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*Moving Beyond Prozac, DSM, and the New Psychiatry: The Birth
of Postpsychiatry*
by Bradley Lewis

**Moving Beyond Prozac, DSM,
& the New Psychiatry**

The Birth of Postpsychiatry

BRADLEY LEWIS



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best served by a strategy of recruitment over conversion. Conversion 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 social status 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 is also a challenge, but it is easier than conversion.

From a Foucauldian perspective, opening the disciplinary boundaries of a discourse will effectively change its power dynamics because it will simultaneously change the power relations among the members. The result will change the outcome, or the course, of what is known and what is considered to be "in the true" (Foucault 1972, 224).

CHAPTER FOUR

Psychiatry & Postmodern Theory

I discuss in chapter 1 how the tropes of "postmodern theory" and "postmodernism" are central designators of theory in the humanities. The postmodern trope adds much to postpsychiatry because it not only signals critiques of language, discourse, and power (as outlined in the last two chapters) but also puts these critiques in a historical context. Postmodern historicization is particularly helpful for postpsychiatry because psychiatry is a quintessentially modernist project. Psychiatry and modernism arose from a very similar mind-set. Indeed, one is not understandable without the other.

In this chapter I use postmodern historicization to consider aspects of modern psychiatry that have been present since psychiatry's inception and that are relevant to each of the historical shifts and divisions in the field. The new psychiatry I've been discussing is only the most recent historical shift in psychiatry. Though the new psychiatry significantly moves psychiatry from a meaning-based practice to a neuroscience-based practice, the new psychiatry is hardly "new."¹ From a postmodern historical perspective, the new psychiatry compulsively repeats more than it changes. Indeed, using a broader historical sweep, the new psychiatry's shift from a psychoanalytic rhetoric to a neuroscience rhetoric is not so much a change as a hardening and further modernist expansion of the worst aspects of the psychoanalytic science that preceded it.

Thus, postmodern theory helps postpsychiatry articulate the intellectual and historical context common to both the new psychiatry and psychoanalysis. Postmodern theory helps put psychiatric practice as a whole in a wider historical frame and provides key tools for theorizing psychiatry beyond current struggles.

an unprecedented and, for many, frightening pace. As a result, adults, children, and the therapists who help them are all being dramatically deskilled in their capacity to resolve relatively minor problems.

Increasingly, psychiatric stakeholders 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, grief, and anxiety. In addition, psychiatry is the only specialty of medicine that has an extensive protest movement organized against it—variously known as the “consumer/survivor movement,” “survivors of psychiatry,” “madness network,” or, my favorite, “mad pride.” These activists are united in their sense that psychiatry has been a traumatic force in their lives. From the perspective of these activists, whatever problems they had when they first engaged with psychiatry, their problems were worse after intervention (Morrison 2005).

Yet in spite of these difficulties, psychiatry continues to organize its core knowledge structures with minimal fundamental changes. What are these core organizing themes of psychiatric knowledge? What are the unspoken commitments that have been made, and how are these 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 earlier. These themes are part of the much larger and more profound context of intellectual and cultural practices within which psychiatry is situated. Rather than focusing on the details of each problem one by one, 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. In fact, psychiatry offers a particularly potent example of the Enlightenment dream of human improvement and perfectibility through the twin goods of science and reason. Yet 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. Psychiatry in particular and medicine in general could benefit greatly from an affirmative postmodern critique.³ Unfortunately, however, because academic medical centers are separated from the main campus by institutional, subcultural, political, and even physical barriers, medical schools and psychiatric training programs have yet to seriously engage postmodern critiques of the Enlightenment. This means that medical and psychiatric institutions have been unable to situate multiple problems in

But why should psychiatry be theorized and reimaged? What’s wrong with things as they are? After all, 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 twentieth century. Increasingly, however, this support is evolving into a chorus of criticisms. These criticisms have been well rehearsed in recent years, but briefly, health care practice is 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, 9)

This list, first drafted by Edmund Pellegrino over two decades ago, has only grown and proliferated. Everything Pelligrino cites remains true, and more. Pelligrino does not include current “health care crisis” critiques of unsustainable expenditures, gross inequities in access, and huge health disparities based on socioeconomic factors. Nor does Pelligrino’s list include the recent biotechnological explosion that threatens to bring a brave new world of genetically and pharmaceutically modified humans. And finally, Pelligrino does not list current public health concerns about the toxic side effects of contemporary scientific medicine—too toxic enough, some argue, to make medicine the third-leading cause of death in the United States.⁴

As a specialty of medicine, psychiatry suffers from all of these problems and more. Psychiatry is simultaneously shrinking and expanding in deeply problematic directions. On the one hand, services are being seriously cut. Psychiatric patients are increasingly found struggling in prisons, in 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 scarce (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

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 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's "An Answer to the Question, What Is Enlightenment?" (Flax 1990, 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, 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 were holding back human advancement. The Enlightenment dream was that through the liberation of reason and experience, knowledge would progress. 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 hero (which became the object of a veritable Western love affair) was rational, scientific, and technological understanding. By the late nineteenth and early twentieth centuries, during the time when modern psychiatry was being organized and before the somewhat sobering effect of the two world wars, Enlightenment modernism was in a 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, I consider three prominent philosophic themes of modernism:

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

health care and, indeed, the "health care crisis" itself within this larger critique of Western thought.

Of all the medical specialties, psychiatry is the least consistent thematically with scientific methods (in spite of the new psychiatry's recent claims) and the closest in subject matter to the arts and humanities. Because of this, psychiatry will likely be the first to seriously engage with postmodern theory. This book is self-confirming evidence of that claim. 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. It frees them from enslavement to Method and Objectivity, and it allows more humane perspectives and approaches to emerge as valued and respected.

I anticipate that postpsychiatric knowledge and practice would change in several ways through an 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, disability studies, and the arts;
2. a grounding of clinical activities in the wisdom of practice rather than the "objective truth" of research;
3. a greater emphasis on ethics, politics, and pleasure as guidelines and goals for clinical progress and knowledge production; and
4. increased democratization of all aspects of psychiatric practice (research, education, and treatment).

In the best scenario, the net result will be the emergence of a new postpsychiatry and a new model for medicine that will be both more enjoyable to practice and more connected to the concerns of patients. But before going further, let me back up for a closer look at psychiatric 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, 1). The intellectual ideals of modernism are the ideals

it 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. At best, forms of psychiatric knowledge coming from non-scientific sources like patient judgment, family opinion, clinical wisdom, case studies, the humanities, social theory, the arts, and so on are seen as hypotheses or conjectures. At worst, forms of psychiatric knowledge not subjected to scientific method are simply dismissed as myth, superstition, or idle speculation. In short, for psychiatric knowledges to be legitimated, they must be tested through scientific method—even if these knowledges are difficult, or even impossible, to operationalize into a testable form. Thus, in psychiatry, as in the Enlightenment, tremendous faith is placed in the scientific method as a route to Objective Truth.

THE TELOS OF PROGRESS & 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 psychiatry, "false knowledge" and "myths" 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 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 dramatically operative in the constantly revised new updates in neuropharmacology, new advances in the psychotherapy for resistant depression, and 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 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. The Enlightenment quest for objective truth rides the same correspondence epistemology and realist ontology I discuss in chapter 2. When psychiatry creates categories like "schizophrenia" or "neurosis," or theories of causality like the "dopamine hypothesis" or the "Oedipal complex," 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 one hundred 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 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—whether

Flax explains, however, postmodern theories are "not a unified and homogeneous field" (1990, 29). Thus, the term *postmodern* can be confusing because it is often used in multiple ways. The three most common usages 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 globalization; and
3. "postmodern theory"—which refers primarily to recent Continental "theory" critiques of Enlightenment philosophy and epistemology.

The focus for the rest of this chapter is on the latter because these theoretical versions of postmodernism are pertinent to rethinking the modernist thrust in existing psychiatric formations. Also, they provide additional theoretical background for the new paradigm I am proposing in this book, postpsychiatry.

Theorists and philosophers grouped primarily under this third category, such as Jean-François Lyotard, Roland Barthes, Jacques Derrida, Michel Foucault, Richard Rorty, and Zygmunt Bauman, have been particularly adept at undermining the foundations of modernist knowledge. Relying on these theorists to guide us, I argue that an affirmative postmodern rewrite could change the modernist concerns dominant in psychiatry today. Working with (and working through) the themes of modernism already discussed, I suggest that postmodernism shifts toward new, more fruitful, ways of thinking. Postmodern theory shifts and rewrites modernism

1. from a quest for objective truth to a *crisis in representation*,
2. from faith in method to an *incredulity toward metanarratives*, and
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 psychiatric consequences, I further elucidate my proposal for a new theory-friendly postpsychiatry.

These three themes of modernism (the quest for objective truth, faith in method, and a telos of progress and emancipation) provide an unreflected background horizon for psychiatric discourse. To illustrate, let me review an example from a contemporary psychiatric journal, the *Journal of Psychotherapy Practice and Research*. The journal describes itself on its front cover as a "peer-reviewed interdisciplinary journal published quarterly by the American Psychiatric Press, Inc., . . . its aim . . . 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 "enhance"—is clearly prominent even in the journal's self-description. But in a typical 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" (Docherty and Streeter 1993, 100, italics added). The authors go on to "review the *progress* in psychotherapy" in order to "provide a useful framework for exploring areas requiring increased attention and research" (1993, 100, italics added). The framework they adopt is proudly "*scientific*." Psychotherapy research, they tell us, needs a "scientific base," a "science of psychopathology," and a "science of psychotherapy."

Prior to the application of scientific method, the authors claim, 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" (Docherty and Streeter 1993, 100). The lack of scientific method in psychotherapy research created a "demoralizing problem for individuals involved with the effort to develop a science of psychopathology" (1993, 100). In other words, the conclusion with regard to psychotherapy for these new psychiatry authors (trying to outmodernize already modernist psychoanalytic psychiatry) is that without proper faith in Scientific Method, there is no Objective Truth. Without Objective Truth, there is no Progress toward human Emancipation.

A Postmodern Rewrite

Postmodernity may be defined, echoing our definition of modernity, as including modes of intellectual formation or organization that emerged in the West from about the 1950s onward and that have rapidly become influential throughout the humanities and certain social sciences. As

THE QUEST FOR OBJECTIVE TRUTH
BECOMES A 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" (Derrida 1974, 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 example, consider some particularly consequential psychiatric words and representations: diagnostic categories. As I discuss in chapter 2, to be intelligible, words and representations divide the world through relational divisions. The most basic example in psychiatric diagnostic 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 mania versus 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 and normal versus abnormal not only work as descriptions but also function as value preferences. These relational hierarchies echo, crystallize, reinforce, and perform other social hierarchies, prejudices, and power relations pre-

sent in the culture—for example, man versus woman, white versus black, straight versus gay, able versus disabled, 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, 196). As a result, postmodernists recommend that meaning-making divisions of linguistic terms be understood and used "under erasure." This 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 a postmodern worldview is to highlight the 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." There is a tendency within Enlightenment thought for the Truth to fall on *either* one side of a binary *or* the other. One is *either* 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 it embraces the use of multiple correlative conjunctions: instead of recognizing only "either/or," it embraces the use of "and/also" and "neither/nor." As we saw in the Van Gogh discussion in chapter 2, 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 advantages to organizing the world through this term, 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 and human interactions that work together to form a perpetually shifting scaffold for perception, thought, desire, and action. As I discuss in the previous chapter, Foucault highlights the interconnection of representational terms with each other and with human perception, practice, and power relations through his notion of "discursive practice." Lyotard's postmodern philosophy makes a similar move by drawing extensively on Ludwig Wittgenstein's concept of a "language game" (Lyotard 1984, 10). A "language game" for Wittgenstein, like a "discursive practice" 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" (1958, 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 either a language of "mentally ill" or one of "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 pluridimensional. 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 that even Lyotard, 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. For Lyotard, "these are games that we can enter into but not to play them; they are games that make us into their players" (1985, 51). However, to rest with Lyotard's conclusion 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 the agency/structure 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)" (1985, 51). Along these lines, in contrast to being forced and played by a single language game into a single subjectivity, recent "postmodern psychoanalysis" has argued that there are degrees of freedom within multiple subjectivities. As a result, one of the goals of therapy can be to increase our autonomy to make choices among these language games that are simultaneously playing us (see, e.g., 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 AN 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" among alternative conceptual possibilities. Psychiatry, like modernism more generally, answers 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 among 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" (1982, xlii). 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, 24, 29)

Thus, from a postmodern perspective, modernist science itself is a world-view, 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 modern or premodern discourse puts faith in a metanarrative, questions of "how to decide" are answered by applying the Method of the metanarrative. Modern discourse looks to reason and science: What would "reason dictate"? What does "scientific method conclude"? Premodern discourse looks to religious faith: What does the "Bible say"? For both moderns and premoderns, 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 metanarrative functions by creating a foundation for belief. Both moderns and premoderns argue vociferously that the foundational metanarrative legitimizes their discourses. However, affirmative postmodern theory undermines these kinds of modernist and premodern foundations. As Lyotard puts it, postmodern discourse is "incredulous toward metanarratives," and as such, postmodernism is an antifoundational discourse (1984, xxiv).

Without modernism's rationalistic and scientific foundation, and without premodernism's religious foundation, postmodernism must answer questions through a case-by-case *judgment* that considers a complex interweaving of *multiple* aspects of knowledge. These aspects include the useful, aesthetic, ethical, and political consequences of knowledge (Lyotard 1985, 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 (Lyotard 1985, 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 modern knowledge in that they avoid the hubris and imperialistic control of certainty.

The advantage of humility, however, does not create for postmod-

ernism a new metanarrative trump card. 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*, which I have been using to characterize modernism and postmodernism, are also a binary. From a postmodern logic reflexively directed back toward its own discourse, 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. In some situations, some people prefer proceeding with certainty. In other situations, the same people may prefer to be humble. For other people, it is best to mix certainty and humility in every situation. Meanwhile, sometimes, or for some people, it is better not to reflect on the distinction at all.

The same flexibility with regard to making distinctions is analogous to the distinction between modernism and postmodernism. Neither has a definitive advantage. In fact, from my perspective, postmodernism does not exclude modernism (or even premodernism). Postmodernism only opens up the possibility of a wider appreciation of the complexities of modernist knowledge. Thus, in a psychiatric context, there can be no external or foundational appeal to postmodernist psychiatry over modernist psychiatry. The only appeal becomes the internal appeal—preference for a psychiatric world that postmodern logics can create and that modernist logics cannot.

THE TELOS OF PROGRESS & EMANCIPATION BECOMES A TELOS OF STRUGGLE & COMPROMISE

The last, and surprisingly most difficult, critique for moderns 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 are easily countered by the equally modernist, only opposite, *Regression* and *Restraint*. For example, increased control over nature through technology is countered by increased environmental pollution, increased destruction of world resources, and increased threat of global catastrophe (through nuclear power, biohazards, or deadly new infections). Similarly, increased political freedoms through "rational" governments are coun-

tered by increased disciplining of human life by "rational" human institutions like schools, barracks, prisons, assembly lines, business management, and bank payments. And finally, increased liberation from superstition and tutelage is countered by increased sensations of alienation, fragmentation, and purposelessness. In all of these examples, modernist progress has led to modernist regress. Modernism is good for some things, but it is bad for other things. Though this seems obvious, it remains a blind spot for most moderns.

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 involve 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, 66; Geertz 1973, 19). Each creates meaning in ways that always contain simultaneous gains and losses. Antitopian in this sense, postmodernism replaces the telos of progress with the telos of struggle and compromise. Humans struggle and compromise with the world—they always make trade-offs between gains and losses of alternative worldviews. And humans struggle and compromise with each other—they always negotiate 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 the "brainless psychiatry" of psychoanalysis to the "mindless psychiatry" of neuroscience and the *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 only a partial progress along the lines of a greater capacity for using neuroscience conceptualizations and social-science operational methods. This increased capacity, though, was a simultaneous loss of capacity (regress). The new psychiatry loses psychoanalytic tools for articulating mental dynamics and therapeutic transferences 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—new psychiatrists and psychoanalysts—would have great difficulty communicating with each other. For better or worse, they work within 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 side's 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). (1988, 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 repairable 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" (1988, 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 psychoanalysis and the new psychiatry. 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. We should see them as common, and we should prepare for the plurality they create.

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, it is a fundamental telos of postmodern logic. However, resolving one differend through a shift in discursive practices frequently creates another differend somewhere else. Thus, compromise and struggle constantly coexist, and there will always be differends in psychiatry that struggle with each other. Rather than fight this phenomenon, Lyotard suggests that we expect it and prepare for it. If psychiatry were to follow this seemingly simple postmodern logic, it would mean that psychiatry must accept multiple and incommensurate forms of practice and knowledge-making. As I argue in the last chapter of this book, that acceptance would result in dramatic changes in the current organization of psychiatric structures.

Postmodern Theory and Postpsychiatry

For me, postmodern theory along these lines is crucial for scaffolding a new paradigm of postpsychiatry. The postmodern theory I have discussed here adds to the theoretical insights of the previous chapters in three vital ways. First, postmodern thinking is critical because of its historicizing thrust. It helps put pragmatic theories of representation and Foucauldian theories of discursive practice in a historical frame. And it offers a historicized understanding of the problematic modernist agenda of current psychiatry.

Second, as I have shown in this chapter, postmodern theory demonstrates the similarities between new psychiatry and psychoanalysis. Although these two psychiatric paradigms are often seen as poles apart, postmodern thinking shows how much these different psychiatric formations share. In particular, it shows the close ties they both have with modernist themes and preoccupations. Both the new psychiatry and psychoanalysis are organized through modernist schemas of a quest for objective truth, a faith in method, and a telos of progress and emancipation.

Finally, and linked to the last point, postmodern theory can help us understand how many of the endemic problems of existing psychiatric formations arise from modernist ways of thinking. Rather than tackle the problems of new psychiatry or psychoanalysis on an individual one-by-

one basis, postmodern theory allows a more fundamental critique that grants more radical and overarching solutions to the problems of existing psychiatric formations. Both the new biopsychiatry and psychoanalysis would benefit from an affirmative postmodern shift toward a crisis of representation, incredulity toward metanarratives, and a telos of struggle and compromise.

Postmodern theory, then, joins pragmatic theories of representation and Foucauldian theories of discursive practice and power to form the bedrock of a theorized postpsychiatry. Taken together, these theories provide serious additional scaffolding for the emergence of postpsychiatry. For postpsychiatry to emerge, the humanities theories examined in these first chapters (in all their complexity and nuance) must be understood and worked through. Nothing less will scaffold the change of mind-set needed to get beyond the problems and impasses of current psychiatric thinking.

But theory alone is not enough. Postpsychiatry also needs to begin specific applications of its theorized thinking to current issues and problems in psychiatry. In the next chapter, I describe how this could happen through a postpsychiatric form of cultural studies scholarship. Such a scholarship would provide the tools and settings in which dominant psychiatric practice and knowledge could be questioned and rethought. By forming alliances with the already postdisciplinary and interdisciplinary domain of cultural studies, postpsychiatry scholarship (in the form of cultural studies of psychiatry) could forge all-important connections between psychiatry and the broader campus.

tural studies of psychiatry offer alternative readings of psychiatric messages and, therefore, potentially reshape the beliefs and assumptions that will be encoded in future psychiatric messages.

Take Bordo's cultural study of "eating disorders," for example. Before her work was published, the only readily available literature on the subject was produced by mainstream psychiatry. Still today, when people come to study "eating disorders" (out of either interest or necessity), such psychiatric literature tends to dominate. However, when people scratch beyond the surface of "eating disorders" knowledge, they easily find Bordo's work. Her analysis of eating disorders provides alternative critical frames of reference that counterbalance the dominant-hegemonic psychiatric readings. Although her work may not have a *dramatic* effect on reshaping the production and encoding of psychiatric messages, it does begin the vital work of offering up alternatives. Such alternative readings alter the wider cultural consciousness about "eating disorders" and, in time, the consciousness of psychiatric practitioners and researchers who will produce and encode tomorrow's messages about "eating disorders."

For these reasons, cultural studies of psychiatry are, in my opinion, vital and effective. They form a crucial scholarly base for postpsychiatry. In the following two chapters, I do my own cultural studies of two key areas in contemporary psychiatry.

CHAPTER SIX

Decoding DSM

Bad Science, Bad Rhetoric, Bad Politics

The 1980 publication of the third edition of the *Diagnostic and Statistical Manual of Mental Disorders (DSM-III)* marks a watershed moment in contemporary psychiatry. Shortly after it came out, new psychiatrist Nancy Andreasen called the *DSM-III* a revolutionary book that would lead "to a massive reorganization and modernization of psychiatric diagnosis" (1984, 155). Andreasen's description has become the mantra of contemporary biological psychiatry. As Gerald Maxmen puts it in his book *The New Psychiatry*, "Perhaps more than any other single event, the publication of *DSM-III* demonstrated that American psychiatry had indeed undergone a revolution" (1985, 35). And contemporary historian of psychiatry Edwin Shorter echoes these same themes when he calls *DSM-III* an "event of capital importance" that resulted in the "turning of the page on psychoanalysis" and "a redirection of the discipline toward a scientific course" (1997, 302).

DSM-III sparked this massive reorganization through one major classificatory innovation. It shifted psychiatric diagnosis from vaguely defined and loosely based psychoanalytic descriptions to detailed symptom checklists—each with precise inclusion and exclusion criteria all meant to be "theory neutral." This may sound merely technical, but Andreasen, Maxmen, and Shorter do not exaggerate when they call the

cumulative effect revolutionary. No other work has had a greater impact on today's formation of psychiatry. *DSM-III* not only revolutionized diagnosis; it legitimized and scaffolded the new psychiatry's embrace of the disease model (Andreasen 1984). Indeed, through *DSM-III* the new scientific psychiatry solidified its position as the premiere paradigm for psychiatry.

Thus, to understand the cultural and political dynamics of today's psychiatry, we must understand the cultural and political dynamics of *DSM-III*. The best way to initiate this kind of cultural/political inquiry is with an insight from Michel Foucault. When Foucault reflected back on his own work unpacking the historical emergence of psychiatry, medicine, and other human sciences, he had the following epiphany. He realized that the best route (the royal road, if you will) to understanding the political and cultural power dynamics of science and reason is to start with *forms of resistance* (Foucault 1983, 211). By "forms of resistance," Foucault meant emergent counterdiscourses that rise up in struggle against an allegedly neutral discourse. Close study of these forms of resistance has several advantages over what might be called an "armchair" philosophical or critical analysis. Studying forms of resistance avoids the often sterile trap of applying reason against reason. It sidesteps the danger of being stuck in the role of "rationalist" verses "irrationalist." It helps intermingle theory with practice and practice with theory (because studying forms of resistance helps propagate that resistance). And, most important, it works better (Foucault 1983, 210).

Foucault found that forms of resistance work like "chemical catalysts" that can bring to light previously hidden power relations. Analyzing them locates political positions, power methodologies, and points of application: "Rather than analyzing power from the point of view of its internal rationality, [this approach] consists of analyzing power relations through the antagonisms of strategies" (Foucault 1983, 211). As Donna Haraway might put it, forms of resistance help articulate "the social relations of science and technology" (1991, 165). They expose whose point of view is being propagated, whose is being silenced, and they explain why and to what effect.

I find Foucault's insights extremely helpful for understanding and decoding *DSM-III*. Accordingly, rather than directly analyzing *DSM-III*, I will follow Foucault's suggestion to offer a cultural studies analysis of prominent "forms of resistance" to the manual. I focus on Stuart Kirk and Herb Kutchins's academic text *The Selling of DSM: The Rhetoric of Sci-*

ence in Psychiatry (1992) and their follow-up popular book *Making Us Crazy: DSM: The Psychiatric Bible and the Creation of Mental Disorders* (1997). These two works are now classic critiques of *DSM-III*, and they provide invaluable resources for decoding the manual's many fault lines. In the course of this chapter, I consider Kirk and Kutchins's main arguments, work through key limitations of their work, and augment their analysis with subsequent critical resistance to the manual. This kind of close reading of "forms of resistance" yields tremendous insight into the manual's development, and it answers a basic question for contemporary cultural studies of psychiatry: What's going on with the *DSM*?

Kirk and Kutchins organize the bulk of their resistance to the *DSM-III* around the "diagnostic reliability problem" that they argue the developers of *DSM-III* created, used, and manipulated for their own interests. Kirk and Kutchins put this "reliability problem" in context by examining 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" warranting serious attention (1992, 13). Kirk and Kutchins show that the scientific and political context of U.S. psychiatry in the late 1960s and 1970s was particularly ripe for the manual's developers. This was a time of serious "self-doubt" in psychiatry and a time of great "vulnerability to public and scientific criticism" (1992, 13).

Though psychiatry had been embattled before—particularly in the 1950s and the early 1960s, around critical and widely distributed exposés of state asylums as places of inhumane and brutal treatment—these earlier attacks were primarily challenges of psychiatric managerial and administrative practices. These managerial attacks, along with other factors, eventually led to the deinstitutionalization of psychiatric asylums. Deinstitutionalization was a major upheaval in psychiatry, but it did not threaten psychiatry's 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.

These additional attacks ranged from the conceptual antipsychiatry critiques of Thomas Szasz's "myth of mental illness" and sociologist Thomas Scheff's "labeling theory" of mental illness to the early historical and political critiques of philosopher Michel Foucault. When these challenges were combined with several high-profile criminal trials (such as that of John Hinckley, in which psychiatrists gave diametrically opposing

testimony) and the widely publicized disagreement in the psychiatric community around homosexuality, it created a climate ripe for 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. (1992, 22)

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

It was in this embattled context that the problem of "diagnostic reliability" took on major proportions within psychiatry. But how, exactly, did this come about? As it happened, simultaneous with these external attacks, psychiatry was embarking on an internal project of revising older forms of 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. The two earlier editions of the *Diagnostic and Statistical Manual, DSM-I* (1952) and *DSM-II* (1968), were small documents with brief descriptions of diagnostic categories. They served largely documentary and administrative purposes. After the 1968 revision, 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*. *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" (Kirk and Kutchins 1992, 13). In addition, *DSM-III* developers translated the reliability problem into a technical problem that they promised to solve through com-

plex social-science research methodology. They 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.

As a result of this process, psychiatry's thick conceptual and political problems (critiques of which were gaining momentum from several quarters) were rearticulated into the thin, but all-consuming, technical problem of reliability. Kirk and Kutchins point to two advantages of transforming psychiatry's problems into technical-reliability problems:

The first was that [they] appeared to be more solvable than problems of validity, at least in controlled research settings. The second advantage, an 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. (1992, 35)

The first advantage was a general one that applied to psychiatry as a profession. The other was an advantage for psychiatric researchers as a subset of the profession. The reliability problem effectively effaced the legitimacy debate about psychiatry as a whole. It turned deep public misgivings about psychiatry into private laboratory investigations of technical psychiatric research questions. In addition, the reliability problem deskilled clinical assessments of mental diagnostic categories and legitimated a new form of diagnostic expert: the research psychiatrist.

Clearly, the reliability problem guaranteed a prominent role in psychiatry for diagnostic 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" (1992, 44). *DSM-III* 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. This placed research psychiatrists center stage. By emphasizing the allegedly sorry state of psychiatric reliability in the past and claiming they could do better, diagnostic research psychiatrists made a place for themselves at the top of the psychiatric hierarchy. In Kirk and Kutchins's words, these 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. (1992, 14)

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

Bad Science

But as Kirk and Kutchins make clear, *DSM-III* developers accomplished this most remarkable transformation of psychiatry through manipulation and distortion of key research findings. Kirk and Kutchins critically examine the developers' repeated claims that the manual was a tremendous scientific improvement over older methods. They focus on the field trials of the manual's diagnostic system, which constituted 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" (1992, 15). Despite all the hype of the new manual, Kirk and Kutchins convincingly show that *DSM-III* developers gave misleading interpretations of their field-trial data, interpretations that greatly exaggerated the new manual's success.

In their reanalysis of the field-trial data, Kirk and Kutchins start with a straightforward question: "Was the new diagnostic reliability as clear and convincing as it was described by the proponents of *DSM-III*?" (1992, 141). They use this question to go back to the reliability data and ask, in effect, "Where's the beef?" *DSM-III* developers said that they had improved diagnostic reliability; what is the empirical evidence for that claim? Kirk and Kutchins find no beef and no empirical evidence. Instead, they find a "gross inconsistency between the answers offered by the developers and the empirical facts" (1992, 141). Rather than a bal-

anced 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" (1992, 74). The developers frequently use 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 (Kirk and Kutchins 1992, 74, 66). In short, the scientific evidence for *DSM-III* does not support the dramatic and bold claims of its developers.

Kirk and Kutchins consider their work to be a rhetorical critique of *DSM-III* because they find the scientific facts of the manual to be rhetorically distorted. But I believe it would be much better to see their work as a straightforward "scientific critique." If we put Kirk and Kutchins's work in the terms used by philosopher of science Sandra Harding, we see that their method primarily involves close empirical analysis of the facts. They do not step back to consider the broader rhetorical frames for the collection and interpretation of these facts. In Harding's terms, Kirk and Kutchins accuse *DSM-III* developers of "bad science" (Harding 1986, 25) because the developers distorted and manipulated their data. They misused their power, and they irresponsibly promoted the self-interests of psychiatrists and researchers. By doing this, *DSM-III* developers violated the internal principles of good science.

Going further, Kirk and Kutchins's implicit solution for the developers' "bad science" is more (and better) science. Harding would characterize Kirk and Kutchins's solution to the problem 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, 51). That is just what Kirk and Kutchins do in *The Selling of DSM*. By more rigorously reviewing the field trials, they correct for the bias of self-interest in the *DSM* developers' reports.

Conceptualizing Kirk and Kutchins's critique of *DSM-III* as a "bad-science" rather than a "rhetorical" critique allows us to better see how Kirk and Kutchins's work fits with other critiques of *DSM-III*. In making a bad-science critique, Kirk and Kutchins join a host of other authors who criticize the scientific details of the *DSM-III*. There has been no shortage of these kinds of critiques of the manual. Diagnostic research psychiatrist Allen Frances once described the scientific critiques of *DSM* as running along a gamut from "A to Z":

Relation of Axis I to Axis II
 Biological and psychological test results
 Categories versus dimensions
 Diagnosis versus definition
 Education
 Field trials
 Generalizability
 Hierarchies
 Illness versus syndrome
 Judgment
 Kultur
 Lumping or splitting
 Mental disorder
 New diagnoses
 Openness
 Prototypes
 Quality control
 Rates of prevalence and incidence
 Subthreshold conditions
 Theoretical neutrality
 Users
 Validation
 When
 Xenophilia versus xenophobia
 Yonder
 Zeal (Frances et al. 1991)

These scientific critiques of the *DSM* have come both from both inside and outside the *DSM* developer community, and they present no lightweight problems for the manual. The most devastating of these critiques comes under *V*, for *Validation*. The validity critique of the manual has been so strong that *DSM* science scholars (both insiders and outsiders) express serious doubts as to whether there is any meaningful connection between the diagnoses of the *DSM* and the "real world" of human mental suffering (Cooksey and Brown 1998; Kupfer, First, and Regier 2002).

When all the scientific problems are taken together, they can leave reviewers wondering if there is any scientific merit to the manual at all. Indeed, senior psychologist Arthur Houts has reached that very conclusion: "after 25 years of following changes in the various editions of the

DSMs, I have concluded that there is far more pseudoscience than real science in the modern *DSMs*" (2002, 17).

But surprisingly, even though bad-science critiques can be quiet harsh, *DSM* developers do not generally discourage this kind of critique. As long as *DSM* critiques are couched in "bad-science" language, *DSM* developers are open and even welcoming to these kinds of critiques and debates. They use them to legitimize perpetual funding for *DSM* research and to justify continued "new and improved" versions of the manual—such as the revised *DSM-III-R* (published in 1987), the follow-up *DSM-IV* (published in 1994), and the planned *DSM-V* (projected to be out in 2010). So far, the actual changes to the manual resulting from these "bad-science" critiques have been relatively minimal. Both the *DSM-III-R* and the *DSM-IV* largely carried over the innovations of the first *DSM-III*. But the situation does not have to stay this way. Bad-science critiques can, at least in principle, lead to major overhauls. Indeed, the kind of tinkering that characterized the *DSM-III-R* and *DSM-IV* may very well stop with the next edition. The *DSM-V* developers, by all indications, have much more ambitious overhaul plans in mind (Kupfer, First, and Regier 2002).

It is important to emphasize, however, that even though *DSM-III/IV/V* developers have been open to "bad-science" critiques, they have not been open to deeper critiques that question the basic research traditions and assumptions of the manual. For example, the developers have not been open to robust "rhetorical" critique that seriously questions the core rhetorical frames of the manual. Kirk and Kutchins unfortunately do not make this deeper level of rhetorical critique, but their efforts do provide the resources needed to take us in that direction.

Bad Rhetoric

Even though the subtitle of Kirk and Kutchins's book is *The Rhetoric of Science in Psychiatry*, they do not sufficiently consider the role of "rhetorical language" in the *DSM-III* developers' methods. Kirk and Kutchins fail to make this move because *rhetoric* for them means something external to the facts: an embellishment or perhaps a commentary on scientific data, rather than something integral to the data itself. Kirk and Kutchins base their "rhetorical critique" on a bright-line distinction between the "facts" of *DSM-III* field trials and the "rhetoric" used to describe these facts. By keeping this distinction intact, they are able to argue that *DSM* develop-

ers rhetorically exaggerated the facts of the manual. But the distinction hurts Kirk and Kutchins as much as it helps them. It prevents them from stepping back from the details to see how the *DSM-III* developers' rhetorical frame significantly affected the facts the developers "discovered." And furthermore, it prevents them from recognizing that alternative rhetorical frames would have produced alternative facts.

Kirk and Kutchins's basic assumptions regarding the relations between "facts" and "rhetoric" have a long heritage in the Western tradition, traceable at least to the ancient Greek distinction between philosophy (love of knowledge) and rhetoric (the craft of persuasion). But there is another way to consider the fact/rhetoric distinction. Recent work in rhetorical theory has built extensively on the implications of the emergence of "theory" across the humanities (Gaonkar 1990). The key conclusion from this recent work—which Barry Brummett calls "post-modern 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; J. Nelson, Megill, and McCloskey 1987).¹

If Kirk and Kutchins had followed this work in rhetorical theory and blurred the fact/rhetoric distinction, they would have been in a better position to critique the rhetorical frames of *DSM-III* research. The critique of rhetorical frames goes beyond an internal bad-science critique and introduces what Harding calls a "science as usual" critique (1991, 58). Science-as-usual critiques open up questions about the very assumptions of science. They highlight the way dominant scientific discourses do not develop neutral methodological models, distinctions, and priorities outside of a field of power and only later hold to these methodological styles with the tenacity characteristic of a battle. The models, distinctions, and priorities themselves are part of the power struggle between dominant and alternative approaches.

Science-as-usual critiques introduce deeper rhetorical questions than Kirk and Kutchins are able to ask. For example, what rhetorical tradition is being followed in pursuit of "the facts"? How is that rhetorical tradition used to perceive, organize, manipulate, and interpret the data? And what are the effects of choosing one tradition over another? As Brummett makes clear in his work on postmodern rhetoric, 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 unspoken choices about the proper "methods," or research traditions, for reaching and legitimizing that reality (Brummett 1999, 166).

Expanding Kirk and Kutchins's work to introduce a deeper rhetorical critique of the *DSM* involves teasing out the rhetorical frame of the current manual and comparing that frame with alternative rhetorical options. The best way to do this is to connect a rhetorical discussion of *DSM* with the literature on "models of madness." Models of madness operate very much like a rhetorical frame: they work as an underlying organizing structure that guides the perception, selection, and methodological manipulation of psychic data. Models of madness frame and select certain aspects of a perceived human reality and make them more salient than others. Each model promotes its own problem definitions, explanatory concepts, research methods, and treatment recommendations.²

Though the *DSM-III* developers claim to use a neutral rhetorical frame, when we connect their work with the models-of-madness literature, we see that they actually use a very rigid "disease model" (also called the "medical model"). The central tenets of the disease model include the following:

- Mental pathology is accompanied by physical pathology
- Mental illness can be classified as distinct disorders that have characteristic common features
- Mental illness is biologically disadvantageous and handicapping
- The causes of mental pathology are explicable in terms of physical illness (Tyrer and Steinberg 1998, 10)

The disease model in psychiatry forces psychiatric observation and research to emphasize signs, symptoms, formal mental-status exams, lab tests, differential diagnosis, pathophysiology, etiology, medical treatments, and prognosis.

The larger rhetorical frame for the disease model is based on natural-science frames of objectivity, precision, and reliability. As philosopher Charles Taylor points out, there has been a long tradition in social science of trying to understand humans through the methods of natural science. Taylor explains that because the natural sciences have been so seemingly successful at explaining the natural world, "the temptation has been overwhelming to reconstruct the sciences of man on the same model" (1977, 105). But a host of philosophers have pointed out the problems with this approach. Human experience, human choice, and human action are sufficiently different from the inanimate physical domain that there exists an unbridgeable gap between human studies

and the natural sciences. Humans may be made of physical material, but attempts to study humans with natural-science methods alone turn out to be ludicrously arid and incomplete (Lewis 1994).³

Despite these serious philosophical reservations, *DSM-III* developers fall straight into the temptations of natural science. With their unbridled enthusiasm for the disease model, *DSM-III* developers wholeheartedly embrace a natural-science rhetorical frame for psychiatric research. This embrace of natural science means there was nothing "neutral" about the frame for *DSM-III*. The manual highlights, prioritizes, and organizes the "facts" of mental illness to suit the particular frame of the disease model. From the time of its publication forward, the disease model legitimized by *DSM-III* has become so dominant that it may seem that there are no alternative models for psychic diagnosis. But that is hardly the case. There is a wealth of treatment varieties for psychic distress. R. Corsini and D. Wedding's *Current Psychotherapies* (1995) lists over four hundred different systems of psychic treatments, and it only scratches the surface. Each treatment variety has its own way of assessing what is wrong and applying that assessment to treatment interventions. Each treatment, in effect, has its own unique rhetorical frame for diagnosis.

Thus, a deeper rhetorical critique of the *DSM* must ask: Why choose one particular rhetorical frame for the manual and disregard all others? Rather than a natural-science frame, why not choose a phenomenological frame? Why not a feminist frame, or a disability studies frame, or a gay and lesbian frame, or a Buddhist frame? Indeed, why must there be a single frame and a single diagnostic system? Why not multiple models of diagnosis based on multiple models of madness? In sharp contrast to the natural-science approach of *DSM-III* developers, postmodern rhetorical theory would not hide rhetorical frames through sleight of hand (like claiming to be theory neutral), nor would it close out alternative rhetorical frames in favor of a single frame. Many models of madness can be applied to psychic distress, and no one model is right. They all have advantages and disadvantages. In the end, the choice of model or frame depends not on science but on the perspectives and values of the person and persons involved.

Though a detailed comparative analysis of the models is beyond the scope of this chapter, the details are not necessary to make this very basic rhetorical claim: *DSM-III* developers ushered in an approach to psychic diagnosis that is not only bad science but also bad rhetoric. To make this argument good, all I need to show is that for many stakeholders in psychiatry the advantages of the disease model do not exceed its disadvantages. The main advantages claimed for the disease model are improved

diagnostic reliability, clarity, and promotion of differential diagnosis (Andreasen and Black 2001, 34–35). It is also fair to say that natural-science models like *DSM-III* have advantages in researching and treating the most clearly physical dimensions of the human psyche: bodies, brains, neurotransmitters. As such, natural-science models will have advantages in developing biological models of psychiatric illness and creating pharmacologic and other somatic treatment interventions.

But beyond these advantages, there are also many disadvantages to the *DSM-III* disease-model approach. Indeed, most critics of contemporary psychiatry focus their critiques on problems with the disease model. These critiques are many, and the problems with disease-model psychiatry are severe. The rhetorical frame of the disease model tends to

- naturalize and reify "mental illness";
- feed into the medicalization of deviance;
- feed into psychiatry as an agent of normalization, state control, and multicultural oppression;
- feed into the pharmaceutical industry boondoggle; and
- rest on a natural-science model approach to humans that excludes other approaches and excludes multiple approaches.⁴

These severe critiques of the disease model are more than enough to convince many that the model is a deeply problematic rhetorical frame for psychiatry.

What must be emphasized is that these critiques of the disease model are also critiques of the basic rhetorical frame of the *DSM-III*. They move beyond Kirk and Kutchins's bad-science critique and open up a science-as-usual critique of the manual, and by doing so they challenge *DSM-III* developers' basic assumption that the disease model is the best rhetorical frame for psychiatry. With so many stakeholders so vehemently against the disease model, it cannot possibly be a good choice to make the disease model the *only* model of psychiatric diagnosis. But that is just what the *DSM-III* developers did. And with that choice, they made a serious mistake—so serious that it is fair to conclude only one thing: the *DSM-III* is not only bad science but also bad rhetoric. Very bad.

Bad Politics

It is not enough, however, to stay at either a "scientific" or a "rhetorical" level alone. Both of these levels remain too textual. They remain too

caught up in books and articles, and they leave out the people involved in writing those texts. My reading of the *DSM-III* thus far reveals the tremendous contingency of the manual. Better science and better rhetoric, or at the very least different science and different rhetoric, would have taken the manual in dramatically different directions. To understand why the *DSM-III* emerged as the dominant text that it is, we must also examine the politics of the manual. We must go beyond textual analysis to what Foucault called "enunciative modalities" (see chap. 3). We have to animate the particular people of the *DSM-III*'s discourse and give them life.

Bruno Latour's work in science studies provides a valuable conceptual resource. 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, 15)

Latour's first rule of method takes "neutral" scientific discourse out of its textual form and puts it in a cartoon-style bubble over the mouth of the speaker. In this way, Latour puts scientific claims back into the "mouth of someone who speaks." From there, he follows what happens next—who listens, who recruits whom, who defects, who is seduced. Latour's first rule of method works to reanimate neutral scientific discourse and helps open the door for a move from a scientific or rhetorical analysis to a *political analysis* of individuals and groups.

A political analysis of science is an extension of the science-as-usual critique. Typical scientific method only allows bad-science critiques, because it assumes it will be sufficient if the participants follow the methods

of science. Going beyond bad-science critiques to science-as-usual critiques, we can also question basic rhetorical assumptions of science (as we did earlier), and we can question the political relations of the participants. A science-as-usual critique that focuses on political issues directs our attention to who gets included in the process. Who gets to sit at the table? Who gets to contribute? Who is excluded? What perspectives do they bring? And what effort is made to include alternative and additional perspectives?

Applying Latour's first rule to the *DSM-III*, we can begin a political critique of *DSM-III* by exploring who gets the new manual's bubble mouth and whom that mouth fetches, recruits, or seduces. Once again, Kirk and Kutchins provide valuable data. Though they do not pursue political issues directly (because they remain primarily focused on a scientific critique of the manual), Kirk and Kutchins do give ample information on the people involved in the manual. If we take their research and add the reports of published insider narratives, we get enough information to answer the "bubble-mouth" question for *DSM*.

Based on Kirk and Kutchins's research, the top *DSM-III* bubble mouth goes to Robert Spitzer. A career psychiatric researcher devoted to problems of nosology and classification, Spitzer was the leader of a group of Columbia University research psychiatrists. These psychiatrists were known for pioneering and developing structured interviews and objective diagnostic criteria. Allen Frances, the psychiatrist in charge of *DSM-IV*, described Spitzer as a "man whose entire life, private and public, personal and professional, is occupied with diagnosis and particularly with *DSM*" (Kirk and Kutchins 1992, 91).

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* was published, and despite his initial support and involvement, Spitzer became one of the manual's biggest antagonists. Spitzer published a 1974 paper offering a scathing critique of the diagnostic reliability of the *DSM-II*. 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" (1992, 63, 90).

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

DSM-III reflects an increased commitment in our field to reliance on data as the basis for understanding mental disorders. . . . [Because of this], a series of field trials were 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*. (American Psychiatric Association 1980, 1, 5).

This is the official narrative of *DSM-III*'s development. If we put a bubble around the quote and clearly identify it with the voice and perspective of Robert Spitzer, we can begin to give the manual a more politically thick background.

Within four months of Spitzer's selection as chair of the *DSM-III* task force, he had fetched (recruited or seduced) all the members of the new committee. The members he chose consisted of a group of five psychiatrists. All had similar research interests, and all believed that psychiatric diagnosis should be based on allegedly theory-neutral objective criteria. One of the members, George Saslow, was known in psychiatry for his coauthored 1965 work entitled "Behavioral Diagnosis" (Kanfer and Saslow 1965). Two other members, Spitzer himself and one of his colleagues, Donald Klein, were known for promoting bioscience psychiatry and objective approaches to diagnosis. The two remaining members, Nancy Andreasen (who is now the editor of the leading professional journal in psychiatry and coauthor of the major psychiatric textbook I discuss in chap. 3) and Robert 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 operational psychiatric nosology and precise objective criteria for diagnosis. This kind of highly scientific approach to psychiatry represented a narrow section of psychiatry. The committee members were, in Kirk and Kutchins's terms, a "minority among a minority" (1992, 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 and Kutchins 1992, 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 (Kirk and Kutchins 1992, 81). In 1978, psychiatrist Gerald Klerman dubbed these psychiatrists "Neo-Kraepelinians" and outlined the young turks' implicit "credo." Klerman's outline of the neo-Kraepelinian credo is worth quoting in full because it demonstrates the overlap between the diagnostic mind-set of the *DSM-III* task force and the disease model I describe earlier. According to Klerman, the neo-Kraepelinian credo includes the following beliefs:

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. (Qtd. in Kirk and Kutchins 1992, 50)

As this credo demonstrates, the stakes for psychiatry were high. Spitzer's *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, and the eventual success of this disease model for psychiatry 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 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 perspectival cleansing. Spitzer's task force carefully eliminated any people with alternative perspectives—including the psychoanalytical psychotherapy perspective, which dominated psychiatry at that time—to create a mono-perspective 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" (1992, 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. 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" (1992, 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 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 scientific claims of the field trials. Here, I want to highlight how the field-trial approach to verification focused on testing already created categories rather than the actual creation of categories. This emphasis on testing effectively covered over the fact that only a very narrow band of participants were involved in the manual's initial creation.

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 and participation in the manual's production. However,

almost all of these names (all but five) are of people who were involved in the field trials rather than of 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 participate in the creation of 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 stakeholder community. The truth was just the opposite. The manual represented the forced will of a very few people and an extremely limited number of psychiatric stakeholders.

And it was not only psychodynamic perspectives that got shut out. So did psychology perspectives, social work perspectives, and other mental health perspectives (Schacht 1985). In addition to professional exclusions like these, *DSM-III* developers also excluded pretty much anyone who was not a privileged, white, male, academic psychiatrist (Malik and Beutler 2002, 6). The most detailed account of these exclusionary practices comes from insider exposés written by feminist psychologist Paula Caplan. In her book *They Say You're Crazy: How the World's Most Powerful Psychiatrists Decide Who's Normal*, Caplan gives a detailed account of the relational dynamics of the manual (1995). She describes the responses she and a group of feminist researchers (therapists, psychologists, and psychiatrists) received when they attempted to contribute to the manual. Throughout the process, they were systematically snubbed, ignored, denigrated, and dismissed.

Caplan and her colleagues got involved with the manual after becoming concerned that key diagnoses under consideration, such as one describing "masochism" (or "self-defeating personality disorder") and one describing "premenstrual dysphoria," were riddled with sexist assumptions. Like Kirk and Kutchins, Caplan couched her concerns in the form of a bad-science critique. She reviewed the scientific literature for these proposed diagnoses and found herself in deep disagreement with the developers' perspective. She argued that these diagnoses were not diseases at all, but simply a pathologizing of culturally produced gender patterns. When she tried to get her scientific conclusions to the *DSM* developers, she was politely but persistently rebuffed and excluded.

Caplan and her colleagues went beyond critiques of the manual's premenstrual and self-defeating personality diagnoses. They also proposed some diagnoses of their own. For Caplan and her colleagues, if the diagnostic manual was going to pathologize culturally produced femininity, then it should be consistent and do the same for masculinity. But when

Caplan and her colleagues suggested some parallel diagnoses, like “testosterone induced aggression” and “macho personality disorder” (which they called “delusional dominating personality disorder”), they got even less respect and were refused even the semblance of an audience.

According to Caplan’s own report, she worked hard to think the best of the *DSM* developers. She was very reluctant to see them as a narrow-minded interest group, and she thought if they could just see good arguments and good data they would come around. Eventually, however, she had to give up her sense that the developers were just doing neutral science. Clearly, they had an agenda, and feminist concerns were not part of it. Rather than argue the merits of the competing claims, the developers stonewalled and excluded their opponents. Caplan categorized the various stonewalling and exclusionary procedures used by the *DSM* developers, and she came up with twenty-five different gate-keeping methods. These ranged from nonresponsiveness, to bait-and-switch tactics, to outright lying and manipulation (Caplan 1995, 222). The stonewalling tactics not only kept out feminist concerns but were also used to fight resistance to other highly problematic diagnoses, like “egodystonic homosexuality,” and to avoid any serious consideration of a “racist personality disorder” diagnosis (Caplan 1995, 221).

I should note that there was some shift in the developers’ exclusionary practices from *DSM-III* to *DSM-IV*. After receiving much criticism on the issue, *DSM-IV* developers were much more sensitive to charges that the *DSM-III* was exclusionary. As a result, they promoted an increased air of “inclusiveness” from the *DSM-III* to the *DSM-IV* (Nathan 1998). But these changes seem to be more window dressing than real inclusiveness. The *DSM-IV* developers’ efforts to include women and racial and ethnic minorities, as well as nonpsychiatric mental health professionals, made little difference in the overall perspective of the next edition of the manual. Since there was no serious rethinking of the manual from the *DSM-III* to the *DSM-IV*, there would be little room for alternative perspectives to actually get in to the manual. And if that were not enough, the inclusion that did happen seemed to be more about including diverse body types rather than genuinely diverse perspectives. Indeed, most of the difficulties that Caplan describes in getting a feminist perspective into the manual involve struggles she had with *DSM-IV* developers—many of whom, at least in the subcommittees that Caplan was working with, were women. Just because more “women” are let into science, that does not mean that more feminists are let into science. *DSM-IV* is no exception.

Caplan and Kirk and Kutchins give us enough information to sketch out the politics of *DSM-III*’s development, but their motivation for doing so is largely wrapped up in a bad-science critique. For both Caplan and Kirk and Kutchins, the relevance of these internal psychiatric politics is that they created bad science and distorted data. But bad science and distorted data are not the whole problem. The *autocratic and exclusionary politics* used by the *DSM* developers must be critiqued directly. Otherwise, the situation perpetually repeats itself. By staying within a “bad-science” critique, Caplan and Kirk and Kutchins make it seem as if there would be no problem if the developers had only agreed with them. But that misses a major issue. These autocratic and exclusionary political tactics are at the core of *DSM-III* and *DSM-IV*. They go straight back to the initial political style set by Robert Spitzer. Changes in the details of the manual will not change this issue. The politics of *DSM* must be changed directly. Spitzer’s autocratic style is a problem not only because it led to bad science but more fundamentally because his politics are bad in themselves. In other words, Spitzer’s politics are bad (have bad consequences) for psychiatry because his politics are bad—too authoritarian and too antidemocratic.

At this level of political critique, the science question and even the rhetorical question are only part of the problem. They are surface manifestations of deeper political problems. Spitzer and the neo-Kraepelinians must also be critiqued on political grounds. Kirk and Kutchins have shown us that Spitzer’s science was bad, and my review of alternative rhetorical options shows that their rhetoric was bad. But improving future *DSM* developers’ science and rhetoric will not improve their politics. That will require specific and direct attention to the politics of science and knowledge in psychiatry.

Just a quick peak into the planned *DSM-V* will give a sense of what I mean. Preliminary *DSM-V* research planning activities can be found in David Kupfer, Michael First, and Darrel Regier’s book *A Research Agenda for DSM-V* (2002), and ongoing information can be found at the *DSM-V* Prelude Project Web site (<http://www.dsm5.org/index.cfm>). One of the most striking things about these early efforts toward creating the *DSM-V* is how willing the developers are to open up questions of science and even rhetoric, at least up to a point. Please do not get me wrong: the developers remain within a very narrow scientific frame. The goal of the *DSM-V*, as the new developers put it, is to “enrich [the] empirical data base” and to incorporate scientific research findings from “animal studies, genetics, neuroscience, epidemiology, clinical research, cross-cul-

tural research, and clinical services research" (see <http://dsm5.org/whitepapers.cfm>). But within that scientific frame, the developers say clearly that an "improved scientific basis" for the *DSM* will likely require major changes. Indeed, they expect it to require "as yet unknown paradigm shifts" (Kupfer, First, and Regier 2002, xix).

Although the new "paradigm" remains uncertain, all indications suggest that the developers plan to move the *DSM-V* from a syndrome-based perspective to a pathophysiology-based one. The new developers set the stage for this by lamenting that current *DSM* categories are "devoid of biology." As such, they plan to develop a system that incorporates multiple forms of biological markers. These markers, they argue, will allow psychiatry to finally achieve more than "reliability." Through a pathophysiological system, psychiatry can achieve "valid" psychiatric diagnoses that do not shy away from etiological explanations.

The domains in which the developers plan to look for etiological diagnostic categories include: "1) better animal models for the major psychiatric disorders; 2) genes that help determine abnormal behavior in animal models; 3) imaging studies in animals to better understand the nature of imaged signals in humans; and 4) functional genomics and proteomics involved in psychiatric disorders, that is the identification of genes or proteins that are regulated in particular brain regions by a given drug or behavioral state." In addition, they plan to include: "1) work to identify disease-related genes from among the 26,000 identified in the human genome project; 2) post-mortem studies to examine circuitry and gene expression; 3) the newer brain imaging techniques; 4) approaches that integrate the use of multiple modalities; and 5) neuroinformatics, the integration and management of large amounts of data produced at various levels of investigation" (see <http://dsm5.org/whitepapers.cfm>).

The genius of this plan is that, should the developers succeed, they will dramatically throw up for revision both the basic science and the rhetorical frame (or paradigmatic model) of the *DSM*, while still staying within the same larger scientist episteme. But for all their openness to change and exploration, what the developers are not throwing up for consideration is the question of politics. Who gets to sit at the table of the creation of *DSM-V*? Why are these people chosen? What kinds of efforts are made to generate diversity? Who gets selected to leadership positions? What kinds of authority do they have? How are differences approached? There is no sign that these questions are given any systematic thought. All the

systematic thought goes into questions of content. None goes into the question of process and inclusion.

Interestingly, there are some nonsystematic efforts at considering questions of inclusion. For example, the early planning conferences are being cochaired by a "distinguished investigator in the same field from a country other than the U.S." (see <http://dsm5.org/planning.cfm>). And there is some reaching into additional disciplines. For example, the developers tell us work-group members were selected "primarily for their expertise in diverse areas such as family and twin studies, molecular genetics, basic and clinical neuroscience, cognitive and behavioral science, development, life span issues, disability, psychopathology, and treatment. In order to encourage thinking beyond the current *DSM-IV* framework, most of the workgroup members had not been closely involved in the *DSM-IV* development process" (see <http://dsm5.org/whitepapers.cfm>). But by what criteria were these efforts at diversity made? Why these new members and not others?

These kinds of inclusion gestures do not come close to a systematic consideration of difference and inclusion. They seem much more like strategically manipulative inclusions based on very narrow special interests. Do the developers really believe that adding different *DSM* developers from these "diverse areas" will truly "encourage thinking beyond the current *DSM-IV* framework"? It seems that they do, but only along very constricted lines.

As a result, the emergent *DSM-V* will in all likelihood both dramatically change and fundamentally stay the same. It will change in its content, but it will stay the same in its basic scientific frame and its fundamental power relations. *DSM-V* will rearrange the science and rhetoric but not change the critical problems with a narrowly scientific disease-model approach. Nor will it change in any meaningful way its elitist and autocratic politics of inclusion.

Conclusion

Kirk and Kutchins provide invaluable tools for critiquing psychiatry's new diagnostic manual. Their work effectively critiques the fundamental scientific premise—increased reliability—on which that manual stands. However, although Kirk and Kutchins's critique is wide-ranging, and although it purports to address the "rhetoric of science," it falls short on both the rhetorical and the political dimensions of the new manual. In

Sandra Harding's terminology, Kirk and Kutchins do a bad-science critique that leaves *DSM-III* science-as-usual unquestioned. Kirk and Kutchins do not seriously challenge the basic assumptions of the manual or the political practices of its developers. From a postpsychiatry perspective, these additional rhetorical and political critiques are exactly what must happen, and science-as-usual in psychiatry must change.

This reading of *DSM* shows that contemporary science-as-usual is creating an exclusionary approach to psychiatric diagnostic research that does not include or respect alternative perspectives. In the next chapter, I move from the *DSM* to Prozac. If we now have some idea what's been going on with the *DSM*, the next cultural studies of psychiatry question I ask is this: *What in the world happened with the advent of Prozac?*

CHAPTER SEVEN

Prozac & 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, *Listening to Prozac* (italics added)

The Epidemic of Prozac Signification

The first edition of Peter Kramer's *Listening to Prozac* came out in 1993. As it happened, it made it to the bookstores about the same time as that year's American Psychiatric Convention. Kramer, a psychiatrist and new book author, was so excited about being in print that he ran to a bookstore near the convention to see himself in print. He found the book selling out as soon as a new shipment arrived: "The staff had unpacked some copies that morning, but they were sold out. . . . When I returned the reshipments had come and gone. I never did manage to catch the books: as soon as they arrived, they were snatched up" (Kramer 1997, 315). *Listening to Prozac* turned out to be a national best seller, but at the time Kramer was surprised and elated by the success of his book. He frantically called his editor to tell her that something *unusual* was going on.

With the advantage of hindsight, we now know that Kramer was right: something unusual was going on with Prozac. But what, exactly, was that something unusual? At the time, the Prozac craze of the 1990s was just getting started, and the fever pitch of the moment made it difficult to