexibility, excitement, posturing and mannerisms}, alogia, poverty of speech, poverty of content of speech, blocking, increased latency of response, perseveration), **affective flattening or blunting**

(unchanging facial expression, decreased spontaneous movements, paucity of expressive gestures, poor eye contact, affective nonresponsivity, lack of vocal inflections), **inappropriate affect, avolition-apathy** (grooming and hygiene, impersistence at work or school, physical anergia), **anhedonia-asociality** (recreational interests and activities, sexual interest and activity, ability to feel intimacy and closeness, relationships with friends and peers), **attention, social inattentiveness, inattentiveness during mental status testing, manic symptoms** (euphoric mood, increase in activity, racing thoughts/flight of ideas, inflated self-esteem, decreased need for sleep, distractability, poor judgment), **depressive symptoms** (dysphoric mood, change in appetite or weight, insomnia or hypersomnia, psychomotor agitation, psychomotor retardation, loss of interest or pleasure, loss of energy, feelings of worthlessness, diminished ability to think or concentrate, recurrent thoughts of death/suicide, distinct quality of mood, nonreactivity of mood, diurnal variation), **anxiety symptoms** (panic attacks, agoraphobia, social phobia, specific phobia, obsessions, compulsions).
(Andreasen and Black, 1995, pp. 52–83)

In this context, therefore, it is these elemental signs and symptoms (rather than the disorders) that are the objects of psychiatry and, if that is so, then the disorders become the concepts. The signs and symptoms (as objects) are grouped together according to a specific psychiatric conceptual schema to create the disorders of psychiatry. The disorders, reinforcing the spiral, are then the objects of psychiatry.

If we stay with the signs and symptoms as the “objects” of the concept of psychiatric disorders, however, then where do these sign and symptom objects arise? Why these objects rather than others? Are signs and symptoms natural objects, which psychiatric observers simply discover, or are they products of psychiatric creation? From the perspective of Foucault’s discursive practice, they are a mangled combination of both. To understand the created side of the signs and symptoms, however, we must get into the

details of the clinical interview. In a fascinating demonstration of the clinical creation of psychiatric objects, Andreasen and Black outline specific interview questions for “eliciting” the signs and symptoms. For example, if the psychiatric interviewer wants to check for “somatic delusions,” she should ask, “Is there anything wrong with the way your body is working?” or “Have you noticed any change in your appearance?” (p. 54). For “grandiose delusions,” she should ask, “Do you have special powers, talents, or abilities?” or “Do you feel you are going to achieve great things?” (p. 56). For “thought insertion,” she should ask, “Have you felt that thoughts were being put into your head by some outside force?” (p. 59). For “bizarre behavior or disorganized behavior” of the “clothing and appearance” type, she should ask, “Has anyone made comments about the way you look?” (p. 61). To check for “incoherence (word salad or schizophasia)” she should ask something like, “What do you think about current political issues like the energy crisis?” If the interviewee answers with, “They are destroying too many cattle and oil just to make soap. If we need soap when you can jump into a pool of water, and then when you go to buy your gasoline, my folks always thought they should, get pop but the best thing to get is motor oil, and, money . . .” the psychiatrist should suspect incoherence (word salad or schizophasia) (p. 65). These questions and answers used to elicit signs and symptoms set up the conditions of accommodation and resistance that define Foucault’s notion of a discursive practice. The signs and symptoms are not created out of whole cloth, nor are they simply discovered; they are organized through a complex negotiation that mangles together human and material agencies (Pickering, 1993).

Notice that these psychiatric questions are not really designed to ascertain whether the interviewee has something wrong with her body or has special talents, and

they are certainly not concerned with what the interviewee thinks about the energy crisis. The psychiatrist uses these questions to elicit object elements of discourse that she can put together into a conceptual grid or schema of disorder. In turn, the conceptual schema drives her questions and her perception of the answers. The conceptual schema selects the signs and symptoms and gives them legitimacy. The conceptual schema also reorganizes the signs and symptoms into another group of objects (the disorders) that gives them the legitimacy of medical science.

In addition to objects and concepts, Foucault also discusses the role of *strategies* in his rules of formation. By “strategies,” Foucault refers to a more abstract level of schematization (beyond objects and concepts) that could be also called “themes” or “theories” (Foucault, 1972, p. 64). As I discussed at length in the first chapter, the main theme or theory of the new psychiatry is (again somewhat paradoxically) “atheoretical science.” In the new psychiatric discourse, the strategy of atheoretical science is applied to (and used to create) the psychiatric disorders through an underlying neurochemical causal theory of mental disorder. There is nothing necessary about this. An “atheoretical” theory of psychiatry could have focused on psychological, political, or aesthetic causes of mental disorder. As Andreasen and Black’s enthusiasm reveals, however, the science of the new psychiatry is primarily neuroscience. (“Neuroscience and psychiatry are exploring the last uncharted territory in the human body. It is an incredibly exciting time to work in these fields”; 1995, p. viii.) Thus, the new psychiatry creates the following discursive complex. The psychiatric disorders, composed of elemental objects of signs and symptoms, are used to create the concepts of disorders, which are then reinforced through the neurochemical causal strategy or theory of disorder. The objects (signs and

symptoms) are the parts, which are interpreted in terms of a whole, which are concepts (disorders), which are further interpreted in terms of another whole, which are the strategies (neurochemical causal theory of mental disorders).

The result of the new psychiatry's emergent discourse of objects, concepts, and strategies is that when the psychiatrist asks questions—"Have you felt that thoughts were being put into your head by some outside force?"—she is using the question not only to elicit and create an order of disorder, but also to probe into the deeper regions of neurological structure. Andreasen and Black put it this way, "ideas and emotions are both the product and the producers of a complex set of chemical events in brains" (1995, p. 15). Because the disorders in the new psychiatry are organized (at Foucault's strategic level) as neuroanatomically caused, what matters to the psychiatrist is brain structure rather than human meaning. Foucault describes a similar process in his history of medicine. With the stethoscope and the autopsy, the physician developed an anatomical gaze, which made invisible bodily depth visible (Foucault, 1973, p. 159). Since that development, and the corresponding switch to anatomical medicine that accompanied it, the medical evaluator's primary goal is to reveal underlying anatomical and physiological pathology. Similarly, in the new psychiatry, psychiatrists rarely talk to people (or even really notice them) with a goal of communicating or connecting. Instead, they probe people's brains through questions, which reveal to the psychiatrist the brain's underlying neuroanatomical structures. In Foucault's terms, they no longer ask "What is the matter?" they ask "Where is it broken?" (Foucault, 1973, p. xix) And, indeed, Andreasen's popular version of the text we've been discussing is titled *The Broken Brain* (Andreasen, 1984).

Rules of Exclusion in Psychiatric Discourse

Let me turn now to some of Foucault's "rules of exclusion"—these are also a significant site of divergence between Andreasen and Black's and Foucault's approaches to the "unity" of psychiatry. Andreasen and Black, of course, do not mention any rules of exclusion. Psychiatric science, for them (like science more generally), is the epitome of open inquiry. For Foucault, however, the rules of exclusion govern what cannot be said in a discourse (including psychiatric science), and they are as important to discursive formations as the rules of formation. In other words, for Foucault, though discursive formations are tightly bound groups of nondeterminate objects, concepts, and strategies, not anything can become an object, concept, or strategy. Strict boundaries are applied to these elements. Foucault articulated two boundaries of discourse that are particularly relevant for psychiatry today.

Reason and Folly

The first boundary I will discuss, the boundary between *reason* and *folly*, is at once the most obvious, the most subtle, and arguably the most pernicious boundary set in psychiatric discourse. The boundary is obvious in psychiatric discourse because the distinction is the very reason for psychiatry's existence. It is subtle because it is so pervasive in psychiatric discourse that it is hard to see (as in the quote often attributed to Marshall McLuhan, "I don't know who discovered water, but I'm pretty sure it wasn't a fish"). And, finally, it is the most pernicious because it has the most pronounced effects on who is allowed to contribute to the discourse of psychiatry.

To be mad, Foucault argues, is to be cut off from legitimate discourse. The mad person's "words are null and void, without truth or significance, [and they are] worthless

as evidence” (Foucault, 1972, p. 216). To be mad, or “in folly,” is to be out of bounds. One’s words do not count. This has been a feature of Western society since the Middle Ages, and it continues to be the defining feature of psychiatric practice and knowledge formation today. The psychiatric clinician, by definition, occupies the position of “reason,” and the patient is relegated to the position of “folly.” This boundary works to create the incredible phenomenon that the patient’s perspective is not a part of psychiatric discourse until the psychiatric observer processes it. What the patient says is “folly.” Only the “reasonable” words of clinicians and researchers are included in psychiatric discourse. Thus, whole worlds of possible contributions to psychiatric discourse are excluded, and the most important stakeholders (the persons whom the discourse is manifestly designed to assist and affect) are excluded from the outset. To illustrate the extent of this exclusion, consider that, although Andreasen and Black have 50 pages of references in their text, as far as I can tell, the people experiencing the problems the text “describes” write none of these references. Even if there were one or two references I missed, it is still Andreasen and Black who have selected these references and not others.

Of course, the new psychiatry’s use of “reason and folly” as a rule of exclusion is not unique in the history of psychiatry. This exclusion was just as dominant, for example, in the psychoanalytic psychiatry that came before it. However, I would argue that the new biological psychiatry is more extreme with regard to the reason and folly boundary exclusion than the psychotherapeutic psychiatry that came before it was. I say this because, in addition to having no contributions from patients in their text, Andreasen and Black also proudly place themselves in line with “Kraepelin, Alzheimer, Nissle, and Brodman”—psychiatry’s early “brain scientists”—and disassociate themselves from the

more phenomenological and dialogic methods inspired by Freud and psychotherapeutic psychiatry (p. 12.) For Freud, one of the main differences between brain science and psychoanalytic science was that psychoanalysis took patients' meaning seriously. Psychoanalysts, by their own accounts, did not immediately reduce patient contributions to neurotransmitter configurations; they tried to understand the meaning for the person involved. Yet, despite taking the patients seriously, psychoanalysts also never brought the patients directly into psychiatric discourse. In analytical psychiatry as much as in biological psychiatry, patients do not write the texts, experts do. The major difference in this regard is that in analytic psychiatry the patient's folly took the form of unconscious conflicts rather than neurochemical imbalances. In either case, the patient remains on the side of folly, with the clinician on the side of reason.

True and False

Beyond reason and folly, another rule of exclusion that has been and continues to be relevant to psychiatry is the boundary between what Foucault calls "*true and false.*" Foucault argues that new scientific discoveries, "great mutations in science," in addition to whatever new knowledge they bring, also bring "new forms of the will to truth" (Foucault, 1972, p. 218). He differentiates the "will to truth" from the "will to knowledge." The will to truth desires more than knowledge, it desires unimpeachably True knowledge. The will to truth imposes its exclusionary force on discourse by prescribing "a certain position, a certain viewpoint, and a certain function" (p. 218). These methodological exclusions limit discursive possibilities by disallowing knowledge not gained through the prescribed method. The will to truth is an indirect exclusion that works through sleight of hand. Inquiry is claimed to be open, but it is only open within a

narrow range of positions, viewpoints, and functions. The will to truth denies its own desire and innocently rides on rhetoric of innocently “gaining knowledge.” Clearly this rule of exclusion is a core feature of the new psychiatry, and as I discussed in the first chapter, the use of the boundary between true and false has been central to the new psychiatry’s rise to a dominant position. Again, however, the new psychiatry is not unique in this way. All the dominant psychiatric formations of this century have used the will to truth as an exclusionary principle. The new psychiatry’s aggressively scientific rhetoric, however, has elevated the will to truth to its most strident form yet.

Summarizing this section, Foucault’s theory of “discursive practice” highlights the multiple discursive ties (rules of formations and exclusion) that bind psychiatric practice together. He emphasizes the semiotic network of objects, concepts, and strategies which is shaped by rigid distinctions between reason and folly and the true and the false. This emphasis on the semiotic network elucidates the simultaneous role of language and practice in cultural formations like psychiatry and provides cultural critics with the reading tool of semiotic analysis for unpacking these formations. At the same time, Foucault’s notion of discursive practice clearly allows us to sidestep the paralyzing binary between relativism and objectivism. Discursive practices are not just about signs; they are also about performative practice. They function to shape, capture, and affect aspects of the world. Simultaneously, though, discursive practices are not universal or necessary. Each historically and locally contingent discursive practice engages the world in a particular way and develops its own particular gains and losses. In that way, Foucault’s “discursive practice” resonates with the metaphysics of “semiotic realism” I

discussed in the last chapter and provides tools to greatly develop and extend this metaphysics.

If we take these many features of Foucault's discourse analysis together, they have a compelling effect, showing the composition of discursive practices. Also, Foucault's rules of formation and exclusion provide powerful analytics for unpacking discursive practices and highlighting key elements of discursive construction. However, discourse analysis does not neutralize discourse, and it does not make it meaningless or insignificant (indeed, just the opposite). Nor does discourse analysis reveal a "truth" underlying discourse or outside of discourse. Discourse analysis, rather, attempts to uncover and free the prediscursive body of rules and relations, which enable objects, concepts, and strategies to form a discourse. It attempts to uncover the conditions of their historical appearance. In Foucault's words, "What, in short, we wish to do is dispense with things. To 'depresentify' them" (Foucault, 1972, p. 47). Foucault's discourse analysis makes it impossible to do just exactly what Andreasen and Black attempt to do—equate the history of a discourse with a history of the referent. Psychiatric discourse is not a story of psychiatric science's increasing knowledge of psychiatric disorders. The referents of psychiatric disorders are not there outside of the rules of discourse that discuss these referents. As a result of discourse analysis, psychiatric "things," the real behind the discourse, fade in importance as the determining cause of the psychiatric discourse. If things fade into the background, though, something else must arise in the foreground.

Power in Psychiatric Discourse

That something else is *power*. Thus far, my discussion of a discursive practice has largely passed over the question of what causes a discursive practice to settle on one particular set of objects, concepts, and strategies rather than another. For Andreasen and Black the answer is correspondence to the real world of things. However, if the world underdetermines the selection of these discursive elements, as Foucault argues, what else contributes to their determination? One answer, as I discussed in chapter 2, is that, beyond correspondence, knowledge formations are also determined by concerns with coherence and consequence. In chapter 2, I argued affirmatively for a conscious inclusion of all three C's in knowledge evaluation. One could also make the case that even so-called pure epistemologies (whether they focus on correspondence, coherence, or consequences) actually utilize all three C's—they just fail to acknowledge the other two. Either way, my pragmatic epistemology must be extended to fully answer the “why this way?” question. The three C's alone provide an incomplete answer because there are multiple ways to combine them. Pragmatic epistemology gives only loose heuristic guides for decision making. By themselves, the three C's still leave open the question of what fixes the combination in one way rather than another.

To answer this question, retheorized psychiatry requires an additional analytic. Foucault supplies this need with his analytic of “power.” Discursive formations, for Foucault, are underdetermined by the world, but they are overdetermined by power relations. A new discursive formation is not just a new “systematicity, theoretical form, or something like a paradigm;” it is a whole new “discursive regime” (Foucault, 1980, p. 113) Accordingly, Foucault argues, knowledge and power must be thought together.

Knowledge is not free from power, but, rather, knowledge is solidified through power and power is solidified through knowledge. Indeed, without power there can be no knowledge, and without knowledge there can be no power. In Foucault's words:

We should admit . . . that power produced knowledge (and not simply by encouraging it because it serves power or by applying it because it is itself useful); that power and knowledge directly imply one another: that there is no power relation without the correlative constitution of a field of knowledge, nor any knowledge that does not presuppose and constitute at the same time power relations. (Foucault, 1977, p. 27)

Without power, the elements of knowledge do not hold together. Thus, it is power that causes one discursive formation to crystallize rather than another.

Power/Knowledge

Foucault's intermingling of power and knowledge, or "power/knowledge," marks a sharp contrast from the way power and knowledge relations are usually understood within Western liberal traditions. In an attempt to clarify this contrast, philosopher of science Joseph Rouse outlines three versions of the "received view of knowledge and power," which are all overturned in Foucault's work (Rouse, 1987, p. 12). One overturned version of power and knowledge is that knowledge creates power, because power is understood as the capacity to dominate and manipulate nature. As Francis Bacon puts it (in his overtly gendered and colonial phrase), through increased scientific knowledge, scientists "bind [nature] to your service and make her your slave" (Rouse, 1987, p. 20). With the rise of social sciences, including psychiatry, power over nature expands to include power over people as well. As a result, according to this view of power and knowledge, knowing yields control over the world and over people; not

knowing yields impotence. In a second overturned version of power and knowledge relations, the situation is reversed. Power in the second version does not yield knowledge as much as power blocks knowledge. In other words, power interferes with or distorts the acquisition of knowledge. Power causes biased belief and allows unjustified and false beliefs to be accepted inappropriately. Finally, a third overturned view flips both of these around. In this version, knowledge does not create or block knowledge. Rather knowledge liberates people from the oppressive effects of power. Knowledge is the great emancipator, or as Galileo put it, a thousand politicians and a thousand philosophers “would be left in the lurch by any average man who happened to hit on the truth for himself” (Latour, 1987, p. 32).

The important point here is that all three of these received liberal versions keep knowledge and power extrinsic to each other. Knowledge and power remain independent and distinct. For each version, as Rouse explains,

Knowledge acquires its epistemological status independent of the operations of power. Power can influence what de facto is known, but its being known, and what it is for it to be known, cannot be subject to the influence of power. That is, power can influence what we believe, but considerations of power are entirely irrelevant to which of our beliefs are true, which of these are known to be true, and what justifies their status as knowledge.” (1987, p. 13)

Rouse argues that these accounts of the exclusion of knowledge and power imply a correspondence theory of knowledge, which, as I discussed in the last chapter, rests on a referential theory of the sign. In a referential theory, knowledge is true if it corresponds to the world independent of people’s opinions or of what they may try to force others to believe. As Rouse further argues, however, these liberal or extrinsic versions of knowledge and power not only assume a correspondence theory of knowledge, they also

assume a particular theory of power. Clearly, the meaning of “power” is not transparent and it can be understood in different ways. From Rouse’s reading, if we are to integrate knowledge and power as Foucault suggests, we must rework not only our liberal understandings of the relationship between knowledge and representation, but we must also rework our understanding of power itself.

Accordingly, turning from questions of knowledge and power relations, Rouse teases out three features of power itself that predominate in most liberal political theories:

First, power is possessed and exercised by specific agents (persons, institutions, classes, groups) from which power is used centrifugally; second, power operates on our representations, but not on the world represented; and third, power is primarily repressive, secondarily enabling, but not productive. (Rouse, 1987, p. 12)

For Foucault, all three of these features of power must be rethought. I will discuss these starting with the latter two, because they both rest on the conception of knowledge and power independence that I have just been considering. With regard to the second feature of liberal political theory, in Foucault’s reworking, power does not operate on representation so much as it is intrinsic to representation. Foucault’s detailed historical analyses of psychiatry, medicine, the human sciences, prisons, and sexuality all undermine the idea that representations and power are exclusive. In reflecting on these histories and their discursive practices, Foucault emphasizes that although it is possible, and even useful, to distinguish between questions of capacity (what knowledge can do, its ability to “modify, use, consume, or destroy” things in the world), questions of communication (the “language, systems of signs, or any other symbolic medium” through which knowledge is represented), and questions of power (the ensemble of actions through which “persons exercise power over others”), it is important to remember that

“capacity-communication-power” do not exist in separate domains (Foucault, 1983, p. 218). Rather, all of these constitute aspects of knowledge representations that create a kind of “block,” through which capacities, communications, and power are “welded together” (Foucault, 1983, p. 219). As a result Foucault concludes that, except for analytic purposes, there can be no possibility of isolating representations and power.

Concerning the third feature of liberal political theory (“power is primarily repressive, secondarily enabling, but not productive”), from the perspective of knowledge/power intermingled rather than separated, it makes little sense to see power as repressive or enabling. Rather, power, for Foucault, is productive. Repressive power, what Foucault refers to as “juridical power,” is power that says no. This is power that works by deduction or subtraction; it tells you what you cannot do: “Power in this instance is essentially a right to seizure: of things, time, bodies, and ultimately life itself” (Foucault, 1978, p. 136). Enabling power, by contrast, is power that liberates, power that says yes. This is the power that the received liberal approaches hope to achieve through knowledge. As I discussed above, this is the power to liberate repressive power regimes through the “truth.” The “truth,” in this case, “will set you free.” Thus, when power is seen as repressive, the liberatory goal is to achieve enabling power through knowledge. If power/knowledge is always intrinsic rather than extrinsic, however, power doesn’t simply repress or enable knowledge, power *produces* knowledge and power *produces* effects. For Foucault,

What makes power hold good, what makes it accepted, is simply the fact that it doesn’t only weigh on us as a force that says no, but that it traverses and produces things, it induces pleasure, forms of knowledge, produces discourse. It needs to be considered as a productive network which runs though the whole social body,

much more than as a negative instance whose function is repression. (Foucault, 1980, p. 119)

Thus, power in Foucault's frame is much more complicated than an authoritative "yes" or "no." Power is intertwined with everything that is known. Thus, the political problem is not the liberal political problem of liberating knowledge from power because power and knowledge are necessarily intermingled. Since power/knowledge is productive, not simply repressive, it is woven into the everyday microfabric of what is known and what is done. There can be no surgical extraction from knowledge of a repressive power leading to a power-free enablement.

Returning to the first feature of power Rouse outlines ("power is possessed and exercised by specific agents . . . from which power is used centrifugally"), Foucault also argues that power is not centrally organized (only possessed and exercised by specific "powerful" persons) in any simple way. Rather, power must be understood as circulating and diffuse. In particular, Foucault argues that analyses of power should not focus on the state as the main locus of politics and power relations. Power, for Foucault, is not primarily located in the state but is embedded in the micropractices of daily life. Accordingly, the state should not be the only or even the primary focus of political attention and analysis, and "nothing in society will change if the mechanisms of power that function outside, below and alongside the State apparatuses, on a much more minute and everyday level, are not also changed" (Foucault, 1980, p. 60). This insight is critical to my retheorized psychiatry. From Foucault's perspective, political practice is not a struggle or battle for or against state government. It is a struggle in the much more mundane and specific domains of daily life—such as clinical psychiatry. These politics of everyday life are as important for Foucault as state politics, and he argues that these

politics require us to recognize a new kind of intellectual—specific intellectuals. In contrast with general intellectuals, who articulate large-scale conditions of oppression and power relations, specific intellectuals work within much more specific domains—such as clinical psychiatry.

However, even when one moves outside the state to look at minute power relations in schools, hospitals, laboratories, factories, business administrations, barracks, and prisons, Foucault argues against a centralized or top-down understanding of power. Emphasizing this point, Foucault distinguishes between power and slavery. Power is a “mode of action upon the actions of others,” but power analysis must always include an element of “freedom” (Foucault, 1983, p. 221). “Slavery,” by Foucault’s definition, “is not a power relationship.” When a person is in chains, “it is a question of constraint,” not power. “Consequently, there is not face to face confrontation of power which is mutually exclusive (freedom disappears everywhere power is exercised), but a much more complicated interplay” (p. 221). Thus, Foucault’s effort here is not to analyze aspects of control operating through direct force as much as to analyze those aspects of power that operate through consent and desire. However, consent, for Foucault, is never total agreement, and his quarry is not voluntary or dupish servitude to power (p. 221). Rather, the power Foucault describes always includes freedom, and, as such, it also always includes resistance. For Foucault, “the relationship between power and freedom’s refusal to submit cannot therefore be separated” (p. 221).

“Agonism” is the term Foucault uses to describe this relation of power and freedom. He coins this neologism based on a Greek word for “a combat.” According to Foucault’s translator, the term “implies a physical contest in which the opponents

develop a strategy of reaction and mutual taunting, as in a wrestling match” (Foucault, 1983, p. 221). Thus, power relations are not understood as slavery, nor are they blind consent. Power is best analyzed as “struggle, conflict, and war” (Foucault, 1980, p. 90). Indeed, Foucault explicitly reverses Clausewitz’s assertion that “war is politics by other means” (p. 90). Instead, he argues that, in peacetime, politics and power are wars by another means. In peace, as in war, relations of power do rest on relations of forces, but these are not top-down forces alone; they are circulating forces. Accordingly, when war ends, the conflicting and circulating relations of force do not stop. Rather, there is a “reign of peace” that reinscribes the prior clash of forces into an “unspoken warfare” (Foucault, 1980, p. 90). And like the war before it, the outcome of later peace—the directions it takes, its winners and losers, the knowledges it produces, the histories written about it, and the ways of life it creates—should all be understood as a contest of strength.

As a whole, Foucault’s discussion of power and its relation to knowledge add another key distinction from Saussure’s discussion of *langue*. With the move to power and the way that power overdetermines the objects, concepts, and strategies of discursive formations (and the rigid distinctions of discursive exclusions), Foucault opens the door to human interactions and human politics. Discursive formations do not just happen; they are the outcome of struggle. Therefore, any “semiotics” must explain not only representations but also the political struggle to create those representations. Although Foucault is very helpful in this way, he remains rather vague about who or what are the protagonists in the political struggles for knowledge. He is clearly not a liberal humanist. He does not idealize the individual as an autonomous actor. However, he is not a

structural determinist who understands individuals as being spoken through language and institutions. Indeed, he sees nothing sacred about the unit of the individual. The struggle inherent in discourse formations is a struggle of “all against all,” and the units in the struggle are subindividual as much as they are supraindividual (Foucault, 1980, p. 208). Foucault’s theory of discursive struggle is neither optimistic nor pessimistic. Change happens, and the future is not determined, but neither is progress guaranteed. Human intentions contribute to change, but not in any linear direction. The future will depend also on “chance, discontinuity, and materiality” (Foucault, 1972, p. 231) Unforeseen events, radical breaks, and the limits of materiality are all factors of change. Thus, human action and intention is only a small part of change. Humans may partially direct their own actions, but they do not direct the larger cultural outcome of those actions. They may know what they do (to a degree), but they do not know what what they do *does*.

Final Rules of Discursive Formation and Exclusion and Their Relation to Power

Having considered Foucault’s analytic of power, I want now to return to his discussion of discursive practice and consider his final rules of discursive formation—“enunciative modalities”—and his final rules of discursive exclusion—“commentary, authorship, and discipline.” Although, as I mentioned, Foucault’s work as a whole is vague about the specific actors involved in discursive struggle, in these rules of formation and exclusion he gives his most detailed articulation of the protagonists involved. Accordingly, these rules of formation and exclusion are the most obvious link between Foucault’s earlier work on discursive practice and his later work on power. Bringing the two together suggests real tactical possibilities for retheorizing psychiatry.

Enunciative Modalities

Starting with *enunciative modalities*, the most important thing to notice about these rules of formation is that they focus on human actors and the social world of people and institutions. By contrast, Foucault's other rules of formation (objects, concepts, and strategies) articulate elements of discourse that exist independent of people—although, clearly, relations between people account for the emergence of these elements of discourse. Without people, these elements of discourse would wither away, and, in reverse, without objects, concepts, and strategies, people would not come into subjective being. Objects, concepts, and strategies are crucial to a discourse and crucial to a people, but these discursive elements are silent until they are articulated through people and through institutions (just as the people and institutions are silent without objects, concepts, and strategies). However (in spite of their clear intermingling with people), objects, concepts, and strategies tend to focus on textual and linguistic aspects of a discourse rather than on the human aspects. As important as these aspects may be, it is the enunciative modalities (the people and their structural positions in a community) that bring unity to a discourse. Unity does not come from the objects, concepts, and strategies alone (though these clearly contribute to its stability).

Rather, unity comes from the enunciative modalities, which animate the other elements of discourse. Thus, to better understand what links the objects, concepts, and strategies together—"what necessity binds them, why these and not others"—one must first ask, "Who is speaking?" (Foucault, 1972, p. 50).

Who, among the totality of speaking individuals, is accorded the right to use this sort of language? Who is qualified to do so? Who derives from it his own special quality, his prestige, and from whom, in return, does he receive if not the

assurance, at least the presumption that what he says is true? . . . medical statements [for example] cannot come from anybody; their value, efficacy, even their therapeutic powers, and, generally speaking, their existence as medical statements cannot be dissociated from the statutorily defined person who has the right to make them. (p. 50)

With the introduction of the “who” and the “who not” of discourse, Foucault opens the door to power/knowledge relations and to a clear connection between power and discursive practice. The people involved and excluded in the process of producing the discourse, and the power interactions and struggles between them, are internal elements of the discourse itself. They are not outside discourse; they are inside. This “inside of power relations” phenomenon is important for understanding discursive practices. If an important element of a discursive practice is who does and who does not speak, then it makes little sense to analyze discursive practice based on its content alone. Discourse analysis must examine who is empowered to speak (and with what effects) and who is prevented from speaking (and with what effects).

In addition to the “who” of discourse, enunciative modalities also include the institutional location of the speakers and the position of speakers in relation to others. It is not enough for a discourse to have objects, concepts, strategies, and speakers. The speakers must be given support and legitimization through institutions and infrastructures. To use psychiatry as an example, the enunciative modalities of psychiatric discourse includes not just “the who” of psychiatrists, it also includes the institutional sites that authorize them. Such institutional sites as universities, conferences, journals, texts, hospitals, private practice offices, laboratories, and libraries scaffold psychiatric discourse and support its unity and its legitimacy. Thus, these institutions, and the rules and rituals that structure them, should not be seen as outside of discourse any

more than the speakers of the discourse are. The discourse and the institutions, like the discourse and the speakers, are commingled.

Beyond speakers and institutional infrastructures, discourse is also given unity through the enunciative position of speakers. In the discourse of psychiatry, for example, the speaker of psychiatric discourse is in an active position in relation to others. The psychiatrist is the “questioning subject” and the “listening subject;” she is the “seeing subject” and the “observing subject” (Foucault, 1972, p. 50). These psychiatric (clinician/patient) relations coconstitute themselves. On the one hand, these relations must be intact for the psychiatric discourse to work. On the other hand, the psychiatric discourse reinforces these relations.

In sum, the unity of clinical psychiatry is not explained in its objects, concepts, and strategies alone but also in “the establishment of a relation . . . between a number of distinct elements, some of which concerned the status of doctors, others the institutional and technical site from which they spoke, others their position as subjects perceiving, observing, describing, teaching, etc.” (Foucault, 1972, p. 53). In other words, one must also understand the enunciative modalities of a discourse: the people, their institutions, and their relational positions.

Commentary

Foucault’s final rules of exclusion also bring us into the world of discursive actors and their relations. These final rules of exclusion, which he labels “commentary,” “author,” and “disciplines,” articulate rules whereby the practitioners of a discourse both limit and expand their discourse (along constrained lines). Like his last rule of formation, these final rules of exclusion focus less on linguistic and textual structures and more on

the particular people and social processes involved in creating and maintaining a particular discursive formation.

Starting with *commentaries*, these are, for Foucault, the secondary texts discursive practitioners create to elucidate the deeper and vital meanings of primary texts.

Commentary rests on the assumption that there is something not explicitly expressed or not easily accessible in primary sources that is, nonetheless, the implicit fundamental meaning intended by the speakers and writers of these primary texts. In the humanities, commentaries often take the form of secondary resources on “great books.” In psychoanalytic psychiatry, the same was pretty much true. Accordingly, key texts from Sigmund Freud or Jacques Lacan, for example, would therefore be a source of extensive commentary. The function of these kinds of commentaries, for Foucault, is to “limit the hazards of discourse” by inspiring similar interpretations of the great works (Foucault, 1972, p. 222).

As the new psychiatry has emulated the sciences more and more, “great books” in psychiatry are rarely published. Accordingly, the new “primary source” of psychiatry has become, like elsewhere in modern science, the research literature. But the role of commentary to limit and constrain the interpretations of research literature is still very much present. Some examples could include Gerald Maxmen’s *New Psychiatry*, which I discussed in the first chapter, or Andreasen’s *Broken Brain*, which I mentioned earlier in this chapter. Neither of these commentaries is about great books. Rather, they are about a collection of research events that the commentaries bring together into a coherent interpretive frame. These commentaries are targeted to both a psychiatric audience and a lay audience, and they work to reproduce what Foucault calls “repetition and sameness”

of interpretation of these events in both of these audiences (Foucault, 1972, p. 222).

Beyond these secondary texts that organize psychiatry into a limited frame, most psychiatrists' daily mail is barraged with a variety of throwaway journals, newsletters, and invitations to special continuing education conferences. These too may be understood as forms of commentary that create repetition and sameness in psychiatry. Drug companies indirectly sponsor many of these commentaries, and they often support the biopsychiatry paradigm that uses drug company products. A related source commentary also comes from the drug companies in the form of lobbying from drug company representatives, direct mail advertisements, trade journal advertisements, and popular media advertisements. Clearly, these are also forms of commentary as Foucault understands the term, but they extend beyond what even Foucault imagines in their slickness and production value.¹ Like other commentaries, these various advertising tools

¹ When I practice psychiatry, I usually use a form of "commentary" to guide my use of prescription medications. The form I use is a handbook of psychiatric medications that comes out every four years or so and collects the author's interpretation of the current standard wisdom regarding which drugs to use for what. This method, though it allows my prescribing practice to evolve, always leaves me just behind the times compared to my peers with regard to the speed with which new drugs become integrated into my psychiatric practice. This is particularly true in academic locations. If other clinicians were using my method, it would take them (as it does me) a few years before a new drug would make it to the handbook and thus into their practice. When I talk with psychiatrists about this, they are very disparaging about using handbooks as their source of commentary for new medications. Rather, they recommend, "prescribing from the journals." By this they mean prescribing from the most recent research data published in those journals. However, most of these psychiatrists are extremely busy, and the idea that they are sitting down reading detailed research articles to carefully parse out which drugs are new and the best to prescribe has always struck me as unlikely. As a result, I have become increasingly suspicious that it is more likely that they are prescribing from the advertising pages of those journals. The American Journal of Psychiatry has approximately 50 pages of (very) professional advertisements for 149 pages of professional articles. Psychiatric Annals has approximately 22 pages of add for 34 pages of articles. Most of those ads are devoted to the "new and improved" versions of medications that are all on patent and thus bring a higher profit for the drug companies than the older medications. The ads tell the clinician what drugs are new, how they are "improved," and what dose to prescribe. They usually also show a happy consumer or a happy clinician—they leave out happy pharmaceutical CEOs.

work to create a sameness and repetition in the minds of both psychiatrists and their consumers. The generalized message is “Psychiatrists give you drugs and that is good.”

Author

For Foucault, the *author*, or more precisely the “author-function” (Foucault, 1984, p. 107), is a rule of exclusion because it constrains and controls discourse, not so much through the individual who writes or gives talks, but through the unifying functions discursive practitioners give to that individual. Again there is some difference between the sciences and the humanities. In the humanities, the author-function goes to the writers of primary sources, not so much the writers of commentaries. For example, in the more humanities-similar psychoanalytic psychiatry, Freud the author and the biographical narratives told about him functioned to unify and limit the Freudian field. By contrast, in science, and the new psychiatry is exemplar, the role of primary “author” is diminished and replaced by research methods and research traditions. There is less of a place for the unifying “author-function” with regard to primary research. However, even in the sciences, the author-function is not completely lost, and it continues to a lesser degree in the commentaries. Thus, for example, Nancy Andreasen’s name, through an assumption of her coherent identity and individuality, functions as a kind of organizing principle for all that she brings together in her texts. The mishmash of material found in *The Broken Brain* and the *Introductory Textbook of Psychiatry* (Andreasen, 1984; Andreasen & Black, 1995) is thus solidified and strengthened in its presumed coherence through the cohesiveness of the name “Nancy Andreasen.”

A related author-function that is significant for the new psychiatry, but which Foucault did not extensively discuss, could be called the “publisher function.” Although

Foucault does mention publishing systems (see the discussion of the *disciplines* below), the American Psychiatric Association (APA) Press is more effective in its capacity to constrain and unify psychiatric discourse than other publishers because it is the publishing house of the association itself. As the official publishing house of the profession, it carries with it a “unifying function” which disparate university and commercial publishers could never achieve. Much of what the APA Press publishes is very friendly to the new psychiatry. For example, both Andreasen and Black’s *Introductory Textbook of Psychiatry* (as their leading textbook of psychiatry) and Andreasen’s more popular version, *Broken Brain*, are published by the APA Press. In addition, APA Press publishes *DSM-IV* and a variety of guidebooks on how to read the manual. And this just scratches the surface. APA Press also publishes multimedia commentaries on categories ranging from anxiety, to APA practice guidelines, to history of psychiatry, to reviews of psychiatry, to trauma and violence (APA Press, Inc., 2000, <http://www.appi.org/>). Clearly the “unifying function” of these APA published commentaries, each carrying the “APA Press” stamp on their cover, is immense.

Disciplines

Foucault’s final rule of exclusion concerns the *disciplines*, which “are defined by groups of objects, methods, their corpus of propositions considered to be true, the interplay of rules and definitions, or techniques and tools: all of these considered to be a sort of anonymous system” (Foucault, 1972, p. 222). Foucault points out that a discipline is “not the sum total of all the truths that can be uttered about something,” and he gives the example of medicine, which “does not consist of all that may be truly said about disease” (p. 223). Disciplines limit their discursive field through their use of the rules of

formation and exclusion I discussed earlier. Thus “disciplines constitute a system of control in the production of discourse, fixing its limits through the action of an identity taking the form of a permanent reactivation of the rules” (p. 224). Disciplinary limitation and control is not, however, simply an effect of language. Rather, disciplines actively achieve limitation and control through a variety of social processes that Foucault articulates as “rarefaction of speaking subjects,” “ritual,” “apprenticeship,” “fellowship of discourse,” “act of writing,” “doctrine,” and “social appropriations” (pp. 224–227).

By “rarefaction of speaking subjects,” Foucault means that “none may enter into a discourse on a specific subject unless he has satisfied certain conditions or if he is not, from the outset, qualified to do so” (Foucault, 1972, pp. 224–225). The restrictive process of determining speakers occurs through a “ritual” process of “apprenticeship” and evaluation that “defines the qualifications required of the speaker . . . ; it lays down gestures to be made, behaviour, circumstances and the whole range of signs that must accompany discourse; finally, it lays down the supposed, or imposed significance of the words used” (p. 225). Those who are so initiated and thus qualified as speaking subjects of the discipline form a “fellowship of discourse.” The function of the fellowship is to preserve and to reproduce discourse “in order that it should circulate within a closed community [and] according to strict regulations” (p. 225).

By “act of writing,” Foucault means the institutionalized process of “books, its publishing systems and the personality of the writer [that] occurs within a diffuse yet constraining, ‘fellowship of discourse’” (Foucault, 1972, p. 226). These writings work together to produce a “doctrine,” which “links individuals to certain types of utterance while consequently barring them from all others. Doctrine effects a dual subjection, that

of speaking subjects to discourse, and that of discourse to the group . . . of speakers” (p. 226). Finally, Foucault turns from these internal disciplinary processes to articulate the more general “social appropriation” of disciplines. Education into a discipline, Foucault points out, is not open to everyone, and the distribution of who may and who may not be educated all too often “follows the well-trodden battle-lines of [broader] social conflict” (p. 227). Thus, education into a discipline is not a neutral internal process but very much a political process of maintaining and integrating the social status of the disciplinary members and the knowledge and the powers associated with such membership.

The relationship between these processes of discourse exclusion and that of current psychiatric discourse is too obvious to linger over. Briefly, it seems clear that rarefaction of speakers occurs in psychiatry through the careful selection process and through the ritual apprenticeships of training, examination, licensing, and board certification. The act of writing captures the author-functions and publisher-functions discussed above, and the doctrine, for the new psychiatry, is the scientistic atheoretical approach. Clearly, the broader social appropriation is the relatively privileged status of initiates into and members of psychiatry’s fellowship of discourse. These processes are never complete, and they go beyond initial training to include the ongoing role of conferences, journals, advertisements, drug representatives, and so forth that I have discussed earlier.

It is crucial, however, that we situate these final rules of formation and exclusion, and the various processes of human and social relationships and interactions that go into creating disciplines and fellowships of discourse, in the context of Foucault’s discussion of power/knowledge. Foucault articulates these social relations of discourse before he

articulates his discussion of power, but if we bring the two together, we can better understand how power relations work in discursive production. Each of these many processes, from selection of initiates, to apprenticeship and evaluation rituals, to the varieties of publishing opportunities and the net of legitimate writings are all open to contest and struggle. According to Foucault's theory of power, top-down control is only one of the many directions power is exercised. For Foucault, the control circulates between the players (and within players and between aggregates of players), and the outcome of these circulating control processes will depend on the outcome of the power dynamics involved.

Read this way, Foucault's twin conceptual tools of discursive practice and power are anything but a counsel of despair. If one wishes to change or influence a discourse, Foucault's concepts certainly do not suggest a Pollyanna naïveté about the difficulties involved. They allow no facile underestimation of the challenge. Rather than suggesting despair, however, they are a *call to action* because they forcefully argue that all the players in a discursive practice can have an effect on the eventual outcome of that discursive practice. It is useful here, I think, to take literally the "course" in Foucault's notion of dis-course. In other words, dis-course is not static but is always "en route." Clearly the power dynamics of a discourse like psychiatry are unbalanced in favor of the few, and therefore psychiatric discourse tends to stay "on path" or to change paths along lines consistent with powerful interests. However, the route psychiatric discourse ultimately takes does depend on the outcome of various power dynamics involved. These routes can be influenced (admittedly with difficulty) from a variety of positions. Foucault's notion of a "specific intellectual" reinforces this conclusion. With this

articulation, Foucault insists that general or abstract philosophical analysis will be less capable of making a difference in a discourse compared with the specific actions and interventions of internal members of the fellowship of discourse (Foucault, 1980, p. 126).

From my perspective, a key leverage point and intervention available to specific intellectuals and like-minded discursive fellows is challenging the rarefaction of speakers. Thus, the goal of discourse change is best served by what I will call “recruitment over conversion.” Conversion, unfortunately, must work by reversing (in individual initiates and in their fellowship community) the whole process of disciplinary limitation and constraint. This is an extremely difficult task, especially because it threatens the whole social status appropriation that initiates gained through their discourse apprenticeship in the first place. By contrast, recruiting new members into a discursive fellowship requires no conversion. Instead, it requires reducing the rarefaction of speakers and opening the boundaries of the discipline. This too is often difficult, but it is easier than conversion. Opening disciplinary boundaries alone will effectively change the power dynamics of a discourse by changing the power relations between the members. The result will change the outcome, or the course, of what is known and what is considered, as Foucault puts it, “in the true” (1972, p. 224).

In later chapters, I will be doing readings of specific psychiatric discursive practices and their intermingled power relations. In the last chapter, I will return to the issue of psychiatric disciplinary practices, particularly with regard to questions of inclusion and the advantages of opening the American Psychiatric Association’s disciplinary boundaries. For the next chapter, however, I want to stay at the “philosophic” level and use the work of another poststructuralist philosopher, Jean-François Lyotard, to

organize and articulate these last two theory chapters (on language, discourse, and power) into a “postmodern manifesto” for a new kind of psychiatric knowledge formation.

Chapter 4: A Postmodern Manifesto for Psychiatry

In chapter 1, I discussed how the tropes of “postmodern theory” and “postmodernism” have become central designators of “theory” in the human sciences. Postmodern theory develops critiques of language, discourse, practice, and power (as outlined in the last two chapters), but, in addition, postmodern theory puts these critiques in a historical context. Postmodern historicization is particularly helpful in understanding psychiatry because psychiatry is a quintessentially modernist project. By evoking the periodizing concepts of “modernism” and “postmodernism,” I frame my discussion of psychiatry in a much larger historical context than when I discussed the recent development of the new scientific psychiatry in chapter 1. Accordingly, in this chapter, I look at aspects of “modern” psychiatry that have been present since its inception and that are relevant to each of the historical turns and divisions in the field.

The “new psychiatry” is only the most recent historical turn. Though it is a significant shift from a meaning-based psychiatry (psychoanalysis) to a neuroscience-based psychiatry (biopsychiatry), the new psychiatry, from a postmodern perspective, compulsively repeats more than it changes. Indeed, from a postmodern perspective, the new psychiatry’s shift from a psychoanalytic rhetoric to a neuroscience rhetoric is not so much a change as a hardening and a further modernist expansion of the worst aspects of the psychoanalytic science that preceded it. As such, postmodern theory helps articulate the intellectual and historical context of psychiatry in a wider historical frame and provides key models for retheorizing and reimagining psychiatry.

But why should psychiatry be retheorized and reimagined? In the United States, both medicine and psychiatry have ridden the crest of modernism and enjoyed tremendous expansion and popular support throughout much of the 20th century. Increasingly, however, this support is evolving into a chorus of criticisms. These problems have been well rehearsed in recent years, but, briefly, health care providers are rebuked for

overspecialization; technicism; overprofessionalism; insensitivity to personal and sociocultural values; too narrow a construal of the doctor's role; too much "curing" rather than "caring"; not enough emphasis on prevention, patient participation, and patient education; too much economic incentive; a "trade school" mentality; overmedicalization of everyday life; inhumane treatment of medical students; overwork by house staff; and deficiencies in verbal and nonverbal communication. (Pelligrino, 1979, p. 9)

This list, first drafted by Edmund Pelligrino almost two decades ago, does not even include the prevailing "health care crisis" critiques that cite an unsustainable rise in expenditures, gross inequities in access, and huge disparities in health depending on socioeconomic factors.

As a specialty of medicine, psychiatry suffers from all of these problems and more. Psychiatry is the only specialty that has an extensive protest movement organized against it (variously known as "antipsychiatry," "survivors of psychiatry," "madness network," or, my favorite, "mad pride"). In addition, paradoxically, psychiatry is rapidly shrinking and expanding in problematic directions. On the one hand, services are being seriously cut. Psychiatric patients are increasingly found struggling in prisons, shelters, or in the streets rather than in clinics receiving care. Psychiatrists are having more and more of their procedures denied by insurance cutbacks, psychiatric hospitals are closing,

research money is dwindling (except for the problematic funds coming from pharmaceuticals), and new trainees are becoming narrower and narrower in their knowledge base and clinical skills. On the other hand, psychiatric expansion is as troubling as psychiatric cutbacks. Psychiatric medicalization and pharmacologization of everyday life (such as medicating mildly depressed adults or inattentive and restless children) are proceeding at an unprecedented and, for many, frightening pace. Adults, children, and the therapists who help them are being dramatically deskilled in their capacity to resolve relatively minor problems. Increasingly they are led to rely on new medications (to the great profit of the pharmaceutical companies), rather than learning ways of working through human problems, suffering, griefs, and anxieties.

Yet, in spite of these difficulties, psychiatry continues to organize its core knowledge structures with minimal fundamental changes (except, perhaps, for a more relentless pursuit of previous approaches). So what are the organizing themes of psychiatric knowledge? What are the unspoken commitments that have been made, and how are those commitments contributing to psychiatry's current problems? This chapter is about going back to the drawing board and reconsidering fundamental assumptions. There are common themes underlying most, if not all, of the problems outlined above, and those themes are part of the much larger and more profound context of intellectual and cultural practices within which psychiatry is situated. Rather than focusing one by one on the details of each problem, I argue that we should back up our perspective in order to locate psychiatry in history, and, most important, within a particular way of thought.

Psychiatry, as a subspecialty of modern Western medicine, is a paradigmatic modernistic application of Enlightenment aspirations. There is no better example than psychiatry of the Enlightenment dream for human improvement and perfectibility through the twin goods of science and reason. Yet, as I discussed in chapter 1, across the main campus, throughout the arts, humanities, and social sciences, there is an increasing postmodern consensus that modernism is a deeply troubled project and an unfortunate (if not tragic) organizing narrative for human activities (Lyotard, 1984). Psychiatry in particular, and medicine in general, could benefit greatly from this postmodern critique. Medical schools and psychiatric residency training programs, however, separated from the main campus by institutional, subcultural, political, and even physical barriers, have yet to seriously engage postmodern critiques of the Enlightenment. Thus, they have been unable to situate multiple problems in health care and, indeed, the “health care crisis” within this larger critique of Western thought.

Of all the medical specialties, psychiatry (in spite of the new psychiatry’s recent claims) is the least consistent thematically with overly scientific methods and the closest in subject matter to the arts and humanities—the current academic locus of postmodern discourse. As of now, however, even with this closeness to humanities (or perhaps because of it) psychiatry has been less subjected to postmodern critique than has medicine (see chapter 5 for some examples). So far, though, even the existing postmodern critiques of medicine are not really part of medicine. They are coming from outside of medicine. Because psychiatry is closer in content to the humanities than other medical disciplines, psychiatry will likely be the first to move beyond external critique toward serious internal engagement with postmodern theory. Indeed, this book is self-

confirming evidence of that claim. How psychiatry emerges from the encounter will foreshadow how medicine itself will be affected. From my perspective, psychiatry, though likely defensive at first, could eventually emerge from an affirmative postmodern critique not only intact but also rejuvenated. Postmodern theory, at its best, provides a liberating effect on modernist practices, freeing them from an enslavement to Method and Objectivity in order to allow more humane perspectives and approaches (all charges of antihumanism notwithstanding) to emerge as valued and respected.

I anticipate that psychiatric knowledge and practice will change in several ways because of the encounter with postmodern theory. These changes include:

1. a shift in clinical knowledge structures away from their recent exclusive focus on neuroscience and quantitative social science toward the more qualitative approaches of philosophy, literary theory, anthropology, women's studies, Africana studies, cultural studies, humanities disability studies, and the arts;
2. a grounding of clinical activities in the wisdom of practice rather than the "objective truth" of research; and
3. a greater emphasis on ethics, politics, and pleasure as guidelines and goals for clinical progress and knowledge production.

In the best scenario, the net result will be the emergence of a new postmodern psychiatry and a new model for medicine that will be both more enjoyable to practice and more connected to the concerns of patients. Before reimagining psychiatry through a postmodern paradigm, however, let me back up for a closer look at modernism and its postmodern critique.

Psychiatry as a Modernist Project

“Modernity” refers to modes of intellectual life or organization that “emerged in Europe from about the seventeenth century onwards and which subsequently became more or less worldwide in their influence” (Giddens, 1990, p. 1). The intellectual ideals of modernism are the ideals of the Enlightenment philosophers. Tireless and vociferous apostles for the then radical “Age of Reason,” the Enlightenment philosophers advocated that humans not rest with intuitive faith, tradition, or authority but that they appraise their universe through rational inquiry, natural experience, and planned experiments. Theorist Jane Flax points out that “perhaps the most succinct and influential statement of Enlightenment beliefs” is in Immanuel Kant, “An Answer to the Question, What Is Enlightenment?” (Flax, 1990, p. 238). In this work, Kant describes and simultaneously prescribes Enlightenment ideals in this way: “Enlightenment is man’s release from his self-incurred tutelage. Tutelage is man’s inability to make use of his understanding without direction from another. Self-incurred is this tutelage when its cause lies not in lack of reason but in lack of resolution and courage to use it without direction from another. *Sapere aude!* ‘Have the courage to use your own reason!’—that is the motto of the Enlightenment” (Kant, 1995, p. 1). Clearly, for Kant, the central focus of the Enlightenment was liberating human reason and experience from the shackles of traditional authority and religious tutelage. For the Enlightenment philosophers, “premodern” life (as I will call it) was rife with superstition and mythical fancy that was holding back human advancement. The Enlightenment dream was that through the liberation of reason and experience, knowledge would progress, and with better knowledge would come advancement in human life through better control of the world.

Thus, the principal villains for Enlightenment modernism were religion and myth, and the principal heroes (who became objects of a veritable Western love affair) were rational, scientific, and technological understanding. By the late 19th and early 20th century, during the time when modern psychiatry was being organized and before the somewhat sobering effect of the two world wars, Enlightenment modernism was in high gear. Multiple advances in science, technology, and rational planning made it seem as if humans were on the verge of mastering the fundamental order of the universe. Caught up in the zeitgeist of the age, psychiatry was an enthusiastic participant in this modernist romance, and consequently, modern psychiatry eagerly came to valorize the ideals of Enlightenment reason. To make this claim clearer, let me start by outlining three prominent philosophic themes of modernism:

1. the quest for objective truth,
2. faith in method, and
3. a telos of progress and emancipation.

These themes of modernism have been prominent in psychiatry since its inception, and they continue to be central for today's "new psychiatry."

The Quest for Objective Truth

As a spiritual child of the Enlightenment, psychiatry attempts to "get it right." Psychiatry understands itself as "founded" on the Truth. Thus, for psychiatry, what counts as "good" knowledge is objectively True knowledge. When psychiatry creates a category like "schizophrenia" or a theory of causality like the "dopamine hypothesis," the idea is that these categories and theories represent the way the world is really structured independent of human subjective constructions. Granted, the categories and theories are

understood as hypotheses, but they are hypotheses of the way the world “really is.” They will change only if there is a better hypothesis. If there are two hypotheses, it is assumed that one will eventually be proved wrong. Inherent in this quest for objective truth is a belief in *universality*. In order to “get something right,” there must be a “right” to get. In other words, there can be only one Objective Truth, the Universal Truth. When psychiatry discovers the Truth about a condition, it is assumed to be true across all cultures and across all historical eras. As such, though the category of “schizophrenia” is only 100 years old, psychiatry assumes the condition has always been a part of human life. Also inherent in the belief in Universal Truth is a belief in the transparency of language. The language of psychiatric discourse is not understood as creating knowledge, or perception, or even substantially affecting the transmission of knowledge; rather, psychiatric discourse only reflects the world “as it is.” Thus, the language of psychiatric categories and knowledge formations, as I discussed in the first three chapters, is minimized in psychiatric discourse, because language is assumed to be an unproblematic medium for transmitting observed categories and reasoned theories.

Faith in Method

For psychiatry, as for the Enlightenment, the route to Objective Truth is the “scientific method.” True knowledge is knowledge that is obtained through the scientific method. Faith in the scientific method helps psychiatry determine “how to decide” whether knowledge is True—that is, whether knowledge actually matches up with the world rather than being an elaborate product of the researcher’s imagination. For psychiatry, as for the Enlightenment, there is minimal emphasis on the usefulness, beauty, ethics, or political value of knowledge. Legitimate knowledge for psychiatry is

independent of the context of discovery and is understood to be “value free.” As such, the only critical question that can be asked of knowledge becomes: “Is it True?” For the Enlightenment, knowledge is True only if it has been tested against the world through the scientific method. Only knowledge that is “verified” (later watered down to “not falsified”) through the scientific method is True knowledge. In psychiatry, this ideal has had a chilling effect on all “nonscience” knowledge. Patient judgments, family opinions, clinical wisdom, or case studies (much less knowledge from the humanities and the arts) are at best seen as hypotheses or conjectures that must pass through scientific method to be legitimated—even if these knowledges are difficult, or even impossible, to operationalize into a testable form. At worst, forms of psychiatric knowledge not subjected to the scientific method are simply dismissed as myth, superstition, or idle speculation. Thus, in psychiatry, as in the Enlightenment, tremendous faith is placed in the scientific method as a route to Objective Truth.

Telos of Progress and Emancipation

As with the Enlightenment philosophers, psychiatry’s overriding justifications for pursuing objective knowledge are progress and emancipation. Modern enlightened thinkers argue that by an ever-improving knowledge of the world, humans will have better control of that world and will be better able to free themselves from the constraints of nature. In the example of psychiatry, “false knowledge” and “myth” about human mental suffering can be abandoned as psychiatry moves toward establishing reliable, value-neutral truths about the objective world of mental illness. “True knowledge,” obtainable at last through the scientific method, will progressively accumulate and allow for increasing human liberation. In psychiatry, this telos of emancipation from mental

illness through progress is clearly operative in the constantly revised “new updates in neuropharmacology,” “new advances in the psychotherapy for resistant depression,” and in the ever-new revisions of the *Diagnostic and Statistical Manual*. Clearly the goal of psychiatric knowledge, like the goal of the Enlightenment, is progress, and the goal of progress is human emancipation.

These three themes of modernism (objective knowledge, faith in method, and telos of progress and emancipation) provide an unreflected background horizon for psychiatric discourse. To illustrate, let me review an example from a current psychiatric journal: *The Journal of Psychotherapy Practice and Research*. The journal describes itself inside its front cover as a “peer-reviewed interdisciplinary journal published quarterly by the American Psychiatric Press, Inc., and its aim is to *advance* the professional understanding of human behavior and to *enhance* the psychotherapeutic treatment of mental disorders” (italics added). The theme of progress (to “advance” and to “enhance”) is clearly prominent even in the journal’s self-description, but in a recent review article (with an associate editor of the journal as lead author), all the themes of modernism are elevated to a highly partisan shrill: “During the past 15 years we have made substantial *advances* in our understanding of psychotherapy research and our ability to conduct this research effectively [read: “scientifically”]” (Docherty & Streeter, 1993, p. 100; italics added). The article authors “review the *progress* in psychotherapy” (italics added) in order to “provide a useful framework for exploring areas requiring increased attention and research” (p. 100). The framework they adopt is proudly “*scientific*”—psychotherapy research needs a “scientific base,” a “science of psychopathology,” and a “science of psychotherapy.” Prior to the application of scientific

method, the authors claim that psychotherapy literature was “shockingly low” in “inter-rater reliability” and could never convince the “skeptical individual that a particular treatment approach has been adequately assessed” (p. 100). This created, Docherty and Streeter claim, a “demoralizing problem for individuals involved with the effort to develop a science of psychopathology” (p. 100). In other words, the conclusion with regard to psychotherapy for these new-psychiatry authors (trying to out-modernize already modernist psychoanalytic psychiatry) is that without appropriate faith in Scientific Method there is no Objective Truth, and without Objective Truth, there is no Progress toward human Emancipation.

A Postmodern Rewrite

Postmodernity may be defined, echoing our definition of modernity, as modes of intellectual formation or organization that emerged in the West from about the 1950s onward and that have rapidly become influential throughout the human sciences. As Flax explains, however, postmodern theories are “not a unified and homogeneous field” (Flax, 1990, p. 29). Thus, the term “postmodern” is often confusing because it is often used in multiple ways. The three most common are

1. “postmodern art, literature, or architecture”—which refers to creative works showing distinctive breaks from their modernist heritage, such as the pop-art work of Andy Warhol,
2. “postmodern culture”—which refers to the recent explosion in world cultures of mass media influence, global village cosmopolitanism, and transnational capitalism, and
3. “postmodern theory”—which refers primarily to recent continental “theory” critiques of Enlightenment philosophy and epistemology.

The focus in this chapter is on the latter. Theorists and philosophers grouped primarily under this third category of “postmodern,” such as Jean-François Lyotard, Roland Barthes, Jacques Derrida, Michel Foucault, and Richard Rorty, have been particularly adept at undermining the foundations of modernist knowledge. These are some of the same theorists I discussed in chapters 2 and 3, and, with the exception of Richard Rorty, all of these writers are representative of French poststructuralism.

However, Lyotard is the only one of these writers, again with the (sometimes) exception of Richard Rorty, who explicitly uses the term “postmodern,” and it is Lyotard’s work I will be using as the main touchstone for the sections of this chapter regarding postmodernism. His book *The Postmodern Condition: A Report on Knowledge* was perhaps the first, and clearly the most important, to put these multiple poststructural critiques of Western philosophy and epistemology in terms of “postmodernism.” Certainly Derrida, Foucault, and Rorty all put their concerns in a historical frame, but Lyotard, more than any other single author, maps the poststructuralist “collapse of certainty” and the “crisis of representation” into a general feature of modern Enlightenment thought (Lyotard, 1984). For Lyotard, the transition to a postmodern condition is marked by a crisis in the status of knowledge in Western societies. In a postmodern frame, God, nature, science, and humanism have all lost their legitimacy as sources of authenticity and truth. Lyotard’s postmodern theory relies on the poststructuralist theories of language I discussed in chapter 2 and, as such, echoes Derrida’s critique of the Enlightenment’s “logocentrism” and “metaphysics of presence” (Derrida, 1973). Like Derrida and other poststructuralists, Lyotard is highly sensitized to the role of language in shaping human beliefs and perceptions, and he emphasizes how

language works as a system of relations rather than a transparent representation. As such, for Lyotard, knowledge is never universal. Knowledge is always partial, limited, and very much shaped by the systems of linguistic categories and power relations from which the world is perceived.

However, Lyotard takes much care to articulate that linguistic power/knowledge systems are not everything nor are they in any way complete. In other words, Lyotard works to counter an overly simplistic reduction of poststructuralist insights into a strawperson position of “nothing but language.” In one of his early works, *Discourse/Figure*, Lyotard takes pains to outline several aspects of psychic life and of the world that are distinct from the linguistic. All of these distinctions from what Lyotard calls “discourse” make up what he calls the “figural” (Readings, 1991, pp. 3–4).¹ His examples of the “figural” include “visual,” “event,” and “unconscious.” Bill Readings defines Lyotard’s use of the figural this way:

The figural is an unspeakable other necessarily *at work* within and against *discourse*, disrupting the rule of representation. It is not opposed to discourse, but is the point at which the oppositions by which discourse works are opened to a radical heterogeneity or *singularity*. As such, the figural is the resistant or irreconcilable trace of a space or time that is radically incommensurable with that of discursive meaning. (Readings, 1991, p. xxxi)

Lyotard’s motivation is similar to mine in turning to Peirce in chapter 2 (avoiding the charge of “nothing but language”), but Lyotard takes his initial inspiration for fine tuning poststructuralism from phenomenology rather than alternative theories of the sign, which I used. Thus, Lyotard’s first distinction, the visible, comes from Maurice Merleau-

¹ Note that Lyotard’s use of the term “discourse” is closer to “linguistic” as I’ve been using it in this section than to Foucault’s concept of discourse discussed in the last chapter.

Ponty's phenomenology of the visible. Like Merleau-Ponty, Lyotard articulates the critical value of making a distinction between the linguistic and the visible. Using this distinction, Lyotard highlights how mental life is not solely composed of linguistic systems; it is also composed of visual images. These visual images, for Lyotard, are not reducible or directly comparable to linguistic forms, and, as such, visual phenomena introduce a radical incommensurability between the linguistic and the visual.

Along similar lines, Lyotard argues for other distinctions from the linguistic. The "event," for Lyotard is an occurrence in the physical or mental world that is "figural" because it is outside and distinct from any referential attempt to capture it. Again, Lyotard introduces incommensurability. This time it is the radical incommensurability between the world and human representation and perception. Thus, the event disrupts any preexisting referential frame from which it could be understood. Also, events have a radical singularity distinct from generalities and abstractions that can be contained within a linguistic form. For Lyotard, to demarcate events from representation or from the visible is to remain phenomenologically humble and open to the wonder and appreciation that something happens (rather than nothing and often rather than the expected) instead of closing down awareness to only the various explanatory structures regarding what happens and how it happens. In Lyotard's terms, this is a distinction between "it happens" and "what is happening." It is the paradoxical capacity to be both surprised and not surprised by the wonder and unexpectedness of the world.

In addition to the visual and the event, Lyotard uses Freud's theory of the unconscious to make another "figural" distinction from the linguistic. Lyotard identifies linguistic structures with Freud's articulation of conscious "secondary process" and the

figural with Freud's unconscious "primary process" (Lyotard, 1989, p. 23). Again, Lyotard is concerned to highlight phenomena incommensurable with (and disruptive to) linguistic structures. Freud's primary process/secondary process differentiation is ideal for this effort. For Freud, primary cognitive/emotional process, in contrast with secondary process, is polymorphously perverse in its affectual cathexis, and it is cognitively indifferent to linear time, rules of noncontradiction, and "reality" (as defined by secondary process). Echoing Freud, Lyotard compares the unconscious, royally approached through a dream, with a rebus—which is simultaneously composed of both visual and linguistic elements. For Lyotard, there can be no complete reduction of this incommensurable complex of visual and linguistic intermingling to language alone. The primary process dream is outside the linguistic. In addition, the polymorphous perversity of the unconscious desire introduces havoc to the secondary process ideal of definitive goals.

For example, the unconscious paradoxically desires the all and the singular simultaneously and completely. But to desire the all completely is to not desire the singular completely. Thus, the unconscious desires and does not desire all and not all at the same time. But, since there is no negation in the unconscious, not desiring does not cancel desiring. Thus, for Freud, to "not desire Mother" is, of course, to desire Mother. Accordingly, for Lyotard, conscious secondary process goals are too linear and noncontradictory to ever fulfill unconscious desires. For Lyotard, unlike for Lacan, these "logical scandals" and unattainable desires of unconscious primary process do not function like a language. They are radically other to language. In other words, Lyotard returns to Freud, without making the Lacanian move of articulating the unconscious as

“structured like a language.” Lyotard argues instead that the unconscious is radically different from language. Or as Lyotard says, “desire does not speak, [desire] does violence to the order of utterance” (Lyotard, 1989, p. 19). And later in the same essay, he puts it this way, “The dream-work is not a language, it is the effect on language of the force exerted by the figural (as image or as form). This force breaks the law” (p. 51).

I linger over Lyotard’s various versions of the “figural” because I find it useful for augmenting and developing Barthes’s use of the term “pluri-dimensional,” which I discussed in chapter 2 and which is essential for my retheorized psychiatry. To recap, Barthes argues, borrowing from Lacan, that the real is a “pluri-dimensional order” that “cannot be made to coincide with a unidimensional order (language)” (Barthes, 1982, p. 465). Lyotard’s “figural” underscores three important features of a pluri-dimensional real that do not coincide with language. First, all linguistic structures are fundamentally inadequate to the complexity and singularity of the visual, of events, and of primary process. Any linguistic structure is always open to disruption from the figural. Second, alternative linguistic orders will capture aspects of the pluri-dimensional order of the real in alternative ways. As such, they are open to figural disruption in alternative ways, and they have alternative advantages and disadvantages in relation to each other. As Readings points out, however, it is essential to note that the figural is not in opposition to the linguistic. The figural does not imply the impossibility of any linguistic connection with the figural. Rather, the figural evokes the way in which language is open to the world’s radical heterogeneity and singularity. Thus, language is to open to connect with the world in alternative ways. Third, because language can change, the relations between the figural

and the linguistic can also change. Or as Barthes so eloquently puts it, “‘to change language’ . . . is a concomitant of ‘to change the world’” (Barthes, 1982, p. 466).

Lyotard’s early use of the discourse/figure distinction is also helpful in understanding his later use of the distinction between “modern” and “postmodern.” The modern, for Lyotard, is an approach to knowledge and to the world that attempts to erase the figural. Through a “metadiscourse”—such as scientific method or rational management—modern discourses legitimate themselves and, most importantly, delegitimize other discourses that do not invoke the same metadiscourse. The postmodern, for Lyotard, is any approach to knowledge and the world that includes the figural and is skeptical of any discourse that erases the figural. Postmodern knowledge is knowledge without guarantees and without the certainty of metadiscourse legitimization.

However, Lyotard, in contrast to many recent Anglo-American scholars (and my definitions given above), explicitly deemphasizes temporal periodizing of the distinction between modern and postmodern. For Lyotard, any discourse that includes the figural, regardless of its temporal location, is considered postmodern: “Freud, Duchamp, Bohr, Gertrude Stein, but before them Rabelais and Sterne, are postmoderns in that they stress paradoxes, which always attest the incommensurability of which I’m speaking” (Lyotard 1993, p28) Clearly, the modern has always had counter-discourses that undermine its certainty, and, just as clearly, there are many discourses in the current “postmodern” moment that are more modern than postmodern. As such, Lyotard’s caution against periodizing is useful. However, Lyotard is sometimes at odds with his refusal to periodize. For example, when he says “the obsolescence of the metanarrative apparatus of legitimation corresponds most notably [to] the crisis in metaphysical philosophy and of

the university institutions which in the past relied on it” (Lyotard, 1984, p. xxiv), he implies a temporal frame which is similar to the more periodizing uses of the term “postmodern” in Anglo-American theory. Perhaps the easiest way to work with this tension is to say that there is nothing necessarily current about postmodern sensibilities, however, recent theory in the academy has raised these sensibilities to a particularly strong pitch.

The most common charge against Lyotard’s postmodern theory made by “promodern” writers, such as Jürgen Habermas, is that without some kind of criteria for Truth, humanity will necessarily sink into the morass of “anything goes” relativity (Habermas, 1995). In other words, for promodern writers like Habermas, without the foundation of objective standards, there will be no way to refute dictators, terrorists, criminals, charlatans, and neoconservatives. The “anything goes” argument against postmodernism, however, turns out to be a strawperson argument that critiques postmodern intellectual thought by holding it to modern ideals and values from which it has explicitly separated itself. As I have discussed in earlier chapters, the poststructuralist “crisis of representation” that inspired Lyotard’s postmodernism does not mean “anything goes;” rather, it means that there is no unmediated representation, no direct access, no possibility of a view from nowhere. All representation is necessarily representation through language. This does not entail relativity (anything goes), however, because it is still as possible as ever to compare, critique, and judge beliefs (represented in language) with other beliefs (also represented in language); it is just not possible for promodern beliefs to “trump” others by claiming that promodern beliefs “match up with the world as it really is” and alternative beliefs do not. Thus, postmodern theory attempts to dislodge

the Enlightenment obsession with objective truth and encourage an acceptance of knowledge as never neutral or true. Knowledge is always bound up with human interests and power relations. Accordingly, postmodernists argue that, instead of being constantly preoccupied with the truth status of knowledge, we should refocus our attention on the uses and abuses of knowledge.

Because postmodern writers have dedicated most of their efforts to critiquing and deconstructing the Enlightenment ideals of reason and empiricism, Lyotard argues that the best way to understand postmodern efforts is as a process of “rewriting modernity” (Lyotard, 1988, p. 24). The “rewriting” trope helps Lyotard avoid being trapped in a periodizing rhetoric: “any periodizing of cultural history in terms of ‘pre-’ and ‘post-,’ before and after, . . . leaves unquestioned the position of the ‘now’, of the present from which one is supposed to be able to achieve a legitimate perspective on a chronological succession” (Lyotard, 1988, p. 24). For Lyotard, periodizing rhetoric is too characteristic of modernistic obsessions with knowing and truth—in this case with knowing the truth of history and historical breaks. He prefers to link “rewriting” with Freud’s concept of “working through” and, accordingly, contrasts rewriting (as a kind of working through) with “repetition” and “remembering.” Lyotard’s invocation of Freud links him closely with my quote in chapter 1 from Zygmunt Bauman. Lyotard, like Bauman, conceptualizes postmodernity as “modernity coming of age . . . psychoanalyzing itself . . . coming to terms with its own impossibility . . . [and] consciously discarding what it was unconsciously doing” (Bauman, 1990, p. 272). In other words, postmodern rewriting of modernism is a working through of modernism.

To clarify the link between postmodern rewriting and Freud, Lyotard returns to Freud's short but critical article: "Remembering, Repeating, and Working Through" (Freud, 1958, p. 147). Repetition, in Lyotard's reading of Freud, "is the result of the 'set-up' which allows the unconscious desire to be fulfilled and which organizes the whole existence of the subject like a drama" (Lyotard, 1988, p. 26). Relating this to psychiatry and postmodern theory, we might say that the (unthematized) themes of modernism are the unconscious "set-up" that is leading psychiatry to compulsively repeat, even through its most recent change into a "new psychiatry," its prior problems and limitations (such as those outlined by Pelligrino at the start of this chapter). Remembering, by contrast, is the attempt to "bring to consciousness, to discover the 'reason' or the 'cause' of the trouble" (p. 27). This kind of "remembering" is in many ways what I have done by outlining the themes of modernity. In that section, I articulated a remembered cause of psychiatry's problems in its modernist set-up.

For Lyotard, though, it is not enough to "identify the crimes, sins, calamities engendered by the modern set-up" (Lyotard, 1988, p. 27). One must also "work through" these insights (obtained through remembering and thematizing) in a *forward* as well as a backward direction. Thus, working through is a "double gesture"—backward *and* forward (p. 30). Working through constantly requires applying the insights of analysis to the present and the future. Of course, the great difficulty (and indeed interminability) of following this concept is that it requires changing not only understandings of the past but also choices and actions in the present and the future.

I do not pretend that my articulation of the themes of modernity in psychiatry are necessary themes, I only suggest they are useful themes that could yield good

consequences if they were adopted and applied. Along this line, Lyotard argues, against the grain of most psychoanalysts, that with the concept of “working through” Freud lets go of a realist understanding of the process of analysis. Lyotard suggests that, while “remembering” emphasizes what truly happened in the past, “working through” is a free-floating concept that pragmatically emphasizes the consequences of remembering or rethematizing for the future. Sidestepping whether Freud was as comfortable in this pragmatist frame as Lyotard suggests, I will stay with Lyotard’s rather “postmodern Freud” to move from thematizing modernity in the last section to working through modernity (emphasizing the consequences of this rewriting as I go along).

Thus, the question arises: How would a postmodern rewrite change the themes of Enlightenment modernism, which are dominant in psychiatry today? What new rewrite would this suggest for psychiatry, and what would be the consequences of this rewrite? Working with (and through) the three themes already discussed, I suggest that postmodernism shifts modernism

1. from a quest for objective truth to a *crisis in representation*,
2. from faith in method to an *incredulity toward metanarratives*,
3. from a telos of progress and emancipation to a *telos of struggle and compromise*.

By rewriting these themes in a postmodern frame, and taking steps toward working through their consequences, I initiate my basic manifesto for a theory-friendly postmodern psychiatry.

Quest for Objective Truth Becomes Crisis in Representation

If psychiatrists practiced from within the worldview of a postmodern “crisis in representation,” they would be much less obsessed with “getting it right.” Psychiatry would understand its knowledges not as universal truths, but as useful heuristics, necessarily formulated through the constraints of a nontransparent language and simultaneously essential to the process of inquiry and intelligibility. From a postmodern perspective, psychiatric knowledge (always mediated through nontransparent language) is understood as, to use Derrida’s term, *sous rature* or “under erasure” (translators’ preface, Derrida, 1974, p. xiv). To place a word under erasure is to write the word, cross it out, and then print both the word and the deletion. Because the word is necessarily inaccurate, it is crossed out. However, since the word (or some other inaccurate word) is needed for articulation and communication, it is left legible through the cross out. By “necessarily inaccurate,” I refer to an inherent incompleteness and instability in representation. In Lyotard’s terms, all representation is necessarily open to figural disruption. As such, words and representations, from within a postmodern “crisis of representation,” are as inaccurate as they are necessary. Similarly, psychiatric words and representations are not True; they are at best evocations of the real. Judging these psychiatric words, therefore, becomes a question not only of reference but also of consequences.

For an example, consider some particularly consequential psychiatric words and representations: diagnostic categories. As I discussed in chapter 2, to be intelligible, words and representations (including diagnostic categories) divide the world through relational divisions. The most basic example in psychiatric categories is “mental health”

versus “mental illness.” Once an initial binary division like this is made, fine-tuning the categories occurs by further dividing the divisions—for example, schizophrenia versus manic depression, unipolar versus bipolar, and melancholia versus dysthymia. These divisions are always to some degree arbitrary and inaccurate, and they always necessarily constrain further meaning making along the lines of the original divisions. In addition, these distinctions (mental health versus mental illness, etc.) are rarely, if ever, neutral. They exist in a hierarchy of relations. Health versus illness is not only a “description” but also a value preference. These relational hierarchies echo, crystallize, reinforce, and perform other social hierarchies, prejudices, and power relations present in the culture—for example, man versus woman, white versus black, straight versus gay, and upper class versus lower class. Accordingly, these contextual social distinctions and hierarchies spill over into and become part of the very meaning of the “mental health” versus “mental illness” distinction. Thus, it is not surprising that most psychiatrists (“mentally healthy” by implication) are upper-middle-class white heterosexual males and most patients (“mentally ill” by definition) are not.

I must emphasize again, however, that concepts and categories created through binary divisions are not only inaccurate and constraining; they are *also* evocative and enabling. Though language never mirrors the world, it does partially “invoke rather than present” the world, and it is necessary because there is no possibility of stepping outside of language (Flax, 1990, p. 196). As a result, postmodernists recommend that meaning-making divisions of linguistic terms be understood and used “under erasure,” which leaves language users more humble and flexible about the ultimate value and worth of any particular binary division.

Another way to understand the difference between a modern and postmodern worldview or mindset is to highlight the role of principles of *noncontradiction* and *clarity* in modernism. In a modernist logic, noncontradiction and clarity are necessary for “objective truth,” because neither contradictory nor muddled representations can be compared with “the world.” Unfortunately, using these principles of clarity and noncontradiction, modernism often limits itself to only one correlative conjunction: “either/or.” Thus, there is a tendency within Enlightenment thought for the Truth to fall on *either* one side of a binary *or* the other. *Either* one is mentally ill *or* mentally healthy. After all, for modernist noncontradictory and clarity-seeking logics there is only one way the world can be. To be “both” mentally ill and mentally healthy, for modernists, would be contradictory and confused. Postmodern logic, however, is less concerned about contradiction and clarity (sometimes maddeningly so) and, as such, embraces the use of multiple correlative conjunctions—instead of recognizing only “either/or,” it embraces the use of “and/also” and “neither/nor.” Thus, to use a term like “mental illness” under the postmodern logic of erasure and multiple correlative conjunctions is to recognize that while there might be many strategic advantages to organizing the world through the term “mental illness,” there might also be many disadvantages. If so, other organizing concepts should be available for consideration.

Of course, representational terms do not exist in isolation; they are part of a whole network of other terms that, working together, form a perpetually shifting scaffold for perception, thought, desire, and action. As I discussed in the previous chapter, Foucault highlights the interconnection of representational terms with each other and with human perception, power relations, and action through his notion of “discourse.” Lyotard’s

postmodern philosophy makes a similar move by drawing extensively on Ludwig Wittgenstein's concept of a "language game" (Lyotard, 1984, p. 10). A "language game" for Wittgenstein, like a "discourse" for Foucault, is more than a set of linguistic representations; it is a complex amalgam of language, being, and action. Wittgenstein uses the notion of a "game," such as chess or "ring-a-ring-a-roses," to evoke the inseparable mixture of linguistic representation and life activities. Wittgenstein puts it succinctly: a "language game . . . is the whole, consisting of language and the actions into which it is woven" (Wittgenstein, 1958, p. 5). The importance of this for my discussion of psychiatric categories is that to change representational terms in psychiatry, say from "mentally ill" to "social critic" or "revolutionary," is to change language games as well. Each linguistic game sets up and shapes the phenomena it evokes, and it simultaneously guides action with regard to that phenomenal evocation. And each game connects terms and actions through a different set of relations. Thus, to use a language of either "mentally ill" or "social rebel" is to play different, and largely incommensurable, games.

Within a postmodern logic, however, clinicians would have no need to limit correlative conjunctions to "either/or" and no need to obsess with "getting it right." Rather, a postmodern perspective would emphasize that mental phenomena, like everything else, are richly complex and, as I have been arguing, pluri-dimensional. From a postmodern perspective, any linguistic approach, which means any human approach, is enabling and constraining: it simultaneously creates possibilities and closes off alternatives. For postmoderns, a person does not have to be *either* "mentally ill" *or* a "rebel," she can be *both* ("and/also") *or neither* ("neither/nor"), depending on the context and the goals of the linguistic construction.

Let me add, however, that I suspect Lyotard himself, were he still alive, might be uncomfortable with aspects of this last paragraph because it implies the possibility of human choice and agency among language games—of which Lyotard would be quite skeptical: “these are games that we can enter into but not to play them; they are games that make us into their players” (Lyotard, 1985, p. 51). To rest with this conclusion, though, is to be trapped in the increasingly tired binary between human “agency” and social/linguistic “structure.” I see no necessary reason, within a postmodern logic, for adopting an either/or relation to that binary. As Lyotard himself points out, circulating multiple language games creates simultaneous multiple subjectivities: “we know therefore that we are ourselves several beings (by ‘beings’ is meant here proper names that are positioned on the slots of the pragmatics of each of these games)” (Lyotard, 1985, p. 51). In contrast to being forced and played by a single language game into a single subjectivity, recent “postmodern psychoanalysis” has argued, persuasively I believe, for the possibility that there is some degree of freedom within these multiple subjectivities and that one of the possible goals of therapy can be to increase our autonomy to make choices between these language games that are simultaneously playing us (see, for example, Benjamin, 1998). Clearly, one cannot step out of language, but there is some possibility of stepping over from one language game to another.

Faith in Method Becomes Incredulity Toward Metanarratives

In a postmodern horizon, where categories and theories are always simultaneously enabling and constraining, there is still the question of “how to decide” between alternative conceptual possibilities. Psychiatry, like modernism more generally, answered this question largely through its metanarrative faith in science and scientific method.

Postmodernism, on the other hand, consistently critiques scientific method for attempting or claiming to be a neutral or value-free arbitrator between conceptual worldviews. As Rorty explains, “There are no criteria [including scientific criteria] that we have not created in the course of creating a practice, no standard of rationality that is not an appeal to such a criterion, no rigorous argumentation that is not obedience to our own conventions” (Rorty, 1987, p. 60). Lyotard similarly points to an inevitable hermeneutic circularity from which even scientific reasoning cannot escape: in the scientific solution

what I say is true because I prove that it is—but what proof is there that my proof is true . . . or more generally “Who decides the conditions of truth?” It is recognized that the conditions of truth, in other words, the rules of the game of science, are immanent in that game, that they can only be established within the bonds of a debate that is already scientific in nature, and that there is no other proof that the rules are good than the consensus extended to them by the experts. (1984, pp. 24, 29)

Thus, from a postmodern perspective, promodern science itself is a worldview, and “scientific method” functions in a modernist discourse as both a circular hermeneutic “metanarrative” and a condition of truth.

Putting scientific metanarrative thinking in a more general frame, we can say that when a promodern or premodern discourse puts faith in a metanarrative, questions of “how to decide” are answered by applying the Method of the metanarrative—what would “reason dictate,” what does “scientific method conclude,” what does the “Bible say?” To follow the metanarrative is to follow the rules of the game. To be outside the rules of the game is to be out of play. Thus, somewhat paradoxically from the perspective of spatial metaphors, faith in “meta”narrative functions by creating a foundation for belief, and promoderns and premoderns both argue vociferously that the foundational metanarrative

legitimizes their discourses. However, as Lyotard puts it, postmodern discourse is “incredulous toward metanarratives,” and, as such, postmodernism is an antifoundational discourse (Lyotard, 1984, p. xxiv).

Thus, for postmoderns, without modernism’s foundation of a scientific or rationalistic metanarrative, the question of how to decide must be answered through a case-by-case *judgment* that considers a complex interweaving of *multiple* aspects of knowledge, aspects that include the useful, aesthetic, ethical, and political consequences of knowledge (Lyotard, 1996, p. 81). Without a metanarrative court of appeal, different people, or even the same people at different times, will make different judgments by weighing these criteria differently. Thus, for a postmodern psychiatry, the goal of inquiry must not be to insist on consensus but to appreciate divergence (p. 95). There must be room and appreciation for a diversity of “legitimate” knowledge structures that are decided among differing mixtures of language games and differing consequential aspects of knowledge. Mushy and indefinite, humble and insecure, postmodern knowledge judgments have the advantage over premodern or promodern knowledge that they avoid (in theory, if not always in practice) the hubris and imperialistic control of certainty.

The advantage of humility, however, does not create for postmodernism a new metanarrative trump card, because, though there are many advantages to humility and uncertainty, these are not necessarily greater than the advantages of confidence and certainty. Postmodern theory is not utopian. Postmodern discourse itself exists within language and is intelligible through the same linguistic binaries that it attempts to theorize. For example, the terms “certainty” and “humility” that I have been using to characterize modernism and postmodernism are also binaries. From a postmodern logic,

reflexively directed back toward its own discourse, however, certainty and humility do not exist in an “either/or” relation. Knowledge makers’ judgments (sometimes conscious, but usually not) to privilege (and therefore choose) “certainty” or “humility” depend on the details of case-by-case situations. For some people, or at some times, it is best to proceed with certainty while also being humble. For other people, or at other times, it is best to be unambiguously certain or unambiguously humble. Sometimes, or for some people, it is better not to reflect on the distinction at all. The same is true for the distinction between modernism and postmodernism. Neither has a definitive advantage. In fact, from my perspective, postmodernism does not exclude modernism (or even premodernism); it only opens up the possibility of a wider appreciation of the complexities of modernist knowledge. Thus, in a psychiatric context, for example, there can be no external or foundational appeal to postmodernist psychiatry over modernist psychiatry other than the internal appeal to create a psychiatric world that postmodern logics can create and that modernist logics cannot.

Telos of Progress and Emancipation Becomes Telos of Struggle and Compromise

The last (and surprisingly most difficult) critique for promoderns to accept is the postmodern critique of Progress and Emancipation. I say “surprising” because, in many ways, this critique is the most obvious. The usual modernist indicators of Progress and Emancipation—increased control over nature through technology, increased political freedoms through liberal governments, and increased liberation from superstition and tutelage—are easily countered by equally modernist, only opposite, *regressions*—increased environmental pollution and destruction of world resources, increased threat of global catastrophe (through world financial collapse, nuclear power, unleashed

biohazards, or deadly new infectious agents), increased disciplining of human life by “rational” human organization, and increased sensations of alienation, fragmentation, and purposelessness. From a postmodern perspective, it is not surprising that the modernist project has brought as much regress as it has progress. Knowledge, and the particular ways of life organized by knowledge, always involves trade-offs. There cannot be progress without loss, emancipation without constraints. Borrowing from the anthropologic notion of “psychic unity,” postmodern theory understands different language games and different ways of life as equally complex (Rorty, 1982, p. 66; Geertz, 1973, p. 19). Each creates meaning in ways that always contain simultaneous gains and losses. Antiutopian in this sense, postmodernism replaces the telos of progress with the telos of struggle and compromise. Humans struggle and compromise with the world (always making trade-offs between gains and losses of alternative worldviews), and humans struggle and compromise with each other (always negotiating competing worldviews that are constantly forced on the less powerful by the more powerful).

For example, this “trade off” dimension of change seems obvious in any fair reading of the new psychiatry’s relation to the psychoanalytic psychiatry that came before. The standing joke among psychiatrists is that psychiatry has moved from a “brainless psychiatry” of psychoanalysis to the “mindless psychiatry” of neuroscience and *DSM-III*. This joke pretty much says it all with regard to a telos of struggle and compromise. The move from one paradigm to the next is not pure progress. The new psychiatry made progress along the lines of greater capacity for using neuroscience conceptualizations and social science operational methods. This increased capacity, though, was a simultaneous loss of capacity (regress) to use the psychoanalytic

phenomenal tools to articulate the complexity of the mental dynamics and the importance of the therapeutic encounter between helper and helped. Thus, there have been trade-offs and compromises between these different psychiatric language games. Neither side can claim to have the absolute advantage over the other. One has advantages along certain lines, while the other has advantages along alternative lines. Each language game struggles with the world, and the players of one game (who Lyotard reminds us are themselves played by the game they have entered) are also in a struggle with the players of the other.

Unfortunately, much of the struggle between psychiatric players is a power struggle that leaves them with little incentive to negotiate. Even if they should desire to negotiate, however, these two sets of players would have great difficulty communicating with each other, because they are working within such different language games. Lyotard introduces an important distinction between what he calls a “differend” and a “litigation” to help articulate this phenomenon. He says,

As distinguished from a litigation, a *differend* would be the case of conflict, between (at least) two parties, that cannot be equitably resolved for lack of a rule of judgment applicable to both arguments. One sides’ legitimacy does not imply the other’s lack of legitimacy. However, applying a single rule of judgment to both in order to settle their differend as though it were merely a litigation would wrong (at least) one of them (and both of them if neither side admits this rule).
(Lyotard, 1988, p. xi)

To sharpen this distinction, Lyotard adds the further distinction between a “damage” and a “wrong:” “Damages result from an injury which is inflicted upon the rules of genre of discourse but which is reparable according to those rules. A wrong results from the fact that the rules of genre of discourse by which one judges are not those of the judged genre

or genres of discourse” (p. xi). Thus, for Lyotard, “damage” is what occurs in a conflict or clash between two parties that can be litigated and therefore addressed and compensated. Wrongs, on the other hand, which occur in a clash between parties of a differend, must remain mute and uncompensatable because there is no language of litigation between the parties.

Using Lyotard’s postmodern terminology in a psychiatric context, in the struggle between brainless psychiatry and mindless psychiatry, the two discourses (and their players) simultaneously wrong each other. Both have their own criteria of legitimacy, but there is no single rule of judgment applicable to both approaches. Therefore, there is no “court of appeal” for litigating the struggle between them. Lyotard argues that the task for differends is not to insist on or force them into a court that is bound to fail one or both sides. Rather, the task is to *witness* the differend and to build structures of tolerance for differends. For Lyotard, differends are not the exception but the rule. This does not mean that language games never shift or that yesterday’s differends cannot become tomorrow’s litigants. Incommensurability between language games is not absolute. Compromise is possible and, as I have said, is a fundamental telos of postmodern logic. However, resolving one differend through a shift in discursive practices often only creates another one somewhere else. Thus, compromise and struggle coexist, and there will likely always be differends in psychiatry that are unlitigatable and must struggle with each other. Rather than fight this phenomenon, Lyotard suggests that we expect it and prepare for it. Unfortunately, from the point of view of many, if psychiatry were to follow this seemingly simple postmodern logic, as I will be arguing in the last chapter, it would result in dramatic changes in the current organization of psychiatric structures.

Some Potential Consequences of Postmodern Thinking for Psychiatry

To conclude this chapter, let me speculate and imagine how psychiatry would change if it embraced a postmodern understanding of itself similar to the one outlined above. Although I will state my speculations in a declarative tone (this chapter is a “manifesto” after all), these speculations are not meant to be necessary consequences of postmodern approaches to psychiatry—they are only an articulation of some possibilities. I do believe, though, that they are possibilities worth struggling toward, developing further, and building coalitions to support.

With that said, I will start my considerations of postmodern consequences by discussing how psychiatry’s knowledge base would change. From a postmodern perspective, it seems to me, psychiatry would be an interdisciplinary “human studies.” In a postmodern context, Lyotard’s “incredulity toward metanarratives” weakens psychiatrists’ obsession with Truth and their faith in science as the *only reliable* Method for knowledge. Once psychiatry’s idealization of scientific method is broken, there remains no reason arbitrarily to limit psychiatric knowledge to scientific knowledge. In a postmodern psychiatry, the entire university (including the arts and humanities and not just the sciences) would be available for psychiatric research. Opening psychiatric knowledges to other branches of academe would bring a wide array of additional methods, beyond science, to the goal of psychiatric knowledge production. Furthermore, additional topics that were considered unapproachable under a scientific “regime” (except through subjective speculation or conjecture)—like the identity of psychiatrists, the experience of mental illness, the dilemmas of clinical uncertainty, the effect of power differentials in the clinical setting, the role of cultural context in clinician and patient

perspectives, and the place of psychiatry within larger social and political trends—all become available to be considered, theorized, and critiqued with the tools of the university as a whole. As a postmodern human studies, psychiatry would seek help with the complexities of clinical interpretation from literary theory, with creating renditions of human experience from the arts and qualitative social sciences, with understanding the historical and philosophical contexts of practice from philosophy and humanities, and with multicultural issues and politics from women's studies, disability studies, Africana studies, international studies, gay and lesbian studies, postcolonial studies, and cultural studies programs.

There is nothing outrageous about this suggestion. It amounts to little more than taking George Engel's modernist "biopsychosocial" clinical healthcare model seriously (Engel, 1977). For Engel, beyond the biosciences, clinical practice must include knowledge and appreciation of the psychological and social domains. Also, in some ways, postmodern theory only articulates and theorizes trends already happening in modernist "preclinical" medical school curriculums—which are rapidly moving away from a purely science-based curriculum toward a more practice-based curriculum. Even from these modernist perspectives, psychiatry must expand its knowledge base into the humanities, arts, and qualitative social sciences if it is to take the psychosocial context of illness and clinical practice seriously. A postmodern perspective intervenes primarily as it disrupts the necessity of making these and other disciplinary divisions so rigid in the first place. Postmodernism helps psychiatry loosen itself and opens the way to a more inclusive knowledge base by undermining the need for a blind, defensive, and dogmatic adherence to the ideology of modernism and a fetishized preference for science.

Rather than appealing to a scientific metanarrative for legitimization, postmodern psychiatry would appeal to the wisdom of the practice community. In this way, postmodern psychiatrists would propagate knowledge in ways similar to bioethics and psychotherapy. Though not usually associated with postmodernism (and often still rhetorically associated with Universality and Objectivity), these discourses are similar to postmodern discourse in that their main legitimacy appeal is not to scientific method, but to what Habermas might call “the force of the better argument” (Habermas, 1984, McCarthy, 1978, p. 285). In a postmodern psychiatry, knowledge would still be accessed and propagated through journals, training institutions, and continuing medical education—the main difference would be that what counts as relevant and useful knowledge for psychiatry would be greatly expanded. Of course, not all knowledge would be considered useful and relevant. Journal editors and psychiatric educators would still make judgments and selections—the difference would be that these selections would be based on judgments of coherence, correspondence, and consequences, rather than scientific method alone.

However, it would still be crucial to remember that, from a postmodern perspective, knowledge and power are never separable and power issues motivate all knowledge. Thus, postmodern psychiatry would be wary about the specifics and particulars of the power interests involved in any knowledge selection. For many postmoderns, the only “force” of a better argument is the force of power relations. As such, for postmodern psychiatry, a major concern would be not only *how* or *what* knowledge selections are made but, even more importantly, *who* is making the selections? Postmodern psychiatry, owing to Foucault’s concepts of power and discourse

that I discussed in the last chapter, would understand that knowledge is always also power/knowledge and would understand that what is accepted as knowledge is always also bound up with the interests of who gets to speak (Foucault, 1980). As such, any psychiatric knowledge base that excluded patients' perspectives would be suspect, and therefore postmodern psychiatric knowledge would be created as much by "patients" as it was by "clinicians." This opens the door to a reconsideration not only of the content of psychiatric knowledge but also of the process of psychiatric knowledge making. I will have much more to say about the infrastructural processes and human relations of psychiatric knowledge production in the final chapter.

Another consequence of postmodern thinking in psychiatry is that psychiatry would move beyond a modernist utopian telos of Progress and Emancipation by deconstructing the current grand clinical telos of Cure and Health. In current psychiatric prioritizing, the telos of Cure results in spending resources on a better world later, rather than on living in this world now. Researchers and funding agencies designed to help the suffering paradoxically end up investing more resources in "scientific" causal relations than they do in people's needs. Obviously, the manifest goal of modernist searches for cures is to help people. As it turns out, however, by following modernist metanarratives, helping people becomes a side benefit rather than a direct priority. For example, how many millions of research dollars are spent on curing schizophrenia someday compared with research on coping with schizophrenia today? Modernist psychiatry believes that Progress and Emancipation for people with schizophrenia will only come through an understanding of the Truth of their illness. But this truth grid is only one approach to schizophrenia. Overemphasizing the truth leaves out the politics, the ethics, the

aesthetics, and the experiences (both painful and pleasurable) of schizophrenia. All of these other aspects of schizophrenia also directly influence the impact of “schizophrenia.” Thus, postmodern psychiatry, based on a telos of struggle and compromise, would shift the clinical goal of curing to one of coping. By *struggle*, I mean a struggle with various social forces to do the right thing for the suffering. By *compromise*, I mean a compromise with the ideal of Cure and a greater appreciation of the need for grief in the face of illness and loss.

Similarly, postmodern psychiatry would deconstruct the related concept of Health, and the sharp distinction between mental “health” and mental “illness.” In a postmodern logic, both patients and clinicians would be seen as always and inescapably an interwoven mixture of both (and neither) mental health and illness. As such, the goal of the clinical interaction would be “living with,” “adjusting to,” “muddling through,” and “coming to peace with” as much as it would be “curing.” Eradication of illness is impossible for postmoderns who assume it to always already be there.

In addition, postmodern psychiatry would be less an “expert” psychiatry and more of a “service” psychiatry. Postmodern psychiatric “servicepeople” would be more comfortable with a middle-class wage and more at ease with equalizing power differentials within the treatment setting. With power differentials closer to equal and with a postmodern telos of coping, psychiatric categories and theories of mental illness would be dereified. Thus, postmodern psychiatry would find it easier to take seriously patient models for suffering and would find it easier to work within alternative strategies for clinical improvement. In addition, postmodern psychiatry would lessen the spirit of seriousness so evident in the clinical world—a spirit that derives primarily from the huge

chasm created between binaries of health and illness. If people are always already both healthy and ill, the fall from health to illness is not so serious. Furthermore, if struggle and compromise are “as good as it gets,” that leaves more room for irony, play, and pleasure along the path of service to others and to oneself.

If psychiatrists were postmodern servicepersons, rather than modernist experts, the microgoals of the clinical interaction and the macro legitimacy of psychiatry as a profession would depend more on human values than on the metanarratives of scientific outcome studies. At the micro level, postmodern psychiatry’s “incredulity toward metanarratives” would advocate for an autonomy-based practice rather than a beneficence-based practice. Let me explain. In an autonomy-based practice, psychiatry would spend less time doing metanarrative treatment “outcome” studies to determine which treatment is beneficently “best” or “legitimate” and more time articulating and exploring the treatment desires and goals of their clients. From a postmodern perspective, it is impossible to test treatment methods for preconceived ideals of beneficent “outcomes,” because there are as many different outcome goals as there are singular clinical interactions. Some people may pursue cure, others may prefer coping. Some will be concerned with maximizing pleasure and others with maintaining beauty. Some may desire longevity and others comfort. Some may feel at ease with machine or synthetic chemical interventions; others prefer only “organic” based treatments. Some may wish to psychotherapeutically weave clinical problems into a new interpretive horizon that reframes and thus lessens the problems (or at least helps organize the problems into a more satisfactory “life story”); others may wish to devote their mental energies elsewhere and approach their clinical problem with as little reflection as possible. Thus, from a

postmodern perspective, the microgoals of the clinical interaction will be determined by the singularities of particular patient desires more than a preconceived calculus of treatment outcomes.

Similarly, from a postmodern perspective, psychiatry does not have to “prove” its legitimacy at the macro (sociopolitical) level through metanarrative scientific measurement of treatment outcomes. Rather, psychiatry achieves sociopolitical legitimacy (or fails to do so) because of more ethical, political, and aesthetic concerns. Thus, the legitimizing justifications needed for maintaining “psychiatry” as a profession that is available for those in mental anguish would be as much ethical, political, and aesthetic justifications as they would be “truth” justifications. Just as there is little need for “science” in justifying hospice care, after-school programs, vocational retraining programs, national parks, or art museums, there is little need for science in justifying psychiatric care. These activities are done, or not done, because there is a sociopolitical consensus that they are “right” to do. In other words, psychiatry should exist as a profession only because it contributes to making the kind of culture we believe in and the kind of world we want to create. Who are the “we” in this case? Whoever believes that there is a role for psychiatry in the service of people with mental pain and suffering, and whoever is willing to struggle and compromise to create such a world.

Therefore, to sum up this chapter and my reflections on postmodern consequences, by overidentifying with the ideals of modernism, psychiatry has developed a horizon of practice that is increasingly besieged by a chorus of criticism. If modernist psychiatry is to emerge from its current difficulties renewed and rejuvenated, it must not only react to insurance cutbacks, it must also rebuild itself from within—assisted in the

process by a scaffold of postmodern thought. Postmodern theory provides a useful corrective to the extremes of modernism and would help psychiatry embrace a wider range of knowledge structures from which to deal with human problems. Postmodern psychiatry would understand itself as a part of human studies and would, therefore, engage in interdisciplinary work with philosophy, history, literary theory, art, women's studies, Africana studies, disability studies, cultural studies, area studies, psychology, anthropology, and sociology. Not only would it open itself to the rest of the university beyond the sciences, it would open itself to patients' perspectives and to alternative cultural perspectives. In a postmodern psychiatry, psychiatry's current interdisciplinary work in the neurosciences would not stop. Rather, it would be balanced by a wider range of concerns. Of course, even a modest reduction in psychiatry's love affair with modernist science and technology would result in some negative trade-offs. There would surely be some slowing in the "Progress toward a Cure" of "mental illness," but there would just as surely be much improvement in psychiatrists' ability to help others, and themselves, in the process of coping.

Chapter 5: Postdisciplinary Coalitions and Alignments

Developing “Cultural Studies of Medicine” as a New Genre

In chapter 1, I argued that human sciences theory is poststructural, postmodern, and postdisciplinary. Accordingly, to retheorize psychiatry it is not enough to bring poststructural and postmodern critiques to bear on psychiatric knowledge. I must also start the process of bringing psychiatry into “postdisciplinary” circles. Psychiatry is already interdisciplinary with a variety of social, neuronal, and clinical sciences. However, it is not postdisciplinary with domains where “theory” is practiced, and it will be difficult to introduce “theory” directly into psychiatric discourse. Thus, strategic questions arise: Where does psychiatric discourse come closest to theoretical engagement? Where can we develop theoretical approaches to psychiatry with the least institutional transformation? Where can coalitions be made that will foster a retheorized psychiatry? My answer to these questions is “medical humanities.” Medical humanities is an interdisciplinary, but largely modernist, humanities domain of inquiry that is largely dominated by bioethics but also includes literature and medicine, history of medicine, and philosophy of medicine. This domain has a national conference, many academic faculty positions and programs, and a variety of journals. Because the “theory” I’m recommending for psychiatry is mostly practiced in the humanities, and because medical humanities includes psychiatry (as a branch of medicine), I argue that medical humanities is the best place to begin postdisciplinary work in psychiatry. Unfortunately, this entails stretching both psychiatry and medical humanities into new interdisciplinary coalitions

and alignments, because, as of now, both psychiatry and medical humanities are resistant to theory.

However, there is hope in the form of an emerging new theory genre in medical humanities, which I will call “cultural studies of medicine.” This genre is particularly interesting for my retheorizing project because it provides a potential bridge site between theory and application. As of now, however, this new genre has not been applied to psychiatry, and there are only a few examples of its application to medicine. Accordingly, my task in this chapter is to articulate and further develop this nascent and fragile postdisciplinary writing form as a viable genre for retheorizing psychiatry. Rather than offer extended definitions of cultural studies of medicine, I have organized this chapter around a reading and analysis of three recently published texts in the field. By bringing these texts together in a single space and developing their similarities in form and content, I intentionally highlight their standing as a new genre. These three texts, dealing respectively with “cyborg” technology, AIDS, and the medical “management” of sexual identity problems, represent excellent examples of the opportunities and possibilities of applying postdisciplinary approaches to medical topics. After working through these texts, and the theories that animate them, I devote my conclusion to a broader consideration of the role of cultural studies of medicine for medical humanities, and I discuss the advantages of strategically initiating and engaging my retheorized psychiatry through the related domain of “cultural studies of psychiatry.”

Cultural studies is a postdisciplinary approach to art, history, philosophy, and literature that combines recent theory with the more traditional humanities to analyze questions and concerns that are difficult to approach from a single humanities discipline

alone. Not content with a transcendental study of the good, the beautiful, and the true, cultural studies shakes the halo from humanities and high (capital c) Culture to ask: For whom is a particular cultural product good, beautiful, and true? Who is being advantaged by any particular constellation of beliefs and priorities? Who is being disadvantaged? What kinds of human subjects and social systems do the cultural products in question create? How and why, and through what set of social manipulations and tactics, are particular cultural products (and not others) with their particular beliefs and priorities (and not others) the ones that are held up for praise and emulation? And, perhaps the most important question for cultural studies: How might it be otherwise?

Cultural studies not only asks different questions than traditional humanities, it also takes on new concerns and new objects of analysis. No longer satisfied to study Shakespeare or Dante alone, cultural studies takes seriously popular culture, folk culture, and even commercial culture. As cultural studies blurs the lines between the disciplines, it also blurs the lines between high-*C* and low-*c* culture. Cultural studies finds popular culture every bit as interesting, as complex, and as worthy of study as high culture. From Madonna to Ice T, from Spike Lee to David Lynch, from *Miami Vice* to *Thirty Something*, from punk youth subculture to African-American gang life, from the suburban mall to the beach vacation, from the Marlboro Man to the Colt Malt Liquor Bull, cultural studies is as rigorous and as diligent in studying popular culture as previous high-culture humanities were in studying the canon of elite cultural products. Cultural studies questions of popular culture, however, are not dissimilar to cultural studies questions of elite culture: What are the meanings (both manifest and latent) in popular cultural products? How are they produced, what meanings are structured into them, and

how are these consumed and used for social and cultural indentifications—through what technical mastery and through what social politics? In other words, what are the effects of these cultural products in the circuits of culture? How might they be otherwise? (Johnson, 1996; du Gay et al., 1997; Barker, 2000).

How, then, could medicine or psychiatry be an object of cultural studies?

Medicine and psychiatry align themselves with science, and, as science, medicine and psychiatry claim to be objective—determined by the real, the facts of nature, not by culture. Cultural studies, however, influenced by theoretical insights into the role of language and power in shaping knowledge (including scientific knowledge), has not been intimidated by the distinction between the sciences and the humanities. As such, cultural studies has recently expanded its postdisciplinary domain to include science and has joined hands with the radical science movement, postpositivist philosophy of science, sociology of science, and feminist studies of science to challenge the very core of the assertion that scientific knowledge is objectively “determined by nature” (Aronowitz, 1996; Keller & Longino, 1996; Penley & Ross, 1991b; Pickering, 1992; Ross, 1996a; Rouse, 1996; Traweek, 1993) These scholarly movements are united in their belief that scientific knowledge (while it certainly produces real effects on the world and is bounded by the limitations of materiality) must be understood as socially determined and culturally specific.

Donna Haraway, perhaps the most representative single author of the new synthesis between cultural studies, social studies of science, and feminist studies of science, puts it this way:

It seems to me that the practices of the sciences—the sciences as cultural production—force one to accept two simultaneous, apparently incompatible

truths. One is the historical contingency of what counts as nature for us: the thoroughgoing artifactuality of a scientific object of knowledge, that which makes it inescapably and radically contingent. You peel away all the layers of the onion and there's nothing in the center. And simultaneously, scientific discourses, without ever ceasing to be radically and historically specific, do still make claims on you, ethically, physically. The objects of these discourses, the discourses themselves, have a kind of materiality: they have a sort of reality to them that is inescapable. No scientific account escapes being story laden, but it is equally true that all stories are not equal here. . . . There are [particular] political consequences to [particular] scientific accounts of the world. (interview response in Penley & Ross, 1991a, p. 2)

For Haraway, cultural studies of science breaks free from standard epistemological questions of science (Is it true? Does it mirror the world?) and opens out to political questions about the effects of scientific discourse.

Reading Examples of the New Genre

When Haraway's cultural studies approach to science is applied to medicine, the new genre "cultural studies of medicine" emerges. The task of cultural studies of medicine is to hold in tension two perspectives: that medical discourses are real and have real effects on the world, and simultaneously that they are social, cultural, and political. As cultural constructions, medical discourses and practices are as open to political critique and analysis as are any other cultural phenomena. Cultural studies approaches to medicine do not acquiesce to medicine's claim to scientific authority. Cultural studies looks behind the bioscience curtain of authority in order to engage more directly the effects of medical discourse. The practitioners of cultural studies of medicine, to date, are primarily students of cultural studies and feminist studies who have become interested in breaking down the barrier between humanities and science and are hoping to unleash the

force of critical thought on the ever-burgeoning power house of biomedical technoscience. As of now, cultural studies of medicine is new enough that most of the practitioners of this emerging genre are dispersed, with minimal sense of connection. The medical community seems to have little awareness of their work, and the cultural studies community, in its more predominant strands, is concerned primarily with popular culture. There is no single place that a reader in this domain can turn for a reliable collection of this work. None of it is available in the medical bookstore, and there is no dependable keyword available for general library searches. A good place to begin, however, is in the cultural studies or the gender studies section of a large academic bookstore.

For example, while browsing the cultural studies section of Labyrinth Books, a Manhattan bookstore that bills itself as a “7,000 square foot independent bookstore devoted to scholarly, academic, and university press books,” I find three current titles with direct reference to medicine: *Technologies of the Gendered Body: Reading Cyborg Women* by Anne Balsamo, *AIDS and the Body Politic: Biomedicine and Sexual Difference* by Catherine Walby, and *Changing Sex: Transsexualism, Technology, and the Idea of Gender* by Bernice Hausman. Here are the pathbreakers in the cultural studies of medicine genre. Though each author is now teaching in the academy, all three books were originally written as PhD dissertations by graduate students in cultural studies programs. Each text uses not only a cultural studies approach but also a feminist approach. On the back of each book, in addition to being classified as cultural studies, each book is also classified as either feminist studies or gender studies. Anne Balsamo refers to her work by the hybrid label “feminist cultural studies.” There is an element of excitement about each of the texts—as if something new is being born. In each book

there is a palpable “casting about for a topic,” which is happily resolved through the discovery of biomedicine as a discourse available for cultural analysis. Cultural studies at this historical moment is sufficiently well developed, and its usual topics are sufficiently well explored, that its scholars are beginning to seek out new material to address. Biomedicine and medical technology are particularly rich writings and cultural practices to explore.

Bernice Hausman is the most explicit about her search for a topic and the excitement she felt on “finding” biomedical discourse as an option. Here is an extended excerpt from her preface.

I fell into this project sideways. In the summer of 1990, I had just passed my comprehensive examinations and was beginning work on a dissertation concerning the idea of identity in feminist theory. It was a topic I had been thinking about for some time and it seemed a natural for a theory-minded feminist graduate student like myself. But no matter how much I applied myself to the task, most of my thoughts on the issue seemed uninspired, boring, even obvious. By October, that project clearly had no future and I was fishing around for another one. I was also relatively homebound, with acute tendinitis in one foot. In a rather despairing mood, I began reading—and this book started to happen to me.

The idea of transvestitism was kicking around feminist literary criticism at the time, and when I finally got to the library to look for primary source material, I inadvertently found texts that dealt with transsexualism. Now that was really fascinating. For about six months I read anything and everything I could find about cross-dressing and sex change. I attended a national conference for transvestites and transsexuals. I started to write critiques of the existent feminist treatments of transsexualism. The possibilities for understanding the construction of “gender” through an analysis of transsexualism seemed enormous and there wasn’t a lot of critical material out there. But it wasn’t until the following

summer, in a seminar on “Theorizing the Body,” that I found my “hook,” my way into the project.

It was simple, really. In thinking about technologies of the body, I realized that certain technologies were essential for the material practices known as “sex change.” After all, without endocrinology and plastic surgery, there could be no hormonal treatments or genital surgery. I hypothesized that transsexualism emerged in the twentieth century at least in part due to advances in medical technologies that made physical “sex change” possible. (Hausman, 1995, p. vii)

With creative leaps like this one, Hausman and other cultural studies scholars are engaging biomedical discourse and developing the new genre I am calling “cultural studies of medicine.”

Walby: AIDS and the Body Politic

Each of the authors of these texts divides them into two sections: a theory section and an applied section, and I have followed that format in my discussion. In *AIDS and the Body Politic: Biomedicine and Sexual Difference*, Catherine Walby titles her theory chapter, “The Biomedical Imagination and the Anatomical Body: AIDS and the Nature/Culture Distinction.” The purpose of this chapter is to set up the “interdisciplinary encounters and epistemological critiques” necessary for the remainder of the book. (Walby, 1996, p. 15) Walby draws heavily on the works of deconstructive and poststructuralist theorists Michel Foucault, Jacques Derrida, Judith Butler, and Elizabeth Grosz to highlight and problematize a core binary distinction she finds throughout the biomedical literature on AIDS: the nature/culture distinction. Biomedical discourse uses a bright line distinction between nature and culture to shore up its own power to determine “what AIDS really is, to draw a line separating the real immutable facts of AIDS from those aspects of AIDS which can be politically contested” (p. 50). The nature/culture

distinction drains off the possibility of interpreting biomedicine's explanatory concepts of AIDS as politically interested and enmeshed in a simultaneous concept of social order.

Using deconstructive and poststructuralist theory, Walby introduces the necessary "ambiguity and metaphoricity" of biomedical language and the related impossibility of that language corresponding accurately and referentially to its preexisting object:

The operation of analogy and narrative is irreducible in scientific practice, and offers points of nonsystematicity through which cultural readings of biomedical texts can be introduced. Analogy is a crucial element in the kinds of idealization and standardization through which biomedical representation produces its anatomical objects, and biomedicine's specifications of the normal and the pathological are always enmeshed in normative social analogies. In this sense biomedicine can be understood as an anatomization of culture, a rendering of concepts of social order into anatomical terms. I term this process of reframing cultural concerns in anatomical terms as the biomedical imagination. (Walby, 1996, p. 50)

In the case of AIDS, biomedicine anatomizes culture in the context of a highly complex and contested field of sexual politics. Consequently, for Walby, any study of the sexual politics of AIDS and AIDS discourse first requires a theoretical "denaturalization" of the biomedical discourse and its claim to scientific authority.

The biomedical imaginary works not only through formal medical knowledge production but also through the privileged relation of biomedical knowledge to popular and public representations of AIDS. Accordingly, in the applied chapters of the book, Walby explores formal medical discourses of immunology, epidemiology, and the HIV antibody test as well as public AIDS education programs, popular promotional ads for condoms, and "sights for resistance and refusal," such as the organizations of Queer Nation and ACT UP (AIDS Coalition to Unleash Power). Throughout her analysis, she

finds a complicitous relationship between medical and public discourse on AIDS and the “phallogentric social order.” For Walby, the phallogentric social order “holds that only the heterosexual masculine is fully equivalent with the human and the normal. It readily represents other sexed interests as allies of the viral and the pathological” (p. 9). Walby’s text explores and outlines in minute detail the intricate and disturbing shape of how culturally desirable is the clean, self-identical, and sharply bounded heterosexual male body. It is the heterosexual male body that the biomedical imaginary consistently represents as the body worthy of protection from virus rather than as a potential source of virus. In contrast to the purity and preciousness of the male body, the biomedical imaginary portrays female and homosexual bodies as culturally horrifying. These bodies are open, permeable, uncontrolled, and prone to harboring and proliferating, aiding and abetting, the infectious HIV villain.

Thus, the biomedical “war against AIDS” is much more than a war against HIV—it is part of a social and moral battle in which biomedicine is aligned with the conservative forces of patriarchy. As Walby explains,

The logic of contagion in AIDS discourse maps itself onto different risks associated with various genital practices, and the sexual identities they are said to denote. The most virulent sexual practice is both receptive and penetrative anal sex between men, which is taken to denote homosexual identity. This is followed by a combination of anal receptivity and vaginal penetration, taken to stand for bisexual male identity, which is in turn followed by indiscriminate vaginal receptivity which stands for promiscuous femininity, and so on. In this way biomedical normativity can be seen to both postulate and moralize sexual identities along particular lines. It simultaneously assumes a relationship between genital capacity and subjectivity, and orders this capacity into a hierarchy of pathology. (1996, p. 41)

In the biomedical imaginary of AIDS discourse, heterosexual men are left unmarked as the pure and the clean. All other bodies are the allies of HIV and the appropriate objects of the war on AIDS. As a result, for Walby, AIDS should not be understood merely as a symptom, or an activity of a virus alone, but as a particular moment in the history of sexual politics.

Balsamo: Technologies of the Gendered Body

In *Technologies of the Gendered Body, Reading Cyborg Women*, Anne Balsamo takes up a similar set of concerns. Like Walby, Balsamo works through a close analysis of medical literatures, public policy documents, and specific biotechnical practices alongside close readings of popular texts. Balsamo titles her theory chapter “Reading Cyborgs, Writing Feminism: Reading the Body in Contemporary Culture.” Inspired by the work of Elizabeth Grosz, Donna Haraway, Ruth Bleier, Paula Treichler, and a feminist rereading of Michel Foucault, Balsamo traces how recent developments in bioscience and biotechnologies are shaped by gender considerations as well as beliefs about race, physical abilities, and economic and legal status. Balsamo understands her work as an elaboration of Elizabeth Grosz’s theoretical project of “corporeal feminism” (Grosz, 1994). For Balsamo, corporeal feminism is a

critical framework which draws its methods from feminist cultural studies broadly to suggest that (a) the body is a central symbolic resource for cultural work [politics]; (b) the discursive, symbolic and material body are mutually determining; and (c) gender is often a submerged discourse within many studies of the body. (1996, p. 11)

Balsamo uses corporeal feminism to wrest the body from bioscience in order to interpret the way in which bodily representations and transformations in high-tech science become the bearers and creators of cultural meanings and desires.

The central trope for Balsamo is Donna Haraway's use of the term "cyborgs" (Haraway, 1991). Cyborgs are techno-bodies. Part human and part machine, cyborgs (shorthand for "cybernetic organisms") are human-machine couplings whose possibilities are proliferating at a dizzying pace. A recent *New York Times* science article, "The Whole Body Catalogue, Replacement Parts Mix and Match," demonstrates how cyborgs are increasingly moving from science fiction to bioscience (and technoculture) familiar. The rhetorical move in medical and popular culture is to make cyborgs "natural" rather than strange by minimizing the difference between cyborg and noncyborg bodies (if such a term still has meaning). For example, according to the *New York Times*,

The body is a complex organic machine designed to renew itself constantly and run with little tinkering for decades at a time. Few machines produced by humans have such stamina and durability, but even the body can falter or break because of disease, accident, overuse or aging and require fixes that exceed its remarkable self-repair mechanisms. People have turned their tool-making and building skills to correcting the medical problems the body cannot fix. The result is a growing array of artificial body parts that are available for immediate use and many more in various stages of development around the world. From the top of the head to the bottom of the feet, it is becoming increasingly possible to use artificial parts to enhance vision and hearing, strengthen weakened bones, bolster or replace faltering organs, replace damaged joints, substitute for disabled nerves or improve appearance. . . . Receiving artificial body parts has become so common that it no longer seems exotic. (Leary, 1997, p. b7)

In articles like this, rhetorical sleight of hand creates the seemingly impossible situation where replacing body parts, “from the top of the head to the bottom of the feet,” becomes everyday and “only natural.”

In *Technologies of the Gendered Body*, Balsamo explores the way cyborg techno-bodies are promoted by bioscience and technoculture as “healthy, enhanced and fully functional—more real than real” (1996, p. 5). Thus, new body technologies are to be desired as life-enhancing and even lifesaving. New body technologies highlight and underscore the impossibility of a definitive nature/culture split in understanding the body by showing the material effects of discourse on the body and the body’s limiting effects on discourse. The body is not nature or culture but a nature/culture hybrid.

Balsamo begins her exploration of the mechanical reconstruction of the body and the effects of discourse on the lived material body with a close reading of the subculture of female bodybuilding. “Perfectly attuned to contemporary culture, the female body builder is a machine dream of cyborg identity, the female form that works to recreate the female form, using the science of weights, resistance, and kinesthetic labor” (Balsamo, 1996, p. 12). Female bodybuilding is a potentially transgressive practice because it undermines the ideological association of strength and masculinity. However, on closer inspection, Balsamo traces the normalizing powers of media representation to reestablish within female bodybuilding new ideals of the feminine such that “muscularity and physical development are heralded as women’s new sex appeal” (p. 12). Balsamo follows her bodybuilding analysis with careful readings of cosmetic surgery, new reproductive technologies, virtual bodies in cyberspace, and the techno-bodies of cyberpunk fiction. In the process, Balsamo takes us further and further into cyborg territory by exploring

technology's impact on and redefinition of the body and human subjectivity in many of the new and unfamiliar areas of high-tech experience that have slithered, almost unconsciously, into more and more of people's daily lives.

Balsamo provides ample evidence that despite the newness of cyborgs and cyborg futures the reimagined techno-body is still very much a gendered and race-marked body caught up in the familiar traps of traditional power relations. With all the gloss and dazzle of biotechnological hype, it is easy to lose sight of political and ethical issues and concerns. The politically conservative subtext of the new technologies is prominent in all the domains Balsamo explores, but it is perhaps most easily highlighted in her analysis of cosmetic surgery. For Balsamo, "the discourse of cosmetic surgery offers provocative materials for discussing the cultural construction of the gendered body because, on the one hand, women are often the intended and preferred subjects of such discourse, and on the other, men are often the bodies doing the surgery" (1996, p. 13). The new biotechnologies of cosmetic surgery exercise a new form of scientific power that effects both the objectification of the female body and the subjection of that body to the surveillance of a normative gaze. Here is Balsamo's summary of her cosmetic surgery analysis:

The medical gaze of the cosmetic surgeon has been transformed into a technological perspective, with the attendant consequence that the female body is itself transformed into a surface for the inscription of cultural ideals of Western beauty. Cosmetic surgery enacts a form of cultural signification where we can examine the literal and material reproduction of ideals of beauty. Where visualization technologies bring into focus isolated body parts and pieces, surgical procedures actually carve into the flesh to isolate parts to be manipulated and resculpted. In this way cosmetic surgery literally transforms the material body

into a sign of culture. . . . Cosmetic surgery is not then simply a discursive site for the construction of images of women but is actually a material site at which the physical female body is surgically dissected, stretched, carved, and reconstructed according to cultural and eminently ideological standards of physical appearance. (p. 13)

Thus Balsamo's study of techno-bodies and popular culture's engagement with the same allows us to investigate how biases and hierarchies of identity, nature, and the body are rearticulated with and through the new biotechnologies—ensuring that traditional and politically conservative narratives of the gendered, race-marked body are socially and technologically reproduced.

Hausman: Changing Sex

In *Changing Sex: Transsexualism, Technology, and the Idea of Gender*, Bernice Hausman follows suit with Walby and Balsamo. Hausman's text explores recent advances in medical technology that have been central in establishing the material and discursive conditions necessary to produce transsexual "demand" for sex change. Hausman titles her theory chapter "Semiotics of Sex, Gender, and the Body," and she builds on many of the same theorists as Walby and Balsamo: Judith Butler, Elizabeth Grosz, and Michel Foucault. Perhaps, the most important theorist for Hausman, however, is Roland Barthes.

Central to Hausman's theoretical semiotics of sex, gender, and the body is Barthes's "mythological" approach to the study of signs. Barthes uses the term "myth" idiosyncratically to mean any commonly held, but relatively unexamined assumption or belief that can be shown to be rooted in the prevailing political order (Barthes, 1957). For Barthes, myth does not imply a distinction from science or truth or reason. Rather, myth

is any speech or writing that attempts to deny its political effects. Unexamined assumptions and beliefs (particularly from the perspective of political implications) are rampant in the modern sciences, such as biomedicine, regardless of their claims to “objectivity.”

Barthes describes two orders of signification that make up a myth. For him, myth is “a peculiar system, in that it is constructed from a [first-order] semiological chain which existed before it” and also from “a second-order semiological system” (quoted in Hausman, 1995, p. 185). Hausman explains:

the second-order signifying chain uses the sign of the first-order counterpart as its signifier, thereby linking the two systems in an apparent homology. The second-order signification negates the first-order semiotics on which it depends, however, and constitutes itself as a myth, the transcendent sign that comes to ground the first-order system. (Hausman, 1995, p. 185)

In a less technical wording, we might say that the “reading” of myth involves a careful alertness to the difference between the denotation of a sign and its connotation(s). The denotation is the literal meaning and the connotation the mythical meaning. A myth is motivated as much by its connotations as by its denotations. The connotations are denied, however, and the producers of myth attempt to present it as simply a denotative system.

Hausman uses a now classic example from Barthes’s *Mythologies* to help clarify. Barthes writes, “I am at the barber’s, and a copy of *Paris-Match* is offered to me. On the cover, a young Negro in military uniform [presumably from one of the French colonial territories] is saluting, with his eyes uplifted, probably fixed on a folded tricolour [the French national flag]. All this is the *meaning* of the picture” (quoted in Hausman, 1995, p. 185). By “meaning,” Barthes refers to the signification resulting from the first semiotic chain—the denotation. He continues,

But . . . I see very well what it signifies to me: that France is a great Empire, that all her sons, without any colour discrimination, faithfully serve under her flag, and that there is no better answer to the detractors of an alleged colonialism than the zeal shown by this Negro serving his so-called oppressors. (quoted in Hausman, 1995, p. 185)

The second-order meaning, or connotation, of a happy family of French imperialism overdetermines the meaning of the picture. Through a mythological reading, however, the picture loses its innocence at both the first and second order. It can no longer simply denote “a Negro is giving the French salute,” nor can it successfully connote a simple benevolent interpretation of French imperialism. A mythic reading highlights ideological dimensions of the picture that are slipped into its supposedly innocent denotative significations. As a myth, the saluting man’s specific racial and political history is denied, and the picture seems to function outside history to naturalize a one-sided interpretation of French imperialism and colonialism. Thus, for Barthes, “myth is constituted by the loss of the historical quality of things. . . . Myth is experienced as innocent [descriptive, objective] speech: not because its intentions are hidden—if they were, they would not be efficacious—but because they are naturalized” (quoted in Hausman, 1995, p. 186).

For Hausman, following Barthes, biomedical “gender” terminology is also a mythic form of signification. By applying a version of Barthes’s semiotic analysis to several discourses of medical technology (endocrinology, plastic surgery, and the medical management of intersexuality and transsexuality) and to the autobiographies of prominent transsexuals (Christine Jorgensen, Nancy Hunt, Renee Richards, and others), Hausman traces the emergence of the term “gender” (as differentiated from “sex”) and its subsequent naturalization and reification in the phenomenon of sex-change surgery. “Gender,” Hausman tells us,

was originally produced as a theoretical concept to guide clinicians treating intersexual [or hermaphrodite] subjects whose physiological sex transgressed the expected binary opposition between male and female. The identification of a gender role [used for patient classification] aided the clinician in designating a correct sex in which to assign the patient. But gender role itself was eventually taken to signify an internal, gender identity that inheres in every individual. (Hausman, 1995, p. 189)

As a result, gender role, initially understood as a social category primarily determined by social factors, is thus reified and naturalized along the previous ideology of male and female sex difference as existing in an absolute binary differentiation. When researchers and clinicians “discovered” intersexuality—ambiguous or mixed physiology—they had the potentially progressive option of upsetting this rigid ideology of sexual anatomy. By creating the term “gender” and its subsequently naturalized second-order meanings, however, researchers and clinicians replaced the physiological object of the body with the idea of “sexed behaviors” (gender role) and consequently erased the ambiguousness of intersexuality in neonatology (and, along a similar fashion, in endocrinology as well).

Once gender was naturalized as either male or female, hybrid experiences of intermingled gender behaviors, sexual preferences, and biological ambiguities could be effectively pathologized. For Hausman,

Transsexuals picked up on this semiotic relation [of naturalized sex and gender], and, claiming the production of an aberrant gender identity, demanded the appropriately “matched” body/sex. Thus gender, a descriptive term, the sign of a first-order signifying system, theoretically came to ground the making of new bodies in a process that simulates the scenario of its original unfolding. (1995, p. 189)

In Hausman's reading, the patriarchal and homophobic medical institution's desire to create heterosexual subjects out of intersexuals underscores how "gender" operated semiotically within the medical literature to maintain heterosexual ideology as the norm of the human body. The medical literature became a source of identity formation for transsexuals, who in turn reinforced and expanded the medical literature's "gender" pathology. A critical aspect of both the medical literature and the transsexual autobiographies Hausman explores is that the source of transsexual suffering should not be understood as related to homosexuality. Both groups are emphatic that the gender and sexual preference problems they address should be narrated as "mistakes of nature" and have nothing to do with the equally plausible story of "painful psychic internalizations" occurring in the context of rampant societal disgust of otherwise healthy homosexuality or bisexuality. As a result, both the medical literature and transsexual autobiographies serve to insure the heterosexual norm. Hausman's detailed analysis of "gender narratives" in the medical literature and in transsexual autobiographies supports not only their codeterminate creation of the phenomenon "transsexualism" but also an interpretation of both groups as simultaneously serving to establish a normative language that regulates compulsory heterosexuality in the lives of all contemporary Western subjects.

Possibilities for the New Genre in Healthcare Discourse

What speculative conclusions can we make from reading these cultural studies of medicine examples? What are some possibilities for this newly emergent genre for healthcare? How might it make a difference, how might it fit into current medical discourse, what kinds of critique would it add, and what possibilities are there that cultural studies critiques would worsen "health care crisis" problems? Finally, how might

“cultural studies of psychiatry” develop from cultural studies of medicine? First, cultural studies of medicine adds an exciting new set of methods, theories, and objects of analysis to the study of medicine in general and medical humanities in particular. Postdisciplinary methods of close reading and discourse analysis, which are able to draw from resources across the academy without limitation to a single discipline or method, have not been available for medical scholars. Nor has medical scholarship taken meaningful advantage of new theories of language and knowledge that have by now been extensively developed on the main campus. Postmodernism, poststructuralism, deconstruction, Lacanian psychoanalysis, feminism, critical-race theory, and postcolonialism are all tools of analysis that can provide medicine with new vistas for self-understanding. In addition, cultural studies of medicine opens medical humanities scholarship out to new objects of analysis. Previously marginalized material, such as popular media, commercial advertisements, alternative healing practices, or the sociopolitical context of medical knowledge, will now have a method of study and a theory of relevance that they have never had before.

Second, medical humanities is the ideal home for this new genre. In many ways, medical cultural studies may be understood as a development of medical humanities, just as cultural studies itself was a development of humanities (particularly literature). In the last couple of decades, there has been a paradigm shift within humanities toward cultural studies. With the emergence of cultural studies of medicine, this paradigm shift is now arriving in medical humanities. Anthony Easthope describes the paradigm of “modern” humanities until the advent of cultural studies as being structured around five interlocking features:

1. a traditionally empiricist epistemology;
2. a specific pedagogic practice, the modernist reading;
3. a field for study discriminating the canon from popular culture;
4. an object of study, the canonical text;
5. the assumption that the canonical text is unified. (Easthope, 1991, p. 11)

Easthope argues that the old paradigm of humanities has collapsed, and the “theory wars” in humanities throughout the 1980s were in many ways a symptomatic register of the collapse. In Easthope’s narrative, modern humanities has fallen into crisis and is being transformed into something else: cultural studies. Transformed postmodern humanities, in a cultural studies paradigm, overturns and displaces each of the paradigmatic features of modern humanities. In Easthope’s reading, though modern humanities continues to be institutionally dominant in North America and Britain, cultural studies has captured the emerging edge of many humanities disciplines. In a related logic, medical cultural studies may be seen as an emergent extension of currently existing medical humanities. Accordingly, medical cultural studies is an ideal institutional site from which to explore the relevance for medicine of the humanities paradigm shift toward cultural studies and to expand the objects of cultural analysis to include a variety of medical artifacts and representations.

Third, the new cultural studies of medicine genre is and will be a “critical multicultural” voice in the study of medicine. As Douglas Kellner has explained, a critical multicultural perspective [in cultural studies] takes seriously the conjunction of class, race, ethnicity, gender, sexual preference, and other determinants of identity as important constituents of culture which should be carefully scrutinized and analyzed in order to detect sexism, racism, classism,

homophobia, and other tendencies that promote domination and oppression.
(Kellner, 1995, p. 96)

All too often, medical writings, including much of medical humanities, lack this kind of multicultural self-critique and portray medicine as an unambiguous good. From my perspective, when medical writings do critique recent problems in healthcare, the refrain too often goes something like this: “Poor, abused, altruistic clinicians do their best to eliminate pain and suffering only to be foiled by greedy, crassly capitalist, and uncaring insurance companies.” This refrain has become so widely popular, presidential candidate Al Gore made it a stump speech. However, a multicultural critique, in contrast to this refrain (or perhaps in addition to this refrain), starting with a critique of class, would demonstrate how this narrative leaves out the ways Western medicine has always been a capitalistic enterprise and, as such, has always been enmeshed with class elitisms and class power structures. Capitalist distortions (and, to be thorough, capitalist possibilities) are nothing new in medicine. Indeed, the recent change in medical rhetoric toward a more commercial (“provider/consumer”) language is in many ways a more accurate representation of healthcare relations than the previously hallowed (“doctor/patient”) language medicine seems so nostalgic to regain. Hidden behind the priestly promise of the “good doctor” have always been power issues of class that medicine has been reluctant to address, or it seems, even acknowledge.

To be sure, the three cultural studies of medicine examples I reviewed in this chapter are not as critical of medical capitalism and class elitisms as they are critical of medicine’s complicity with patriarchy and compulsory heterosexuality. Medicine’s “men of science” and the discourses they have created are as protective of the “medical man’s” sexual privilege as they are “his” class position. Walby, Balsamo, and Hausman have all

critiqued medical discourse primarily from the perspective of patriarchal preference and bias. There has been progress in admitting women into medical practice, but the patriarchal bias of medicine is by this time so deeply entrenched in the language and practice of medicine that it is not sufficient to critique medicine's traditional exclusion of women from professional practice. Though exclusion continues to be a major concern with much ground yet to gain (particularly in administrative, research, academic, and specialty medicine), and always in danger of backsliding in the areas where progress has been made, the challenge of feminist cultural studies of medicine goes beyond critiquing issues of equality. In addition to equality concerns, feminist cultural studies of medicine critiques the linguistic and social structures of medicine and the kinds of human subjectivity and life experiences created by those structures. Feminist approaches in cultural studies of medicine question not only who we should include in medical practice but, more fundamentally, what kind of people we want to be.

Beyond questions of class and gender, future cultural studies of medicine will also critique medical discourse and practice in terms of race, ethnicity, postcoloniality, age, and so forth. Medical misogyny, homophobia, and class elitisms are also complicitous with racist, colonialist, and ageist biases. The specifics of Western medicine's "othering" of the "diseased" and how medicine's images of illness have arisen from a need to separate the clinically clean white upper-class medical man from a host of frighteningly horrific poor and pitiable patients will have to be worked out in much detail. Medical cultural studies as it develops will increasingly learn to think of gender, race, class, and so on together, rather than as simply parallel or analogic relations. Thinking gender, race and class together will not be meaningful in terms of abstract theory alone. Rather,

specific medical and popular discourse and practice will have to be analyzed in all their complications and mixed features. The three cultural studies of medicine we read above may all be faulted, perhaps, for interpreting medicine as too monolithic and totalizing in its effects. These early cultural studies of medicine present a kind of demonic medicine that in many ways mirrors medicine's own angelic self-image. As cultural studies of medicine develops, the binary between "good" and "evil" medicine will be less compelling. Modern medical practice is a cultural practice, and as such it brings empowering possibilities even as it extends traditional structures of control and domination. A primary role for cultural studies of medicine will be to serve as a source of critique toward these very structures of control and domination within healthcare, while simultaneously seeking elements of empowerment and enhancement.

A multicultural critique of medicine will hardly weaken it. The "crisis in healthcare" is not simply about medical economics anymore than the rapid rise in alternative "new-age medicine" is simply about "scientific illiteracy." Medicine has problems, and people are alienated by it in ways that require deep rethinking rather than superficial tinkering with economics of delivery systems. These problems are very much tied up with the cultural and political position of so much of the "objective" medical research and practice to date. Opening up medical inquiry beyond the sciences and traditional humanities to the postdisciplinary critique of cultural studies has the potential to be a regenerative force of change in medicine rather than a negative acid of destruction. Though critical self-analysis can often be a painful process, the rewards and possibilities for redirection can be tremendous. Reading even these early examples from the new cultural studies of medicine genre can go a long way toward improving what is

perhaps the worst of the “crisis in healthcare”—the hubris and arrogance of medical knowledge.

Finally, as a branch of cultural studies of medicine, cultural studies of psychiatry is an excellent place to bring psychiatry into postdisciplinary interaction. Cultural studies of psychiatry has a potential in psychiatry similar to what cultural studies of medicine has in medicine. In addition, using medical humanities as a bridge to cultural studies of psychiatry provides a way to “retheorize” psychiatry without requiring major institutional changes. By bringing psychiatry into the cultural studies orbit, significant new theoretical tools and academic coalitions become available for new approaches to psychiatry. Like cultural studies of medicine, cultural studies of psychiatry does not require a dramatic paradigm switch within the clinical domain. Instead, these new cultural studies genres can be put into circulation through the institutional infrastructure that already exists for cultural studies, women’s studies, postcolonial studies, gay and lesbian studies, and so on and by further developing the connections between these institutional sites and the infrastructure of medical humanities. Admittedly, from my perspective, larger scale revolution in psychiatry would be preferable to somewhat marginal cultural studies readings located in the also marginal domain of medical humanities. In chapter 8, I will explore what a large-scale “paradigm” switch toward a retheorized psychiatry might be like, but retheorizing psychiatry should not wait for such a revolutionary paradigm switch to initiate its efforts. Retheorized psychiatry can begin now, through a “cultural studies of psychiatry” genre located within medical humanities, to build a resource of critical material and to build new postdisciplinary coalitions. These materials and coalitions can be used to develop and sustain alternative readings and approaches to psychiatry. It is in

this spirit that, in the next two chapters, I will apply the cultural studies of medicine genre to issues in psychiatry.

Chapter 6: Prozac and the Posthuman Politics of Cyborgs

*I phoned my editor and left a message on her voice mail. I said, I know you are tired of hearing this sort of thing from authors, but something **unusual** is happening out here.*

—Peter Kramer (1997, p. 315; emphasis added)

The Epidemic of Prozac Signification

When Peter Kramer's *Listening to Prozac* was published in 1993, Kramer, a psychiatrist and first-time book author, ran to a local bookstore to see himself in print. When he got there, he was amazed to see his book selling out as soon as a new shipment arrived. *Listening to Prozac* turned out to be a national best-seller. At the time, however, Kramer was so surprised by the success of his book that he excitedly called his editor to tell her that "something unusual" was going on. But what is that "something unusual," and how can it be articulated? Certainly, part of that something unusual is the recent epidemic of Prozac prescribing. When Prozac hit the market in 1987, it was widely hailed as a "wonder-drug," and its prescriptions have dramatically increased ever since—with Lehman Brothers predicting sales of \$4 billion a year by the turn of the century (Better than well, 1996, p. 87).¹ But beyond the epidemic of prescriptions, another, less analyzed,

¹ This number turned out to be an overestimate. As of this writing, Eli Lilly's third quarter earnings statement reports that Prozac is on track for sales of \$ 2.5 billion in 2000.

part of the Prozac phenomenon is an epidemic of signification that has simultaneously surrounded Prozac in the last few years.² Following are some examples.

The *Handbook of Psychiatric Drug Therapy* puts the “something unusual” of Prozac as:

The recognition that specific neuronal uptake mechanisms for serotonin were present in the CNS suggested, as early as the late 1960s, a potential target for the development of antidepressants. By the early 1970s, the technology existed for the screening of molecules that could selectively inhibit serotonin uptake. In 1972, fluoxetine (Prozac) was shown to produce selective inhibition of serotonin uptake in rat synaptosomes. This drug, the first in its class . . . was approved for release in the United States in December 1987. [Its] impact . . . on the treatment of depression has been extraordinary, with more than 10 million people prescribed . . . by 1994. The success appears to derive mainly from side effect advantages over older agents . . . [which has] generated wide patient and prescriber acceptance. (Hyman, Arana, & Rosenbaum. 1995, p. 62)

Psychology Today puts Prozac’s “something unusual” this way:

Slowly, stealthily, Prozac is slithering into more and more of our lives and finding a warm place to settle. Even the most casually aware citizen can feel the shift in thinking brought about by the drug’s ability to “transform” its users: We speak of personality change, we argue over the drug’s benefits over psychotherapy (all those expensive hours of parent-bashing as compared to a monthly dash to the pharmacy); and we let ourselves imagine a world in which our pain is nullified, erased as easily and fully as dirty words on a school blackboard. (Mauro, 1994, p. 44)

Tribune Business News puts the “something unusual” as:

² I borrow the phrase “epidemic of signification” from Paula Treichler, who has used it in a different context to refer to the “fragmentary and often contradictory ways we struggle to achieve some sort of understanding” of a new and dramatic medical phenomena (Treichler, 1988, 31).

Feeling despondent? Beset by burning stomachaches? Are your arteries hopelessly clogged? Well, you're not alone. Prescription medications for depression, ulcers, and high cholesterol dominated the list of best-selling drugs last year with six of the top ten entries. . . . What's more, these half-dozen drugs generated \$8.1 billion, or an impressive 9.5% of the \$85.4 billion in prescription drugs sold in 1996. . . . Overall, the sale of prescription drugs to pharmacies rose by 10% in 1996. . . . Eli Lilly's Prozac was the third leading bestseller overall with sales of 1.7 billion, a 14% rise [from 1995]. . . . Pfizer's Zoloft was fifth with sales of \$ 1.1 billion. (Silverman, 1997, p. 216)

Finally, Andrew Weil, in his *New York Times* best-seller, *Spontaneous Healing*, puts Prozac's "something unusual" this way:

What about depression, which is now epidemic in our culture? I experience depression as a state of higher potential energy, wound up and turned inward on itself. If that energy can be accessed and moved, it can be a catalyst for spontaneous healing. The psychiatric profession treats depression almost exclusively by prescribing drugs, especially a new class of antidepressants called serotonin reuptake inhibitors, of which Prozac is a prototype. The pharmaceutical industry markets these drugs aggressively and successfully, partly by convincing people that they cannot know their full human potential unless they use them. Recently a woman friend of mine in her early fifties went for a routine checkup to her gynecologist, also a woman. After the examination was over, the gynecologist asked her, "Well, do you want me to write you a prescription for Prozac?" "Why should I want to take Prozac?" my friend replied. "I'm not depressed." "How do you know?" asked the doctor. (Weil, 1995, p. 201)

What are "we" to make of Prozac in light of this epidemic of signification? Is Prozac a straightforward example of medical progress? Or is Prozac a complex cultural phenomenon? Is Prozac just good business? Or is Prozac symptomatic of a medical system out of touch with healing and obsessed with technology? How, in other words, should Prozac be narrated with such a diversity of options? Should we be concerned for

the coming of a “Prozac nation,” or jubilant for new and improved treatments for depression? Is Prozac progress or regress—panacea or Pandora? Should the clinical science discourse have priority over all others? If not, why not? What are the ethical issues of Prozac signification? What are the political ones? Who should answer these questions, for whom, and with what claim to legitimacy?

The Time of Cyborgs

To approach these questions and to get some perspective on the Prozac phenomenon, let me start by considering Prozac within the context of a range of new sciences and technologies (or technoscience for short) that have dramatically infiltrated many of our daily lives. Just think about the amount of time you spend in some kind of synergistic interface with a machine. How much time in your day are you *not* on the telephone, at the computer, watching TV, listening to the radio, in the car, on the train, or in a climate-controlled environment? How many thousands of advertisements and commercials have you seen in which happiness is promised through a technological interface—a long-distance phone call, an exciting new car, an opportunity to sit by the ocean (simultaneously connected to a global network on your personal laptop computer). These messages are always the same—technology enhances life and brings smiles . . . for a price.

Increasingly technoscience has infiltrated medicine too. Of course, technology in medicine is nothing new, but the recent explosion of technical capacities in medicine has created a qualitative shift in the practice of medicine. Indeed, we may increasingly understand medicine as a kind of applied technoscience. New biotechnologies—including advanced imaging techniques, genetic manipulations, organ transplantation, artificial

limbs, expanding cosmetic surgeries, and an array of new psychopharmaceuticals—are rapidly turning medicine into technomedicine. In addition, not only has technoscience become a staple of medical diagnosis and treatment, technoscience has also catapulted medicine into an era of physical and mental enhancement. With the further developments of the dawning biotech century, human lifespan, mental and physical abilities, and even personality will be molded in ways that were previously unimaginable (Rifkin, 1998). In this environment, clinicians are in danger of becoming glorified distributors of the new technologies—sort of like new car dealers with a medical certificate.

From my perspective, the twin epidemics of Prozac prescribing and Prozac signification need to be understood in the context of this explosion of technoscience into medicine. Indeed, Prozac is one of the first of the new psychopharmaceuticals to sit uncomfortably between a treatment and an enhancement, between a medication and a mental cosmetic (Kramer, 1997, p. xvi). Situating Prozac within the context of the new technomedicine is not immediately helpful, however, because, unfortunately, the technoscience invasion of medicine has happened so fast, and is so controlled by dominant interests, that the standard medical literature has not caught up with the full complexities of medicine as technoscience or even begun to develop a critical discourse of this phenomenon. Certainly, with regard to specific biotechnologies like Prozac, medical science (working within the rules, norms, and expectations of its own discourse) can tell us something about the drug's pharmacology, therapeutic effects, and common toxicities. And certainly, with regard to prescribing Prozac, medical ethics (working within its usual frame) can help us sort out questions of autonomy and beneficence in the dyadic relation between physician and patient. Neither medical science nor medical

ethics, however, even scratch the surface of articulating the social, cultural, and political dimensions of a medical technoscience like Prozac. At best, there is information available about the use of technomedicine but practically nothing about the creation or cultural effects of technomedicine.

Thus, before further embracing the joys and smiles of technoscience body enhancement, medical science and bioethics should seek a discursive enhancement to better cope (and struggle) with the rise of technomedicine. One discursive option I have found extremely useful in sorting through the Prozac phenomenon is the work of cultural studies of science scholar Donna Haraway. If asked, Haraway might categorize herself as a postmodern feminist science historian of the present. In her writings, she has initiated a great expansion of the cyborg metaphor, and she is a major initiator of what many are calling cyber-feminism and others are calling posthumanism (Braidotti, 1994, p. 102; Halberstam & Livingston, 1995). Before discussing Haraway's cyborg metaphor in more detail, however, it is worth explaining what I mean by "metaphor" in this context, because the surest way to misunderstand Haraway's work is to approach it too "literally" or too "metaphorically" without rethinking the usual meanings of these terms.

Haraway (in the company of most postmodern philosophers and antifoundational theorists and consistent with my discussions in the first four chapters) reverses, rejects, and ultimately displaces the notion that "metaphorical" meaning is significantly different from "literal" meaning. According to Haraway, there are not "metaphorical" meanings and "literal" meanings (separable on deep ontological or epistemological grounds); there are only different possible meaning formations. For Haraway, the proper questions for particular meaning formations (like bioscience), which are always already metaphorical

and literal, are not simply the scientific and epistemological questions of whether the meanings mirror the world independent of human constructs. Rather, the proper questions also include ethical and political questions of what world this kind of meaning formation will create. What effects will this meaning formation have on particular living narratives, and who or what is benefiting (and why) by making meaning this way rather than another way?

Thus, when Haraway says, “By the late twentieth century, our time, a mythic time, we are all chimeras, theorized and fabricated hybrids of machine and organism; in short we are cyborgs. The cyborg is our ontology; it gives us our politics” (1991, p. 150), she means to be both literal and metaphorical at the same time. For Haraway, there is a literal truth to her cyborg claim—something worth struggling over and fighting over—and simultaneously the cyborg metaphor is an “imaginative resource suggesting some very fruitful couplings” (p. 150). In other words, cyborgs make for productive thinking in the current age of dramatic technoscience proliferation. Cyborgs, for Haraway, are cybernetic organisms—systems that embrace living and technological components. Always and inseparably organic and machinic, the cyborg displaces, and renders nonessential, crusty Western binaries like nature/culture, fact/value, pure/contaminated, inorganic/organic, and real/artificial. These distinctions, while useful in the past, do not work well in the current technoscience moment—which effectively blurs all these distinctions. Thus, Haraway uses the cyborg to enter the fray of science politics not by arguing for a repudiation of science or technology (it is way too late for that) but by arguing for mixing up the scientific and technological with the ethical, political, and aesthetic. Considering herself a “child of antiracist, feminist, multicultural, and radical

science movements,” Haraway “yearns for knowledge, freedom, and justice *within* the world of science and technology” (Haraway, 1997, p. 267; italics added). For Haraway, cyborgs effectively cut through much of the theoretical baggage associated with technoscience binary thinking that can inhibit her yearning. The issue for Haraway is not whether the organic and machinic are mixed but *how* they are mixed and to what effect. Also, who is doing the mixing; who is being affected? What are the ethical and political relations between the participants and stakeholders? For Haraway, we may all be cyborgs, but not all cyborg mixings are the same.

Haraway argues that behind the seemingly “natural” evidence of a supposedly objective scientific method, biomedical science is not only culturally constructed, it is also big politics and big business. “Biology,” she reminds us, “is not the body itself but a discourse of the body” (Haraway, 1997, p. 217). For Haraway, bioscience discourse is far from neutral (and far from “progressive”) in its political and cultural alliances in what she calls the “New World Order, Inc.” (p. 2). Indeed, bioscience, while legitimating itself on a rhetoric of “new scientific progress,” is simultaneously bedfellows with many of the old politically regressive power structures of patriarchy, racism, classism, ableism, neocolonialism, and homophobia. These alliances remain invisible, however, if bioscience is able to proceed free and aloof from other critical discourse—free from deep and serious ethical and political questioning, not only about the technical applications of bioscience, but also about what projects to take up, who should develop them, and what the consequences are of handing over so much authority to a realm of science independent of politics.

The Political Dynamics of Prozac

With Haraway's cultural studies of science in mind, let me return to the question of Prozac. What is the relevance of the cyborg metaphor for the recent epidemic of Prozac prescribing and Prozac signification? How do we go from theoretical analysis to practical politics? How can we assess and understand the legitimacy of Prozac and the dominant psychopharmaceutical trends in psychiatry (which Prozac metonymically represents)? Who (and what) are the "we" who will do all of this? For starters, Haraway's cyborg theory helps sort out what won't work. It is of little use to decry the impurity or artificiality of Prozac induced mental states. From Haraway's perspective, humanity (or what may be called "posthumanity" in a cyborg age) in the New World Order, Inc. is too intertwined with technoscience for these distinctions to be of much use. That means that we can get little help from an appeal to grand narratives that attempt to decide, independent of the details, whether Prozac is an appropriate or inappropriate choice. The usual grand narratives for legitimizing or delegitimizing Prozac are narratives of the True or narratives of the Good. With the many Prozac significations available, however, there is no one grand Truth of Prozac. There are instead many situated truths about Prozac. Similarly, there is no single judgment of the Good with regard to Prozac. In some discourses, Prozac is a dawn of light for millions of depression sufferers; in others it is one of world's newest and most insidious evils. This undecidable situation does not mean, of course, that anything goes, and certainly not that all technology should be embraced or rejected. Both technobliss and technophobia are held in tension in a cyborg reading. Because there are multiple undecidable significations of Prozac, we must develop an alternative discourse—besides the natural or the artificial, the true or the false,

the good or the bad—to scaffold and navigate questions of legitimacy in the posthuman world of cyborgs and cyborg technology.

Therefore, what alternatives for legitimizing technoscience discourse arise from Haraway's cyborg philosophy? In short, without recourse to universal truth or universal good, questions of legitimacy come down to local and largely political questions of *effect* and *inclusion*. What are the effects of Prozac? For whom? Who is included and empowered to create legitimate psychiatric knowledge? Who is excluded and why? Analyses of effects and inclusions are mid-level discourses. They do not give permanent or universal solutions, only temporary and situated ones. They result in messy analyses because effects are diffuse and often go in contradictory directions and questions of inclusion are always transient as stakeholder groups are constantly emerging and disbanding.

In the case of Prozac, let me consider the question of effects first. If I start at a broad discursive level, what might be called a cultural semiotic level, one effect of Prozac is to support a psychopharmacologic, or biopsychiatric, discourse of human pain and suffering that has deeply conservative political ramifications. Biopsychiatry as a way of talking about and organizing human pain minimizes the psychological aspects of depression—personal longings, desires, and unfulfilled dreams—and it thoroughly erases social aspects of depression—injustice, oppression, lack of opportunity, lack of social resources, and systematic denigration. Not only that, but biopsychiatry mystifies and naturalizes the scientific (and pharmaceutical) contribution to the discourse on depression, leaving alternative opinions increasingly difficult to sustain. Biopsychiatry, like other scientific discourse (and this is perhaps their most insidious hegemonic effect),

presents itself as a discourse from nowhere. No one claims to decide that depression should be organized primarily around neurophysiology; it is supposed to just be “the way it is.” Alternative opinions become just that, “opinions,” compared not to other opinions but to “facts.”

As a deeply conservative discourse, biopsychiatry benefits the currently dominant groups. To state the case polemically, anyone unhappy with the status quo and the emerging New World Order, Inc. should shut up and take a pill. Of course, who is most unhappy and who represents the highest percentages of depressed persons? Women, people of color, the poor, and other victims of societal biases (Kleinman, 1988, chap. 4). Who would stand to benefit the most from a change in the social order? The same folks. In the bioscience discourse of depression, however, the personal is not political, it is biological. If we plug human suffering, misery, and sadness into the calculus of bioscience, there is no need to make changes in the social order; instead, we need only to jump-start some neurotransmitters. There is no need to reduce social harassment, discrimination, and gross inequities in opportunity; instead, let them have pills. There is no need for workers to take time out from the job for personal healing, reconsidering life choices, or making life changes, no need to build an infrastructure to support those who are unable, for whatever reason, to find ways to support themselves; instead, all people/machines need is to take a pill and get back to the New World Order of hyperactive consumption/production.

All this being said, however, it must be added that it is tricky to polemically read effects directly from a discourse. If semiotic readings are done in a heavy-handed way, they leave out the possibility of negotiated and oppositional readings of dominant

discourse (Hall, 1993, p. 102). Thus, rather than rest with a broad discussion of the discursive currents of Prozac and biopsychiatry, let me try to articulate more specifically who wins and who loses in the case of Prozac.

One of the most clear and least contradictory sites of Prozac effects is the pharmaceutical company Eli Lilly. It can be argued that, more than anyone else, Eli Lilly benefited from the advent of Prozac. Eli Lilly sold \$2.3 billion worth of Prozac in 1996—which was 32% of Eli Lilly's total sales (Eli Lilly and Company, 1998).

To put that in perspective, if that money was spent on psychotherapy, it would employ 23,000 psychotherapists a year (at \$100,000 gross income) to provide 46 million psychotherapy hours. I'm not suggesting that psychotherapy is a simple good, any more than Prozac is a simple good. Psychotherapy, no different from technoscience, is also intertwined in political forces that are barely articulated and critiqued within the psychotherapy discourse community. Perhaps the only thing one could say in favor of psychotherapy is that, compared to biopsychiatry, the earlier era of psychotherapeutic psychiatry was not backed by a major bioscience industry. Indeed, pharmaceutical companies are increasingly taking advantage of their size and capital to aggressively market their products. According to the *New York Times*'s business page, pharmaceutical companies are rapidly transforming themselves from "research-driven companies" to ones that operate "more like Procter & Gamble, the maker of Tide." For these drug companies it is now the "marketing executives, not scientists, who are in charge" (Petersen, 2000, p. 3-1). To give an example of the effect of this change, IMS Health reports that

pharmaceutical company promotional spending directed toward physicians and consumers in the U.S. reached \$13.9 billion in 1999, an 11% increase over 1998.

Total promotional spending includes physician detailing, sampling, and both consumer and physician advertising and promotion. Direct-to-consumer advertising, which accounts for 13% of audited promotional spending, totaled \$1.8 billion, up 40% from the previous year. (IMS Health, 2000)

Though Eli Lilly's Prozac was not in the top 10 in promotional spending in 1999, it was in 1998. My point here, however, is not to get into a detailed comparison of the relative effects and marketing strategies of psychotherapeutic and pharmacologic psychiatry but to show, through the comparison with alternative treatment options like psychotherapy, that, whatever other effects Prozac has, it produces an enormous benefit to Eli Lilly. The money spent on Prozac is money not spent on other options, and the profit to Eli Lilly for their promotional efforts is huge. In 1996, Prozac contributed one-third to Eli Lilly's \$1.5 billion profit Eli Lilly and Company, 1998). With this kind of profit, unless we are to get into the slings and arrows of wealth, there seems to be little need for further discussions of the benefits of Prozac for Eli Lilly.

Studying the further effects of Prozac becomes increasingly muddled because the vectors of effect are less unidirectional. For example, what is the effect of Prozac for clinical psychiatrists? They too benefit in many ways. Clinical psychiatrists are often members of dominant groups (white, male, heterosexual, upper-middle class), and thus they benefit from the general status quo that biopsychiatry supports. In addition, they can charge around \$60–\$75 for a half-hour visit for prescribing Prozac. That's not bad money. \$120 an hour, 40 hours a week, 50 weeks a year, comes to around \$240,000 gross income per year. Not only that, but through their prescription privileges, they get a leg up on their guild rivals—psychologists and social workers. On the other hand, clinical psychiatrists may eventually lose out. No longer known as having skills in

psychotherapy, that service is rapidly going to their rivals. As for the prescribing service they provide, that may eventually be taken over by primary care clinicians, neurologists, psychologists, or nurse practitioners. Thus, clinical psychiatrists are not clear winners here, at least not in the long run. Of course, psychiatrists are no longer (if they ever were) a single group, and clinical psychiatrists are rapidly becoming the group with the least voice among psychiatrists. As if it were coming from a textbook in colonial conquest, psychiatry is being divided into dramatically unequal status groups. These may be articulated as clinical, research, and administrative psychiatrists. Out of these groups, the research and administrative psychiatrists are benefiting the most from Prozac and biopsychiatry: (a) research psychiatrists because of grant money and academic power and (b) administrative psychiatrists because they use biopsychiatry to justify limiting other clinical psychiatric expenses, thus increasing profits for healthcare systems and enhancing their own positions within these systems. Consequently, among psychiatrists, clinicians are most likely to lose out, and this pretty much seems to be the case.

What about consumers? Technomedicine, or more precisely, technoscience capitalism in medicine, like capitalism generally, is rather complicated with regard to consumer benefit. The mantra of business seminars is “Win, Win.” That phrase is supposed to mean that when a business wins, the customer wins as well, and the other way around. Therefore, by this logic, companies do not exploit consumers, companies only help consumers achieve their desires—otherwise a smart consumer would not buy the company’s product. However, as Jean Baudrillard has so effectively pointed out in his “autopsy of homo economicus,” the loophole of the Win-Win mantra is that, in a postmodern consumer society, desire is not fixed and businesses can use a variety of

methods to stimulate desire (Baudrillard, 1988, p. 35). Consider cigarette companies, or auto companies, or soda companies, or computer software companies. Are the desires these companies create necessary? Can those desires be said in any logical way to rest in the consumer? There is a tremendous fluidity of consumer desire, and Baudrillard makes a compelling argument that it is better not to view needs as the stimulus for production, but production as the stimulus of needs: *“the system of needs is the product of the system of production”* (Baudrillard, 1988, p. 42; italics in original).

If Baudrillard is even partially correct, there can be no simple analysis of the effect of Prozac for consumers. How much do “Prozac needs” start with consumers and how much are they stimulated by psychiatry and the pharmaceutical companies? This is an undecidable question, as it is impossible to determine authentic individual needs outside of their cultural context. Thus, there is little theoretical (or political) advantage in celebrating consumer “euphoria.” However, there is certainly no more advantage in a grand critique of consumer “dupes.” In spite of the general conservative discourse of biopsychiatry, the clear advantage to the pharmaceutical industry and powerful psychiatrists, and the capacity of the psychiatric/pharmaceutical alliance to stimulate individual desires, there are many ways that Prozac, like other technoscience, can also empower consumers. For example, consider the situation of the abused woman who gets enough energy and hope through Prozac to stand up to or leave her man. Or, at the larger political level, perhaps the next Simone de Beauvoir, Adrienne Rich, Kwame Nkrumah, or Angela Davis will be on Prozac. Perhaps, without Prozac they will be vulnerable to curl up in a depressive self-loathing rather than change the world.

Still, although consumers may benefit, they are right to be wary of technomedicine. In the case of Prozac, it seems clear that, at the bottom line, Eli Lilly and the most powerful psychiatrists benefit as much if not more than consumers. At best, consumers can hope for a kind of trickle-down benefit. Consumer wariness is also warranted by the unequal power relations between the pharmaceutical companies, powerful psychiatrists, clinical psychiatrists, and consumers. In a conflict between what is good for the consumer and what is good for the pharmaceutical companies or powerful psychiatrists, who do you think will win? Pharmaceutical companies and powerful psychiatrists are likely to put their interest first. This choice to privilege their own interests may be conscious and Machiavellian, but just as likely it may occur in the form of unconscious blind spots to other people's needs relative to their own. That seems to leave two positions for consumers (and, from my perspective, for clinical psychiatrists as well)—outright paranoia and general skepticism. There seems little room for blind trust.

One thing should begin to be clear in this very limited analysis of the “effects” of Prozac. The picture is much more complicated and problematic than the biopsychiatry literature or the drug company advertisements would suggest. Eli Lilly's advertising slogan, “Neuroscience: Improving Lives, Restoring Hope,” may well be true. But improving whose lives and restoring whose hope? Whatever Prozac may be, it is not simple progress, and it cannot claim to be a necessary or a universally true discourse on depression. Biopsychiatry does not have a divine right to the discourse on depression. To be a legitimate discourse of depression, Prozac and biopsychiatry cannot hide behind the curtain of science. They must play fair with other possible discourses.

The Politics of Cyborgs

This brings me to the question of inclusion or what I call the posthuman politics of cyborgs. Cyborg politics are politics of inclusion. If we follow Haraway and other theorists into the “politics of truth,” it becomes clear that one of the most consistent effects of power on truth is the disqualification and prohibition of local and alternative forms of knowledge. As a result, dominant knowledge formations too often arise from dominant groups. As Sandra Harding has put it, “Women and men cannot understand or explain the world we live in or the real choices we have as long as the sciences describe and explain the world primarily from the perspectives of the lives of dominant groups” (Harding, 1991, back cover). In the sausage factory of knowledge production, subordinate knowledges are excluded, thus silencing subordinate groups. In liberal societies, knowledge disqualifications are achieved not primarily through the legal authority of censorship, but, as Foucault reminds us, by the “ensemble of rules according to which the true and the false are separated and specific effects of power are attached to the true” (Foucault, 1980, p. 132). In short, as I discussed at length in chapter 3, knowledge/power works through the existence of a particular politico-economic regime of the production of truth. From this standpoint, the key task in confronting the politics of technoscience is not that of restoring the purity of scientific practice by criticizing its ideological contents, nor, for that matter, attempting to emancipate truth from power. Rather, the task is to “detach the power of truth from the forms of hegemony (social, economic, and cultural) within which it operates at the present time” (Foucault, 1980, p. 133).

Thus, a central task in a posthuman politics of Prozac is to challenge the hegemonic regime of bioscientific (and increasingly administrative and research) psychiatry and their pharmaceutical company supporters. Because there are diminishing opportunities for challenging biopsychiatry within the current psychiatric discourse (the reigning ensemble of rules separating the true and the false no longer permit it), the only remaining opportunity is a politics of activism. Models for this kind of activism exist already in medicine. The medical activisms I have in mind start from the perspective that medicine is, all too often, part of people's problems rather than part of their solutions. These are activisms that build on the strategies midwives have used in their battle against organized OB-GYN physicians and hospitals, that La Leche League groups have used to help make breastfeeding a possible alternative, and that ACT UP (AIDS Coalition To Unleash Power) has used in their battle with medicine over HIV treatment and research. Perhaps the best rallying cry for these activisms has come from the newly emerging disabilities movement: "Nothing about us without us" (Charlton, 1998). This is a cry for inclusion in knowledge formation more than anything else. It rests on the experience that knowledge that excludes key stakeholders too often shifts toward the interests of those included over those excluded. Indeed, in all of these activisms, it is not that medicine is simply wrong or bad, it is more that medicine is too powerful, too hegemonic, too self-serving, and too unresponsive to alternative points of view. In the face of medicine's political power, these medical activist groups, like feminism and other new social movements before them, adopt a variety of strategies. They strive to change people's consciousness. They build networks of opposition and support. They lobby for protective

legislation. In general, they provide a community of resistance to dominant forms of truth and a community of support for alternative knowledge structures.

In the case of Prozac, this kind of “posthuman activism” would ideally have sources and coalitions both internal and external to psychiatry. Internal activism would involve lobbying dominant psychiatry to reduce its alignment with technoscience and with pharmaceutical companies. Activist politics, after all, is a politics of alignment. It is about forming coalitions. Presently, psychiatry is too aligned with the pharmaceutical companies and the technoscience they produce and encourage. Twenty percent of the APA’s budget comes from pharmaceutical companies, and pharmaceutical companies are major supporters of psychiatric research (Breggin, 1991, chap. 15). These bioscience industry dollars, in spite of blanket claims of “unrestricted research support,” profoundly affect the direction of psychiatric knowledge. Internal activism in psychiatry would attempt to loosen the alignment to the drug companies and increase psychiatry’s alignments to patients, consumers, and clinicians. Rather than dominant psychiatrists creating knowledge as unofficial representatives of the drug companies—at conferences funded by drug money or presenting research funded by drug money—psychiatrists would attempt to get more consumer and clinical contribution into psychiatric knowledge. Psychiatry would try to create a knowledge base that includes a variety of points of view. Some of this knowledge would be informed by science, but it would also include knowledge informed by humanities, interpretive social inquiry, and the arts.

New alliances in psychiatry would likely reduce rather than increase consensus in the field. This would occur in direct opposition to the more usual, post-Kuhnian, understanding of progress in science. Consensus in posthuman politics is not seen as a

sign of advance as much as a sign of exclusion. Thus, the goal of psychiatry at the present moment should not be increased consensus but increased appreciation of diversity. To make this work, as I will discuss extensively in the last chapter, the American Psychiatric Association (APA), for example, would have to become (much more than it is now) a forum for diverse opinions about mental suffering rather than continue its attempts to create a single truth about mental illness and a single standard of care. Funding for research inquiry, according to this view, must not be decided by experts within scientific psychiatry alone. Research inquiry must be decided by a more democratic and inclusive process. The resulting APA would be made up of a patchwork of overlapping alliances and knowledges, not one knowledge formation based on a single authorized truth. In this situation, it would be best to speak in the plural and rename the APA as the American Association of Psychiatries.

External activism to psychiatry has already begun. This activism takes the form of grass roots organizations that provide an alternative discourse to psychiatric treatments. One such group is the Survivors of Psychiatry, and another, more specific to Prozac, is the Prozac Survivors group. These groups have Web pages, local chapters, newsletters, conferences, protest rallies, and so forth, and they use them as a kind of cultural politics. Similar to consciousness-raising functions of activists groups, they provide a source of critique to dominant power structures. They read technoscience psychiatry against the grain, deconstruct ideological hierarchies, satirize and poke fun at the dominant position, explore alternative possibilities, and in general form their identity in opposition to the “Other” of psychiatric science.

Both internal and external psychiatric activists must eventually increase their efforts to lobby Congress for protective legislation. As in regulating the cigarette industry, regulating biopsychiatry and the pharmaceutical industry will require many fronts of activity. On the legislative front, we need laws that reduce the capacity of drug companies to advertise and to support conferences and organizations in which they have a direct conflict of interest. Legislation is needed that gives people better work benefits to deal with emotional problems—for example, more time to process a depression rather than being forced back to work as soon as possible. We need legislation that would allow nonbiomedical treatments the same insurance support that mainstream bioscience treatment is given. Legislation is needed that would improve mental health benefits generally—particularly benefits for psychotherapy—which have all but eroded over the same years of Prozac's rise to dominance. Finally, we need legislation that takes seriously the fact that social ills and community distress are huge factors in mental health and well-being.

In(con)clusion. I must admit that the political tasks I have presented here are more suggestive than programmatic. In its simplest form, what I am seeking boils down to a call for the priority of democracy over science in psychiatric knowledge production. Prozac, like other kinds of technoscience, is not clearly oppressive or liberatory. It is both—sometimes one more than another, but always both. This makes the problem not Prozac itself but the politics of knowledge surrounding the discourse of Prozac. Who is getting to speak? Who is being silenced? How can the knowledge production proceed on a more level playing field? How can more diverse groups get involved with the production and application of psychiatric knowledge? Waiting until the technoscience

knowledge is produced, then attempting to regulate knowledge use, is like trying to delete an email after it has been sent. The challenge of technomedicine like Prozac is not only to insure its safe and ethical use, but to create a more level playing field for its knowledge production.

Chapter 7: Selling DSM

Introduction

In the last chapter, I used the “epidemic of signification” surrounding Prozac to illustrate the undecidability of Prozac discourse. My aim was to use a “cultural studies of psychiatry” genre to provide an analytic and cultural wedge for reading the clinical literature on Prozac (and biopsychiatry) against the grain. In this chapter, I use the cultural studies genre to address psychiatry’s diagnostic manual, *Diagnostic and Statistical Manual of Mental Disorders (DSM)*. I shift tactics, however, in that rather than reading *DSM* directly, I will be focusing on a prominent critique of *DSM*. Stuart Kirk and Herb Kutchins’s *The Selling of DSM: The Rhetoric of Science in Psychiatry* (1992) is one of the few sustained critical analyses of *DSM*, because within the psychiatric community (the domain most interested in the topic) the manual has been largely rubber-stamped. There has been considerable disagreement within psychiatry on the details of individual diagnoses but little critical attention to the manual itself. Kirk and Kutchins, however, start from a different position. They are social work professors who have followed with rising concern the transition of *DSM* from relative obscurity to “the most frequently used book among all mental health professionals” (Kirk & Kutchins, 1992, p. ix). Their critique is both focused and wide-ranging. It is at once a reporterly exposé, a scientific analysis, and a disciplinary struggle with psychiatry. As such, it provides a rich resource for detailed consideration of key tensions in recent psychiatric discourse.

My inspiration for starting with Kirk and Kutchins's critique comes from Michel Foucault's suggestion that to understand power relations, in particular the power relations of reason (and, I would add, science), it is most useful to start with "forms of resistance" (Foucault, 1983, p. 211). By "forms of resistance," Foucault means emergent counter-discourses that rise up against an allegedly neutral discourse. In the case of psychiatry, there are many such counter-discourses to choose from: antipsychiatry literature, survivors of psychiatry grass roots groups, women's health movement literature, and critiques of psychiatry from other professional perspectives (like Kirk and Kutchins's). Foucault argues that starting with any of these forms of resistance has several advantages over what might be called an "armchair" philosophical analysis: (a) it avoids the often sterile trap of applying reason against reason, (b) it sidesteps the related trap of being stuck in the role of "rationalist" or "irrationalist," (c) it helps to intermingle theory with practice and practice with theory, and, perhaps most importantly, (d) it works better (p. 210). In other words, rather than trying to do armchair analyses of power relations, Foucault recommends going to the source of conflict within specific discourses.

This method of analyzing discourse, Foucault argues, uses forms of resistance as "chemical catalysts so as to bring to light power relations, locate their position, find out their point of application and the methods used. Rather than analyzing power from the point of view of its internal rationality, it consists of analyzing power relations through the antagonisms of strategies" (1983, p. 211). Accordingly, my goal in turning to Kirk and Kutchins's critique is not to determine the Truth of *DSM* or to develop an alternative to *DSM*. Rather, in Donna Haraway's terms, I seek to articulate "the social relations of [*DSM*'s] science and technology" (1991, p. 165): Whose point of view is being

propagated with *DSM*? Whose is being silenced? Why and to what effect? To articulate these relations, as will become clear, I must not only use Kirk and Kutchins but also critique Kirk and Kutchins.

Kirk and Kutchins's *DSM* Critique

Kirk and Kutchins organize their critique of *DSM* around the “diagnostic reliability problem” that, they argue, the developers of *DSM* created, used, and managed for their own interests. Kirk and Kutchins examine how the “making and selling of *DSM* came about” and how a handful of “influential researchers were able to use a historical moment to claim effectively that diagnostic inconsistency was a serious matter that should be attended to” (Kirk and Kutchins, 1992, p. 13). They start with a review of the scientific and political context of U.S. psychiatry in the 1960s and 70s, which, they argue, the manual’s developers were able to use to their advantage. For Kirk and Kutchins, this was a time of serious “self-doubt” in psychiatry and of great “vulnerability to public and scientific criticism” (p. 13). Though psychiatry had been embattled before—particularly in the 1950s and the early 1960s around critical and widely distributed exposes of state asylums as places of inhumane and brutal treatment—these earlier attacks were primarily challenges of psychiatric managerial and administrative practices. These attacks, along with other factors, led to the deinstitutionalization of psychiatric asylums.

Deinstitutionalization was a major upheaval in psychiatry, but it did not threaten its social foundations. As the 1960s went on, however, several additional attacks arose, attacks that Kirk and Kutchins argue threatened the very foundation of psychiatry’s medical and scientific legitimacy.

The post-deinstitutionalization attacks ranged from the conceptual critiques of Thomas Szasz's "myth of mental illness" and sociologist Thomas Sheff's "labeling theory" of mental illness to the broader historical and political critiques of philosopher Michel Foucault. When these challenges were combined with several high-profile criminal trials (such as the John Hinckley trial—in which psychiatrists gave diametrically opposing testimony) and the widely publicized disagreement in the psychiatric community around homosexuality, it created a climate ripe for the *DSM* developers to exploit. In Kirk and Kutchins's words,

these pointed attacks constituted a much more fundamental attack on psychiatry than criticisms of clinical effectiveness or its hospitals. Services can always be improved, access to them for the poor arranged, and patients' rights protected. On the other hand, if mental illness does not exist, if psychiatric symptoms have little to do with medical science, if the entire mental health enterprise is a carefully structured fiction about life's normal troubles, and if psychiatrists are policemen in white coats, then psychiatry confronts a much more serious problem. (p. 22)

Kirk and Kutchins argue that these attacks effectively challenged the conceptual integrity of psychiatry as an enterprise, and this left many psychiatrists feeling that psychiatry itself was in critical condition.

In this context, the problem of "diagnostic reliability" began to take on major proportions within psychiatry. But how, exactly, did that come about? By historical coincidence, simultaneous with these external attacks, psychiatry embarked on an internal project of revising its diagnostic manual. Diagnostic revision had happened in the past, but this particular revision of the manual was to change greatly the fortunes of *DSM*. Through the 1960s, *DSM* served a minimal role in psychiatry. *DSM-I* (1952) and *DSM-II* (1968) were small documents with brief descriptions of diagnostic categories that served

largely documentary and administrative purposes. After the 1968 revision of *DSM-II*, however, there was a push for a major overhaul and a call for a much more extensive manual. Kirk and Kutchins argue that the push for change drew momentum from psychiatry's insecurities and vulnerabilities. A key feature of this argument centers on how the *DSM-III* developers transformed psychiatry's multiple conceptual and political problems into a new form and a new problem: *the reliability problem*. Kirk and Kutchins detail how *DSM-III* developers claimed that "without diagnostic reliability" no further progress could be made in psychiatry and psychiatry could not stand up to its critics. Thus, *DSM-III* developers transformed the reliability problem into the key "symbol of the profession's self-doubts" (p. 13). In addition, *DSM-III* developers translated the reliability problem into a technical problem that they promised to solve through complex social science research methodology (the details of which proved convoluted and complicated). In the end, the developers used these social science research methods to demonstrate that prior psychiatric reliability was unacceptable, that more complex criteria of evaluation and measures of agreement were needed, and that only those investigators with sophisticated research backgrounds could be expected to solve psychiatry's dire reliability problem.

Consequently, psychiatry's thick conceptual and political problems (whose critique was gaining momentum from several quarters) were rearticulated into the thin, but all-consuming, technical problem of reliability. Kirk and Kutchins point to two advantages (one for psychiatry as a profession and the other for psychiatric researchers as a subset of the profession) of transforming psychiatry's problems into technical reliability problems:

The first was that it appeared to be more solvable than problems of [conceptual] validity, at least in controlled research settings. The second advantage, and unintended by-product of many scientific advances [like *DSM-III*], was that the technical solutions proposed and the gauge developed to measure their success were beyond the easy comprehension of clinicians and public alike. (p. 35)

Thus, transforming psychiatry's conceptual and political problems into a technical problem of reliability has the effect of deskilling clinical and lay assessments of mental and emotional suffering. The reliability problem effectively moved the debate from public concerns about psychiatry as a whole to private laboratory investigations of technical psychiatric research questions.

As an added bonus, the reliability problem simultaneously guarantees a prominent role in psychiatry for researchers. Kirk and Kutchins explain that "the [reliability] problem was embedded in a closely knit research community, which accepted responsibility for solving the problem, on its own terms and in its own territory" (p. 44). *DSM* developers created a world in which the mysteries of psychiatry, once transferred into narrow questions of reliability, were to be solved by superior techniques, rigorous control, and the right kind of training. Thus, the reliability problem and the new manual (*DSM-III*) designed to address it placed research psychiatrists center stage. By emphasizing the sorry state of psychiatry in the past and claiming they could do better, research psychiatrists made a place for themselves at the top of the psychiatric hierarchy. For Kirk and Kutchins, research psychiatrists effectively

undermined the objections of their opponents, particularly psychotherapists with a Freudian orientation, who constituted the majority of the APA. The eventual coup, led by psychiatric researchers, successfully used the language, paradigms, and technology of research to gain influence over clinical language and practice.

Thus, *DSM-III* was presented not only as a solution to the problem of psychiatric reliability, but as the embodiment of a new science of psychiatry. (p. 14)

With great political savvy, research psychiatry used the reliability problem to transform psychiatry and to place themselves at the top of the psychiatric heap.

According to Kirk and Kutchins, *DSM-III* developers accomplished this most remarkable transformation of psychiatry through strategic rhetorical manipulations and distortions of key facts and statistics. Kirk and Kutchins devote almost half of their text to critically examining the often-repeated claims of *DSM-III* developers that their new manual was a tremendous improvement over older methods. They focus much of their attention on the field trials of the manual, which constitute the linchpin of the developers' evidence for having improved diagnostic reliability. Kirk and Kutchins's reanalysis of this data concludes that "even using the modest standards [of improvement] suggested by the developers, we find that the studies so frequently cited to claim success in resolving the reliability problem were flawed, incompletely reported, and inconsistent" (p. 15). Kirk and Kutchins reach this conclusion by a compelling demonstration of the way *DSM-III* developers gave misleading interpretations of their field trial data, interpretations that greatly exaggerated the new manual's success. The developers' usual discursive modus operandi was to make "bold claims about equivocal data" and to shift the standards of success in a direction that "made it easy for sophisticated and respected investigators to control the strategic use of these data" (p. 15). As a result, the field trial data became mystified, and "would-be critics found themselves persuaded by the sanguine interpretations offered by the developers of *DSM*. Despite the apparent weaknesses of the scientific evidence supporting the bold claims of its developers, *DSM-III* capped a successful revolution in psychiatry" (p. 15).

In their analysis, Kirk and Kutchins make a bright-line distinction between the “facts” of *DSM-III* field trials and the “rhetoric” used to describe these facts. When they examine the field trials, their question is straightforward: “Was the new diagnostic reliability as clear and convincing as it was described by the proponents of *DSM-III*?” (p. 141). Kirk and Kutchins go back to the reliability data and ask (in effect), “Where’s the beef?” *DSM-III* developers say that they have improved diagnostic reliability; what is the empirical evidence for that claim? Kirk and Kutchins find no beef. Instead, they find a “gross inconsistency between the answers offered by the developers and the empirical facts” (p. 141). In other words, Kirk and Kutchins conclude that the facts of the field trials have been rhetorically distorted. Rather than a balanced report of the results, *DSM* developers use a “language which is all positive. Even in the text where they acknowledge [equivocal data], the authors quickly obscure them in a tide of good news” (p. 74). Kirk and Kutchins frequently find the developers’ “rhetoric of interpretation” uses evaluative terms like “very high, quite satisfactory, and amazingly high” in a grossly misleading fashion in order to vastly inflate the results of their field trials, and they contrast these misleading interpretations with more “accurate summaries” of data that could have been given (pp. 74, 66).

Thus, Kirk and Kutchins’s critique of *DSM-III* is an internal critique that uses many of the same assumptions as the *DSM* developers. The main difference between Kirk and Kutchins’s analysis and the *DSM-III* developers’ is that Kirk and Kutchins reach different conclusions, which they argue are more true to the facts of the research. The developers’ scientific method itself is not questioned by Kirk and Kutchins. They are concerned about the way the scientific method was used. Thus, the problem for Kirk and

Kutchins, in feminist philosopher of science Sandra Harding's terms, is not "science as usual;" it is "bad science" (Harding, 1986, p. 25). For Kirk and Kutchins, *DSM-III* developers usurped their power and irresponsibly acted in a way that promoted the self-interests of psychiatrists in general (in relation to other mental health providers) and research psychiatrists like themselves in particular (in relation to other psychiatrists). For Kirk and Kutchins, this is a methodological and ethical violation of the principles of science. It is not a problem with science itself as used in the making of DSM. Kirk and Kutchins's solution for "bad science" is more (and better) science. Harding would likely characterize Kirk and Kutchins's position as follows: "if scientists would just follow more rigorously and carefully the existing methods and norms of research" any bias in scientific knowledge would correct itself (Harding, 1993, p. 51). Indeed, that is what Kirk and Kutchins attempt to do. By more rigorously reviewing the field trials, they hope to correct for the bias of self-interest in the *DSM* developers' reports.

Critiquing Kirk and Kutchins's Critique

Although I find many of Kirk and Kutchins's internal empiricist critiques persuasive, when I approach *DSM-III*'s development from a retheorized perspective, two additional issues, beyond internal critique, also emerge in the foreground. The first and perhaps most important (since it sets up the second) is reconsidering the sharp "fact" versus "rhetoric" distinction from which Kirk and Kutchins are working. The second is a more active critique of the manual's natural science model and of the consequences of organizing the manual along natural science lines.

Starting with the fact/rhetoric distinction, even though Kirk and Kutchins subtitle their book "The Rhetoric of Science in Psychiatry," they do not, from a retheorized

perspective, sufficiently consider the role of rhetorical language in the *DSM-III* developers' scientific method of knowledge production. They consider the rhetoric of the results but not the rhetoric of the method. This happens, I believe, because *rhetoric*, for Kirk and Kutchins, is an embellishment or perhaps a commentary on the data; it is not integral to the data itself. Kirk and Kutchins do not argue that the *DSM-III* developers' rhetorical approach to diagnosis significantly affects their facts, and they do not argue that alternative rhetorical approaches to inquiry would have produced alternative facts. Rather, they argue that the facts and the method of producing the facts are fine. The problem is that *DSM-III* developers reported these facts with misleading rhetorical excess and exaggeration.

Kirk and Kutchins's perspective on "facts" or "truth" and "rhetoric" has a long heritage in philosophy, science, and rhetorical theory, but there is another way to consider the fact/rhetoric relationship. Indeed, recent work in rhetorical theory has built extensively on the rhetorical implications of the emergence of "theory" across the human sciences to do just that (Gaonkar, 1990). The key conclusion from this recent work—which Barry Brummet calls "postmodern rhetoric" and John Nelson and Allan Megill call the "rhetoric of inquiry"—is that the relation between "rhetoric" and "facts" (or "rhetoric" and "truth") is better seen as intertwined than as extrinsic (Brummett, 1999; Nelson, Megill, & McCloskey, 1987).

Literary theorist Paul de Man's work crosses over between this recent "rhetorical turn" and the postmodern theory I have been advocating. Thus, to highlight the importance of this recent rhetorical theory, let me contrast Kirk and Kutchins's approach to Paul de Man's discussion of rhetoric in his paper "The Resistance to Theory" (1986).

De Man details the role of rhetoric in the classical trivium—which divided the science of language into logic, grammar, and rhetoric. Of these three, it was logic that linked the trivium with the quadrivium (the “nonverbal” sciences of number, space, motion, and time). In logic, the rigor of linguistic discourse about itself was thought to match up with the rigor of mathematical discourse. Accordingly, in classical thought, logic and facts are linked. Seventeenth-century epistemology further idealized this connection and came to hold that, the more one’s reasoning is geometrical or logical, the more it is reliable and infallible. Indeed, in the words of philosopher Blaise Pascal, geometrical reasoning is “the only mode of reasoning that is infallible because it is the only one to adhere to the true method, whereas all other ones are by natural necessity in a degree of confusion of which only geometrical minds can be aware” (quoted in de Man, 1986, p. 102). Thus, there is a link in modern Western thought between the “science of language conceived as definitional logic, the precondition for a correct axiomatic-deductive, [and] synthetic reasoning” (p. 102). If there is a link in classical and 17th-century thought between logic and natural science, or logic and fact, however, what has been the link between logic and the other two divisions of language: grammar and rhetoric?

De Man argues that logic is further linked with grammar in the classical trivium, and this link continues to dominate through the present day. For de Man, there has been a “persistent symbiosis between grammar and logic. . . . The grammatical and the logical functions are coextensive. Grammar is an isotope of logic . . . [and] grammar stands in the service of logic which, in turn, allows for the passage to the knowledge of the world” (p. 103). From this perspective, grammar, like logic, is a necessary precondition for scientific and humanistic knowledge. Rhetoric, by contrast, is seen as distinct from grammar and

logic. Rhetoric is a “mere adjunct [and] a mere ornament” to the epistemological functioning of language (p. 103). Grammar and logic serve to link language to the real world outside language, and in classical thought up to the present, both forms of language serve to secure knowledge and facts. From this perspective, however, rhetoric is very different. As an ornament and adjunct to knowledge and facts, rhetoric is separated from logic and grammar. This separation also separates rhetoric from fact, and the functioning of rhetoric is removed from the epistemological realm. This tradition is consistent with Kirk and Kutchins’s approach to rhetoric. For Kirk and Kutchins, the *DSM* developers’ use of “rhetorical excess” is not part of the epistemological realm of the facts of the *DSM*. The rhetorical excess is a mere adjunct and, in this case, a misleading adjunct at that.

De Man outlines how difficulties in this tradition occur with the rise of “theory” in the humanities. Theory, de Man explains (and as I discussed at length in the first three chapters), introduces Saussure’s relational approach to language and introduces the inherent tropological dimensions of language. Because recent theory has seen the relational and the tropological as central to the functioning of language and because these dimensions of language fall under the rhetorical category of language, theory has the effect of *reworking the separation* of rhetoric from grammar and logic and, simultaneously, the separation of rhetoric from fact. In other words, rhetoric after theory is no longer separable from the epistemological dimensions of language. Similar to my discussion of Saussure’s relational theory of the sign in chapter 2, de Man argues that the tropological is internal to the functioning of language. Language is the medium of knowledge, and the possibility of separating language from knowledge is blocked. Thus,

for de Man, “tropes pertain primordially to language” (p. 103) and they are inherent in the text.

De Man makes an additional connection between the tropological dimension of language and the process of reading. For de Man, the reason many people resist theory is that they resist exposing the choices and organizational alternatives that are unleashed through the inherently tropological and relational dimensions of language. De Man develops this idea by connecting resistance to both theory and the tropological dimensions of texts to a fundamental “resistance to reading” (p. 103). In this context, “reading,” for de Man, is an active process that exposes the choices being made in how knowledge is organized. Thus, acknowledging the tropological reverses the usual hierarchy between authors and readers. Authors, from this perspective, do not have complete authority over the organizational tropes in their texts. Through the tropes they use, authors make organizational selections, but once the selections are recognized, readers are not forced to agree with these selections. They may select alternative possibilities.

Similarly to Lyotard (see chapter 4), de Man argues that the tropic is the figural. The tropic is that unavoidable aspect of linguistic signs that works through comparison and linkage rather than correspondence. Comparison and linkage, in contrast to correspondence, are more fluid because central linguistic tropes, such as metaphor and metonymy, organize meaning through similarity and association. If I refer to the man at the lunch counter as a “ham sandwich,” I am organizing the way the man is perceived by making a connection between him and the ham sandwich. This connection is not, de Man would argue, simply ornamental. It is epistemological as well. Nevertheless, it is different

from a purely correspondence epistemology in that there is no single necessary essence of the man independent of the tropological. I may read the man at the counter differently. If, for example, I refer to the same man as a “schizophrenic,” I make a new set of links. Both or neither of these designations (“ham sandwich” and “schizophrenic”) may be intelligible or useful within a given cultural and linguistic context. Key for de Man is that some kind of trope is required for meaning, but neither the particular choices of “ham sandwich” nor “schizophrenia” are necessary. Which particular trope is used matters a great deal, however, because how the man is known will depend on the tropological dimensions of the language used. There is no reaching the man without the tropological and the “truth” of the trope is always undecidable. Thus, the difference between the two possible descriptions is structured by the tropes involved, and it matters which trope is used. Accordingly, the reader must decide and cannot leave it to the author’s choice. As de Man would argue, this is “not only an exercise in semantics, but in what the text actually does to us” (p. 105).

De Man gives several reasons this figural or rhetorical dimension of language is resisted: “It upsets rooted ideologies by revealing the mechanics of their workings; it goes against a powerful philosophical tradition, . . . [and] it blurs the borders of literary and nonliterary discourse” (p. 101). As a result, it exposes the connections between ideologies and allegedly neutral discourse. If one puts these reasons together, they become de Man’s “resistance to reading” (p. 103). Resistance to reading is a resistance to uniting rhetoric with logic and grammar and, ultimately, with the sciences. It is a resistance to the inescapable contingency at the heart of all discourse (even a theoretical discourse like de Man’s), and, simultaneously, it is a resistance to the inescapable

totalizing effects of discourse. All discourse, even theoretical discourse, has an inescapable will to power. Resistance to the rhetorical dimensions of language is a resistance to its human authorship and a resistance to responsibility for that authorship. Resistance to the rhetorical dimensions of language holds on to the illusion that something nonhuman forced the discourse in the singular direction it has taken. Clearly, human authorship cannot go anywhere it pleases, but human authorship has many possibilities open to it. Resistance to reading is a resistance to the freedoms of authorship.

As should be clear, de Man's discussion of rhetoric overlaps with the retheorized rhetoric of inquiry approach I am recommending in this text. Thus, from a retheorized perspective, Kirk and Kutchins are as resistant to the rhetorical dimension of language as are *DSM-III*'s developers. Kirk and Kutchins do hope to expose ideological connections within *DSM-III*'s allegedly neutral discourse, but they do so without challenging the powerful philosophical tradition of separating facts from values and truths from fictions. As such, Kirk and Kutchins, like the *DSM-III* developers, attempt to keep rhetoric separate from science. And like the *DSM-III* developers, Kirk and Kutchins resist reading their own authorship. They present their critique as simply forced by the facts of the matter, not by their own preferences, values, alternative perspectives, and so on. Thus, Kirk and Kutchins resist the contingency of their work, and they resist their own will to power.

What implications would follow if Kirk and Kutchins had taken a retheorized turn and accordingly blurred the fact/rhetoric distinction? This question opens up my second main difficulty with Kirk and Kutchins's efforts. For simplicity, I will organize my concerns along the "three C's model" I discussed in chapter 3. In a nutshell, if Kirk and

Kutchins had blurred the fact/rhetoric distinction, they could have better opened up their critique to the three C's (correspondence, coherence, and consequences). Because they resist rhetoric, Kirk and Kutchins devote the greatest part of their critique to the principle of correspondence alone. I do not mean this as a complete dismissal of their work. Indeed, their efforts are a good example of how correspondence critique can be done. Staying within the language of the developers, Kirk and Kutchins show how the data collected and analyzed by the developers' own methods does not match (does not correspond) with the developers' claims regarding that data. By staying within the same theoretical frame, however, Kirk and Kutchins ignore the problem of rhetorical (tropic, figural, and associational) shaping of knowledge. In other words, they ignore the problem of "theory-laden" data. A correspondence critique does not question the data's theory-laden aspects. Rather, it questions the data's accuracy within the theoretical frame being used. The same theoretical frame found in the original work is included in the critique. Algebraically, one might say, the theoretical frame cancels itself out because it is present on both sides of the analysis. Therefore, I see no way to do a "correspondence" critique from a different theoretical frame. Critiquing theoretical frames and recommending changed theoretical frames introduces a coherence critique.

The principle of coherence, then, as I am using it, asks, What rhetorical tradition is being followed? What rhetorical tradition is being used to perceive, organize, manipulate, and interpret the data? In other words, with what rhetorical tradition or theoretical frame does the knowledge being analyzed cohere? Kirk and Kutchins do not raise the question of research tradition concerning *DSM-III*. As Brummett makes clear, however, rhetorical choices are always "double" choices. On the one hand, they represent

choices about the “reality” they advocate, and on the other hand they represent choices about the proper “methods,” or research traditions, for reaching and legitimizing that reality (Brummett, 1999, p. 166). Perhaps the most obvious (but certainly not the only) alternative method, or alternative research tradition, Kirk and Kutchins fail to bring up is the interpretive tradition in human research. *DSM-III* developers stay within a natural science tradition of human research, but it is far from obvious that the natural science tradition is the best way to do human research (Braybooke, 1987; Dallmayr, 1977; Polkinghorne, 1983). If *DSM-III* developers used interpretive methods, such as qualitative research or action research models (not to mention critical methods or cultural studies methods), the manual would have turned out very differently. Because Kirk and Kutchins do not bring in the question of alternative research traditions, the only coherence critique available for them is the question of whether *DSM-III* developers are consistent with the natural science model from which they work. The short answer to this question seems to be “yes.” There does not seem to be a problem with *DSM-III* developers’ coherence to the natural science model. *DSM-III* developers are working within a natural science model of social science, and their work coheres to that model. Thus, Kirk and Kutchins do not argue the coherence question. From my perspective, this is a significant loss for their critique. I say this not because I find *DSM-III* incoherent with a natural science model but because I question whether a natural science model is the best model to use.

In addition, if Kirk and Kutchins had addressed alternative research traditions, they could have expanded their consequential critique. Kirk and Kutchins’s primary consequential critique, as I have discussed, is that through distortion and manipulation of

the data, *DSM*'s developers artificially raised the prestige of psychiatry relative to other mental health professions and created a situation in which inordinate control over the psychiatric profession went to research psychiatrists. In this critique, Kirk and Kutchins are limited to the consequences of *DSM-III* developers misusing their data and distorting their results. They fail to address the additional consequential issue of what the consequences are of a single research tradition dominating the field of psychiatry. The rise of the *DSM-III* has been intimately connected with the rise of scientific psychiatry. What are the consequences, both positive and negative, of such a model for psychiatry relative to other possible models? What would be the advantages of alternative or additional models? These very important consequential questions do not arise in Kirk and Kutchins. It is here that we see that the limitation of Kirk and Kutchins's coherence critique is also a limitation of their consequential critique.

How could Kirk and Kutchins expand their coherence and consequential critiques of *DSM-III*? This question takes me back to Foucault's insight that initiated this chapter, and it opens the door to insights found in recent sociology of science work. As I discussed earlier, Foucault argues that the power relations inherent in forms of reason can be demonstrated most clearly by exploring those areas of emergent critique that rise against the dominant form. The power relations between these discourses (dominant and alternative) reflect the power relations encoded within the dominant discourse. In other words, the dominant discourse's repression, suppression, and denial of key issues and concerns within the alternative discourse reveal the power motivations of the dominant discourse. The dominant discourse does not develop neutral methodological distinctions, priorities, and heuristics outside of a field of power and only later hold to these

methodological styles with the tenacity of a battle. The very distinctions, priorities, and heuristics are part of the power struggle between dominant and alternative approaches. Thus, when Kirk and Kutchins limit themselves to a correspondence critique, they are able to critique the *DSM-III* developers' results but not their methods. They miss the opportunity to argue for alternative discourses and the opportunity to discuss the negative consequences of the *DSM-III* developers' exclusive reliance on a natural science tradition for human study.

It is not enough, though, to speak of dominant and alternative "discourse" alone. In other words, it is not enough to stay at a "rhetorical level." If we are going to go further than Kirk and Kutchins's correspondence critique, and open their work to a more robust coherence and consequential critique, we need to animate the *DSM-III* discourse and give it life. In other words, we must further develop Kirk and Kutchins's political critique. Bruno Latour's recent work in the sociology of science provides a key resource here. In his discussion of ethnographies of science, Latour concludes that the "first rule of method" in studying seemingly neutral claims within science is to

start with a textbook sentence which is devoid of any trace of fabrication, construction or ownership; we then put it in quotation marks, surround it with a bubble, place it in the mouth of someone who speaks; then we place them all in a specific situation, somewhere in time and space, surrounded by equipment, machines, colleagues; then when the controversy heats up a bit we look to where the disputing people go and what sort of new element they fetch, recruit or seduce in order to convince their colleagues; then we see how the people being convinced stop discussing with one another: situations, localizations, even people start being slowly erased; on the last picture we see a new sentence, without any quotation marks, written in a textbook similar to the one we started with in the first picture. (1987, p. 15)

By putting “neutral” discourse back into the “mouth of someone who speaks,” Latour’s first rule animates the discourse in question and moves us from textual or discourse analysis to political analysis of individuals and groups.

If we apply Latour’s first method to *DSM-III*, what mouth gets the bubble and who does this mouth fetch, recruit, or seduce? Although Kirk and Kutchins do not pursue political issues as political issues per se (because they remain primarily focused on a scientific critique of the manual), they do give us ample information to answer the “bubble-mouth” question. Based on their research into the new manual’s development, Kirk and Kutchins give the top *DSM-III* bubble mouth to Robert Spitzer. A career psychiatric researcher devoted to problems of nosology and classification, Spitzer was the leader of a group of New York academic psychiatrists associated with Columbia University who developed structured interviews and behavioral diagnostic criteria. Allen Frances, the psychiatrist in charge of *DSM-IV*, has described Spitzer as a “man whose entire life, private and public, personal and professional, is occupied with diagnosis and particularly with *DSM*” (p. 91). Indeed, Spitzer’s involvement with the diagnostic manual came early in his career and dates back to the 1960s, when he was a major participant in developing *DSM-II*. After *DSM-II*’s publication, however, Spitzer became one of the manual’s biggest antagonists. In 1974 Spitzer published a paper offering a scathing critique of past diagnostic reliability studies based on *DSM-II*, and later that same year he was chosen to head the *DSM-III* task force. Kirk and Kutchins argue that Spitzer’s task force appointment was “one of the most important committee assignments in psychiatry in the twentieth century” and that his “role cannot be ignored in any discussion of the evolution of modern psychiatric diagnosis” (Kirk & Kutchins, 1992, pp. 63, 90).

Spitzer wrote the introduction for *DSM-III*, and it is from this text that I have chosen the quotes to be bubbled:

DSM-III reflects an increased commitment in our field to reliance on data as the basis for understanding mental disorders. (American Psychiatric Association, 1980, p. 1)

[*DSM-III*] task force members, and consultants from the fields of psychology and epidemiology, were selected because of their special interest in the various aspects of diagnosis. Most had made significant contributions to the literature on diagnosis. (p. 2)

In the past, new classifications of mental disorders have not been extensively subjected to clinical trials before official adoption. The task force believed that field trials using drafts of *DSM-III* should be conducted during the development process to identify problem areas in the classification and to try out solutions to these problems. In addition, because of the many proposed changes in the classification, it was important to demonstrate its clinical acceptability and usefulness in a variety of settings by clinicians of varying theoretical orientations. For these reasons, a series of field trials was conducted, beginning in 1977 and culminating in a two-year NIMH-sponsored field trial from September 1977 to September 1979. In all 12,667 patients were evaluated by approximately 550 clinicians, 474 of whom were in 212 different facilities, using successive drafts of *DSM-III*. . . . The results indicated that the great majority of participants, regardless of theoretical orientation, had a favorable response to *DSM-III*. (p. 5)

This is the official narrative of *DSM-III*'s development. If we clearly identify this narrative with Spitzer's authorship and give a more historically thick background, what do we find? Within four months of Spitzer's selection as chair of the *DSM-III* task force, he had fetched (recruited or seduced) the members of the new committee. He chose a group of five psychiatrists. All had similar research interests and all believed that

psychiatric diagnosis should be based on allegedly theory-neutral behavioral criteria. One of the members, Saslow was known in psychiatry for his coauthored paper in 1965 entitled “Behavioural Diagnosis” (Kanfer & Saslow, 1965). Two other members, Spitzer himself and one of his colleagues, Klein, were from the Columbia University research group. Klein, similar to the other Columbia researchers, was a leading spokesperson for bioscience psychiatry and behavioral approaches to diagnosis. The two remaining members, Andreasen (who is now the editor of the leading professional journal in psychiatry and author of the major psychiatric textbook I discussed in chapter 3) and Woodruff, were associated with a team of psychiatric researchers at Washington University in St. Louis. Like Spitzer’s Columbia group, the St. Louis researchers were devoted to psychiatric nosology and behavioral criteria for diagnosis. At the time, this highly behavioral and scientific approach represented a narrow section of psychiatry. They were, in Kirk and Kutchins’s terms, a “minority among a minority” (Kirk & Kutchins, 1992, pp. 49, 98.).

Thus, Spitzer’s task force was composed of an “invisible college” of like-minded researchers chosen from a narrow band of available possibilities (Kirk & Kutchins, 1992, p. 98). They represented a new direction for psychiatry, and they were so aggressively sure of the superiority of their methods that they referred to themselves as the “Young Turks.” These young turks made it their project not only to redo the manual but to revamp psychiatry (p. 81). In 1978, psychiatrist Gerald Klerman dubbed these psychiatrists “Neo-Kraepelinians” and outlined the young turks’ implicit “credo.” Klerman’s outline of the Neo-Kraepelin credo is worth quoting in full because it demonstrates the overlap between the diagnostic mindset of the *DSM-III* task force and

the eventual “scientific psychiatry” that I have been trying to retheorize. According to Klerman, the Neo-Kraepelin credo is:

1. Psychiatry is a branch of medicine.
2. Psychiatry should utilize modern scientific methodologies and base its practice on scientific knowledge.
3. Psychiatry treats people who are sick and who require treatment for mental illness.
4. There is a boundary between normal and sick.
5. There are discrete mental illnesses. Mental illnesses are not myths. There is not one but many mental illnesses. It is the task of scientific psychiatry, as of other medical specialties, to investigate the causes, diagnosis, and treatment of these mental illnesses.
6. The focus of psychiatric physicians should be particularly on the biological aspects of mental illnesses.
7. There should be an explicit and intentional concern with diagnosis and classification.
8. Diagnostic criteria should be codified, and a legitimate and valued area of research should be to validate such criteria by various techniques. Further, departments of psychiatry in medical schools should teach these criteria and not depreciate them, as has been the case for many years.
9. In research efforts directed at improving the reliability and validity of diagnosis and classification, statistical techniques should be utilized. (quoted in Kirk and Kutchins, 1992, p. 50)

As this credo demonstrates, the stakes for psychiatry were high. Thus, the *DSM-III* task force was not simply developing a new scientific nosology; it was also creating a new kind of psychiatry. Cleansed of subtlety, conflict, ambivalence, and uncertainty,

Neo-Kraepelinian scientific psychiatry is a polemic that passes itself off as neutral. The eventual success of this model was wrapped up in the eventual success of *DSM-III*.

Spitzer's choice of membership for the initial task force demonstrates an added dimension of the Neo-Kraepelinian theoretical cleansing. Spitzer was not only cleansing ideas, he was cleansing people. Spitzer's cleansing was not so much ethnic cleansing (at least not on a manifest level) as ideological cleansing. Spitzer's task force carefully eliminated any theoretical diversity, including the psychoanalytical psychotherapy perspective, which dominated psychiatry at that time, to create a mono-ideological committee. Kirk and Kutchins put it this way, "Among the five original psychiatrists on the task force, there was a remarkable congruence of interest. More importantly, there were no major divergent viewpoints, and the primary psychodynamic perspectives in psychiatry had no representative at the table" (p. 98). Once Spitzer recruited his task force, he wasted no time reworking the manual in his Neo-Kraepelinian image. Within one year after the *DSM-III* task force was formed, they completed the first draft of the new manual. The draft was officially tentative, but it was no mere rough draft or provisional starting point, because it successfully incorporated all the major innovations that were eventually included in *DSM-III*. As Kirk and Kutchins point out, "Although another five years passed before the manual was published, the essential decisions about its approach, structure, and contents were made quickly by Spitzer and this small group" (p. 99). All the basic conceptual schemata and distinctive features of the new manual were put in place by this powerful and strategically placed minority of like-minded psychiatrists.

After such a quick start, what happened over the next five years? To put it bluntly, during this period the initial task force covered its tracks. The initial draft was followed by a long tortuous process of refining the manual and obtaining official approval. Key to this process was the use of field trials to test the manual. I have already discussed Kirk and Kutchins's concern regarding the exaggerated claims of the field trials. Here, I want to highlight that the field-trial approach to verification focused on justification rather than discovery. The emphasis on justification effectively covered over the fact that only a very narrow band of participants were involved in the manual's initial discovery. Spitzer says that "12,667 patients were evaluated by approximately 550 clinicians," and the back appendix of the *DSM-III* lists hundreds of contributors to the manual. This gives the appearance of a broad base of involvement in the manual's creation. However, almost all of these names (all but five) are people who were involved in the field trials rather than people involved in the initial draft of the manual. These people "tested" the manual according to the rules, norms, and priorities of the initial task force. They did not create the manual. Thus, the five-year period between the *DSM-III*'s initial draft and its subsequent ratification and publication gives the false impression that the manual was developed by a broad base within the psychiatric community.

Though Kirk and Kutchins give enough information to sketch out the politics of *DSM-III*'s development, their main motivation for doing so is still wrapped up in their correspondence critique. For Kirk and Kutchins, the relevance of these internal psychiatric politics is that Spitzer and the Neo-Kraepelinians falsely elevated themselves above alternative approaches through distorted data. If Kirk and Kutchins had more fully included the role of rhetoric in knowledge formations, however, they would also have

been able to critique *Spitzer's autocratic and exclusionary politics*. They could have done this not simply because Spitzer's data do not support such a politics but more fundamentally because these politics are problematic at the level of the politics themselves. In other words, Spitzer's politics are bad (have bad consequences) for psychiatry because his politics are bad—too totalitarian. At this level of critique, and this is key, the science question is beside the point. Spitzer and the Neo-Kraepelinians need to be critiqued not only on scientific grounds but on political grounds as well. Kirk and Kutchins may have persuaded us that Spitzer's science was bad, but improving his science will not improve his politics. That will require specific attention to the politics of science and knowledge.

Conclusion

Kirk and Kutchins provide invaluable tools for critiquing psychiatry's new diagnostic manual. Their work effectively questions the fundamental premise—increased reliability—on which that manual stands. From a retheorized perspective, however, though their critique is wide-ranging and though it purports to address the “rhetoric of science,” it falls short on both the rhetorical and political dimensions of the new manual. Thus, Kirk and Kutchins do a much better job with a correspondence critique, as I have outlined that term, than they do with a coherence or a consequences critique. Kirk and Kutchins do not challenge science as usual in psychiatric research. If we take retheorized perspectives seriously, however, science as usual in psychiatry must change. As Kirk and Kutchins show (though they do not theorize it), science as usual is creating a psychiatry that does not include or respect alternative perspectives. In the next chapter, I will turn to the theoretical and practical issues involved in creating a more inclusive psychiatric

research community. When Foucault looked back at the history of the human sciences, he found an intermingling of power and knowledge. Similarly, Kirk and Kutchins's work allows us to see power/knowledge interweavings in the contemporary making of *DSM-III*. What are the implications, then, of Foucault's histories and Kirk and Kutchins's contemporary analysis for the *future* of psychiatric research? In other words, what would it mean to build questions of power and politics into the process of nurturing future psychiatric knowledge formations?

Chapter 8: A Feminist Successor Science for Psychiatry

As I mentioned at the end of the last chapter, Michel Foucault's detailed philosophical inquiries into the histories of psychiatry, medicine, human sciences, criminal punishment, and sexuality repeatedly revealed a complex interweaving between historical knowledge formations and social power relations. For Foucault, these mangled interweavings of knowledge and power were so complex and so unavoidable that it became impossible to think of these historical knowledge formations without also thinking of the power relations of their birth and propagation. Thus, Foucault's work has been highly instructive for overturning the Enlightenment illusion of "value-free" knowledge and for situating historical knowledges within specific power relations. Thus, Foucault opens the door to complex cultural studies readings of psychiatry that would not be possible within the current psychiatric discourse community. In this chapter, I will argue that the value of Foucault's power/knowledge insights for psychiatry does not stop with his backward looks at discursive formations or the cultural studies readings he can inspire. Rather, to use a metaphor from the video age, Foucault's insight should be "run forward"—ideally fast forward—and used in organizing future knowledge-making structures in psychiatry. To help articulate how this might be possible, I propose adding to Foucault's insights on discursive practice and power the work of recent feminist epistemologists and applying the combination toward future psychiatric knowledge production. Feminist epistemologists are essential in this task because, like Foucault, they have used insights into the co-occurrence of power and knowledge to critique historical

and current knowledge formations. But, unlike Foucault, feminist epistemologists have gone beyond critique to construct alternative visions for future knowledge-making practices.

Both Foucault's work and feminist epistemologies overturn the notion of "value-free" science and the once hallowed fact/value distinction on which it stood. Donna Haraway once again sums this up accurately and precisely in a phrase: "Facts are theory laden, theories are value laden, and values are history [and politics] laden" (Haraway, 1981, p. 477). If psychiatry were to follow through on this reversal and destabilization of the fact/value distinction, future psychiatric research would have to be restructured. In the current context of psychiatry, the fact/value distinction, along with the fraternal distinctions of objective/subjective, truth/myth, science/pseudoscience, knowledge/conjecture, context of justification/context of discovery, are the key starting points for knowledge inquiry. Indeed, the fact/value distinction undergirds not only individual research projects but also the entire infrastructure of psychiatric research. In this infrastructure, psychiatric knowledge production is divided into the separate domains of scientific knowledge production and bioethical knowledge oversight. Thus, in psychiatric research centers we have "research committees" and "ethics committees," each composed of separate people and separate procedures. Scientific research committees determine the pursuit of knowledge (the facts), and medical ethics committees determine how that knowledge should be used (the values).

Of course, there are some "ethics of medical research" committees devoted to the proper values at issue between psychiatric researchers and their subjects. For the most part, though, ethics and science are so divided during the stages of knowledge production

that there is no systematic infrastructure available to ask and negotiate: What kinds of psychiatric knowledges are good to pursue and for whom are they good to pursue? Which of the available methods of knowledge inquiry are best for psychiatry? And on what ethical or political grounds do we exclude possible contributors to psychiatric knowledge? Instead, we have an infrastructure in which scientific experts, coming from a narrow stratum of society, make all of these key value decisions among themselves and effectively decide the psychiatric knowledge agenda for everyone else. The situation is only getting worse in the context of multinational pharmaceutical and biotech corporations directly or indirectly funding much of psychiatric research. As a result, bioethics ends up entering the process of psychiatric knowledge production too late to make a sufficient impact. When bioethical value considerations are relegated to questions of knowledge *use*, rather than questions of knowledge *production*, it is like closing the barn door after the cows have run through—or, to update this metaphor for a posthuman age, it is like trying to undo electric shock treatment through the production of reverse seizures.

Running Foucault forward, recognizing that power/knowledge interminglings are inescapable in knowledge production, would begin to change this situation. Foucault's theory of power/knowledge implies that political and ethical choices are at play throughout the process of knowledge production (not just at the points of knowledge use). To respond to this insight, psychiatry must build an infrastructure that includes politics and equitable power relations into the process of psychiatric knowledge production. That means the field of participants in psychiatric knowledge production must be greatly expanded, and "peer review" can no longer be limited to a narrow scientific evaluation by

a narrow band of scientific insiders. There must be more stakeholders involved. Running Foucault forward, we must admit that, without such changes in the research infrastructure, the United States has an unethical system of psychiatric knowledge production no matter how much attention it pays to the bioethics of knowledge use. If bioscience and bioethics programs cannot come together to take up this charge, it is time to set up “biopolitics coalitions” and “biopolitics centers” (to augment the minimal effects of current “bioethics centers”) across the country and across the world to pick up where bioscience and bioethics are falling short. In the larger domain of “life sciences,” this kind of biopolitical action is already gaining much momentum in Europe and India in the crisis and controversy over genetically modified crops. Before psychiatry reaches its own crisis over the misuse and mistrust of science, psychiatric research-as-usual must change.

Introducing Democracy

The single most important rallying cry for retheorizing psychiatric research infrastructure can be summed up in a sound bite: “Democracy in Psychiatry.” Historically, the call for democracy has been perhaps the most powerful political imaginary for change. Like other discourses, the discourse of democracy is open ended and its meaning flexible. What it means and where it is applied is open to creative insight and collaborative struggle. However, democratic theorists Ernesto Laclau and Chantal Mouffe understand the language of democracy as a “fermenting agent” that has successfully motivated a variety of recent progressive politics, from the women’s movement, to African-American civil rights, to gay and lesbian liberation, to environmental activism (Laclau & Mouffe, 1985, p. 155). Going back further, this is the

same democratic imaginary abolitionists cited to combat slavery. suffragettes used in their struggles for the vote, and anti-imperialist resistance fighters mobilized against their colonial rulers (Smith, 1998, p. 9). Psychiatry, seen since its inception as a science and therefore separated from politics and power, has not fallen under the influence of a democratic imaginary. Retheorizing psychiatry, however, undoes the illusion of value-neutral and politics-free psychiatry. Retheorizing psychiatry reveals that power and politics are very much at the heart of psychiatric knowledge and that there is no escape from power/knowledge intermingling. The goal, therefore, in putting a retheorized psychiatry into practice is not to cleanse power and politics from psychiatric science (that was in many ways the goal of scientific psychiatry in the first place). The goal is to build politics and equitable power relations into the practice of psychiatric inquiry, education, and practice.

Laclau and Mouffe point out, however, that there is no necessary or inevitable connection between power and resistance to power. The subordinated must recognize their subordination, understand it as a limit to themselves and their world, and organize and struggle for effective resistance. Power rarely gives itself up. However, it is possible, Laclau and Mouffe argue, for democratic discourse to function as a rallying cry for collective action in ever new domains, even those previously removed from democratic language. As Laclau and Mouffe put it:

egalitarian discourses and discourses on rights play a fundamental role in the reconstruction of collective identities. At the beginning of this process in the French Revolution, the public space of citizenship was the exclusive domain of equality, while in the private sphere no questioning took place of existing social inequalities. However, as de Tocqueville clearly understood, once human beings

accept the legitimacy of the principle of equality in one sphere they will attempt to extend it to every other sphere. (Laclau & Mouffe, 1990, p. 128)

Putting a retheorized psychiatry into practice means extending the goal of inclusion and the principle of equality beyond the public sphere and into the private domain of the psychiatric profession. “Democracy in Psychiatry” becomes the rallying cry for this effort.

While democratic discourse in psychiatry would privilege the themes of *inclusion* and *equality*, it would also effectively reverse two of the key themes in the new psychiatry: reliability and scientific rigor. This is not immediately clear, because inclusion/reliability and equality/rigor are not obvious opposites. To see the inverse relation of these binary couples, we must work through the new psychiatry’s micropractices of achieving reliability and rigor. The new psychiatry achieves reliability through carefully selecting and training a narrow band of observers who see things along a narrow set of criteria for observing and organizing data. All others are excluded. Thus, in practice, reliability requires exclusion (which, of course, is the more obvious inverse of inclusion). Similarly, the new psychiatry achieves scientific rigor through a disciplined chain of command. Psychiatric research communities, journal editorships, and teaching institutions are organized in a military fashion, with multiply-graded roles equivalent to privates, sergeants, and officers. Increasingly, clinical communities are also regimented, with strict oversight of clinical activities. In all of these settings, rigor (defined as exact standards of production and reproduction) is maintained through institutional hierarchy. Those who stray are punished through lack of funding, lack of publication, and lack of employment. In other words, rigor, in practice, requires hierarchy (which is the more obvious inverse to equality). Thus, retheorizing psychiatry reverses the new psychiatry’s

unspoken values of exclusion and hierarchy and replaces these with values of inclusion and equality.

This reversal is not surprising. Indeed, it is expected that retheorizing psychiatry will reverse priorities. Using Derrida's language, one could argue that retheorizing psychiatry entails a deconstructive "reversal and displacement" of the key distinctions and priorities of the new scientific psychiatry (Derrida, 1981, pp. 41–42). Other key reversals might include:

- where the new psychiatry privileges science, retheorized psychiatry privileges humanities;
- where the new psychiatry privileges expert knowledge, retheorized psychiatry privileges lay perspectives;
- where the new psychiatry privileges biological determinants to mental suffering, retheorized psychiatry privileges psychosocial determinants;
- where the new psychiatry privileges the biomedical reduction, retheorized psychiatry privileges conceptual holism;
- where the new psychiatry privileges one truth, retheorized psychiatry privileges multiple truths.

The list could go on and on. The point here is that retheorized psychiatry has the effect of reversing the priorities of the new psychiatry. Retheorized psychiatry demonstrates that the priorities and values of the new psychiatry are not given by the complaints of consumers as much as *taken* by the interests of providers. Retheorized psychiatry denaturalizes these seemingly necessary priorities and opens the door to alternatives. This is needed because the current priorities and values inherent in the new psychiatry are arguably not so much good for the consumer as they are good for new psychiatry administrators, researchers, and providers. Lest I be considered polemic, however, let me add that the new psychiatry's priorities (and the practices that embody them) may be good for consumers as well. As I discussed in the Prozac chapter, however, when there is

a conflict between what is good for consumers and what is good for providers, consumers are likely to lose out.

Of course, retheorizing psychiatry would also create priorities and values that would be good for some more than others. Perhaps the greatest beneficiary, as of the moment I am writing, would be me. After all, it is my idea. Echoing Bruno Latour, one might say that if the status of an idea depends on the status of the statements that follow, then the status of the person or persons articulating the idea is also tied up with the same set of variables. Because this whole exercise of retheorizing psychiatry has largely been a thought experiment, however, and because, to my knowledge, there is little chance the idea will catch on, then I am unlikely to benefit much. One could argue, therefore, that my benefit is not a major issue in the discussion. Still, just working through the problem of retheorizing psychiatry benefits me even if the greater psychiatric community never picks it up. It will give me the feeling of a project completed. It will count toward my PhD requirements, and it will give me material for further publications and conferences. Regardless of the psychiatry community as a whole, I will benefit. Thus, a better way to approach the interest and values issue would be to accept up front that retheorizing psychiatry is an interested and motivated recommendation. After all, retheorized psychiatry is hardly neutral. This approach would be in keeping with the structure of reversal I have been discussing. Where the new psychiatry privileges “value-neutral” knowledge, retheorized psychiatry privileges “value-laden” knowledge. Accordingly, my interest in retheorized psychiatry is not a problem in and of itself, because it is assumed that all knowledge is interested. The question then becomes not “Is it interested?” but “How is it interested and does it try to conceal its interest?”

However, retheorizing psychiatry is not only a reversal of priorities. Like Derrida's deconstruction, retheorizing psychiatry has a "double moment." It does not stop with reversal but goes on to displacement. Thus, retheorized psychiatry not only reverses the priorities mentioned above, it also displaces them. But what does displacement mean in this context? Let me explain by way of example. Let's take the one-truth/multiple-truth distinction. Retheorizing psychiatry would not stop with reversing this distinction, or this priority; it would also potentially displace the distinction. In other words, it would allow the possibility that the distinction be given up altogether and replaced with alternative distinctions. A community does not have to privilege one truth or many truths. Instead, it is possible for a community not to think with that distinction at all. Put another way, if the distinction is displaced, it is no longer consciously relevant. If the distinction is no longer problematized, it is no longer considered. Obviously, it is not possible to do away with all distinctions (there could be no meaning without them), but it is possible to leave behind specific distinctions in favor of alternatives.

I do not mean to imply that an individual or a community can consciously reverse or do away with a distinction in any simple way. Distinctions are structured into a language practice, and they have reverberations far beyond the particular distinction involved. When a distinction is structured in a language, it is relatively permanent. The operative word, however, is "relatively." Of course, distinctions can change, evolve, and transition. New distinctions can emerge and old ones can fade. This transitional process occurs gradually, or sometimes with a sudden discontinuity, but it is something that can be recognized only in hindsight. A group or an individual can wish it to happen, will it to happen, even fight for it to happen, but these efforts in and of themselves will not make it

happen. Sometimes they will work and sometimes not. It depends on the material conditions and forces of the linguistic community. By “material conditions and forces,” I mean the materiality of the natural world (not any distinction will work in the natural world) and the materiality of the social world (not any distinction will make sense in a world structured by concrete rituals, practices, and institutions).

Feminist Epistemologists’ Call for a Successor Science

If psychiatry cannot will itself into reversed and displaced distinctions, how can new distinctions be fostered? How can psychiatry go from exclusion and hierarchy to inclusion and equality and, even further, to a displacement of these concerns on to other concerns? In the end, these are applied practical and policy-related questions. From my perspective, several feminist epistemologists (and like-minded scholars) have done the most work along these applied lines, and it is to their work I will turn for guidance. The feminist epistemologists I have in mind for this section include Evelyn Fox Keller, Sandra Harding, Helen Longino, Donna Haraway, and a handful of other feminist-minded scholars who have taken an interest in what Harding calls the “science question in feminism” (Harding, 1986). Though there is obviously much diversity in feminist writings on science, these scholars as a whole recommend moving beyond the sensational, but ultimately empty, debates of realism and antirealism (Rouse, 1996). In the words of Donna Haraway, they “hope to avoid the commercialized and rigged epistemological Super Bowl where the only teams on the globe are Realism and Relativism” (Haraway, 1997, p. 128). The realism/relativism debates are commercialized because those who structure the controversial issues of science along these lines are the legitimizers of current approaches to science understood as value-neutral and internally

motivated inquiry, and, simultaneously, they are defenders of high-profit technology and the elaborate interweaving of technoscience with consumer capitalism. The debates are rigged because, once the debate has moved to the realism/relativism binary, feminist epistemologists and others who hope to displace the binary have already lost out. Any argument within the terms of the binary reinforces the binary. Or, as Haraway puts it, engaging science within the terms of this realism/relativism binary is “more like spreading an epidemic than conducting debate on important issues in science, history, politics, and culture” (Haraway, 1997, p. 123).

Feminist epistemologists also recommend moving the debate from critique to reconstruction. They do not denigrate critique—it is a necessary step. For example, much of this book has been devoted to critique. In the early chapters, I critiqued the new psychiatry’s version of knowledge representation as theory neutral. I tried to show that knowledge representation, including scientific representation, is always part of a linguistic culture and practice and part of historically specific relations of power. In the later chapters, I moved from an abstract or philosophical critique to more specific critical readings of the politics of medical and psychiatric knowledge formations. Feminist scholars of science have spent much time critiquing science along similar lines, but they have not stopped with critique. The limits of critique are the limits of its temporal focus. Critique addresses the past or, at best, the present. Feminist and cultural critique of science starts with already worked out representational artifacts and practices of science and subjects them to scrutiny. But feminist epistemologists have also been interested in a future orientation (Rouse, 1996). As Sandra Harding puts it, feminist epistemologists are interested in a “successor science” (Harding, 1986, p. 142). Accordingly, feminist

epistemologists have made several initial steps toward creating a new model for science that can overcome the multiple problems of “science-as-usual.”

Like my retheorized psychiatry, feminist attempts to outline a plausible successor science start with the premise that knowledge is intermingled with values, practices, ways of life, and politics. Feminist epistemologists argue against universal knowledge or knowledge from nowhere. For feminist epistemologists, knowledge is always situated; it is always created from a particular standpoint (Haraway, 1991, pp. 183–203; Harding 1993). Feminist epistemologists have it “both ways” in that they argue it is possible “to have simultaneously an account of the radical historical contingency for all knowledge claims and knowing subjects, a critical practice for recognizing our own semiotic technologies for making meanings, and a no-nonsense commitment to faithful accounts of a real world” (Haraway quoted in Harding, 1993, p. 50). Thus, though there has been some controversy around the term, feminist epistemologies are usually some version of “standpoint epistemologies.” In standpoint epistemologies, knowledge, to be real, does not have to transcend historical and geographical interests, values, or agendas. Feminist standpoint theories embrace the idea of real knowledge as socially situated. For feminist epistemologists, this knowledge/power premise is not a problem to be overcome but an opportunity to be utilized and developed.

When knowledge/power intermingling is assumed along these lines, it follows that differential power locations will have differential knowledge perspectives. Thus, feminist standpoint epistemologists argue not only for including marginalized perspectives in scientific practices, they also “argue for starting off thought from the lives of marginalized peoples” (Harding, 1993, p. 56). Marginalized people provide

alternatives to the standpoints of dominant groups, and, because dominant groups are the most represented in scientific research communities, starting off with marginalized perspectives provides a corrective to the dominant perspective. Because dominant perspectives have been much longer at the center of knowledge production, they are by now thoroughly embedded in what is accepted as knowledge. As Harding puts it, “in societies stratified by race, ethnicity, class, gender, sexuality, or some other such politics shaping the very structure, the activities of those at the top both organize and set limits on what persons who perform such activities can understand about themselves and the world around them” (1993, p. 54). Dominant knowledge groups are unable to interrogate their own advantaged social situation and the effect of such advantages on their beliefs and scientific practices. Feminists argue that, far from being a hindrance to knowledge production, adding the perspectives of marginalized groups is an advance because it counterbalances the blind spots of the dominant groups.

Feminist epistemologists explicitly move theories of science away from an individual focus toward a community focus. They speak less of “a knower” or “the scientist” and more of “knowers” and “scientists.” They deliberately adopt these plural terms to counter the more prominent epistemological individualism of scientific method and philosophy of science. As Lynn Hankinson Nelson explains,

Feminist have argued that a solipsistic knower is implausible [and] have challenged the view that beliefs and knowledge are properties of individuals; and many have argued that interpersonal experience is necessary for individuals to have beliefs. And for more than a decade feminists have argued that a commitment to epistemological individualism would preclude reasonable explanations of feminist knowledge; such explanations (or, on some accounts, justifications of that knowledge) would need to incorporate the historically

specific social and political relationships and situations, including gender and political advocacy, that have made feminist knowledge possible. (Nelson, 1993, p. 122)

Thus, feminists focus on epistemological communities and subcommunities rather than individuals. This feminist focus on community complements their theories of perspectival and situated knowledge because the corollary of situated knowledge (knowledge situated within a linguistic and political community) is that knowledge production is communal.

Accordingly, the focus of a feminist successor science is not changing individual scientific behavior as much as it is diversifying scientific subjects and reorganizing scientific practice. For feminist epistemologists, scientific method includes more than hypothesis testing by individuals. Scientific method also includes conceptual criticism of collective background assumptions. However, background assumptions are often invisible to the members of a community because it is by internalizing background assumptions that one becomes a member of a community. Only some of this internalization is conscious; most of it is unconscious. Consequently, alternative points of view are required to effectively criticize background assumptions. People cannot effectively criticize their own unconscious points of view. Without diversity in the scientific community, the knowledge that community generates is always distorted by its own collective assumptions. When alternative points of view are excluded from the community, shared values within the community will not be identified as shaping observation and reasoning. As Helen Longino explains,

scientific knowledge, on this view, is an outcome of the critical dialogue in which individuals and groups holding different points of view engage with each other. It is constructed not by individuals but by an interactive dialogic community. A community's practice of inquiry is productive of knowledge to the extent that it

facilitates transformative criticism. [Thus,] the constitution of scientific community is crucial to this end, as are the interrelations among members. (Longino, 1993, p. 112)

Therefore, the best way to characterize a feminist successor science might be to say that it shifts scientific emphasis from representations to relations. Current scientific method, and the new psychiatry is no exception, focuses on the reliability and validity of representations. By contrast, feminist successor science focuses on the way members of a scientific community deal with inclusion and difference. In other words, they focus on relational issues. Feminist successor science puts relations first and representation second. Their implicit assumption is that if a scientific community sufficiently achieves diversity and treats differences with respect and appreciation, then the representations will work themselves out. Quality representations will flow from quality relations. Thus, the emphasis for a feminist successor science is not scientific representations as much as scientific relations. Quality representations are seen as a by-product of the way the communities go about recruiting difference and the way they deal with conflicts.

Far from “anything goes” relativism, feminist epistemologies are both “normative” and “objective.” Helen Longino argues that tending to the relations of scientific knowledge involves not only describing how scientific communities are set up but also prescribing how scientific communities should be set up (Longino, 1993, p. 102). For Longino, feminist observation of epistemic exclusiveness is also a demand for epistemic inclusiveness. And, she argues, advocates of feminist epistemology should be willing to struggle for the dissolution of noninclusive models of scientific method. Similarly, Sandra Harding argues that feminist epistemology involves not less stringent

objectivity requirements but strengthened standards for objectivity. As Harding puts it, feminist epistemologies

call for recognition that all human beliefs—including our best scientific beliefs—are socially situated, but they also require a critical evaluation to determine which social situations tend to generate the most objective knowledge claims. They require, as judgmental relativism does not, a scientific account of the relationships between historically located belief and maximally objective belief. So they demand what I shall call strong objectivity in contrast to the weak objectivity of objectivism and its mirror-linked twin, judgmental relativism. (Harding, 1991, p. 142)

Although, unlike Harding, I would not see feminist epistemologies as “more objective” than science-as-usual, I would agree that they have just as much right to a discourse of normativity and objectivity. I would argue that they are differently objective and, as such, they can lay claim to objectivity as much as the current approaches to scientific inquiry. I would add that, if a community values inclusion and equality, feminist epistemologies are more likely to build a knowledge structure consistent with those values. In that way, one can say that they have Harding’s “strong objectivity” for the progressive goals that she values.

Along these lines, Harding has taken the feminist epistemological focus on relationships a step further by explicitly substituting the trope of “democracy” for the trope of “feminism” (Harding, 1991). It is here that the feminist epistemologists’ approaches to science line up with the new democratic movements I discussed at the beginning of this chapter. As a result, Harding’s question for a successor science becomes, “What can be done to enhance the democratic tendencies within the sciences and to inhibit their elitist, authoritarian, and distinctively androcentric, bourgeois,

Eurocentric agenda?” (Harding, 1991, p. 217). The trope of feminism does not drop out for Harding, but her consistent use of the trope of democracy highlights that a feminist successor science is not only about women’s issues. As Linda Alcoff and Elizabeth Potter put it,

Growing [feminist] awareness of the many ways in which political relationships (that is, disparate power relations) are implicit in theories of knowledge has led to the conclusion that gender hierarchies are not the only ones that influence the production of knowledge. Cognitive authority is usually associated with a number of markings that involve not only gender but also race, class, sexuality, culture, and age. Moreover, developments in feminist theory have demonstrated that gender as a category of analysis cannot be abstracted from a particular context while other factors are held stable: gender can never be observed as a pure or solitary influence. . . . [Thus,] feminist epistemology should not be taken as involving a commitment to gender as the primary axis of oppression, in any sense of primary, or positing that gender is a theoretical variable separable from other axes of oppression and susceptible to a unique analysis. (Alcoff & Potter, 1993, pp. 3–4)

Harding’s use of the trope of democracy and science is the logical extension of these insights.

The feminist focus on democracy helps unite feminist approaches to science with other activist groups concerned with antidemocratic consequences of current scientific practices. One of the most interesting of these democratic science activist groups is the LOKA Institute. As Richard Selove, the organization’s current director, explains: “The LOKA Institute is dedicated to making science and technology more responsive to democratically decided social and environmental concerns” (Selove, 1995, p. 338). LOKA combines an interest in science, technology, and democracy for the following “simple” reasons: “Insofar as (a) citizens ought to be empowered to participate in shaping

their society's basic circumstances and (b) technologies profoundly affect and partly constitute those circumstances. it follows that (c) technological design and practice should be democratized" (Sclove, 1995, p. ix). The institute fosters this goal by providing resources for democratic choice and participatory research in scientific processes and technological design. For Sclove, "a technology is democratic if it has been designed and chosen with democratic participation or oversight and . . . is structurally compatible with strong democracy and with citizens' other important common concerns" (Sclove, 1995, p. 338).

Reminiscent of Harding's call for "strong objectivity," Sclove's call for alternative science is also a call for "strong democracy." Sclove borrows this term from democratic theorist Benjamin Barber, and, like Barber, he distinguishes "strong democracy" from "thin democracy" (Barber, 1984, pp. 3, 117). Advocates of strong democracy argue that, as a matter of justice, people should be able to influence the basic social circumstances of their lives and that society should be organized along relatively egalitarian and participatory lines. Sclove gives examples of New England town meetings, self-governing Swiss villages, and Anglo-American trial by jury. Thin democracy, by contrast, is "preoccupied with representative institutions, periodic elections, and competition among conflicting private interests, elites, and power blocs. Within thin democracies power is less evenly distributed; citizens can vote for representatives but ordinarily have little direct influence on important public decisions" (Sclove, 1995, p. 26). Strong democracy contains both a "procedural standard" (commitment to egalitarian participation) and a "substantive standard" (priority to common interests). For Sclove and the LOKA Institute, science-as-usual fails on both

standards. Science is too exclusive and it gives too much priority to economic and bureaucratic self-interest. Today's science is at best consistent with thin democracy. Only by subordinating science to democratic prerogatives can science and technology be consistent with a strong democracy.

Again, we see the normative element in this discussion. These two "strongs" (strong objectivity and strong democracy) go together. Though neither strong democracy nor strong objectivity is necessary, they do entail each other: It is certainly possible to organize large parts of society in nondemocratic ways. And it is certainly possible to arrange knowledge so that it contains multiple hidden interests and blind spots. If strong democracy is desirable and if it is worth fighting for, however, and the history of the democratic imaginary would suggest that it is, then strong objectivity is also desirable and also worth fighting for. As I see it, normativity derives from the entailment of these two strongs. It is difficult to be normative about strong objectivity without also being normative about strong democracy. And advocating strong democracy without also advocating strong objectivity (and the other way around) is a sham.

Both Longino and Sclove outline possible ways to organize scientific practice that would be more consistent with strong objectivity and strong democracy. Longino focuses on four community-level criteria needed to achieve a "transformative dimension of critical discourse" within scientific practice:

1. There must be publicly recognized forums for the criticism of evidence, of methods, and of assumptions and reasoning.
2. The community must not merely tolerate dissent, but its beliefs and theories must change over time in response to the critical discourse taking place within it.

3. There must be publicly recognized standards by reference to which theories, hypotheses, and observational practices are evaluated and by appeal to which criticism is made relevant to the goals of the inquiring community. With the possible exception of empirical adequacy, there needn't be (and probably isn't) a set of standards common to all communities. The general family of standards from which those locally adopted might be drawn would include such cognitive virtues as accuracy, coherence, and breadth of scope, and such social virtues as fulfilling technical or material needs or facilitating certain kinds of interactions between a society and its material environment or among the society's members.
4. Finally, communities must be characterized by equality of intellectual authority. What consensus exists must not be the result of exclusion of dissenting perspectives; it must be the result of critical dialogue in which all relevant perspectives are represented (Longino, 1993, pp. 112–113).

Sclove takes Longino's criteria the next step and gives several specific examples of community approaches to democratic scientific inquiry. The example that is most in line with Longino's criteria involves setting up "citizen tribunals." These tribunals follow a general model in which an inclusive and diverse group of participants work together and on an equal playing field in the process of technoscientific inquiry. Citizen tribunals involve "(i) technical experts, (ii) experts in technologies' social dimensions and effects, and (iii) representatives of organized interest groups (including public interest groups) playing vital roles" in considering new and ongoing science and technology. (Sclove, 1995, p. 218). In one such tribunal, the Danish government's Board of Technology selected a panel of ordinary citizens from varying backgrounds to consider questions of genetic manipulation in animal breeding. The panel attended background briefings and then spent several days hearing diverse presentations on the scientific and social issues involved. As Sclove reports, "After cross-examining the experts and deliberating among

themselves, the lay panel reported to a national press conference their judgment that it would be entirely unacceptable to genetically engineer new pets but ethical to use such methods to develop a treatment for human cancer” (Sclove, 1995, p. 217). This information was then used to help determine future legislative and funding decisions.

Feminist and Democratic Successor Science Applied to Psychiatry

How could these principles and examples of democratic science be applied to U.S. psychiatry? On the one hand, it seems impossible. Because psychiatric science, like so much other rapidly emerging technoscience, is a subset of the general U.S. capitalist economy, it might at first seem that we would require a new country, one with a truly strong democracy, to have a truly democratic psychiatry. But on the other hand, it is possible to separate aspects of psychiatric healthcare from the general free-market economy. Many, if not most, bioethicists argue that a just and fair society (where “equal opportunity” is more than slogan) requires a decent minimum of healthcare services for all (Nelson & Nelson, 1999, p. 289). The decent-minimum idea suggests that healthcare should be organized according to two protocols: (a) basic services that are publicly funded and distributed according to need and (b) additional (luxury) services that are privately funded and are distributed according to ability to pay. Without a decent minimum of basic medical services, it is argued, those who have access to care will have a clear unfair advantage over others. I need this argument because it seems to me that even thinking about a democratic psychiatry requires at least some component of psychiatric care to be publicly funded. Otherwise, psychiatric services become no different from other free-market services. Free-market services put profits at the “bottom line” rather than democracy. I do not see how Microsoft, for example, could be

compelled to organize itself along democratic lines without completely revamping the larger economic system within which Microsoft exists. Of course, one can argue against the decent-minimum idea that there is as much of a need for a decent minimum in computers as there is in healthcare. From there, one can easily argue that there is a need for a decent minimum in lots of things for a just and fair society. For example, housing, safety, cultural capital, job opportunities, transportation, and information (just to name a few) are arguably just as pressing for equal opportunity as healthcare. My point here, however, is not to argue the decent-minimum issue in detail: rather, it is to start a discussion of democratic psychiatry that will not be shot down right away because it would be infeasible in a capitalist system. Healthcare from a decent-minimum perspective is only partly capitalistic. The remainder is public—it is that remainder where the possibility of a democratically organized psychiatry exists.

If we start, then, with a publicly funded psychiatry (or at least some part of psychiatry as publicly funded), I believe we can begin to organize it democratically. My proposal for doing so must be considered a kind of “posttheoretical thought experiment.” I do not claim to have worked out a completely new infrastructure. I only hope to initiate a dialogue of possibilities. These ideas would need to be developed and fine tuned considerably, but that process cannot happen in a vacuum and without an initial proposal. It seems to me that the current psychiatric infrastructure can provide some initial assistance for working out alternatives. The American Psychiatric Association, for example, could continue to be the main organizational body for the psychiatry community. However, the APA, if it were to function as a strong democracy, would have to re-form its membership and its organization. The APA’s current working definition of

the psychiatric community would have to be revamped so that it could recruit more diversity into the community. Currently the APA community is composed of only professional psychiatrists, but the relevant stakeholder community for psychiatry is much broader. The APA should include representatives from all stakeholder groups: patients, family members, interested citizens, clinicians, administrators, researchers, legal personnel, government officials, police, and interested scholars of many types. From this perspective, the psychiatric community must be seen as a subset of the country and as such it should “look like America.”

To be democratic, the APA should also have membership representation weighted according to the size of the stakeholder group and the degree to which psychiatry affects a particular group. Thus, the largest single group represented in the APA community should be patients. Let me make a brief digression here on the word “patient.” The term “patient” has been increasingly unsatisfactory from within various critiques of psychiatry. Many are suggesting that the neologism “c/s/x” be used. C/s/x is an abbreviation for “consumer/survivor/ex-patient.” One psychiatric activist defines the term this way, c/s/x is

a progressive term, in that one begins with the illusion of being a consumer, is subjected to one or more of the horrors of psychiatric/therapeutic abuse and becomes a survivor (if he is lucky), and quickly realizes that the best way in which to extend his survival and avoid a repetition of the nightmare is to remain permanently an ex-patient. (“Shoshanna's”)

However, putting these different identity positions (consumer/survivor/ex-patient) all together into a single neologism (c/s/x) rather than using only “ex-patient,” implies that the relationship between these identity positions is not simply linear. People often shift from one identity position to another, and back again, or inhabit more than one at the

same time. Thus, many folks involved with the mental health system, or attempting to avoid involvement with it, are often a hybrid mixture of these multiple identifications. In addition, many still take up (or are put into) very passive “patient” identity roles as well. Perhaps, the abbreviation should be “p/c/s/x.” Whether this makes sense or not, rather than coin a new term, I will follow the activist literature on this point and use the term “c/s/x” rather than “patient” for the remainder of this discussion.

After c/s/x, the next largest group represented would be family members, followed by clinicians, administrators, scholars (from all areas of the university and from outside academe), and clinical researchers (from academe and private industry). Lastly, the APA should include representatives from the government, the police, and the legal community because of the many ways in which psychiatry works as a functional component of these other domains within the country. However, these representatives would be relatively small in number compared to the other stakeholders. The APA would have to diversify in other ways as well. In addition to belonging to psychiatrically defined groups, such as “c/s/x” or “clinician,” each member will be part of other identity groups and marked by race, ethnicity, gender, sexual preference, class, and age. Although these groups should be assumed to be fluid rather than fixed (members have hybrid identifications rather than essential identities), these identity markings are important, and the re-formed APA should make ongoing efforts to represent these groups in proportion to the wider society.

The APA members should be paid for their efforts, because the reformed APA would be the governing and regulating body for psychiatry. These functions must be considered part of the price of maintaining psychiatry. The membership could hold a

general meeting once a year, as they do now, at an annual conference. The expense of organizing the members and reimbursing their participation would be part of the expense of administering psychiatry. Members would be elected representatives who would represent local districts and function for psychiatry similarly to the way a congress or parliament functions for some nation-states. Once at the APA convention, the members would select an executive branch from among itself. Only the executive branch would be paid full time. Those serving on the executive branch would effectively be on sabbatical from regular livelihood. Other members of the association would be paid only for their efforts related to the annual convention.

Borrowing from Sclove's Danish example, the reformed APA's annual convention would be set up as a kind of psychiatric community tribunal. The role of the tribunal would be greatly expanded, however. Rather than giving a press conference on their findings, these community tribunals would be empowered with authority to make binding decisions. Sample decisions made by this community would include ongoing refinement of APA structure, practice guidelines, covered services, training requirements, training accreditation, continuing education meetings, kinds of journals (and their editorial boards), research projects (with "research" defined very broadly), brick and mortar needs, and general budget issues. APA members dealing with one of these issues would be given background information in the form of hearings. They would cross-examine presenters and deliberate among themselves. Their eventual decisions would be binding until the next tribunal on that topic. In between these times, the executive branch would carry out the decisions. Longino's community-level rules of engagement could form a basic guideline for members' interactions and how they took up their deliberative

processes. However, from my retheorized perspective, I would argue some with Longino's criterion number three. Whatever "community-level" standards are used should be considered to be in process (rather than fixed) and situated (rather than universal). I do not argue against trying to work out some standards. Indeed, my "three C's" are an attempt to achieve a kind of community-level standard. However, if the APA is to truly tolerate dissent (Longino's first criterion) and have equality of authority (criterion four), then it must realize that not everyone involved will be working with the same evaluative standards.

Obviously, these kinds of structural changes in the make-up and organization of the APA could have dramatic consequences for psychiatry. Rather than c/s/x being people who are discussed and managed by experts but never allowed to speak or to lead, they would become the major force in psychiatry. Joined by the other new members of the reformed APA, they would make policy in all areas of psychiatry. However, just because c/s/x are given the major power to shape psychiatric policy does not mean that psychiatric policy, practices, and research methods would necessarily change. In other words, it is entirely possible that the reformed APA would decide to continue psychiatry on exactly the same course it is following now. It is possible that the new members would select the same kinds of practice guidelines, the same kinds of research and scholarship, and the same kinds of administration of programs psychiatry has today. Things would stay the same if a significant proportion of the reformed APA membership felt, after extended hearings and deliberations, that the current approaches were working and were good for the people they represent. If things did stay the same, the reformed APA would still have an advantage over the old APA because its members would have a much clearer

sense that the approach it is following is supported by the stakeholders most affected by the system and not just a narrow band of elite researchers, administrators, and clinicians. In addition, the members would know that, if the current system turned out to have unforeseen negative effects, they would be able to make changes as needed in the future.

It is my impression, however, that psychiatry would change, and rather quickly, as a result of this new organizational structure. The biggest change I predict would be the integration of c/s/x into every element of therapy, administration, research, training, and continuing education. With the majority of power in the APA given to the c/s/x, they would no longer be content to stay in a passive role. Obviously, there is some risk that at first, like other colonized peoples, they would have so internalized the hierarchies of their previous masters that they would continue to privilege the priorities and values that went before (Fanon, 1967). Over time, however, the reformed APA would, I believe, begin to find ways in which c/s/x could participate in treatment teams (including being paid for their caretaking services), in administration (where they would have improved insight into the ways provider systems thwart people's needs), in research (after all, who knows more about painful emotional problems than people who have experienced them?), in training programs (who is a better mentor than someone who has been there before?), and in continuing education (through writing in journals, giving talks, leading conferences, and so on). And of course c/s/x would not be the only group better integrated into these activities. There would also be better integration of family members (who would be the second-largest group in the reformed APA) and clinicians (who are the biggest group in the current APA, but because the current APA is a thin rather than a thick democracy,

they have little power). Research and administrator activities as they exist today would continue only if the membership desired their continuation.

Another likely consequence for the reformed APA would be what Longino calls the “dilemmas of pluralism.” If strong objectivity requires strong democracy and if strong democracy depends on consensus among participants, what happens when consensus cannot be reached? In other words, what about the elements of conflict within the reformed APA that are not resolvable through debate and deliberation? If conflict is choked off and stifled through a procedural mechanism, such as “the majority rules,” then the psychiatry produced by the reformed APA will be as problematic as the psychiatry of today. The dilemma of pluralism is a dilemma we have met before in retheorizing psychiatry: the dilemma of multiple truths. If the reformed APA insists on the goal of a single truth for psychiatry, and consequently a single way of organizing practice, training, research, and so on, then it will have to do so at the cost of denying strong objectivity. Strong objectivity requires pluridimensionality. Longino offers this solution: “My strategy for avoiding this dilemma is to detach scientific knowledge from consensus, if consensus means agreement of the entire scientific community regarding the truth or acceptability of a given theory. This strategy also means detaching knowledge from an ideal of absolute and unitary truth” (Longino, 1993, p. 114).

Longino supports her detachment of knowledge from consensus and from the goal of one truth through two philosophic moves: “one of these is implicit in treating science as a practice or set of practices: the other involves taking up some version of a semantic or model-theoretical theory of theories” (Longino, 1993, p. 114). Both of these moves have been well rehearsed in my efforts to retheorize psychiatry. If knowledge is always

also part of practice, then knowledge is part of a way of life and not simply an abstract representation. Ways of life can be contrasted with other ways of life, but they are difficult to order into a clear hierarchical grid with one “right way” on the top. If knowledge is linguistically mediated—containing metaphorical and relational dimensions of meaning beyond straightforward reference—then knowledge is always wrapped up in language. One language can be compared with another language, but alternative languages, like alternative ways of life, are difficult to arrange in a hierarchical grid with a superior language or way of life on the top. Thus, through these two philosophic moves, Longino opens the door for multiple truths to emerge from a community of inquirers. This dissensus is not a problem (or a sign of immaturity) but an expected outcome of knowledge understood as part of language and practice.

With these issues of pluralism in mind, the reformed APA must have provisions for multiple approaches to defining, researching, practicing, and teaching psychiatry. Designing these provisions is a difficult problem, and I see no way to resolve it in an ideal way. The problem is related to the old, and tired, realism/relativism debate in scientific inquiry. If realism is one correct truth and relativism is anything goes, and if neither of these perspectives is satisfactory, how can a knowledge community design itself such that this binary is held in tension rather than being constantly collapsed from one side to the other? The difficulty in finding such a design is related partly, I believe, to the problem itself and partly to the repetition of the realism versus relativism (“science wars”) debate. If this debate had not become such a cottage industry, then there would have been more effort devoted toward solving the problem rather than constantly propagating an endless debate. Feminist epistemologists recommend moving past this

distinction, but it will take time and effort before a nuanced organization of scientific practice can do this. Thus, in my view, the pluralism problem is difficult because it is difficult, but, also (and this is crucial because it is most open to change) it is difficult because few people have really worked on it.

My provisional solution for the reformed APA would be to hold votes during the “consensus tribunals.” The reformed APA should expect that these votes will rarely be decided through unanimous consensus. However, I would also argue against a majority-wins approach to the votes. In contrast to the more typical election outcome in which the winner takes all, I would suggest a multiple-winners approach. In other words, if a knowledge perspective could get, say, 20% of the reformed APA vote, then that would be enough to be considered a valid knowledge and a valid, though admittedly controversial, approach to a psychiatric concern. By “valid,” I mean it would be written up in teaching materials, included as a genuine perspective in training programs, offered as a real possibility in practice situations, funded for further research, and so on. By “controversial,” I mean that it would be acknowledged that there is uncertainty on the issue and that the APA community differs on how to approach it. This uncertainty would not be seen as a problem; it would be expected that there would be much uncertainty and conflict about important issues. Of course, 20% is just a starting number. Perhaps the reformed APA would prefer 10% or perhaps 30%. Right away, however, one sees the impossibility of even this solution. What if over 20% of the APA thought that the percentage should be 5% and 20% thought 30% and 20% thought 50% and so on. It seems like opening the door to chaos if both procedural questions (like this one) and substantive questions are decided through multiple truths. The door to chaos cannot be

avoided, however, because, as I have been arguing all along, procedures are part of the practices that shape the outcome of truth; so they are far from neutral. Thus, we see the provisional nature of this solution. It is at best a starting point.

For an example of how this solution might work on a less procedural issue, however, consider the highly publicized APA vote on homosexuality in the 1970s. The question before the APA was “Is homosexuality an illness?” The vote came out “no,” but (unfortunately, from my perspective) it was relatively close: 58% no and 37% yes (Kirk & Kutchins, 1992, p. 88). Voting on “scientific questions” is unusual for the current APA, and this particular vote was considered by many to be an embarrassing chapter in the history of psychiatry. For me, it was embarrassing that the vote was so close, but with regard to the vote itself, I find it was one of the more strongly objective moves the current APA has ever made. However, if this vote were repeated in the reformed APA, I see two very important differences. First, it would have had a very different outcome because the membership would be so dramatically different from the current APA. My hope would be that such a vote in the reformed APA would not get the sufficient 20% to be considered valid knowledge. However, even if it did, the second difference in the way the reformed APA would handle the situation is that the answer would not have to be one or the other. In a situation in which greater than 20% voted yes, then “yes” would be accepted as knowledge and taught as a controversy. Homosexuality for some, on this outcome, is an illness. For others, it is not. The reformed APA would not attempt a procrustean solution to the question. By the tenets of strong objectivity, trying to decide yes or no in such a situation is inaccurate. A reformed APA would work with (teach, practice, research, and train) the controversy.

The reformed APA, like retheorized psychiatry more broadly, is not intended to be utopian, because reforming the APA would clearly involve losses as well as gains. There would be losses for those people currently doing well in the APA as it is now formulated. There would be losses in the values the current APA prioritizes. For example, there would be losses in the emphasis on scientific psychiatry and on the development of biopsychiatric interventions. These losses would be offset by gains for other people and the advancement of alternative values. Still, loss and imperfection would occur. Another loss, or imperfection, would be a risk of bureaucratic bloating. If the reformed APA had a bigger bureaucratic machine to organize difference and orchestrate alternative approaches, there is a danger that the APA would suffer from bureaucratocentric forces—which would lose sight of the APA's *raison d'être* and spend most of their energies self-propagating. In addition, the reformed APA is at risk to have unequal power relations between members distort the possibility of strong democracy (as it seems to do in most functioning political democracies). These last potential losses—bloating and power distortions—could be minimized through various protocols designed to limit them, but the point here is that the reformed APA will be no utopia and it will result in multiple trade-offs.

Therefore, reforming the APA cannot be motivated by a goal of global progress. Rather, the reformed APA can be motivated only by limited gains and a willingness to make sacrifice along particular lines. This does not mean, however, that there is no ethical or political weight, or no effective rallying cries, to the recommendation that the APA reform itself. When I say that “the APA should reform itself with priority given to strong objectivity and strong democracy,” I am making a normative recommendation. I

take a stand on preferred values and priorities. This call for “Democracy in Psychiatry” does not claim to be the only or the best way to go. There are certainly other ways to go, and there may be better ways to go, where democratic values are not given top priority or even better ways to achieve democratic values. However, it is a start, it matters, it makes a difference, and it is worth recruiting, enlisting, and fighting over.

Also, the many paradoxes and conundrums of my “thought-experiment” efforts at the end of this book should not be a deterrent to democratic reform in psychiatry. The basic light at the end of the tunnel for a retheorized psychiatry is that the process of psychiatric knowledge and practice needs to be more open to diversity and more representative of stakeholder groups. Though working out the details of how to do that is difficult, and though full democracy may never be possible, basic moves in that direction are very doable and are very possible. Thus, for me, reforming the APA along more democratic lines is a worthwhile struggle that can begin now, and, from my position inside the current APA, nothing short of a fight and a struggle would ever succeed in achieving these kinds of democratic changes.

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