

Purple in the Morning, Blue in the Afternoon, Orange in the Evening  
A Genealogical Analysis of Depressive Disorders in the American Psychiatric  
Association's Diagnostic and Statistical Manual, Fifth Edition

by

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## ABSTRACT

The American Psychiatric Association's *Diagnostic and Statistical Manual*, the official guidebook to psychiatric diagnosis in America, currently exempts the recently bereaved from being diagnosed with depression unless their experiences are marked by feelings of extreme worthlessness, significant functional impairment, psychotic symptoms, psychomotor retardation, or suicidal ideation. Ordinary symptoms of depression, such as sleeplessness or loss of appetite, are considered healthy, functional emotional responses to the loss of a loved one. The bereavement exemption is slated for removal in the upcoming fifth edition of the *Diagnostic and Statistical Manual*, functionally redefining the emotional distress of bereavement as a psychiatric disorder. This study employs genealogical analysis to expose the multiplicity of forces that shape modern psychiatry and the ways that the redefinition of depression functions strategically in the social negotiation of truth and power. Under the guise of etiological and prescriptive neutrality, the redefinition of depression promotes a deeply biological model of psychiatric disorder, a medicalized understanding of human emotion, and a pharmacological approach to the treatment of emotional distress. Through genealogical analysis, this project seeks to enable informed, meaningful ethico-political responses to these developments.

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## Chapter 1

### INTRODUCTION AND THEORETICAL BACKGROUND

In February of 2010 the American Psychiatric Association released a draft of proposed changes to the upcoming fifth edition of its *Diagnostic and Statistical Manual*, the official guide to psychiatric classification and diagnosis in America. Due to the ubiquity of psychiatry and psychopharmacology in our society, these changes have potentially vast and far-reaching consequences. As Michael First, editor of the *DSM-IV-TR* explains, “[a]nything you put in that book, any little change you make, has huge implications not only for psychiatry, but for pharmaceutical marketing, research, for the legal system, for who’s considered normal or not, for who’s disabled” (Carey, 2010). This paper draws on the work of Michel Foucault to genealogically recast the *DSM* revision project and analyze its function as a strategy in the social negotiation of power and knowledge.

In his first major work, *Madness and Civilization*, Foucault sought to study the workings of power and knowledge by looking at different understandings of madness throughout European history. From the repurposed leper houses of the Middle Ages to the asylums of the nineteenth century, *Madness and Civilization* is a story about the social exclusion and physical confinement of Otherness. The word *madness* has been used to describe different groups of people in different times and places, but the opprobrium of madness remains unchanged. The precise methods used to isolate the mad differ from one culture to the next, but the impulse to segregate madness at the margins of society is an historical constant. Whatever the nature of those experiences described as madness, Foucault argues that the descriptor itself is a malleable social construct used to mark the boundaries of the

social order. Sometime around the end of the eighteenth century, he tells us, these boundaries began to shift. Madness came to be understood as a medical phenomenon:

[T]he doctor takes a preponderant place, insofar as he converts it into a medical space. However, and this is the essential point, the doctor's intervention is not made by virtue of a medical skill or power that he possesses in himself and would be justified by a body of objective knowledge. It is not as a scientist that *homo medicus* has authority in the asylum, but as a wise man... as a juridical and moral guarantee (p. 270).

Although Foucault does not focus extensively on the grounds of the doctor's institutional authority in *Madness and Civilization*, he takes up this theme in *The Birth of the Clinic* and *The Archeology of Knowledge*. In the figure of the doctor, the moral authority of the asylum, Foucault argues, we can see the workings of power/knowledge exposed. The doctor as moral authority is afforded a right to define madness as abnormality and violation, while the identification of madness as illness or disorder reinforces the institutional authority of the doctor as the agent who can eliminate the problem and rectify the transgression of the social order.

It is precisely because of the way that psychiatric knowledge is linked to the social negotiation of power and knowledge that Foucault chose to study madness in the first place. As he explained in a 1977 interview:

[I]f, concerning a science like theoretical physics or organic chemistry, one poses the problem of its relations with the political and economic structures of society, isn't one posing an excessively complicated question? Doesn't this set the threshold of possible explanations impossibly high? But on the other

hand, if one takes a form of knowledge (*savoir*) like psychiatry, won't the question be much easier to resolve, since the epistemological profile of psychiatry is a low one and psychiatric practice is linked with a whole range of institutions, economic requirements, and political issues of social regulation? Couldn't the interweaving of effects of power and knowledge be grasped with greater certainty in the case of a science as 'dubious' as psychiatry? (1980c, p. 109).

Following Foucault this paper takes psychiatric discourse about depression, as outlined in the draft changes to the *Diagnostic and Statistical Manual*, as an apt point of departure for studying the negotiation of power and knowledge in contemporary society.

### **Psychiatry Today**

The public profile of psychiatry has changed a great deal in the half century since Foucault began his work on madness. Advances in neuroscience, genetics, molecular biology, psychopharmacology, and related fields have changed the way that psychiatry is practiced, contributing to the deeply biological understanding of mental health that defines the modern psychiatric discipline. Institutional changes in the medical field have altered the nature of psychiatric treatment, encouraging psychiatrists to meet with patients for brief prescription visits while outsourcing psychotherapy to psychologists and social workers. The time a behavioral health patient spends interacting with service providers is likely to be divided up among more specialists than fifty years ago, with each providing more narrowly defined services. Most importantly, psychopharmacology has become a ubiquitous feature of modern life. Pharmaceutical companies, not physicians, represent the public face of

psychiatry in the twenty-first century, and the popularization of psychiatric drugs has changed the way we view mental distress, mental disorders, and even ourselves.

Depression is the most commonly diagnosed psychiatric condition in the world today. The World Health Organization claims that the societal burden created by depression is greater than any other psychiatric condition, and the fourth greatest among all known medical conditions. The same report projects that depression will be the second most burdensome of all medical conditions within the next fifteen years (Blazer, 2005). Looking back just fifty years, depression as we know it today was all but non-existent. The incidence of depression in Europe and America is now estimated to be fully one thousand times what it was thought to be only half a century ago, an increase of 100,000% (Healy, 2002). Whatever else this is, it is a story about psychiatric classification. A change of this magnitude is utterly incomprehensible without looking to the meaning of the term *depression* and the standards we use to classify mental distress.

This is also a story about the growth of psychopharmacology and the intersections between science, medicine, and corporate interest. The United States accounts for roughly 43% of all pharmaceutical marketing and sales (Gagnon & Lexchin, 2008), and more than two-thirds of global antidepressant sales (Ratner, 2008). According to investigative journalist Evelyn Pringle (2009), the number of Americans who are officially considered disabled by mental illnesses has skyrocketed since the release of Prozac in 1987, increasing at a rate of 150,000 people each year. Between 1996 and 2006 prescriptions for psychiatric drugs increased 50% for American children and 73% for American adults. Mental health care spending during the same ten-year period increased by 30%, almost entirely

due to drug prescription costs (Pringle, 2009). As of 2002, nine out of ten American children treated by a child psychiatrist were taking one or more psychiatric medicines (Diller, 2006), and medical spending on mental health care now outpaces spending on any other class of medical conditions for American children (Pringle, 2009).

In this environment, it is perhaps unsurprising that the scientific pedigree and validity of psychiatric practice, and psychopharmacology in particular, has come under increasing attack (Greider, 2003). While some psychiatrists argue that increases in psychiatric diagnoses and psychopharmacological prescriptions reflect better diagnostic methods or a genuine increase in the occurrence of psychiatric disorders, critics charge that these numbers are a reflection of harmful self-promotion or worse, iatrogenesis (Illich, 1975; Linneman & Zahle, 2007). Moynihan and Cassels (2006), for instance, criticize the role that drug companies play in the definition of disease, arguing that pharmaceutical companies create a market for their own products through the redefinition of disease. Although not written from an explicitly Foucauldian or poststructuralist vantage, Moynihan and Cassels highlight the malleability of disease as a construction, the ways that phenomena such as hyperactivity can move fluidly between the registers of medicine, behavior, and simple human diversity. They also highlight the role of power/knowledge in this process: those in power literally (re)define what counts as a disorder in order to create a larger market for their drugs, and in so doing shore up their own institutional authority and expert status as purveyors of a cure.

In this vein, both Critser (2007) and Angell (2005) are critical of the marketing practices employed by major pharmaceutical companies, including



direct-to-consumer advertisements intended to convince consumers to self-diagnose and aggressively pursue pharmaceutical intervention from their doctors. Greider (2003) likewise criticizes the broad commercialization of the pharmaceutical enterprise and the attendant conceptual shift from an industry that is primarily responsive to existing medical needs to one that is in the business of actively inventing new marketing niches. The pharmaceutical industry now spends upwards of \$57.2 billion a year on product promotion in the U.S. – almost twice as much as it spends on research and development – a figure that does not account for ghostwriting fees, off-label promotions, educational grants, “seeding” trials, or other forms of promotional spending that defy simple categorization as marketing expenditures (Gagnon & Lexchin, 2008). Of this, \$22 billion a year is spent marketing directly to providers, amounting to \$25,000 per U.S. physician per year (Ratner, 2008).

These marketing efforts clearly pay off: the most advertised fifty prescription drugs, a meager 5% of all prescription drugs on market in the U.S., account for half of all pharmaceutical spending in this country. The remaining half of American pharmaceutical spending is divided among roughly 9,850 other drugs, or 95% of the different prescription drugs on the market (Medawar & Hardon, 2004,). Shockingly, these numbers *drastically downplay* the actual role of pharmaceutical advertising in shaping demand because an unknown portion of the money spent on the latter group of drugs is effectively spillover from the advertising of the former, money spent on unadvertised generic versions of heavily advertised brand name drugs.

Following Foucault, this paper proceeds from an understanding of the *DSM* revision project as the site of even more fundamental contestation over issues of truth, power, and authority. What is at stake here is not only the definition of normality or illness, but the very ability to define normality or to speak with authority on matters of illness. This project is concerned not simply with what counts as normal under the new schema, but with the process through which normality is schematized. This project will examine how some voices are authorized to speak about normality and disorder while others are not, and the ways that some perspectives are accorded the status of genuine knowledge while others are devalued and disregarded. Most importantly, this project will explore the processes and strategies by which the meaning of disorder is socially negotiated through the *Diagnostic and Statistical Manual*, the ways that the *Diagnostic and Statistical Manual* helps to shape what is considered authoritative and true.

### **Theoretical Background: The Critical Rhetorical Project**

The theoretical underpinnings of this project can be traced to the strand of rhetorical scholarship known as critical rhetoric, with roots in Phillip Wander's (1983) landmark essay *The Ideological Turn in Modern Criticism*. Following the conversation initiated by Wander and Raymie McKerrow's (1989) call for the development of a critical rhetoric, a growing body of work in the field of rhetorical studies has striven to incorporate the insights of critical theory into the rhetorical tradition.

Wander's (1983) essay, written in a time of intense public anxiety over the end of détente and the re-escalation of the U.S.-Soviet arms race, argues that rhetorical critics should move beyond simple evaluations of public address and the

traditional Greek canon to engage in unabashedly ideological criticism that responds to the social, historical, and political exigencies of the time. Wander (1984) further outlines the entailments of socially responsible and responsive ideological criticism by way of a concept he dubbed the Third Persona: the subject position occupied by those who are neither involved in rhetorical production (the First Persona) nor hailed as an audience to that production (the Second Persona). "The Third Persona, therefore, refers to being negated. But 'being negated' includes not only being alienated through language... negation extends beyond the 'text' to include the ability to produce texts, to engage in discourse, to be heard in the public space" (p. 210). For example, trans- and intersexual people are excluded, silenced, and overlooked, their very status as political subjects negated, by numerous legal and social institutions that are based on the simple division of all people into two sexes. Wander argues that ideological criticism should work to interrogate these types of abjection and produce new, more democratic, modes of public engagement.

Klumpp and Hollihan (1989) take up this line of argumentation, suggesting that criticism can be ideologically conservative or ideologically critical, but not ideologically neutral. Because of the way that certain subject positions, certain personae, are excluded from legitimized discourses, rhetorical criticism that does not work to expose the network of power relations underlying such discourses serves a fundamentally conservative ideological role, masking and entrenching the operations of power. Crowley (1992) further expands upon this model by problematizing the role of the critic in the production of knowledge. The failure to engage in ideological criticism, according to Crowley, represents not just a form of

political complicity, but a form of epistemological complicity with extant relations of power and the marginalization they effect. As Dana Cloud (2001) explains:

[C]riticism that shuns overt politics is either ignorant of or masking its own investments in the status quo... Affirmation bears the privilege of invisibility; it possesses the luxury of never appearing "heavy-handed"... Nonetheless, affirmation is an act of advocacy. Even when criticism claims to be descriptive of social reality rather than offering normative correctives to unethical or malign rhetorical practices, the retreat into description is profoundly ideological. As Kenneth Burke (1966) has warned us, the language we use to describe the world necessarily filters our audience's perception of that world – foregrounding what aspects of it are deserving of our attention and backgrounding aspects which are not. Those whose work is visible as political critique – often representing minority, subordinated, or marginalized interests and positions – simply have different, and therefore more visible politics than those masquerading in "descriptive" or "apolitical" criticism (p. 1).

McGee (1990) notes that these arguments belie a profound paradigmatic shift in the field of rhetorical studies. In just a few years, the discipline's millennia-old focus on the criticism *of* texts had shifted to the study of criticism *as* text.

McKerrow's 1989 essay, *Critical Rhetoric: Theory and Praxis*, was one of the first in the field of rhetorical studies to explicitly draw upon the model of power/knowledge articulated by Michel Foucault. It is certainly the most influential early work to do so, and it is recognized both as a foundational essay in the development of a critical rhetoric and a key point of intersection between rhetoric

and the human sciences through a shared history and vocabulary of continental theory (Greene, 1998). Here, McKerrrow (1989) calls for the development of a critical rhetoric, a mode of criticism that “seeks to unmask or demystify the discourse of power. The aim is to understand the integration of power/knowledge in society – what possibilities for change the integration invites or inhibits and what intervention strategies might be considered appropriate to effect social change” (p. 91). To do this, he suggests an engagement with two strands of thought running through the works of Foucault: a critique of domination and a critique of freedom. Though critically inclined rhetorical scholars have embraced McKerrrow’s goals, many also levy criticism at his reading of Foucault.

Ono and Sloop (1992), for instance, criticize McKerrrow’s bifurcation of domination and freedom. They argue that domination and freedom are inextricably connected in the Foucauldian model. Power relations only function as constraint in instances where those affected are, in the first instance, unconstrained. As such, freedom and resistance are always present in relations of power. Moreover, and in part because of this, power in the Foucauldian model is never reducible to domination. Although there can be domination within relations of power, power itself is productive, a force that circulates, making knowledges and subjects useful.

Barbara Biesecker (1992) is similarly critical of the way McKerrrow tries to operationalize Foucault’s model of power/knowledge in rhetorical studies. Biesecker explains the productive nature of power through a lengthy narration of the repressive hypothesis. Popular readings of European history describe a profound shift in attitudes about bodies and sex that began to take root sometime in the latter part of the seventeenth century. Once treated with relative openness,

sexuality was increasingly suppressed, hidden away, and rendered taboo, culminating in the severe sexual repression and hypocrisy of nineteenth century Victorianism. Sexuality was confined to the marital bedroom where it was joyless and utilitarian. The taboo on sexuality stifled it, suppressed it. People were secretly obsessed with sex, but it was dirty, illicit, and forbidden, driving them to pathology; in this narrative, Jack the Ripper comes to be an icon for Victorian sexual repression. Following Freud, the repressive hypothesis holds that the answer to these problems can be found in talking openly about sexuality. To challenge the taboo on sexuality is to liberate oneself from the confines of repression. Talking openly about sex and sexuality thus becomes a revolutionary act, both a form of political resistance and a cure for pathology.

Foucault (1978) sees in the repressive hypothesis a fundamental misunderstanding about the nature of power. Here, power is understood as simple and repressive, as distinct from and at odds with the truth about sex. Contrary to the simplistic narrative of repression, Foucault argues that there was something else going on alongside the Victorian suppression of sexuality. While sexuality was disappearing from public everyday speech, there was a proliferation of other, official discourses about sexuality – the development of a *scientia sexualis*. Sex became an object of medical and psychiatric knowledge. Nearly any medical problem a woman might have was sexually pathologized as hysteria. Bawdy jokes and open talk about sex in places like the school and the military barracks were replaced with official proclamations and meticulous regulation. Everywhere there were warnings about the evils of sex. For Foucault, the repressive hypothesis misses half of the story. Sexuality was confined, repressed, and made taboo, but existing discourses about

sexuality were not simply suppressed or eradicated; they were channeled and transformed. What was once freely the object of vernacular, public discourses became the object of an explosion of official discourses that defined, measured, recorded, theorized, pathologized, and otherwise regulated sex. New discourses came into being with the sole aim of controlling sexuality. Far from eradicating sexuality as an object of discourse, increasing concerns about the dangers of sexuality motivated people to see sexuality everywhere, to think and talk about sexuality where they had not before. The relations of power that the repressive hypothesis understands as merely constraining were in fact *productive* of discourses about sexuality, only discourses of a different type.

The most important consequence of this for Foucault is the way that the repressive hypothesis valorizes “resistance” against the repression of sexuality. As Biesecker explains, understanding power as repression “is dangerous therefore not simply because it limits what can be said, but more importantly, because it incites speakers to believe that the very discourses it has effected are both of their own making and directed against it” (p. 353).

With this important corrective to McKerrow’s model of rhetorical production, Biesecker articulates a slightly different vision of critical rhetoric informed by Foucault. Her model, one that informs the genealogical criticism here, sees “the critical rhetorician’s task [as] ‘mak[ing] these virtualities visible’ by the strategic and deliberate codification of those points of resistance. In other words, the task is to trace new lines of making sense by taking hold of the sign whose reference has been destabilized by and through those practices of resistance” (p. 361). Enabling this type of sense making is the task taken up by this project.

## The Text

The primary text for this project will be the *DSM-5* Development Website located at <http://www.dsm5.org>. In particular, this project will focus on the section of the manual dedicated to depressive disorders. The website is maintained by the American Psychiatric Association. The *DSM-5* Development Website houses a variety of information related to the fifth edition of the American Psychiatric Association's *Diagnostic and Statistical Manual of Mental Disorders (DSM-5)*, slated for publication in May 2013.

Most importantly, the *DSM-5* Development Website details the changes proposed to-date by the thirteen *DSM-5* Work Groups, each comprised of around a dozen psychiatric professionals. Draft changes were posted on the website on February 10, 2010. They are arranged by disorder according to the existing seventeen-section *DSM-IV-TR* taxonomy with a separate section for structural, cross-cutting, and general classification issues in the *DSM-5*. The webpage for each disorder details the proposed text of the *DSM-5* revision, the rationale for the revision, the proposed severity criteria for the disorder (a new feature in the *DSM-5*), and the *DMS-IV* text.

In addition to these draft changes, the *DSM-5* Development Website contains information about the American Psychiatric Association; the *Diagnostic and Statistical Manual*; the Task Force, Study Groups, and Work Groups that contributed to the *DSM-5*; *DSM-5* development conferences and research; progress reports; and a newsroom. Before April 20, 2010 registered users were able to submit comments on the draft changes, although these comments were not displayed on the *DSM-5* Development Website or otherwise made public, and thus are unavailable for use as



rhetorical text. The *DSM-5* Development Website is currently available for viewing only.

The American Psychiatric Association website says of the *DSM*:

The *Diagnostic and Statistical Manual of Mental Disorders (DSM)* is the standard classification of mental disorders used by mental health professionals in the United States. It is intended to be applicable in a wide array of contexts and used by clinicians and researchers of many different orientations (e.g., biological, psychodynamic, cognitive, behavioral, interpersonal, family/systems)... It is also a necessary tool for collecting and communicating accurate public health statistics.

The most notable change in the *DSM-5* section on depressive disorders is not to the diagnostic criteria for the codeable depressive disorders, but to the definition of a depressive episode. The criteria for depressive disorders refer to the occurrence of depressive episodes. In the current edition of the *DSM*, there is an exemption that disqualifies an experience as a depressive episode if the symptoms are “not better accounted for by Bereavement, i.e., after the loss of a loved one, the symptoms persist for longer than 2 months or are characterized by marked functional impairment, morbid preoccupation with worthlessness, suicidal ideation, psychotic symptoms, or psychomotor retardation.” The proposed update to *DSM-5* removes this clause because “the evidence does not support separation of loss of loved ones from other stressors.”

The *Diagnostic and Statistical Manual* has been revised six times since the 1952 publication of the first edition. The last full revision to the *DSM* was the *DSM-IV*, published in 1994. In 2000 the APA published a text revision of the fourth edition

(*DSM-IV-TR*), which made few changes to diagnostic criteria but updated the text sections explaining each diagnosis. Though referred to through parts of its development as the *DSM-V*, the *DSM-5* will be first edition to use a standard Arabic numeral rather than a Roman numeral. Scientific knowledge is advancing at an increasingly rapid pace, and modern technology renders the traditional publishing model that governed previous versions of the *DSM* all but obsolete. The American Psychiatric Association changed the current *DSM* naming convention in anticipation of future incremental revisions to be identified with decimal notation (*DSM-5.1*, *DSM-5.2*, and so forth).

### **The Foucauldian Model: Toward an Analytics of Power**

In the Foucauldian model, truth and power are co-productive, or mutually constitutive. What counts as truth within a given discursive formation, an historically situated field of knowledge defined by a set of common rules governing intelligibility, both shapes and is shaped by existing relations of power. Neither is outside of or prior to the other. Those in positions of institutional authority are accorded the ability to define what counts as true knowledge, which in turn shapes the characteristics and distribution of power relations within institutions. Foucault (1980c) writes:

Truth isn't outside power, or lacking in power: contrary to a myth whose history and functions would repay further study, truth isn't the reward of free spirits, the child of protracted solitude, nor the privilege of those who have succeeded in freeing themselves. Truth is a thing of this world: it is produced only by virtue of multiple forms of constraint. And it induces regular effects of power. Each society has its regime of truth, its 'general

politics' of truth: that is, the types of discourses which it accepts and makes function as true; the mechanisms and instances which enable one to distinguish true and false statements, the means by which each is sanctioned; the techniques and procedures accorded value in the acquisition of truth; the status of those who are charged with saying what counts as true (p. 131).

The inter-relationship between power and knowledge is absolutely central to Foucault's model of power.

Foucault (1978) most completely sketched out his understanding of power during the last years of his life. He was careful to frame this work as an "analytics of power," explicitly contrasted with theory. Foucault's analytics of power is derived from his genealogical analysis of specific historically situated texts. It is not intended to be an ahistorical, objective, or context-free account. In fact, such a thing is anathema. "Power in the substantive sense, *le pouvoir*, doesn't exist... power means relations, a more-or-less organized, hierarchical, co-ordinated cluster of relations" (Foucault, 1980a, p. 198). Power here is intersubjective. It exists in the relations between subjects. Power functions through a process of doing, in the operation of political technologies. It is not an independent thing with a beginning outside of and beyond the subjects in whose name it operates. "If one tries to erect a theory of power one will always be obliged to view it as emerging at a given place and time and hence to deduce it, to reconstruct its genesis. But if power is in reality... [a] cluster of relations, then the only problem is to provide oneself with a grid of analysis" (Foucault, 1980a, p. 199). Even if power could be theorized as such, this is simply not Foucault's concern; the goal is not to understand the *what* of power, the

nature or being of power understood for its own sake, it is rather to understand the *how* of power, the mechanisms and the processes through which power functions.

Foucault's (1978, 1980c, 1980d, 1982) sketch of an analytics of power begins from the recognition of power as a matrix of force relations. In this understanding, power is not a thing that is possessed or wielded, but a system of relations similar to a web or a capillary system. These relations of power are non-egalitarian and mobile, meaning that power has the ability to spread, to manifest in the exercise of new political technologies. Despite being asymmetrical, power relations are multidirectional. Power is exercised both from the top down and from the bottom up. As Dreyfus and Rabinow (1983) explain:

In the prison, both the guardians and the prisoners are located within the same specific operations of discipline and surveillance... The guards in Mettray prison had undeniable advantages in these arrangements; those who constructed the prison had others; both groups used those advantages to their own ends. Foucault is not denying this. He is affirming, however, that all of these groups were involved in power relations, however unequal and hierarchical, which they did not control in any simple sense. For Foucault, unless these unequal relations of power are traced down to their actual material functioning, they escape our analysis... maintaining the illusion that power is only applied by those at the top to those at the bottom (p. 186).

Critically, this means that power is never equivalent to domination or reducible to domination, even if power can, and often does, produce relations of domination.

An important corollary to this point is that the Foucauldian model recognizes latent tension or resistance in all relations of power. In traditional zero-

sum models of power, power is regarded as a thing to be possessed. Here, one can be cast as powerless, quite literally without and other from power. If power is relational, however, then all actors, even those subjected to domination, are situated in a field of power relations. And if power relations are never simply unidirectional, wielded by one actor over and against others in a strictly top-down manner, then there are in every relationship of domination possibilities for the exercise of power by those being dominated. This is sometimes referred to as strategic reversibility, a potentially misleading phrase as the strategic reversibility of power relations does not imply the possibility of *effective* resistance nor does it imply that all relations of power can be flipped on their heads. The strategic reversibility of the power relations between prisoners and prison guards, for instance, might not extend beyond the ability of prisoners to hunger strike or to refuse minor orders at the risk of punishment. The tension inherent in relations of power is a precondition for political contestation because it prevents the overdetermination of agency at the site of power's material operation, but Foucault's point here is *not* about political practices – his claim is a mechanistic one about the character of those technologies through which power invests itself in the social field.

The operation of power in the Foucauldian model is both intentional and non-subjective. Power relations have an order, a rationality that allows them to be analyzed systematically, but that rationality is not by necessity the design of a knowing, choosing subjectivity. People may behave in ways that are more or less intentional vis-à-vis the power relations in which they are situated for reasons that they generally understand, but this local and immediate knowledgeability does not necessarily extend to the broader systemic forces that affect individuals' actions or

the system-level implications of these actions. “Let us not, therefore, ask why certain people want to dominate, what they seek, what is their overall strategy. Let us ask, instead, how things work at the level of on-going subjugation... how it is that subjects are gradually, progressively, really and materially constituted through a multiplicity of organisms, forces, energies, materials, desires, thoughts, etc.” (Foucault, 1980d, p. 97). Cultural regimes of gender nicely illustrate the intentional and non-subjective nature of power relations, as well as the futility of searching for a ghost in the machine rather than examining power in its material functioning.

Power here is understood as diffuse, extending to the smallest crevices of the social fabric. Even the largest, most abstract systems of power are merely amalgamations of individual power relations that manifest in the daily actions and iterations of individual actors. Analyses of power should therefore “be concerned with power at its extremities, in its ultimate destinations, with those points where it becomes capillary, that is, in its more regional and local forms... where power... invests itself in institutions, becomes embodied in techniques, and equips itself with... means of material intervention” (Foucault, 1980d, p. 96). Some critics have claimed that this characterization of power forecloses the possibility of agency (see Taylor, 1984). As Kulynych (1997) explains, however, Foucault’s account actually *expands* the possibilities for agency by locating the operation of power in the slight, mundane repetitions of everyday life,

explod[ing] the distinction between the public and private, between the political and the apolitical... [W]hat was formerly considered apolitical, or social rather than political, is revealed as the foundation of technologies of state control. Contests over identity and everyday social life... create the

very character of those things traditionally considered political. The state itself is “superstructural in relation to a whole series of power networks that invest the body, sexuality, the family, kinship, knowledge, technology, and so forth.” Thus it is contestations at the micro-level, over the intricacies of everyday life, that provide the raw material for global domination and the key to disrupting global strategies of domination (pp. 337-338).

Traditional models of power locate its operation in behemoth, edificial ideologies or the workings of formal institutions, far beyond the ordinary sphere of influence accorded to the people affected by it. By tracing the operation of power to the sites of its most minute reproductions, Foucault’s model reveals previously unrecognized spaces for the exercise of political agency, and with them, new possibilities for micro-political practices of resistance.

Finally, power in the Foucauldian model is productive. Power here is understood not simply as a form of negation or constraint that regulates or limits pre-given subjects, but as a force that circulates, shaping subjectivities, making bodies and discourses useful. As Foucault (1980c) explains,

If power were never anything but repressive, if it never did anything but to say no, do you really think anyone would be brought to obey it? What makes power hold good, what makes it accepted, is simply the fact that it doesn’t only weigh on us as a force that says no, but that it traverses and produces things, it induces pleasures, forms knowledge, produces discourse. It needs to be considered as a productive network which runs through the whole social body, much more than as a negative instance whose function is repression. In *Discipline and Punish* what I wanted to show was how, from

the seventeenth and eighteenth centuries onward, there was a veritable technological take-off in the productivity of power. Not only did the monarchies of the Classical period develop great state apparatuses (the army, the police and fiscal administration), but above all there was established at this period what one might call a new 'economy' of power, that is to say procedures which allowed the effects of power to circulate in a manner at once continuous, uninterrupted, adapted, and 'individualised' throughout the entire social body (p. 119).

As we have already seen with the repressive hypothesis, this facet of power is often concealed through a sort of double production whereby the productive effects of power are rendered as natural and pre-existing, masking the foundational processes through which they are produced. Butler (1989) explains:

Juridical notions of power appear to regulate political life in purely negative terms... But the subjects regulated by such structures are, by virtue of being subjected to them, formed, defined, and reproduced in accordance with the requirements of those structures... through certain exclusionary practices that do not "show" once the juridical structure of politics has been established... In effect, the law produces and then conceals the notion of 'a subject before the law' in order to invoke that discursive formation as a naturalized foundational premise that subsequently legitimates the law's own regulatory hegemony (pp. 4-5).

This strategic concealment of power's productivity is a theme we will return to in the next section.



## Biopower

Beyond the generalized, abstract sketch of power outlined above, Foucault (1978) gives us a highly specific and detailed account of the predominant form that power takes in modern society. Traditional models of power derive from the figure of the monarch. Here, power is figured as hierarchical and monolithic, the tool with which a sovereign forces subjects to comply through prohibition, limitation, denial, and punishment. Though theories of law are often understood as challenges to the rule of sovereignty, they operate within the same basic problematic of sovereign power. Theories of legal right simply attempt to fix the limits of sovereign power, to locate its philosophical basis and define its legitimate uses. “[W]hen this legal edifice... is turned against that control [of the monarch], it is always the limits of this sovereign power that are put into question, its prerogatives that are challenged... the King remains the central personage in the whole legal edifice of the West” (Foucault, 1980d, p. 94). Foucault suggests that this may be a more or less adequate way to understand the operation of power throughout much of European history, but recent centuries have been witness to the evolution of a new form of power – biopower: a power of life, a power over life, “an explosion of numerous and diverse techniques for achieving the subjugations of bodies and the control of populations” (Foucault, 1978, p. 140). In contrast to traditional juridical understandings of power, Foucault’s analysis of biopower strives

to reverse the mode of analysis followed by the entire discourse of right from the time of the Middle Ages... to show not only how right is, in a general way, the instrument of this domination – which scarcely needs saying – but also to show the extent to which, and the forms in which, right (not simply

the laws, but the whole complex of apparatuses, institutions, and regulations responsible for their application) transmits and puts in motion relations that are not relations of sovereignty, but of... the manifold forms of domination that can be exercised within society. Not the domination of the King in his central position, therefore, but that of his subjects in their mutual relations: not the uniform edifices of sovereignty, but the multiple forms of subjugation that have a place and function within the social organism (Foucault, 1980d, pp. 95-96).

Although the discourse of right and sovereign power persists to this day, it is these multiple forms of subjugation, the diverse techniques that act upon bodies to define, shape, and control subjectivity, that hold the key to understanding the distinctive form of political rationality operative in modern society.

Foucault traces the historical emergence of this political rationality by situating it amidst larger developments in the evolution of Western political thought. From the time of the ancient Greeks, political thinking in the West was centrally concerned with the metaphysical. Political philosophy was understood as an exercise in defining the good life. Governing, in its ideal form, at least, was understood as an exercise in virtue. Political treatises outlined the values that should inform political decision-making, and political decision makers could be judged by their adherence to these transcendental notions of right. This way of thinking about the political ended abruptly in the sixteenth century, when traditional political rationalities gave way to Machiavellianism. This new political philosophy concerned itself exclusively with the preservation of the prince and his state. Under the influence of Machiavelli, the calculations of the prince became

divorced from transcendental values and otherworldly ends. Politics became a justification for the most self-serving uses of sovereign power. Beginning around the same time as Machiavellianism, Foucault identifies a second major turning point in Western political thought: the development of *raison d'état* as a political philosophy. Political philosophers often treat the logic of *raison d'état* as co-extensive with Machiavellianism, but Foucault sees a critical disjuncture here. Where Machiavellianism simply redefined the proper ends of state power, the philosophy of *raison d'état* made state power an end in itself, an end requiring new techniques and practices to form bodies in the service of power. It is here that we can see the historical roots of biopower beginning to take shape as a coherent political technology.

According to Foucault, biopower developed along two different axes of concern: control of the body and control of the population. The first axis of biopower is the control of individual bodies. Biopower renders bodies docile and then makes them useful through subtle, meticulous techniques of observation, spatialization, hierarchization, and normalization that carefully define the most specific and minute elements of the individual's existence. The second axis of biopower is the control of populations. The development of biopower was attended by a profound shift in thinking about the practice of ruling and the interweaving of political philosophy with the human sciences. Rulers, administrators, and social scientists began to focus on problems concerning the growth and health of populations. People began to study populations, gather data on them, and theorize about threats to their existence. Demographics, administrative record keeping, the history of populations, and reproductive trends all became objects of great concern.

Most pertinent for the study at hand, one of the areas where we can see these two axes of biopower – control over the social collectivity and control over individual bodies – coming together most clearly is in the negotiation and disciplining of madness in the modern age. For most of European history, the treatment of madness has been marked by practices of differentiation and exclusion, from the leper houses of the Middle Ages to the asylums of the early Classical Age. During the Renaissance, the mad were put on the *Narrenschiff*, the Ship of Fools, and sailed around Europe, where each village would pay the captain of the ship to keep sailing. Madness was always kept at arms' length, the constitutive Otherness against which subjectivity was given meaning. Under the political rationality of biopower, madness came to be refigured as an object of public interest and control. The mad were still seen as a threat to the social order, but now their bodies could be made productive if only they could be cured or fixed. Thus they had to be counted, sorted, recorded, tracked, and locked away until their pathologies could be diagnosed and treated, until they could be made useful. Foucault tells us that this focus on bodies – on counting bodies, on disciplining bodies, and on making bodies useful – was paralleled in institutions all across society. In schools, in factories, in hospitals, in the military: everywhere the process was the same. Bodies were subjected to discipline to make them useful for society through a meticulous process of observation, spatialization, hierarchization, and normalization.

Although biopower is the predominant form of power operative in modern society, tropes of sovereign power, natural law, and political right still dominate our political discourse. This is no simple mistake or oversight; this discourse functions to strategically conceal the operation of biopower. "Power is tolerable only on the

condition that it mask a substantial part of itself. Its success is proportional to its ability to hide its own mechanisms” (Foucault, 1978, p. 86). Power works most effectively when people do not even realize they are being subjected to it. Traditional juridical power can only render compliance through force or threat of force, but power that works through subtle normalization and hierarchization is more innocuous. It is harder to know when one is being subjected to it and it is harder to resist.

This is the form of power that Foucault believes predominantly characterizes our modern condition, for better or for worse. For when power operates at the level of life, both the protection and the destruction of life become matters of political choice. Biopower made possible the rise of capitalism, industrialism, and the emergence of the nation-state. It also made possible the holocaust and the specter of universal nuclear annihilation: “If genocide is indeed the dream of modern power, this is not because of the recent return to the ancient right to kill; it is because power is situated and exercised at the level of life, the species, the race, and the large-scale phenomena of the population” (Foucault, 1978, p. 137).

### **Research Questions**

Drawing on the Foucauldian model, this project will genealogically reframe the proposed changes to depressive disorders in the American Psychiatric Association’s upcoming *Diagnostic and Statistical Manual of Mental Disorders, Fifth Edition* in order to understand how these changes function as strategies of power. While the primary text of this study is the proposed changes from *DSM-IV-TR* to *DSM-5*, this is not a comparative study. Rather, this project views the proposed

changes to the *DSM-5* as strategies of power in the social negotiation of meaning, legitimacy, and authority. The focus of this project is not on the effects of these changes per se – on who will count as normal and who will count as abnormal if/when these changes are instituted – but on the processes by which normality is schematized, on who gets to say what normal and abnormal are.

Accordingly, this project will seek to answer the following research questions:

R1: What do the proposed changes to depressive disorders in the *DSM-5* reveal about the rules and power relations that determine who can speak with authority about matters of mental health?

R2: What do the proposed changes to depressive disorders in the *DSM-5* reveal about the way depressive disorders are understood in our society?

Chapter two will outline the methodology of the study. Drawing from Foucault's later historiographic work, this is not a simple narrative history but a genealogy that seeks to highlight how the current situation came to be and how it could have been otherwise.

## Chapter 2

### THE GENEALOGICAL METHOD

The history of psychiatry is a treacherous terrain to map, riven by both ideological and paradigmatic differences. Historian of science Thomas Kuhn (1962) argues that the sciences are governed by paradigms, or exemplars of scientific processes that give way to a common understanding about the methods, content, rules, and ends of legitimate scientific investigation – and hence scientific knowledge – within a particular discipline at a particular time. Each paradigmatic framework is governed by an internal logic that does not necessarily correspond to immutable Truth or objective reality. Quite the contrary, Kuhn argues that there are anomalies and explanatory gaps in all paradigms. Knowledges that do not fit within the internally referential system of the currently dominant paradigm are often rendered marginal or altogether unintelligible, but even when these are acknowledged a paradigm can continue to function despite the presence of anomalies or errors. The work of the normal sciences is to find a way for these anomalies to be reconciled with the dominant paradigm.

According to Kuhn, a paradigm is only thrown into crisis when large numbers of people begin to question its validity and support a competing paradigm. This generally happens when the accumulation of anomalies reaches a critical mass or when there are broad changes to the perceived need for the current paradigm to account for some previously recognized and accepted anomaly. A paradigm shift, or scientific revolution, is said to have occurred when a new paradigm, whose validity is internally referential and thus by definition cannot be fully reconciled with the old paradigm, gains widespread support and the paradigm that existed before it falls

into disuse. Kuhn's notion of the paradigm overlaps considerably with the somewhat more narrow Foucauldian concepts of the *episteme*, *dispositif*, and discursive formation, and neatly parallels the theoretical model employed in this analysis.

Psychiatry has undergone several major paradigm shifts during its short history, significantly complicating the historian's task. Consider, for instance, the radical discontinuity between two authoritative histories of psychiatry written just a quarter century apart: Henri Ellenberger's *The Discovery of the Unconscious: The History and Evolution of Dynamic Psychiatry*, published in 1970, and Edward Shorter's *A History of Psychiatry: From the Era of the Asylum to the Age of Prozac*, published in 1997. Ellenberger, writing at the height of the psychoanalytic period, traces the roots of psychiatry through a variety of traditional healing practices and esoteric traditions, including shamanism, exorcism, temple healing, and Mesmerism. Shorter, writing after the eclipse of psychoanalysis by modern biological psychiatry, understands psychiatry as a form of medical science whose roots lie squarely in the Western medical tradition. By both accounts the discipline we now recognize as psychiatry was born during the latter part of the eighteenth century, but they agree on little more than this broad periodization. In Shorter's version, the significance of this date owes to the advent of the therapeutic asylum, founded on the novel conviction that the asylum could be converted from a strictly custodial institution into a curative one. This conversion of the asylum space was critical for the development of biological psychiatry because it laid the groundwork for institutionalized psychiatric experimentation. Psychoanalysis, on the other hand, developed in large part beyond the walls of the asylum and owes little to



experimental methodologies. In Ellenberger's account, the importance of the late eighteenth century for the story of psychiatry can be traced to the physician Franz Mesmer's discovery of biomagnetism and the role it played in winning public acceptance for the idea of rational, rather than magic, healing.

Although both writers generally agree about the main cast of characters and the breadth of themes that find articulation in this story, the relative importance of these differs drastically. The main players in Ellenberger's narrative are primarily psychoanalytic theorists, with neurologists and chemists playing smaller, supporting parts. In Shorter's account these roles are reversed; the key players are neuroscientists and chemists, with the analysts relegated to a single chapter entitled "The Psychoanalytic Hiatus." Perhaps most telling of all, the index to the Ellenberger book lists twenty-eight distinct subheadings under the category of "unconscious," thirty subheadings for "dream, dreams" and forty different subheadings for "hypnosis" and "hypnotism." Shorter's index lists a single entry for "dream analysis" and one for "hypnosis." The unconscious – the discovery of which is the crowning achievement of psychiatry in Ellenberger's narrative, both the title and the underlying theme of his book – does not even appear in the index to the Shorter text. By contrast, Shorter sees effective drug treatments as the pinnacle of psychiatry's disciplinary evolution. Shorter's index lists over one hundred separate entries related to pharmaceuticals, pharmaceutical companies, and psychopharmacology. The index to the Ellenberger text lists two.

Nor are the differences between these texts merely topical. *A History of Psychiatry* is a book about experimental science, written for an audience conversant in the language of science and sympathetic to the biological view of psychiatric

illness. *Discovery of the Unconscious* is a book about psychoanalysis written by an analyst for an audience favorably inclined toward psychoanalytic methods. Where the substructure of the Shorter book is mainly chronological, much of Ellenberger's text is arranged thematically within a broader chronological framework. Shorter's prose moves quickly from one point to the next, trading liberally in the language of hypothesis testing and scientific objectivity. The Ellenberger book, on the other hand, is filled with dense biographical narrative and lengthy philosophical exposition, all framed in the language of psychoanalysis. The detailed character sketches in Ellenberger's 932-page text are like case histories, with frequent anecdotes intended to illustrate some aspect of a subject's personality. At points he muses aloud about the proper psychoanalytic interpretation of this or that occurrence in a subject's childhood. From the vantage of the scientist these are meaningless details; knowing about a thinker's childhood or family life is all but irrelevant to understanding the contributions that person made to psychiatric knowledge. For the analyst these things are nigh inseparable. Although Ellenberger and Shorter were narrating the same history less than three decades apart, there is a very real sense in which their paradigmatic differences have produced two entirely different and at times conflicting stories.

### **The Politics of Textual Interpretation**

If this degree of paradigmatic discontinuity is possible within a period of several decades, texts at a much greater historical remove can appear altogether unintelligible to later readers. Foucault (1963) gives the following example:

Towards the middle of the eighteenth century, [Pierre] Pomme treated and cured a hysteric by making her take "baths, ten or twelve hours a day, for ten

whole months.” At the end of this treatment for the dessication of the nervous system and the heat that sustained it, Pomme saw “membranous tissues like pieces of damp parchment... peel away with some slight discomfort, and these were passed daily with the urine; the right ureter also peeled away and came out whole in the same way.” The same thing occurred with the intestines, which at another stage “peeled off their internal tunics, which we saw emerge from the rectum. The oesophagus, the arterial trachea, and the tongue also peeled in due course; and the patient had rejected different pieces either by vomiting or by expectoration.” (p. ix)

It is not at all clear what Pomme might possibly have observed, or what standards we would appeal to in order to evaluate the veracity of such a claim. This creates great difficulty for the historian, whose goal is ostensibly to give an accurate accounting of what happened in the past.

There are several ways for the historian to negotiate a situation like this. One method is for the historian to recount the event as it is presented in the archival record, ignoring the likelihood that such a thing falls outside the realm of what a modern readership understands as possible, or even coherent. A second option is to reconcile the historical account with modern sensibilities through a sort of phenomenological bracketing, presenting the reader with the historian’s reinterpretation of the archival account. This is the strategy often taken by historians of psychiatry, although no amount of reinterpretation is likely to allow a modern reader to make sense of the passage above (which, of course, is precisely why Foucault chose this example). A third option is for the historian to re-present the account as it originally appears in the archival text without endorsing it as true

or valid. At first blush this might appear the clearly superior choice, presenting the reader with what was said by the archival source without claiming that such an account should be taken seriously. This approach betrays the historian's ultimate purpose though. Historians are not just collectors of old texts, but narrators and guides. The historian's task involves making choices and presenting the reader with an ostensibly accurate account of what happened in the past.

This problem is particularly acute for a genealogy of mental disorders, and the two guides I have relied upon most heavily in the construction of this genealogy take decidedly different approaches to this problem. Shorter tends toward the second approach, though not without due caution. For instance, he says the following about the purported rise in schizophrenia during nineteenth century:

The vast bulk of asylum admissions consisted of patients receiving diagnoses that in retrospect are not transparent, "epileptic insanity," "hysterical madness," and the like. To peel back contemporary diagnostics from the underlying reality of illness, we need something more than these global diagnostic terms of former years: We need to make our own assessments of individual patients. Retrospective diagnosis on the basis of patients' charts is required, making a reassessment from the signs and symptoms reported in the file. Yet this kind of scholarship is very time-consuming, requires a knowledge both of context and illness, and has only just begun. Therefore what I have to say... is somewhat tentative. Yet I think there is enough evidence to justify the conjecture that the incidence of schizophrenia rose significantly during the nineteenth century (1997, p. 60-61).

By contrast, David Healy tends toward the third approach, contextualizing source claims when possible and often encouraging the reader to take these on face. He regularly cautions against the tendency to assume that people in the past behaved as they did because of superstition, prejudice, or simple ignorance while people in our own time are guided by reason and science. Insulin coma therapy, for instance, is often considered one of the more shameful aspects of psychiatry's history, a defunct therapy that never really worked and killed a number of patients along the way. Historical reports of successful treatment with insulin coma therapy are conventionally understood to be examples of the placebo effect; insulin coma therapy, though therapeutically ineffective, is thought to have created some hope of treatment in asylum wards where there was none previously. Healy (2002) writes:

This is a very powerful explanation, but the traditional account is probably wrong. ICT is actually quite mysterious. Clearly, it must somehow have changed glucose levels, and often patients appeared to come out of the coma in response to glucose infusions. But amyl nitrate and amphetamines could also restore patients. Glucose wasn't necessary. Furthermore, it is now known that insulin, far from being simply a treatment for diabetes, is present in the brain, where a number of insulin-like nerve growth factors function as neurotransmitters... It should also be remembered that ICT was used for twenty years before the introduction of chlorpromazine. A therapy that did not produce some good surely would have faded away, given the intense amounts of labor involved and the risk of fatalities (53).

When I was unsure how to proceed with the genealogy here, I more often than not found myself following Healy's lead. This genealogy incorporates little primary

source material. It was constructed largely through the use of secondary sources; that is, through the use of other historians' research. Of these, Healy was the author I relied upon most. Both the structure and content of this analysis are deeply indebted to his historical narration in *The Antidepressant Era* and *The Creation of Psychopharmacology*.

### **From Archaeology to the Genealogical Method**

For Foucault as archeologist, the point of this exercise is not to distinguish reliable, modern accounts of medical phenomena from this historical account which, "lacking any perceptual base, speaks to us in the language of fantasy" (Foucault, 1963, p. x), but rather to challenge the very possibility of such a distinction:

[B]y what fundamental experience can we establish such an obvious difference below the level of our certainties, in that region from which they emerge? How can we be sure that an eighteenth-century doctor did not see what he saw, but that it needed several decades before the fantastic figures were dissipated to reveal, in the space they vacated, the shapes of things as they really are? (pp. x-xi).

Foucault is essentially asking after the conditions and the governing rules of the paradigm shift that inaugurated recognizably modern medical science. If the rules governing serious statements two hundred years ago could produce a medical description that appears completely absurd to the modern reader, what basis do we have for assuming that the rules governing the production of serious statements in our own time bear any necessary relationship to objective Truth, to the world as it actually is? At this point in his career, Foucault was primarily concerned with the systems of rules that govern discourse. The goal of the archaeologist is to bracket

serious meaning in order to move beyond interpretation and analyze the structure of all discursive systems with the same critical distance that we bring to Pomme's description of his patient.

In the late 1960s, the focus of Foucault's work began to shift from the social practices that were the subject of his first two books to the archaeological method itself. Following a long period of methodological introspection, Foucault (1973) would eventually find the archaeological approach lacking. He had struggled to insulate his own analysis of the human sciences from the rules of truth and meaning he described, but in so doing found himself without any ground left from which to speak. It also seemed to Foucault increasingly unlikely that such a thing as a purely objective description was possible. The task of bracketing serious meaning now appeared infinitely reductive; all descriptions involve choices and no analyst of the human sciences can ever claim to have an omniscient perspective on the object of study. "For the moment," he wrote, "and as far ahead as I can see, my discourse, far from determining the locus in which it speaks, is avoiding the ground on which it could find support" (1973, p. 205).

The early 1970s marked a significant turning point in the body of Foucault's work. He gave up trying to reposition himself outside the systems that determine truth and meaning in the human sciences, and he refocused his analysis on social systems and cultural practices rather than the rules of discourse. In his preoccupation with the formation rules of discursive structures, he felt that he had overlooked the potentially more important question about how these relate to a larger field of power relations:

What was missing from my work was the problem of 'discursive regime,' the effects of power proper on the enunciative play. I confused it too much with systematicity, the theoretical form, or something like a paradigm. Between *The History of Madness* and *The Order of Things*, there was under two different aspects, the problem of power which had not yet been well located (Foucault, 1980c, p.105).

The archaeological method alone was not up to this task. What Foucault needed was a new method for examining systems of power from within. He needed a new methodological tool that would allow him to trace the dispersion of power relations throughout the social field and identify the technologies through which truth and power are socially negotiated, but without conceding away the possibility of making serious claims himself. He found such a tool in genealogy. Foucault (1971) outlined his genealogical method in an essay titled *Nietzsche, Genealogy, History* and embraced it fully in the remainder of his major works.

### **Nietzsche: Helping Sell Antidepressants Since 1872**

Foucault's genealogical method was heavily influenced by Friedrich Nietzsche's *On the Genealogy of Morals*. This was Nietzsche's response to philosophical hypotheses that modern morality can be accounted for through the utility of altruism. According to the advocates of this view, morality originated in enlightened self-interest, in the utility of altruism that was forgotten when altruistic social norms began to ossify and compel behavior without reference to the originary value of the norm. Nietzsche finds this idea absurd. People do not forget the value of social practices, he argues. Quite the contrary, the value we ascribe to social practices is constantly affirmed and reinforced; when something has been deemed



valuable by society, we are continually reminded of that fact. Society would no more let us forget the use value of altruism than it would let us forget the value that has been ascribed to normative sexuality. *On the Genealogy of Morals* tells a very different story, outlining three central propositions that together comprise a counter-history of morality.

The first of Nietzsche's propositions concerns the binary good/evil, which he claims developed out of and in opposition to the binary good/bad. *Good*, in the naturalistic sense, refers to strength, fitness, and power, while *bad* refers to weakness and powerlessness. A good predator is one that runs fast and kills mercilessly. In Nietzsche's view, the dichotomy good/evil developed from a "slave morality" based in resentment and hatred of those who are strong and powerful. Those who were not good in the naturalistic sense had to invent a new meaning for the term. The model of good/evil thus reverses the concepts good/bad, making the bad *good* and the good *evil*. He argues that slave morality only serves the weak. The strong and fit have no need to demonize the weak. They vanquish the feeble and meek and then go about their lives. The predator has no need to feel contempt or to denigrate its prey. It is the weak who feel resentment toward the strong. It is the powerless, unable to triumph over the powerful, who need a concept of evil, a grand deception to make people think that weakness will eventually be rewarded and strength punished. Nietzsche finds this outrageously unfair. The strong cannot choose to be weak any more than the weak can choose to be strong. The predator cannot help what it is and it would be a mistake, he says, to hold the predator accountable for being the only thing it knows how to be.

Nietzsche's second main proposition is that the origins of punishment are neither virtuous nor noble, but rather lie in basic economic relations and instinctual human sadism. He argues that the idea of guilt (*schuld*) has nothing to do with a moral impulse. It derives from debt (*schulden*). When a debtor does not repay a creditor, the creditor can inflict punishment on the debtor. This is the whole idea behind arrangements of credit. As Nietzsche sees it, this is only an agreeable arrangement for creditors because it is naturally pleasurable to inflict punishment on others. Punishment does nothing to bring back the creditor's money, but the pleasure the creditor derives from inflicting punishment on the debtor makes up for the loss. Nietzsche believes that those who see punishment as having some high moral purpose suffer from historical amnesia. Humans have inflicted brutal, torturous punishment on each other for millennia, and the only common theme running through this long and bloody history is that people like to inflict pain on others. Whatever else punishment does, it does not make people remorseful or turn them into better citizens. Punishing people hardens them. Far from instilling a sense of caring or connectedness with others, punishment alienates people further. But if humans are naturally so sadistic, why do we feel bad when we wrong others? Conscience, he claims, is a form of self-hatred that occurs because civilization constrains our primal instincts, our will to power, and this forces our natural violence and sadism to turn inward. Lacking an outlet for their destructive tendencies, people begin to internalize the myth of slave morality and punish themselves psychologically for their natural inclinations.

Nietzsche's third proposition is that asceticism is a product of slave morality. Here he looks at a multiplicity of different meanings that the ascetic ideal holds for

different types of people, finding beneath each one emptiness, a void of meaning. The whole point of asceticism is to suppress desire, which leads to resentment. If the ascetic had no worldly desires, there would be no need to condemn those who act upon theirs. Like conscience, he says, the ascetic ideal turns our resentment inward toward the self. It tells us that it is wrong to desire what we desire and that the source of our unhappiness is not to be found in those things that prevent us from getting what we desire, but in ourselves, in the sins we commit by harboring desires in the first place. Through all of this, Nietzsche wants to show that the historical development of morality was shaped by the will to power, by cruelty and desire and hateful jealousy, not by benign intentions or altruism masquerading as enlightened self-interest.

### **Foucault's Genealogical Method and the History of the Present**

Following the model of Nietzsche's argument, Foucault's genealogical method attempts to open space for new ways of thinking through counter-history. He attempts to shed light on ideas and practices that people tend to view as being ahistorical, existing always as they are without a history, such as sexuality, punishment, or the regimentation of the body. He does this through historical analyses that demonstrate how these things came to be and how their histories could have developed otherwise. Originally Foucault intended for the genealogical method to supplement archaeology. The archaeological method allows the analyst to look at a particular idea or practice – a form of knowledge proper, a pattern of action, or maybe the spatial arrangement of a building – and identify the ways it functions within the discursive system, the ways it is made intelligible and accorded legitimacy. The genealogical method then enables the analyst to recast it

historically, to isolate the artificiality and non-essentiality of the object of analysis, those places where its history could have developed differently, and to situate its working amidst a larger field of power relations. In practice, the genealogical method soon supplanted the archaeology method as the basis for Foucault's work. He never abandoned the archaeological method per se. He just began to concern himself with different aspects of social practices.

This form of genealogical analysis is sometimes referred to as a "history of the present," a reference to Foucault's declaration in *Discipline and Punish* that:

I would like to write a history of the prison with all the political investments of the body it gathers together in its closed architecture. Why? Simply because I am interested in the past? No, if one means by that writing a history of the past in terms of the present. Yes, if one means writing a history of the present (p. 31).

This form of analysis begins with an incitement in the present and attempts to identify how the ritual of power in question developed into its current form. It is neither an attempt to give a complete accounting of what happened in the past, nor is it an attempt to step outside of one's own perspective to speak about what happened in the past with complete and total objectivity. A history of the present does not attempt to read the current situation back into the past. It is rather a political tool that seeks to expose alternatives by denaturalizing that which is understood to be natural and pre-existing, by showing how the current situation took shape and how it could have happened differently.

This is the form of genealogical analysis employed in the present study. The goal of this project is to explore how we came to this particular historical moment so

as to enable people to meaningfully respond to the political exigency represented by the formal re-envisioning of depression in *DSM-5*. This project is not an attempt to tell the whole story of psychiatry, or even the whole story of depression. Neither is this a polemic against psychiatry or an attempt to delegitimize the proposed changes to the upcoming version of the *Diagnostic and Statistical Manual*. This is not a criticism that seeks to close off dialogue or end erroneous thinking by saying how things actually are. The point of genealogy is to open space for new dialogue and new understandings, to locate the historical fissures and cracks that make it possible to think otherwise. It is, in the words of Biesecker (1992), to enable the critic to “‘make these virtualities visible’ by the strategic and deliberate codification of those points of resistance... to trace new lines of making sense by taking hold of the sign whose reference has been destabilized by and through those practices of resistance” (p. 361).

The genealogy here is broken up into four chapters. Chapter three will trace the history of modern understandings of disease, transitioning over time from a model of medical holism to one based on an atomized understanding of the person and the idea of specific diseases with specific cures. It also examines the conditions that made possible to development of biological psychiatry. Chapter four will focus on the politics of diagnosis, the history of psychiatric classification, and the various ways depression has been understood throughout the ages. Chapter five will describe the evolution of psychopharmacology, including the specific roles played by major psychiatric drugs, changing ideas about the nature of medicine, the development of organic chemistry, and the birth of the pharmaceutical industry. Finally, chapter six will examine the history of the *Diagnostic and Statistical Manual*

and the role it has played shaping the present state of psychiatry, as well as the relationship between contemporary biological psychiatry and psychopharmacology.

## Chapter 3

### MEDICAL HOLISM, DISEASE SPECIFICITY, AND BIOLOGICAL PSYCHIATRY

Our genealogy will begin by looking at different understandings of disease and illness across history and the ways these have evolved into the highly particulate model of distress that characterizes modern Western medicine. This chapter will also examine the development of biological psychiatry. This is the predominant approach to understanding psychiatry today, and as we will see in later chapters, the removal of the bereavement exemption in *DSM-5* owes much to the biological model of psychiatric illness. Consonant with Foucault's observations about biopower, this chapter begins to sketch out a story about the increasing particularization of the body and the expansion of the medical gaze over ever more specific and minute aspects of the bodily organism.

#### **The Humoral Model and Medical Holism**

For most of recorded history, Western medical thinking reflected the assumptions of the humoral model most commonly associated with the teaching of Hippocrates of Kos. Born in the fifth century B.C.E., Hippocrates is often referred to as the father of Western medicine in recognition of his efforts to establish medicine as a discipline separate from philosophy or theurgy. Medical practitioners around the world still take a modified version of the ethical oath taught by Hippocrates and his students. In the Hippocratic view, the human body is understood to be comprised of four humors, or fluids, that exist in a natural state of equilibrium: blood, phlegm, yellow bile, and black bile. Health in the humoral model is understood as the natural result of a balance or harmony between the humors, illness the result of imbalance or disharmony (Yapijakis, 2009).

Galen of Pergamon extended the humoral theory of Hippocrates and molded it into a logical system that eventually became better known than the Hippocratic corpus itself. Galen lived during the second century, following the decline of ancient Greece and the rise of the Roman Empire. According to Healy (1997), Galen took the aphorisms and commentaries of the Hippocratic system and transformed them into a systematic medical model wherein the physician would use knowledge about the relationships between the humors to deductively arrive an understanding of the patient's condition. Galen's writings were some of the most well known ancient texts and this medical system persisted long after his death. In time, the Galenic understanding of health spread to Arab physicians. By the end of the Middle Ages, it had spread to Europe, where Galen's teachings were some of the first texts published after the invention of the printing press. These ideas would continue to influence the way people understood health well into the eighteenth and nineteenth centuries, over a millennium and a half after his death. As Healy (1997) notes, this system

was all but irrefutable in that superficially discrepant observations could be reconciled in an overall synthesis of humoral, personality, and environmental factors... [W]hereas Renaissance discoveries in physics led to the overthrow of Aristotelian physics and the Ptolemaic cosmology, the emerging anatomical knowledge of the seventeenth century on the circulation of the blood, for instance, or demonstrations of the existence of nerves were incorporated into updated versions of Galen's models... all such systems... remained true to Galen in that they attempted to relate local problems to disorders of the body as a whole (pp. 8-9).



Other well-known humoral systems include the Ayurvedic system traditionally practiced on the Indian subcontinent and the Chinese Yin and Yang system. The basic principles of humoral thinking can also be seen in the modern Western alternative health movement and the health food industry (Healy, 1997).

Health in the humoral view is understood as a characteristic of the whole bodily organism. In this way of thinking, the health of one organ or body part is related to the health of the whole body such that a lesion on the liver or kidney, for instance, would be understood not just as a defect in that particular organ but a defect in the overall harmony of the body. This was the predominant medical view until the latter part of the nineteenth century, when medicine began to shift to a focus on localized, semi-mechanical problems rather than the treatment of the whole person. Healy (1997) identifies three primary factors in this transition: the idea of localized diseases that act on particular parts of the body; the idea of therapeutic specificity, that different diseases might require different types of treatment; and an interest in evaluating treatment outcomes.

### **Specific Diseases and Specific Cures**

The idea of disease specificity had been around since at least the middle of the eighteenth century, but it was not until the emergence of bacteriology in the late nineteenth century that it would become a viable medical paradigm. Prior to this, there were no scientific techniques for detecting localized pathologies in living beings, nor were there medical procedures to combat localized diseases. The statistical methods that now enable us to study the progression of diseases in populations had not yet been invented. Thus evidence that would eventually be understood as disconfirming the holistic view of disease – such as autopsy findings

suggesting that specific diseases were associated with lesions or trauma in a specific organ, or even a specific tissue type in an organ – were incorporated roughly into the dominant paradigm or simply overlooked. Neither Joseph Lister’s demonstration that antiseptic reduces infections nor Louis Pasteur work suggesting that micro-organisms are involved in fermentation would initially be accepted as evidence of the germ theory of disease (Healy, 1997).

It was not until Robert Koch isolated anthrax, tuberculosis, and cholera in the years 1877 to 1884 that the germ theory of disease began to gain widespread acceptance. It is common for new ways of thinking about a medical problem to be rejected until solutions can be discovered within the new model, and we will see several more variations on this theme throughout the course of this analysis. Koch and other bacteriologists helped secure acceptance for the germ theory of disease because they were able to demonstrate not only the existence of a pathogen, but also how it could be cultured and used to develop an antitoxin. One of the key events that won support for the bacteriological model was the isolation of diphtheria and the creation of a diphtheria antitoxin in the 1890s. Although the germ theory of disease is a medical milestone in its own right, it was underwritten by two broader but rarely discussed conceptual shifts in thinking about disease. First was the idea of disease specificity, which challenged the long-standing holistic model of disease. Second was the idea of therapeutic specificity. Where previously treatments had been targeted at the relief of symptoms, it was now widely understood that different patients presenting with similar symptoms (nausea and a fever, for instance) might actually be suffering from distinct diseases with different cures (Healy, 1997).

### **Concern for Therapeutic Outcomes**

The third development that helped move medical thinking from a focus on the whole patient to a focus on specific semi-mechanical defects was an increased concern with therapeutic outcomes. In the Greek worldview, the logical syllogism was the foundation of scientific knowledge. Early medical practitioners worked through deductive analysis, comparing the patient's symptoms with extant theories of medicine to arrive at a course of action. Healy (1997) explains:

Medical practice involved efforts to reconcile the logic of the system with the observed state of the patient, and so people who looked flush were bled. But given a choice between the certain knowledge provided by the system and the uncertainty of striking out alone and going by the signs visible in the patient, physicians chose to adhere to the system, even though bleeding, for instance, regularly led to circulatory collapse and death. To choose to do otherwise was to be an empiric – which was a term of opprobrium (p. 80).

This way of thinking underpins the split between physicians and surgeons that began in the Late Middle Ages; surgeons studied new scientific developments with an eye toward producing better outcomes, while physicians studied ancient texts and valued logical reasoning above all else. There had thus long been advocates for medical empiricism, but it was not until the development of statistics that this view gained broad acceptance. As discussed in chapter one, the seventeenth and eighteenth centuries witnessed a dramatic conceptual shift in thinking about populations, attended by a new focus on the accumulation of population data. Problems of governance, including most notably problems related to issuing insurance and annuities, necessitated new forms of administrative record keeping.

This led to the development of statistical techniques for making practical, probability-based judgments about populations, and these in turn begat new ways of thinking about medical outcomes. With these new statistical measures, the predictability of outcomes became untethered from the process of logical deduction. The old medical empiricism was essentially a form of anecdotal decision making, with all the uncertainty and unpredictability that implies. Statistics made possible, for the very first time, reasonably reliable predictions based on observations. By conceptually detaching the predictability of judgments from the deductive process, the new statistical techniques helped to secure the first broad support for outcome- rather than logic-based science (Healy, 1997).

### **The Development of Biological Psychiatry**

As medical science was transitioning from a discipline focused on the treatment of the whole individual to one focused on the correction of localized, semi-mechanical defects, psychiatry was becoming increasingly biologically oriented. “What made the first biological psychiatry distinctive from previous humoral theories was not the belief that psychiatric illness possessed an underlying neural structure,” Shorter (1997) explains, “... but the desire to lay bare the relationship between mind and brain through systematic research” (p. 70). While Lister and Pasteur were working for medical recognition of the germ theory of disease, Wilhelm Griesinger established what would become the model for the modern psychiatric department at Charité hospital in Berlin, a teaching hospital where residents practiced psychiatry as a cross-covering medical discipline. According to Shorter (1997):

In 1867, Griesinger founded the flagship journal of the new neurologically oriented psychiatry, the *Archive for Psychiatry and Nervous Diseases...* The preface to the first issue of Griesinger's *Archive* contained one of those programmatic statements that in terms of historical resonance was comparable to Lenin's *What Is to Be Done?* "Psychiatry has undergone a transformation in its relationship to the rest of medicine," Griesinger said. "This transformation rests principally on the realization that patients with so-called 'mental illness' are really individuals with illnesses of the nerves and brain." ... Psychiatry must therefore "emerge from its closed-off status as a guild and become an integral part of general medicine accessible to all medical circles." These are among the most portentous words ever uttered in the history of psychiatry (p. 76).

Psychiatry had long been transitioning out of its old custodial role and into the role of a healing discipline, but the nature of the asylum space complicated these efforts. What Griesinger had accomplished at the Charité was the beginning of psychiatry's move out of the asylums and into universities and teaching hospitals, the first of two critical developments in the evolution of biologically oriented psychiatry.

The second, related story in the development of biological psychiatry concerns advances in neurology and the uptake of neuroanatomical research by psychiatrists in the latter part of the nineteenth century. Neurosyphilis, once called the "disease of the century," had been identified as a neurological disease often manifesting in manic or demented behaviors. Paul Fleishig mapped out the functions of the different regions of the cerebral cortex and Eduard Hitzig established that electrical stimulation produces responses in the brain. Both men

opened prominent university clinics in the model of Griesinger's during the 1880s. Shortly after the turn of the century, Aloys Alzheimer discovered the disease that bears his name.

Ironically, it was this overlap between the domains of neurological and psychiatric research that would eventually contribute to the eclipse of biological psychiatry by psychoanalysis. As organic neurological diseases were discovered and localized in the body, they became neurological problems, the proper domain of the neurologists, and all the disembodied symptoms that could not be accounted for in the anatomy of the brain were left to psychiatry (Shorter, 1997). These tensions still exist within modern psychiatry. The only condition that is being removed outright from *DSM-5* is Rett syndrome, a disorder of the nervous system caused by mutations in the MeCP2 gene on the X chromosome. Rett Syndrome is currently classified as part of the autism spectrum because it often manifests in autistic symptoms, but “[l]ike other disorders in the *DSM*, Autism Spectrum Disorder (ASD) is defined by specific sets of behaviors and not by etiology (at present) so inclusion of a specific etiologic entity, such as Rett's Disorder is inappropriate.”

By the beginning of the twentieth century, the idea of disease specificity had laid the theoretical foundations for the practice of medicine as we know it today, and psychiatry was well on its way to establishing a place for itself under the broader canopy of the medical sciences. Health, treated for over two thousand years as a facet of the holistic bodily organism, had become a function of atomized and specialized parts working and breaking independently, as cogwheels in a clock. It was increasingly understood that organic diseases could produce changes in thought and behavior, and that afflictions of the mind were sometimes marked by

aberrations in the brain. Psychiatry's long transition from a strictly custodial role into a curative one was nearing completion.

## Chapter 4

### DIAGNOSING DEPRESSION

Diagnosis is such an integral component of the modern medical model that it can be difficult to imagine a world in which diagnosis was thought unnecessary. In actuality, this was the case for much of the history of medicine. Chapter four will trace the progression of this change, and alongside it the evolution of the depression concept. This chapter will also begin to unpack some of the ways that anonymous technologies of power are woven through the history of psychiatry. We will see how abstract ways of thinking about medicine or psychiatry can produce powerful institutional changes and the profound cultural resonance of seemingly innocuous scientific ideologies. In addition, this chapter will begin to develop the theoretical importance of Kuhn's model on the project here, with several important examples of Kuhn's observation that normal sciences undergo paradigm shifts not because the new paradigm necessarily provides a better or more complete explanation, but because it better fits the needs of a particular community.

#### **The Politics of Medical Diagnosis**

Far from being thought of as a medical necessity, diagnosis was understood for most of history as a strictly academic pursuit opposed to actual treatment. Woven through this history is a radical shift in the way people conceive of medicine and the role of the doctor. Faced with ill health, people often seek to understand *why*. *Why is this happening to me?* For most of history, this was the question medicine sought to help answer. In modern medicine the question *why* is usually referred to others, to teachers and epidemiologists, philosophers and religious authorities. The modern physician seeks to understand the *what* and *how* of illness,



not the *why*. *What is wrong with the patient? How will this illness progress? What can be done to treat it?* (Healy, 1997).

As recently as the 1970s, psychiatrists were concerned principally with understanding the *why* of depression. Psychotherapeutic approaches sought to identify the traumatic events or subconscious conflicts that made people depressed, while social psychiatric approaches looked for explanations in the social environment, in factors like structural racial inequalities that by their nature breed unhappiness and discontentment. Today that focus has been lost. The removal of the bereavement exemption in *DSM-5* is clear evidence of this. It is not the relevance of bereavement to the onset of depression that is being questioned, but the relevance of cause altogether. The *why* of depression no longer matters.

In ancient times there was not much to be gained or lost from diagnosis. Before specific diseases and specific treatments, diagnosis played little meaningful role in shaping treatment. But neither was it particularly dangerous. Modern diagnosis has more benefit for the patient, but greater risks as well. Healy (1997) explains:

In the humoral framework, the very word *disease* implies illness – the presence of symptoms. In contrast, modern diseases such as hypertension or cancer are asymptomatic for the greater part of their courses... Being told that one is HIV positive or that one's blood pressure is elevated may extend the experience of illness. Once diagnosed, some people become symptomatic even if they were not symptomatic at the time of diagnosis (p. 10).

There is no such thing as asymptomatic depression, but the symptoms of depression involve subjective emotional states that can be affected by diagnosis. In

fact, direct-to-consumer antidepressant advertisements aim to do just that. The target market for antidepressants is comprised of people who are symptomatic but do not view themselves as needing treatment, not people unaware of antidepressants. For these advertisements to work, they *have* to change the way people think of themselves, to diminish the faith people have in their abilities to cope without reliance on medicine. Diagnosing depression presents a central ethical dilemma that would have been thought conceptually incoherent just a few hundred years ago.

### **Mania and Melancholia Throughout the Ages**

What we now call depression was known by two different names before the eighteenth century. The first of these was *melancholia*. In the ancient Greek model, *melancholia* referred to a state of madness involving fear, sadness, restlessness, and so on (Blazer, 2005). Madness here was understood as a totalizing affliction. Before the atomized view of human health, insanity was something that affected the whole person. The mind was understood as a spiritual entity guided by the soul. To be mad was to be deprived of one's faculties, and this was not something that happened by gradation. Madness was divided into two different states. Insanity involving overactive states was dubbed mania, while insanity involving underactive states was called melancholia. The term *manic* now has a different clinical meaning, but these associations with overactive insanity still persist in the meaning of words like *maniac* and *maniacal*. Melancholia included what is now understood to be major depression, as well as the conditions we know as social phobia, obsessive-compulsive disorder, negative schizophrenia, and so forth (Healy, 1997).

This basic distinction between mania and melancholia, overactive and underactive forms of madness, remained in place from the time of the Greeks until the eighteenth century, but understandings of melancholia and its causes varied greatly across the centuries. In the Hippocratic model melancholia, or biliousness, was a disorder associated with the humor of black bile, as well as cold, dryness, and the spleen. Galen used this understanding of melancholy as the basis for the melancholic type, one of the four temperaments, or personality archetypes. In medieval Europe, melancholia was understood primarily through the lens of religion, as a reaction to a trial or punishment from God. What is particularly noteworthy about this view is the idea of melancholia as a reaction to something external rather than an intrinsic feature of the melancholic person. This way of conceptualizing madness was at odds with dominant understandings both before and after the medieval period, but it would form the basis for nineteenth and twentieth century theories of depression. As melancholia began to break away from religion during the Renaissance, it was increasingly associated with the passions. Following the Renaissance, melancholia came to be understood as a mainly physiological phenomenon. Descartes, for instance, proposed that melancholia was caused when the animal spirits of the nervous system moved and agitated the brain (Blazer, 2005).

Dominant explanations of melancholia in the seventeenth and eighteenth centuries fell into two distinct schools: humoral and chemical. The humorists echoed Hippocrates and Galen, locating melancholia in an imbalance of the humors, while advocates of the chemical view proposed alchemical imbalances as the cause of melancholia and chemical concoctions as its remedy. In its general form, this

model of melancholia is largely indistinguishable from the biological view of depression so predominant today. In 1621, humorist Robert Burton published an influential tome titled *The Anatomy of Melancholy*. Burton subscribed to the standard humoral view that melancholia was caused by an excess of black bile, but he also “developed a detailed description of a multicausal web that would do a [twenty-first] century conceptual model proud” (Blazer, 2005, p. 42). Burton suggested that melancholia could be triggered by life events such as bereavement or enslavement, as well as a dysfunction of the spleen, blows to the head, excessive sleep, poor diet, shame, pride, envy, and even love of learning. He also left room in his schema for religious explanations of melancholia. *The Anatomy of Melancholy* generated great interest among contemporary readers, and it is still recognized as a work of great literary value (Blazer, 2005). Burton lived at the end of an era in medical thought. Although medical thinking about the causes of melancholia varied greatly from one age to the next, its meaning as a nosological concept had remained stable since the time of the ancient Greeks. This began to change in the eighteenth century.

The need for a new system of classification arose from challenges to the holistic view of mental disorder. The traditional categorical binary of mania and melancholia was grounded in a totalizing view of madness that recognized no degrees or levels of insanity. Madness was an affliction of the mind and the soul, neither of which had parts or gradations. Madness could take different forms, but all had the same totalizing impact; the mad were by definition wholly mad, no longer in control of their own minds. Blazer (2005) notes that the first shift in the meaning of the word *melancholia* occurred when “the term... seemed gradually to have become

restricted to a disease, whereas *melancholy* remained a synonym for *melancholia* and a popular term (with breadth and diffuseness of use much as *depression* is used today)" (p. 42). This subtle linguistic distinction between the affliction of melancholia and the experience of melancholy is evidence of a profound change in understandings of mental disorder. One could conceive of experiencing symptoms of melancholy without being melancholic.

The shift from a holistic view of mental disorder to an atomized one was made possible by several critical developments in the eighteenth and early nineteenth centuries. The first of these was the development of faculty psychology. Thomas Reid first proposed the concept of mental faculties simply as a philosophical heuristic. Even if the mind was a unified whole, it made practical sense to imagine it as an aggregate of different faculties that could be isolated and talked about separately. Of course, once people began to think of the mind as having different faculties, it became possible to think about the impairment of some faculties but not others. Soon thereafter, advances in neurology suggested that the brain does, in fact, have distinct parts responsible for controlling different functions. The discovery of the reflex arc demonstrated that some parts of the human body could act reflexively without input from the soul, and subsequent work in neurology revealed reflexivity at increasingly higher levels of the nervous system. The more that neurologists learned about the working of the brain, the more apparent it became that there were different parts to the brain and that some of these functioned without conscious direction from the soul. The basic assumptions that had informed the holistic view of disorder were fast becoming scientifically untenable (Healy, 1997).

The development that finally led physicians to abandon the old categories of melancholia and mania was the advent of the therapeutic asylum. There had long been institutions where the raving mad could be confined, but these were small facilities dedicated to isolating a handful of dangerous lunatics from society. Following the moral movement of the eighteenth and nineteenth centuries, new, larger asylums were built with the ostensible goal of treating insanity rather than just warehousing the insane. The circumstances surrounding the construction and operation of the new asylums is a point of ideological contestation within the literature, but whatever one's views on the asylums, the new hospitals brought together large numbers of the insane in one place for the first time. Faced with hundreds of these people, it became clear that there was more than one type of insanity with two forms. The idea of a single fundamental madness would quickly prove inadequate for the new asylum psychiatrists. If the psychiatrists were to make sense of their patients' afflictions, they would need a better system for categorizing mental disorders. All of this was in keeping with the scientific trends of the day, an echo of the classificatory efforts of contemporary naturalists and somatic physicians (Healy, 1997).

### **Nervous Illness**

There have always been behavioral disorders that did not fit the simple binary of mania and melancholia, but these were not considered forms of madness. These conditions, which would mostly be considered nervous illnesses today, were thought of as bodily diseases, and they fell under the purview of organic medicine or other types of healing. Shorter (1997) writes:

[E]very era has had its vocabulary of psychoneurosis. The ancient Jews thought “love sickness” to be capable of reducing a man to a skeleton. Later epochs would contribute such additions to nosology as “hysteria” in the sixteenth century and “nerves” in the eighteenth. All these expressions were quite nonspecific and could envelop just about any symptom imaginable... Eighteenth century alienists covered only a narrow segment of the broad spectrum of disorders that the term psychiatry embraces today. The rest of the spectrum was taken over by practitioners who shunned any public identification with madness and sailed under the flag of spa doctor or society nerve doctor (pp. 22-26).

In historical retrospect, it is clear that the classificatory boundaries between madness and nervous conditions were porous and indistinct. Whether someone was understood to be suffering from melancholia or hysteria might have had more to do with her social status or the wishes of her family than with anything resembling objective diagnostic standards, and the labels were often used for prescriptive as much as descriptive effect. Thus Shorter (1997) describes an eighteenth century physician who attributed the problems of his female patients to nervous vapors that arise in the uterus and “derange all functions of the brain.” Such vapors, Shorter notes, “sound like fairly significant disorders, deranging the functions of the brain and whatnot. But were these women ‘insane,’ in the sense of being candidates for admission to an asylum? Not at all. Their families would keep them at home” (p. 26). Regardless of her symptoms or demonstrable mental state, the patient’s sanity or lack thereof was determined by the name given to her experiences; the psychiatric

label determined the nature and severity of the disorder rather than the other way around.

These arrangements benefited both doctors and patients. Specializing in “nerves” or “nervous illness” gave early psychiatrists a way out of the asylum. “Psychiatrists... underwent a big leap up in their social stature and income as they became urban nerve-specialists, electrotherapists, neuropsychiatrists, and the like.” For patients, meanwhile, “this camouflage presented an opportunity to escape the opprobrium of madness... patients experienced the vast relief of knowing that their problems were really ‘organic’ rather than constituting evidence of hereditarian family degeneration or being ‘all in their heads’” (Shorter, 1997, pp. 113-114).

By the end of the nineteenth century, the future of these nervous conditions was in jeopardy. The major classes of non-psychiatric behavioral disorders had been around for centuries. Hysteria was the name for the psychosomatic complaints or emotional instability of women, hypochondria was the equivalent diagnosis for men, and everything else was nerves. When the nervous system was not very well understood, these had been deemed organic conditions with a psychiatric presentation rather than actual madness. The problem was that nineteenth century advances in neuroanatomy had made it possible to identify biological conditions in living people. By the latter part of the century, physicians could perform a test to distinguish multiple sclerosis from hysteria, or to determine if a patient had neurosyphilis rather than nerves. With each organic neurological disease that was identified and distinguished from the mass of “nervous complaints,” it became increasingly difficult to maintain that what remained as hysteria or hypochondria



were genuine organic diseases. This created an acute crisis for the nerve specialists.

Shorter (1997) writes:

The climate of medical uncertainty about what was really organic and what was just hypothetically organic was a mirror image of patients' uncertainty about what was madness and what was nerves. Both doctors and patients required a bridge between these two uncertainties, an organic-*sounding* disease term to explain psychiatric-*looking* illness behavior. In 1869, New York electrotherapist George Beard supplied this bridge with his announcement of the discovery of neurasthenia (pp. 129-130).

Neurasthenia, Beard claimed, was an exhaustion of the nerves, something that happens when "the central nervous system becomes dephosphorized, or perhaps, loses some of its solid constituents" (Shorter, 1997, p. 130). Unlike general paresis of the insane or neurosyphilis, neurasthenia was an organic condition of the nervous system that could not be seen under a microscope. It was a functional nervous condition, based in biology but lacking in physical biological markers. Neurasthenia thus could only be identified by its outward symptoms.

The significance of neurasthenia to this story is twofold. Neurasthenia was, first of all, one of the names given to those who display the complex of symptoms that would today be diagnosed with depression. Histories of depression often trace the evolution of modern depression from the concept of melancholia, one of the two classic forms of madness. It is likely, however, that the overwhelming majority of people now considered depressive would never have been labeled as melancholic. To feel unfulfilled in one's station or unsatisfied with one's marriage was simply a fact of life for most people for most of history, not a sign of madness. To be

melancholic was to feel deeply and profoundly despondent, sometimes to the point of catatonia or suicide. Melancholy was a form of insanity. There is no doubt that some people who are diagnosed with depression would have been considered melancholic, but they represent a small portion of the most severely depressed. The average modern depressive, suffering from occasional difficulty sleeping, fatigue, or loss of appetite, would more likely have been diagnosed with neurasthenia a hundred years ago, and before that nerves, hysteria, or one of the other generic nervous conditions. That is, of course, if the average modern depressive would have been diagnosed with any condition at all. The boundaries of modern depression have widened appreciably in the past forty years, and some of those who are now considered depressed would probably not have been viewed as abnormal or needing treatment at all.

The second reason for neurasthenia's importance is the role it played in the development of psychoanalysis. As neurasthenia became the quintessential nervous condition, there arose an increasing need for an effective cure. Silas Weir Mitchell happened upon such a cure in 1875 while treating a neurasthenic patient. Thus was born the Weir Mitchell rest cure, involving bed rest, massage, a warm milk diet, and electrical stimulation. This was a costly and time-intensive therapy usually carried out at private retreats where physicians would send neurasthenic patients for six to twelve week stays. "Because the rest cure was so ideal for a private sanatorium, a symbiotic relationship soon developed among the spa clinics, the urban nerve doctors, and the nervous middle-class females of the Atlantic community" (Shorter, 1997, p. 132). As the Weir Mitchell rest cure spread in popularity, it became apparent to physicians that the main reason for the rest cure's effectiveness was

psychological, not biological. This was not, however, understood as a simple psychosomatic cure in the modern sense. Rather, the prevailing medical view was that the rest cure operated through submission to the doctor's authority. The will of the neurasthenic patient had to be worn down through an extensive personal treatment regimen until she could be made to surrender to the medical authority of the physician. The specific elements of the Weir Mitchell rest cure came to be seen as secondary. Electrical stimulation could be used or not, and the patient would still recover. What was important was the patient's acceptance of the doctor's authority. This recognition would underwrite the rise of psychoanalysis. In addition, the rest cure laid the institutional groundwork for psychiatry's transition from biology to psychology by moving psychiatrists out of the asylums and into office practices, where they concerned themselves increasingly with the problems of the urban middle-class rather than the indigent who populated the asylums (Shorter, 1997).

It is one of the great historical ironies of psychiatry that psychoanalysis and neurology would end up at opposite ends of the psychiatric spectrum with modern biological psychiatry decidedly closer to neurology. Before the neurological system was well understood, minor behavioral conditions were thought to be located in the body because genuine insanity was an affliction of the mind and soul.

Psychoanalysis was born out of neurology, though there was little meaningful difference between a neurologist and a psychiatrist at the time; before professional certification physicians were free to call themselves whatever they wished.

Practically, psychiatrists or alienists, so-called because they dealt with mental alienation, were based in asylums. Neurologists, in contrast, were those who trained in general pathology or internal medicine. Freud, Janet, and Charcot, the preeminent

theorists of hysteria, were all neurologists. Modern medical science now recognizes the most debilitating forms of mental disorder – dementia, schizophrenia, and so forth – as organic diseases of the nervous system, while hysteria and hypochondria are now thought of as essentially fictitious illness that exist only in the mind, but such terms as *nervous illness*, *neurosis*, *neuroticism*, and *nervous breakdown* are a lasting relic of this confused history (Shorter, 1997).

### **Psychiatric Classification in the Nineteenth Century**

Most prominent medical thinkers of the early asylum period had something to say about the classification of mental disorders, but the first practical system for classifying asylum patients came from Jean-Étienne-Dominique Esquirol. Esquirol outlined his views on the matter in a series of pamphlets that were collected into a book in 1838. He advocated a division between insanity, as it had been understood in the past, and partial insanity. Once it became possible to think of the mind as having different faculties, people began to realize that some of the insane could carry on normal conversations and even reason logically in support of a position. Esquirol dubbed these people partially insane, deluded or dysfunctional in certain ways but not in others. In Esquirol's model, one form that partial insanity could take was monomania, madness in only one way, a delusion or dysfunction of a single faculty. Monomanias associated with the faculty of will, for example, included kleptomania, pyromania, and nymphomania. Most important for our purposes is lypemania, a disorder associated with the faculty of mood. Esquirol's lypemania is a direct precursor of modern depression, the first mood disorder marked by sadness that is not by necessity a dysfunction of the whole person, or even the whole mind (Healy, 1997).

The primary difficulty with Esquirol's classification scheme was the sheer number of monomanias that could be included. There was potentially a new monomania for each distinct symptom. This approach was also plagued by longitudinal evidence suggesting that some diseases might cause different symptoms at different stages of progression. For example, it had been established that general paresis of the insane (tertiary syphilis) caused grandiosity at one point and delirium later. How were physicians to treat one physical condition that caused different monomanias at different points in its progression? And could a disease with multiple clinical presentations really be considered a *monomania* at all? The concept of monomania has been discarded from modern psychiatry, but the *-mania* suffix that persists in the names of many disorders is a holdover from Esquirol's original system of classification (Healy, 1997).

One of the disorders that challenged the monomania concept was manic-depression, described separately around the middle of the century by Jean-Pierre Falret and Jules Baillarger. What Falret called *folie circulaire* and Baillarger called *folie à double forme* appeared to be a cycling of two different monomanias. The link between mania and melancholia had been noted as far back as the ancient Greeks, but this was always understood as fluidity between the overactive and underactive states of madness. What Falret and Baillarger described was a disorder of mood that cycled predictably between two extremes. Around the same time, Benedict Morel made a similarly groundbreaking discovery in the identification of a disorder that caused young patients to mentally deteriorate into dementia. Morel first described this condition in 1853 and gave it the name *démence précoce* in 1860. Today Morel's *démence précoce* is known as schizophrenia (Healy, 1997).

## **Emil Kraepelin and Classification by Prognosis**

Emil Kraepelin came to psychiatry as the biological psychiatry of the nineteenth century was coming to an end. "It was not necessarily discredited by research findings," says Shorter (1997). "That's not the way paradigms change within medicine. People simply lost interest in brain anatomy once a new way of looking at psychiatric illness appeared on the horizon" (p. 99). The new way of looking at psychiatric illness treated it vertically rather than cross-sectionally. Instead of looking at symptoms in isolation and trying to draw connections with extant knowledge about neurology, it looked at symptoms in the context of the patient's life history. Kraepelin was critical in this transition (Shorter, 1997).

Kraepelin's career began in the early 1880s working with the experimental psychologist Wilhelm Wundt, one of the founders of modern psychology. Like his contemporary, Sigmund Freud, Kraepelin needed money to marry so he published "a slim *Compendium* of no particular merit" (Shorter, 1997, p. 102) in 1883 and took a post as an asylum physician a year later. After a brief stint at an Estonian university, Kraepelin moved to a professorship in psychiatry at the university clinic in Heidelberg in the year 1890. Here he began keeping note cards on each of the patients to come through the hospital. Kraepelin developed an interest in the progression of illnesses, something that was of little interest to the mainstream academic psychiatry of the day, and his note cards allowed him to track patients' histories all the way up to discharge. Following the model established by Griesinger at the Charité, Kraepelin wanted to surround himself with the best researchers. To accomplish this he began recruiting friends to join him at the university, a list that

“read[s] like an honor roll of turn-of-the-century German neuroscience,” including both Franz Nissl and Aloys Alzheimer (Shorter, 1997, p. 102-103).

Kraepelin was keenly interested in using drugs to uncover the workings of the mind. He coined the term *psychopharmacology*, although this fact seems largely lost to history and most accounts now date the term to the 1950s. As an assistant researcher, Wundt had worked with Hermann von Helmholtz, who established that the nervous system works by way of electrical signals. Helmholtz even established the precise speed of nervous transmission. In his later work with Wundt, Kraepelin proposed to use drugs to alter the speed of neurotransmission. They tested a variety of substances, including morphine, cocaine, alcohol, bromine, trional, coffee, and tea. Writing in 1892, Kraepelin summed up their work: “Herein lies, it seems to me, a not insignificant benefit of this psychopharmacology, in that it may lead us at times to recognize the true nature of certain psychological processes from the special effects of an already accurately known drug” (Healy, 1993). This interest is especially noteworthy because of the central role Kraepelin’s work has played in defining modern biological psychiatry, though it was largely ignored in his own day. This was half a century before scientists understood that some nervous transmission involves chemicals rather than electricity, and the predominant view was that only poisons could affect mental processes. It was thus believed that there was little or nothing to be learned about normal mental functioning through experimentation with drugs (Healy, 1997).

Kraepelin’s central classificatory innovation was to design a nosological system around the progression that diseases took rather than their origins or outward symptoms. Karl Kahlbaum had proposed to classify diseases based on their

outcomes several decades before Kraepelin, but it was Kraepelin, aided by his extensive note cards, who actually constructed such a system. Kraepelin shared the biological psychiatrists' belief that mental disorders are the product of underlying organic diseases, but he was unimpressed with their methods. Half a century of looking at brain cells under microscopes had produced few pathological descriptions of disease and little useful diagnostic data. Kraepelin strove instead to discern the nature of these diseases inductively, using the longitudinal data from his cards to identify patterns and discontinuities. Comparing note cards allowed him to determine the progression of diseases, or to figure out when a particular set of symptoms might be associated with multiple diseases. By checking the initial diagnoses on his cards against subsequent patient histories, he could look back and see which diagnoses had been incorrect and why. Rather than publishing his work in academic journals, Kraepelin released these findings in updated versions of his 1883 textbook. Each new edition generated great excitement among the psychiatric community (Shorter, 1997).

Kraepelin's system first began to come together in the 1893 edition of his book. This classificatory system was rather conventional, mostly drawing on established concepts. It also included *dementia praecox*, or premature dementia. This was the *démence précoce* Morel had identified thirty years earlier. Kraepelin's extensive description of the condition as a distinct disease and its inclusion in his system of classification is considered an important milestone in the history of psychiatry (Shorter, 1997).

The 1896 edition of Kraepelin's text made a drastic change, abandoning clinical presentation as a basis for classifying diseases to focus exclusively on



prognosis. This was a monumental decision. Kraepelin declared himself agnostic about cause, arguing that diagnostic criteria should be etiologically neutral unless the cause of a disease was known for certain, as it was with neurosyphilis, for example. He maintained that this was not only good science, but also the best way to help patients and their families. The families of psychiatric patients do not care what other diseases are symptomatically similar; they want to know whether their loved ones are going to get better (Shorter, 1997).

Kraepelin proposed the final form of his prognosis-based classification system in the 1899 update to his textbook. This is the classificatory scheme that serves as the model for the modern *Diagnostic and Statistical Manual*. Kraepelin divided diseases into thirteen categories based on prognosis. Most of these were familiar, such as mental retardation. The great innovation of the Kraepelin's 1899 model was the decision to collapse all the psychoses without an established biological cause into two different categories. Those illnesses that had an affective component became *manic-depressive psychosis*. This was a greatly enlarged form of the *folie circulaire* of Falret or the *folie à double forme* of Baillarger. Kraepelin had determined that patients presenting with symptoms of a mood disorder – despondency, nervousness, compulsiveness, difficulty eating, hyperactivity, lethargy, and so on – all basically had the same prognosis. Their diseases would likely take a cyclical course and eventually improve. Those patients whose psychotic symptoms did not have an affective component were deemed to have *dementia praecox*, or schizophrenia. They were not likely to get better. The decision to reduce all major psychotic symptoms into two categories received a predictably mixed reception from Kraepelin's contemporaries. Over time, however, this became the

predominant way of thinking about psychiatric disorders around the world. To this day manic-depressive illness and schizophrenia occupy the top of the psychiatric hierarchy, the cornerstones of modern psychiatric practice. Likewise, antidepressants and antipsychotics are the two primary classes of psychiatric pharmaceuticals. The clearest evidence of Kraepelin's lasting influence on psychiatry though can be seen in the pages of the modern *Diagnostic and Statistical Manual*, which we will examine in more detail in chapter six (Healy, 1997; Shorter, 1997).

### **Depression as a Reaction and Individualized Diagnoses**

Adolf Meyer is generally recognized as the central figure in American psychiatry between the turn of the twentieth century and World War II. Meyer argued that the term *melancholia* should be discarded because it implied an understanding of an underlying disease entity that had yet to be established, suggesting the term *depression* instead, as it describes the outward symptoms of the disorder, a depressing of one's energy, without implying a cause (Healy, 1997). Meyer also challenged the idea that psychiatric disorders are caused by discrete disease entities. He believed that depression was fundamentally a reaction pattern, a response to something in the patient's life. In Meyer's view, a person's life experiences and social environment were critical components of depressive reactions, and he emphasized that maladaptive reaction patterns are much easier to fix than a purportedly biological disease with unknown pathology (Blazer, 2005).

This type of psychological reaction is known in psychiatry as a neurosis. As mentioned earlier, the term *neurosis* originated in the eighteenth century to designate those situations where patients behaved in ways that were supposed to

be associated with the nerves – lethargy, convulsions, fainting – but where no physical lesion could be identified. It developed as a complement to the concept of neuritis, nervous symptoms that were associated with damage to the nervous system in autopsies. When a disease was known to leave post mortem traces of nervous damage, it was classified as neuritis. When a disease was believed to cause nervous damage but its physical pathology had not yet been identified, it was neurosis. The discovery that certain actions take place reflexively led psychiatrists to hypothesize that neuroses could be related to defective reflexive regulation in the nervous system, and Wilhelm Wundt's work toward the end of the nineteenth century suggested that problems with a patient's nervous reflexes might be associated with a loosening of mental associations. Thus when Freud and Janet recognized a loosening of mental associations among hysterics, these were dubbed *hysterical neuroses* (Healy, 1997).

Freud originally distinguished between *actual neuroses*, which were conditions associated with an actual disorder of the nerves (he believed nervous trauma resulting from masturbation to be the most prevalent form), and *psychoneuroses*, which were psychological in nature. Over time the idea of psychoneurosis became the cornerstone of psychiatric practice. In popular usage *neurosis* came to mean a problem with psychological causes, precisely the opposite of what the term originally signified. Whereas *psychosis* and *neurosis* had once referred to disorders originating in the mind and body, respectively, both words had come to mean essentially the same thing. As a result, some psychiatrists in the early twentieth century consciously pushed to restrict the meaning of *psychosis* to something more like the original meaning of *neurosis*; disorders associated with the

nervous system, the body, or something vital. Accordingly, when the newly developed electroconvulsive therapy, a physical treatment, produced promising results in people with severe depression but not on people with mild depression, it was taken to suggest that there are two fundamentally different forms of depression, psychotic and neurotic. Psychotic depression, the more severe form that responded to electroconvulsive therapy, was called *endogenous depression*. Neurotic depression, the less severe form that did not respond to electroconvulsive therapy became *exogenous depression*. After World War II, these concepts were sometimes overlain with the distinction between depressive disease and depressive reaction (Blazer, 2005; Healy, 1997).

Following Meyer's lead, psychiatric diagnosis in America emphasized the uniqueness of the individual's reaction pattern rather than categorical similarities. Numerous new forms of depression were named and quickly assimilated under the broader spectrum of "affective reaction types." "[I]n the heyday of psychoanalysis," says Shorter (1997), "many psychiatrists were simply indifferent to diagnosis, and believed that ascertaining the presumed psychodynamic cause was more important than classifying the presenting symptoms" (pp. 296-297). As the uniqueness of the individual's situation was accorded an increasingly central place in the treatment process, classification became all but meaningless. Healy (1997) writes,

Under Meyer's and later Freud's influence, American psychiatry drifted toward an extreme diagnostic nihilism. The schizophrenia concept expanded hugely, probably owing partly to Bleuler's view that schizophrenia involved a loosening of associations – loosening of associations being something that can rather readily be detected in anyone with nervous problems... In the

process, even the boundaries between depression and schizophrenia were blurred and there was increasing recourse to the concept of a schizoaffective disorder (p.41).

The first two editions of the *Diagnostic and Statistical Manual*, published in 1952 and 1968, respectively, did little to reverse this diagnostic nihilism, as we will see in chapter six. By the 1970s, psychiatric diagnosis was in a state of crisis.

### **The Rosenhan Experiments**

In 1973 professor of psychology and law David Rosenhan published an article titled “Being Sane in Insane Places” detailing the results of two experiments into the nature of psychiatric diagnosis. In the first experiment, Rosenhan and seven others sought admission to twelve different psychiatric hospitals in five states, complaining of hearing voices. In eleven cases the participant was admitted with a diagnosis of schizophrenia. The sole exception was the pseudopatient presenting at the only private hospital in the sample, who was diagnosed with manic-depressive psychosis – a condition with a significantly more favorable prognosis. Upon admission, the participants stopped pretending to hear voices, resumed behaving as they normally would, and attempted to get themselves discharged. The point of the experiment was to test the stickiness of diagnostic labels, the extent to which the diagnosis would “stick” despite disconfirming evidence. It took the pseudopatients an average of nineteen days to get themselves discharged, with a range from one week to fifty-two days. When the pseudopatients were able to secure themselves discharge from the hospital, it was always with a diagnosis of schizophrenia (or psychosis) in remission. As Rosenhan explains,

The label “in remission” should in no way be dismissed as a formality, for at no time during any hospitalization had any question been raised about any pseudopatient’s simulation. Nor are there any indications in the hospital records that the pseudopatient’s status was suspect. Rather, the evidence is strong that, once labeled schizophrenic, the pseudopatient was stuck with that label. If the pseudopatient was to be discharged, he must naturally be “in remission”; but he was not sane, nor, in the institution’s view, had he ever been sane.

Interestingly, although none of the hospital staff detected any of the pseudopatients at any point in the experiment, other patients frequently identified them as imposters:

During the first three hospitalizations, when accurate counts were kept, 35 of a total of 118 patients on the admissions ward voiced their suspicions, some vigorously. “You’re not crazy. You’re a journalist, or a professor (referring to the continual note-taking). You’re checking up on the hospital.” While most of the patients were reassured by the pseudopatient’s insistence that he had been sick before he came in but was fine now, some continued to believe that the pseudopatient was sane throughout his hospitalization. The fact that the patients often recognized normality when the staff did not raises important questions.

Rosenhan notes that other patients attempting to get themselves discharged “not only singled us out for attention, but came to imitate our behaviors and styles.” He spends considerable page space detailing the experience of psychiatric hospitalization, including intense powerlessness, depersonalization, and occasional

abuse, but the thrust of his criticism focuses on the inescapability of the psychiatric diagnosis.

Once admitted, the pseudopatients found that everything they said and did was read through the lens of their initial diagnoses. One pseudopatient's relational history was "unintentionally distorted by the staff to achieve consistency with a popular theory of the dynamics of a schizophrenic reaction," reading ambivalence and affective instability into a history of normal and healthy relationships with his parents, friends, and spouse. The pseudopatients initially tried to take notes on their experiences in secret, but upon realizing that no one much cared they wrote notes openly, often in public spaces like the day room. Although hospital staff never questioned them about these notes or bothered to look at what they were writing, nursing records showed that the staff interpreted the pseudopatients' frequent note-taking as a manifestation of the compulsive behaviors associated with schizophrenia. Rosenhan writes:

One tacit characteristic of psychiatric diagnosis is that it locates the source of the aberration within the individual and only rarely in the complex of stimuli that surrounds him... The notes kept by pseudopatients are full of patient behaviors that were misinterpreted by well-intentioned staff. Often enough, a patient would go "berserk" because he had, wittingly or unwittingly, been mistreated by, say, an attendant. A nurse coming onto the scene would rarely inquire even cursorily into the environmental stimuli of the patient's behavior. Rather, she assumed that his upset derived from his pathology... One psychiatrist pointed to a group of patients who were sitting outside the cafeteria entrance half an hour before lunchtime. To a group of

young residents he indicated that such behavior was characteristic of the oral-acquisitive nature of the syndrome. It seemed not to occur to him that there were very few things to anticipate in a psychiatric hospital besides eating.

Rosenhan suggests that the overwhelming failure of the hospital staff to detect sanity in psychiatric settings may stem from a strong preference among medical professionals for Type II error (false positives) rather than Type I error (false negatives). This is a perfectly rational bias for practitioners of somatic medicine, but what holds true for traditional medicine does not necessarily hold true for psychiatry, where diagnoses carry debilitating personal, social, and legal consequences.

In an effort to see if this bias could be reversed, Rosenhan conducted a second experiment “at a research and teaching hospital whose staff had heard these findings but doubted that such an error could occur in their hospital.” Rosenhan told the hospital staff that one or more pseudopatients would present themselves for admission in the next three months, and asked each staff member to rate each patient admitted during that time based on the likelihood that the patient was an imposter. Of the 193 patients admitted during the experiment,

[f]orty-one patients were alleged, with high confidence, to be pseudopatients by at least one member of the staff. Twenty-three were considered suspect by at least one psychiatrist. Nineteen were suspected by one psychiatrist and one other staff member. Actually, no genuine pseudopatient (at least from my group) presented himself during this period.



Rosenhan concludes that “[i]t is clear we cannot distinguish the sane from the insane in psychiatric hospitals. The hospital itself imposes a special environment in which the meaning of behavior can easily be misunderstood.”

### **A Growing Crisis**

An additional concern with psychiatric diagnoses was the growing recognition of incompatible national and regional traditions. The French had a disorder called *eruptive delusional disorder* that had no counterpart elsewhere in the world, and they were fond of diagnosing feeble-mindedness. The English diagnosed neuroses liberally. Americans preferred to diagnose schizophrenia (Shorter, 1997). In one study, a group of researchers from both sides of the Atlantic showed diagnostic interviews with eight patients, three American and five British, to large groups of psychiatrists in the eastern U.S. and different parts of the British Isles. The Americans diagnosed schizophrenia markedly more than their British counterparts, including one case where 69% of the American psychiatrists diagnosed schizophrenia and only 2% of the British psychiatrists did. The researchers concluded that the American concept of schizophrenia “is much broader than the British concept, embracing not only part of what in Britain would be regarded as depressive illness, but also substantial parts of several other diagnostic categories – manic illness, neurotic illness, and personality disorder” (Kendell et al., 1971, p. 123).

Growing concern about the state of psychiatric diagnosis throughout the 1970s had a great deal to do with the availability of targeted psychiatric drugs as well. As recently as fifty years ago, whether one was diagnosed as schizophrenic or depressed had little practical import for either the patient or the physician (Shorter,

1997). Guze and Goodman (1989), two of the principle authors of the DSM-III note that “[w]ith the availability of lithium and neuroleptic drugs distinguishing between mania and schizophrenia – once an interesting academic exercise – might now determine how a patient was treated” (p. vii).

### ***DSM-III and the Age of Depression***

This was the state of diagnosis in American psychiatry prior to the 1980 publication of *DSM-III*, which abandoned the psychoanalytic framework of the first two editions in favor of a classification system based on Kraepelin’s model. The *DSM-III* is recognized as a revolution in psychiatry, the defining point in the modern history of the discipline. Chapter six will review these events in greater detail, but three aspects of the disciplinary transformation surrounding the publication of *DSM-III* are of particular interest to us here.

The first of these is the elimination of reactive depression. Following Kraepelin, the authors of *DSM-III* declared themselves agnostic about cause. The conceptual distinction between endogenous and exogenous depression was no longer necessary. This move of purported etiological neutrality had the primary effect of ensuring that all depression was now understood to originate in biology rather than environment or the patient’s life experiences (Blazer, 2005). We will explore the parallels between the elimination of reactive depression and the removal of the bereavement exemption in the final chapter.

Around the same time, depression came to be seen as an affliction requiring medical intervention. Since the days of mania and melancholia, even very severe forms of depression had been understood as largely self-limiting, conditions that usually went away on their own. One 1964 review of the depression literature, for

example, notes that “depression is, on the whole, one of the psychiatric conditions with the best prognosis for eventual recovery with or without treatment. Most depressions are self-limited and the spontaneous or placebo-induced improvement rate is often high” (Cole, 1964, p. 124). Medawar and Hardon (2005) suggest that the primary reason for this change in medical opinion was pharmaceutical marketing. Reframing depression as a deficiency state made drug intervention all but imperative. Pharmaceutical companies explained that some people need antidepressants just as some diabetics need insulin, and industry-sponsored “awareness” campaigns helped to create a climate of fear surrounding depression, emphasizing that untreated depression places patients at increased risk of suicide. Over time, physicians came to think about depression as a state of chemical imbalance rather than a neurotic reaction, and the fact that past depressions usually cleared up on their own seems to have been largely forgotten.

The final point to take note of is that diagnoses of depression increased dramatically in the years following the publication of *DSM-III*. As recently as forty years ago, depression was diagnosed at roughly 0.1% the rate it was by the turn of the twenty-first century, meaning that diagnoses of depression increased a thousand-fold in a period of under three decades. This is the result both of the increasing psychologization of distress, bringing more people under the fold of psychiatric diagnosis than were in the past, and of shifting diagnosis patterns. Although psychoanalysts maintained a model of depressions and depressive reactions, the forms of distress that are today associated with depression were diagnosed primarily as symptoms of anxiety disorders or neurosis more generally, not depression. One factor in this shift is likely the *DSM-III* itself. *DSM-III* broke

anxiety neurosis into a variety of different conditions, including generalized anxiety disorder, panic disorder, social phobia, post-traumatic stress disorder, and obsessive-compulsive disorder, while the depressions were collapsed into a single category. In addition, the *DSM-III* provided insurance companies with a meaningful standard for determining coverage, which in turn shaped diagnoses – depression is a much sturdier, medical-sounding diagnosis than anxiety neurosis, and therefore more likely to be covered by insurance companies. Yet another factor that contributed to this change was the availability and marketing of psychiatric drugs. It is this latter force that we will examine in the remainder of the chapter (Healy, 2004).

### **Antidepressants and Depression in Modern America**

It is important to distinguish here between the way a disorder is conceived of, the way people think about what the disorder is and where it comes from, and the way the disorder is categorized, the formal definition of its boundaries. Although the *Diagnostic and Statistical Manual* has tremendous societal import, it probably shapes how most people understand depression only indirectly. The *Diagnostic and Statistical Manual* is designed to provide etiologically neutral, operational criteria for mental disorders. It does not tell us what depression is, in the sense of defining its nature or cause, nor does it tell us what it means to be depressed. It tells us who counts as depressive. It defines the boundaries of the category, but it does not purport to tell us anything about the meaning of the category itself. Although the *Diagnostic and Statistical Manual* does not tell us where psychiatric disorders come from or how to treat them, it tells us how institutional rules are applied; what they do, who they affect, and under what conditions. It determines the billing codes used

by insurance companies, which in turn decide the kinds of treatment a provider can be reimbursed for, and what types of drugs a patient is eligible to get. It affects the rules that determine which students have access to special testing accommodations. It shapes the laws that determine who the state can forcibly confine, and which criminal offenders can be held legally accountable for their actions.

Pharmaceuticals, more than anything else, shape the way modern Americans understand depression. We now live in a serotonin culture, where many people understand it to be a scientific fact that depression is caused by low levels of serotonin in the brain. Healy (2004) says:

In a way unimaginable ten years ago, popular culture takes it for granted that serotonin is low in depressed people... Talk-show hosts kick off programs by mentioning the recognized serotonin deficiency in depression. I have repeatedly brought such shows to a crashing halt by saying there is, in fact, no truth in this. Neither the questioner nor I know what to say next. How do I even begin to explain that there was never any basis for this idea? How does the host cope with someone breaking one of the fundamental rules of the game? (p. 263-264).

When the first antidepressants appeared, there was undoubtedly some scientific basis for the idea that neurotransmitters might be associated with depression, but the orthodoxy of serotonin deficiency that pervades popular discourses about depression has now been thoroughly scientifically discredited.

The serotonin theory of depression is a specific form of the more general monoamine theory of depression. Monoamines are neurotransmitters, chemicals that transmit signals throughout the brain and central nervous system. Following

the discovery of antidepressant effects in imipramine, an antihistamine, and iproniazid, a tuberculosis drug, in the late 1950s, psychiatrists hypothesized that depression had something to do with the actions of these drugs on neurotransmitters. Chemical transmission occurs when a nerve cell, or neuron, releases a chemical into the synaptic cleft connecting it to other neurons. Molecules of the chemical bind to receptors on the other neurons, stimulating them and signaling them to pass along the chemical signal. The remainder of the chemical used for transmission is either re-absorbed by the neuron that released it or it is broken down metabolically. Iproniazid is a monoamine oxidase inhibitor (MAOI). Monoamine oxidase is an enzyme involved in the metabolism, or breaking down, of neurochemicals left in the synaptic cleft after neurotransmission. Inhibiting the production of monoamine oxidase therefore is theorized to allow these chemicals to build up in the brain, increasing the stimulation of receptor cells. Imipramine is known as a tricyclic antidepressant owing to its three ring chemical structure, a class of antidepressants that acts by preventing the re-absorption, or reuptake, of neurochemicals by sender cells. It was known that neurotransmitters such as dopamine, adrenaline, and serotonin are associated with pleasure and energy, so the antidepressant effects of these drugs led scientists to hypothesize that depression might involve a natural deficiency of these chemicals in the brain (Healy, 2002; 2004).

Selective serotonin reuptake inhibitors (SSRIs) are the most popular modern antidepressants. This is the class of drugs that includes Prozac, Zoloft, Paxil, and Celexa, among others. These act by inhibiting the reuptake of serotonin, which is known chemically as 5HT. SmithKline Beecham launched Paxil much later than the

other major 5HT reuptake inhibitors. In an effort to distinguish it from the other drugs that inhibited serotonin reuptake, they marketed Paxil as a *selective* serotonin reuptake inhibitor, highlighting the fact that it does not act on catecholamines (the neurotransmitters involved in the “fight or flight” mechanisms of the sympathetic nervous system, including dopamine, adrenaline, and noradrenaline). Selectivity obviously sounds like something that one would want in a drug, and *SSRI* rolls off the tongue much easier than *5HT reuptake inhibitor*, so the other drug companies quickly followed suit. Today the whole class of reuptake inhibiting antidepressants whose parent molecules have little effect on catecholamines are referred to generically with the acronym SSRI, even though none of them are actually exclusive to serotonin reuptake. The SSRIs are a chemically diverse group of compounds with a range of active neurochemical principals and effect profiles. Fluvoxamine (Luvox/Faverin) is the most effective at inhibiting serotonin reuptake, while citalopram (Cipramil) is the most specific, meaning that it has the least effect on other monoamines (Healy, 1997).

### **The Catecholamine Hypothesis**

Although modern SSRIs are designed to avoid interaction with catecholamines, the catecholamines originally showed much greater promise as antidepressant targets. Aside from LSD, which had been demonstrated to affect the serotonin system, there were few drugs capable of acting on serotonin in the 1950s and scientists had little other indication that serotonin was important to mood. By contrast, the role of catecholamines in regulating anxiety and fear had been well established, and there were numerous drugs capable of acting on the catecholamines. The antipsychotic reserpine blocks the uptake and storage of

catecholamines, effectively depleting them from the patient's system, and reserpine had been reported to cause some patients to become depressed, even suicidal. Following the discovery that a best-selling antidepressant called desipramine blocks catecholamine reuptake but not serotonin reuptake, Joseph Schildkraut formally articulated the catecholamine theory of affective disorders in 1965 (Healy, 1997).

From the beginning, Schildkraut referred to the catecholamine hypothesis as "at best a reductionist oversimplification" that "may soon require revision." According to Healy (1997), there were indeed several basic problems with this idea. For one, the reports of people becoming depressed on reserpine had never been scientifically established. This is not to say that they were de facto invalid, but the only formal trial to monitor the depression level of patients on reserpine had found no causal link between reserpine and depression and, in fact, suggested that reserpine lowered levels of anxiety and depression. Furthermore, it had been established that the compound isoniazid has antidepressant effects, but isoniazid does not inhibit monoamine oxidase or the reuptake of monoamines. A third problem with the theory was that there was no proof that inhibiting the reuptake of catecholamines would lead to increased levels of catecholamines in the brain. Reuptake is a conservation mechanism, so it is just as logical to expect that inhibiting it might lead to a net decrease. Though it was not established at the time, this idea has been supported by research on the brain chemistry of non-human animals given monoamine reuptake inhibitors; chronic use of these drugs decreases, rather than increases, the amount of monoamines in the brain. Fourth, therapies designed to increase the amount of catecholamines in the brain proved generally ineffective as depression treatments. The final problem with this theory is that if



depression is a function of brain chemistry, and if this imbalance can be chemically corrected to produce mood changes, then effective depression therapies should at the very least be ineffective in treating mania. As it happens though, the tricyclic antidepressants are effective in treating mania as well as depression. Lithium and electroconvulsive therapy have also been demonstrated to treat both depression and mania (Healy, 1997).

Despite these problems with the catecholamine theory, it was quickly taken up by some psychiatrists, polarizing the psychiatric community. The mere existence of antidepressant drugs was never a challenge to psychoanalysis any more than the existence of aspirin is a challenge to chiropractics. There had long been physical remedies for mood disorders, such as alcohol, St. John's wort, and narcotic drugs. These could be used to treat symptoms without imputing causation, allowing psychoanalysts to maintain both the psychogenic origin of mood disorders and the effectiveness of physical treatments. The catecholamine theory changed this, forcing psychiatrists to align with one school of thought or the other. Each community formed its own professional associations, held its own conferences, and published its own journals. The catecholamine hypothesis pointed toward a group of specialists who spoke an entirely different language from the psychoanalysts.

But why was a theory with such significant problems embraced so enthusiastically as to create a fissure through the entire discipline? Invoking Kuhn, Healy (1997) writes:

Certain hypotheses and theories hold sway at particular times, not necessarily because they are the most comprehensive, the most sophisticated, and the best, but because they make some attempt to explain

particular sets of facts that a scientific community at that time feels need explaining and they do a better job than their competitors... the catecholamine hypothesis... made some effort to account for the fact that antidepressants worked and afforded some predictions that could be tested... the catecholamine hypotheses gave the pharmaceutical industry a clear goal to aim at... Once companies' production efforts turned this way, industry scientists had a vested interest in talking up catecholamines... There was, furthermore, a certain market-copy quality to the amine hypothesis that was also important. The idea that depression was "known" to involve low levels of biogenic amines was something that fitted neatly into the snappy format in which truths have to be conveyed on advertisements to physicians... When prescribing any pill it is useful to have a model of what the pills do to convey to patients, and for the past thirty years, physicians prescribing antidepressants have found it very easy to sell a package of low brain amines, which the pills are designed to correct. In part it has been so easy to sell this because the idea of low amines became public knowledge very quickly... Where once lay people had gone to psychiatrists expecting to hear about sexual repression, they now came knowing that something might be wrong with their amines or with some brain chemical (pp. 160-161).

The idea that pharmaceuticals are correcting an imbalance has a particularly powerful intuitive appeal. Many people find the idea of taking a drug to correct a chemical deficit, to restore a naturally existing balance, more palatable than the idea of taking of a drug that artificially induces "unnatural" emotions. All these points are

relevant to understanding the serotonin theory of depression, which has never had a more firm scientific foundation than the catecholamine theory.

### **The Serotonin Hypothesis**

The idea that serotonin might be involved in depression first appeared around the middle of the twentieth century, but it took more than two decades before the pharmaceutical companies began to take seriously the idea of a serotonin-targeted mood treatment. Research on LSD in the 1950s showed that LSD affected the behavior of the serotonin system, leading scientists to hypothesize that serotonin might play a role in mental functioning. Tryptophan is a serotonin precursor, meaning that it interacts with other chemicals to produce serotonin. In the early 1960s, researchers in England discovered that adding tryptophan to MAOIs appeared to increase recovery rates, and that serotonin is linked to aspects of mood and personality such as irritability and impulsivity. On this basis, Alec Coppen proposed the serotonin theory of depression as a close cousin of the catecholamine theory in 1967 (Healy, 1997).

American research was focused primarily on catecholamines in the 1970s, but serotonin research increased at a rapid pace in Europe. Pharmaceutical firms began developing serotonin reuptake inhibitors, and two developments in particular stimulated renewed interest in serotonin worldwide. The first came from Marie Åsberg in Sweden, who found that 5HIAA, the primary metabolite of 5HT, was lower in the cerebrospinal fluid of depressed patients who were suicidal. As other groups attempted to replicate Åsberg's findings, however, they failed to confirm such a link. Åsberg and colleagues attempted to account for these findings, first claiming that the suicidal depressives have lowered 5HIAA, then all depressives

whether or not they were suicidal, then all depressives who had ever been suicidal, then all people who had ever been suicidal whether or not they were depressive, and finally all people who were impulsive regardless of whether they were suicidal or depressed (Healy, 1997).

The second major development came from Steven Paul and Solomon Langer in France, who found that the number of ipramine binding sites on the neurons of depressed patients had less receptors than those of non-depressed patients. For the first time, it appeared as though an objective diagnostic test for depression might be possible. As with Åsberg's discovery of lowered metabolites though, others' attempts to replicate these findings failed. Ipramine binding, it turned out, varied with not only demographically, but seasonally and diurnally (Healy, 1997).

Against this backdrop, Eli Lilly developed the compound fluoxetine (Prozac). At the time depression was still rarely diagnosed, and Eli Lilly was considering marketing the drug as an antihypertensive or maybe a weight loss drug. Trial evidence suggested that patients on fluoxetine lost weight, which almost certainly played a role in the enthusiastic public reception Prozac received once it went to market. Alec Coppen, who first proposed the serotonin theory of depression, recounts a meeting with company executives in the early 1970s where he suggested that the drug might be used to combat depression: "I'll always remember the Vice President of Research saying 'I thank Dr. Coppen for his contribution but I can tell you we won't be developing fluoxetine as an antidepressant'" (Shorter, 1997, p. 322).

When, with great trepidation, Eli Lilly finally took fluoxetine to the market as an antidepressant, what resulted was nothing less than a cultural revolution. No one was prepared for the response generated by Prozac, least of all the pharmaceutical

companies. In many ways, Prozac just happened to be the right drug at the right place and time. It was not the first SSRI on the market as an antidepressant. That was zimiledine (Zelmid), developed by Merck. Zelmid went to market in Europe in 1982, but it was withdrawn before being approved for sale in America due to findings that it caused an autoimmune disorder called Guillan-Barré syndrome. Duphar's fluvoxamine (Luvox/Faverin) was the second SSRI, marketed in Europe in 1983. Eli Lilly's fluoxetine (Prozac) was the third, licensed for sale in the United States in 1987. Unlike its predecessors, the name *Prozac* was not derived from the clinical name for the molecule. *Prozac* combines a prefix that refers to professionalism and a suffix associated with the treatment of a specific defect. Prozac came to market at a time when the mass of nervous complaints previously treated as anxiety or neurosis were being described by clinicians as depression, shortly after the American public turned against the benzodiazepines that had previously been used to treat these symptoms. One of the reasons clinicians favored Prozac is that it acts across a range of affective conditions other than depression, such as obsessive-compulsive disorder or panic disorder. Prozac was also the only antidepressant on the market with the side effect of weight loss, and it enmeshed itself quickly in American culture. The drug was popularized in best-selling books, like Elizabeth Wurtzel's *Prozac Nation* and Peter Kramer's *Listening to Prozac*, and it even made the cover of *Newsweek*. Above all though, Prozac's success owes to the widespread belief that depression is caused by a deficit of serotonin in the brain (Healy, 1997; Healy, 2004; Shorter, 1997).

## **The Failure of the Serotonin Theory**

The serotonin theory of depression, like the catecholamine theory before it, can no longer be considered scientifically tenable. The basic foundation of the serotonin hypothesis is the observation that inhibiting serotonin reuptake can reduce symptoms of depression. It is a reasonable hypothesis, but by no means is it based on a strong form of reasoning. The same logic could be used to support the aspirin theory of headaches, for instance: because aspirin relieves headaches, headaches must be caused by an aspirin deficiency. As with the catecholamine theory, the serotonin theory assumes the blocking the reuptake of serotonin will lead to increased levels of serotonin in the brain, but tests on non-human animals have not borne this out. Another problem this model shares with the catecholamine theory is the inability to account for therapies that treat both depressive symptoms and manic symptoms. There is no evidence that depressed people have lowered levels of serotonin in their brains, and some drugs with antidepressant properties even prevent serotonin from binding to neural receptors. Further, SSRIs act immediately on the serotonin system, but the mood-altering effects of the drug take several weeks to kick in (Healy, 1997; Healy, 2004). As Carlat (2010) explains:

Researchers have found evidence of abnormalities in serotonin, norepinephrine, dopamine, cortisol, thyroid, growth hormone, glutamate, and brain-derived neurotrophic factor — yet no specific defect has been identified. Straying outside the world of chemistry, other researchers have tried to find the causes of depression through neuroimaging scans. But this research has been just as inconclusive. Some of the major findings include decreased activity in the left frontal lobe, a shrunken hippocampus, an

oversized amygdala, disrupted circuits around the basal ganglia, and miscellaneous abnormalities in the thalamus and the pituitary gland... A typical brain contains one hundred billion neurons, each of which makes electrical connections, or synapses, with up to ten thousand other neurons. That means a quadrillion synapses are active at any given time — the number of people on 150,000 Earths. It is therefore no surprise that we know almost nothing definitive about the pathophysiology of mental illness — the surprise is that we know anything at all (p. 6).

All these problems complicate attempts to account for the observation upon which the serotonin theory is based; that drugs which work to inhibit the metabolism or reuptake of serotonin seem to have antidepressant properties. As it turns out though, there are problems with this basic observation as well.

One of the problems with the claim that serotonin-targeted antidepressants work to reduce depression is that the SSRIs are in fact no more effective than any other class of antidepressant with different chemical properties. Drugs that block the reuptake of serotonin do make some people feel better, but drugs that block the reuptake of noradrenaline or dopamine work just as well. There is substantial evidence that SSRIs are actually less clinically effective than older antidepressants, a somewhat paradoxical consequence of developments in neuroscience. As Healy (1997) explains:

There has been astonishing progress in the neurosciences but little or no progress in understanding depression...[T]he emergence of an ability to clone receptors [has] permitted, indeed almost compelled, a different form of drug development. Specific compounds are now designed to target

specific receptors... These new compounds are as selective as magic bullet mythology ever could have wished to be... But this is the chemists' rather than the biologists' vision... Such compounds... in the case of both the antidepressants and the antipsychotics, seem if anything less effective than the older, "dirtier" drugs. This has led to something of a crisis in that the chemists cannot readily engineer compounds when more than two targets are indicated (pp. 174-175).

To understand the current state of antidepressant research, it is important to remember that the first antidepressants were discovered entirely by accident, when the tuberculosis drug iproniazid made some patients feel "too well," causing them to overexert themselves or discontinue their medications prematurely. Once discovered, it took a number of years for these drugs to stimulate new research. The pharmaceutical industry was initially ambivalent about producing medicines for depression, mostly because of "uncertainty about what 'depression' was. Until these drugs were discovered, the extent of depression was unknown, and there were no good estimates of market size" (Medawar & Hardon, 2004, p. 45). Research into depression began to pick up pace once the pharmaceutical companies discovered the substantial size of the antidepressant market, but nearly all antidepressant research for the past sixty years has been focused on understanding and reverse-engineering these early accidental successes. Given this trajectory then, why do pharmaceutical researchers continue to invest considerable money and effort chasing the golden calf of serotonin? Healy (1997) likens the situation of the pharmaceutical companies to the joke about the drunk and the lamppost:



One night, a police officer happens across a drunken man scouring the ground underneath a lamppost for his keys. The police officer tries to help the man locate them, but to no avail. After a few minutes of searching the officer turns to the drunk and asks whether he is sure that this is the same lamp where he dropped his keys. “No, of course not,” he responds. “I didn’t lose my keys under a lamppost. I lost them somewhere in those bushes.” “But then why are you looking for them under the lamppost?” the officer asks. The drunk cocks his head quizzically and stares at the police officer for a moment before answering. “Where else would I look? This is where the light is.”

The final problem with the basic observation upon which the serotonin hypothesis is based is that it is simply not supported by the existing scientific evidence. Some individuals may feel better after taking SSRIs, but that alone proves neither causation nor efficacy. Studies across large test populations have repeatedly found that these drugs are ineffective at treating depression. For moderate and mild forms of depression, which comprise the overwhelming majority of diagnoses in America today, SSRIs have proven to be, on balance, no more effective than placebo. A 2008 meta-analysis of trial data submitted to the FDA found that the benefit of SSRIs over placebo “falls below accepted standards for clinical significance.” The authors write:

Drug-placebo differences in antidepressant efficacy increase as a function of baseline severity, but are relatively small even for severely depressed patients. The relationship between initial severity and antidepressant efficacy is attributable to decreased responsiveness to placebo among very

severely depressed patients, rather than to increased responsiveness to medication (Kirsch et al, 2008).

Similarly, a 2010 meta-analysis published in the *Journal of the American Medical Association* looked at FDA-approved, randomized, placebo-controlled trials that did not include a placebo washout period. The authors conclude that “[t]he magnitude of benefit of antidepressant medication compared with placebo increases with severity of depression symptoms and may be minimal or nonexistent, on average, in patients with mild or moderate symptoms” (Fournier et al. 2010).

Placebo washout is a standard phase in SSRI trials where all participants are put on a placebo for several weeks and anyone who responds is removed from the study before groups are randomized. This is quite obviously bad science, but the pharmaceutical industry defends the practice on the grounds that it helps to prove their drugs are actually doing *something*. Owing to the high levels of placebo response and spontaneous remission for depressive patients, it is not uncommon for a trial to lose half the participants or more during the placebo washout. The few studies that have been done using an active placebo – placebo pills laced with an active chemical to mimic the side effects of an actual drug – have shown no benefit at all to taking SSRIs over placebo (Medawar & Hardon, 2004).

Joanna Moncrief, a psychiatrist at University College London, argues that the meager benefits of SSRIs over placebo in conventional placebo studies can be accounted for by two factors: “One is the possibility of an amplified placebo response when people know they are on an active drug. The drugs will also induce some drowsiness, which might improve depression-rating scores.” Not only do people feel better when they are well rested, but sleep-related items can account for

as much as 12% of the total score on the industry-standard Hamilton depression rating scale. Moncrieff explains “[i]f you give someone a glass of alcohol, their score will change. None of these scales validly record depression or mood. They respond to any drug that makes people feel different” (Scott, 2008). In this vein, stimulants and antipsychotics both produce changes on depression rating scales even though neither class of drug demonstrates antidepressant properties (Moncrieff and Kirsch, 2005).

### **Understanding Depression**

It is clear that selective serotonin reuptake inhibitors do *something* that seems to combat depression in at least some patients some of the time, but the exact mechanism of these mood effects remains a mystery. It may be that mood is the result of a complex interaction of different neurotransmitters, or that antidepressants act on something that is antecedent to mood disorders. In a book published earlier this year, Shorter and Fink (2010) review evidence that the endocrine system may be responsible, or partly responsible, for causing mood disorders. There was some promising research done in this field in the 1980s, but researchers abandoned the endocrine system when serotonin came into vogue. It might also turn out that depression does not originate in biology after all.

Blazer (2005) suggests that the dichotomy between psychological and biological understandings of depression is a false one:

Most episodes of moderate to severe depression derive from a biological predisposition to depression coupled with the stress associated with the social environment, such as a loss or threat (or even the ongoing hassles of everyday life). Even the biological predisposition undoubtedly is

conditioned over the years. In addition, under enough stress, most people will experience some type of emotional suffering, and many of these will express that suffering through symptoms of depression. Virtually all depression is at once exogenous and endogenous (pp. 51-52).

Healy (1997) suggests that one potentially promising area of research incorporating biological, environmental, and psychological understandings of depression is the group of theories known as circadian rhythm models. These are based on the idea that the dislocation of social routines can disrupt the body's natural rhythmic homeostasis. In this type of model, either biological processes or external events have the potential to trigger a state of de-synchronization or stress that might be comparable to the experience of having the flu or being jet lagged. Individual response patterns can either promote resynchronization or a de-synchronizing spiral into depression. Exercising regularly, eating well, and having a strong emotional support system are all factors that increase the likelihood someone will recover from circadian disruption on their own, while poor sleep or cycles of poor performance and self-blame might push an individual into a state of self-sustaining de-synchronization that is labeled depression. One benefit of circadian rhythm models is that, unlike both the psychogenic model of depression and the biological model, they are not premised on the effectiveness of any particular treatment strategy (Healy, 1997).

One of the very few things we know for certain about depression is that the serotonin theory is largely unsupported by the existing science. That the serotonin theory of depression is widely understood as a biochemical fact is a triumph of savvy marketing and auspicious circumstances, not sound science or rational public

discourse. It is also a testament to Kuhn's observation that scientific ideas become popular not necessarily because they are correct, or even logically sound. Scientific ideas are embraced when the explanations they provide meets the needs of a particular community. We will continue to see variations on this theme throughout the next chapter.

## Chapter 5

### THE DEVELOPMENT OF PSYCHOPHARMACOLOGY

As we saw in chapter four, psychopharmacology has played a large and decisive role in shaping the contours of modern biological psychiatry. This chapter will genealogically recast the roots of modern psychopharmacology, tracing the evolution of medical and psychiatric substance use throughout history as well the specific institutional developments that gave rise to the modern pharmaceutical industry. Of chief importance among these is the birth of organic chemistry. This chapter will also examine the development of food and drug regulation in America. Throughout chapter five we will see a number of poignant examples of the interrelationship between power and knowledge and the ways that the critical distance of history can expose the non-essentiality of once taken for granted ways of knowing.

#### **The First Drugs**

Humoral systems generally treat health as a matter of natural balance and moderation, something that can be achieved by exercising, eating the right types or portions of food, moderating one's sexual activity, and so on. Medical interventions in this model usually aim to mimic or induce the body's own healing methods such as bleeding, blistering, vomiting, sweating and diuresis. Thus the earliest drug therapies were primarily emetics, laxatives, and purgatives; foods, drinks, or other herbal substances that helped to visibly remove unbalancing elements from the body. Physicians strove to produce the most dramatic effects possible; the stronger the effect the more potent the cure was understood to be. Today physicians generally attempt to avoid or minimize overt physical drug effects in the treatment

of illness. These are mostly considered “side effects,” secondary consequences unconnected to the active healing principle of the drug. Even when a drug is prescribed and used to induce specific physical effects, such as alertness or weight loss, these are thought to be separate from the active principle of the drug. For early humorists though, the physical effects were both the primary goal of the therapy and the proof that it was working (Healy, 1997).

Alcohol was one of the first substances used by humans to produce specific effects in the body, but dating the medicinal use of alcohol is problematic because the earliest evidence of alcohol consumption substantially predates recorded history (Patrick, 1970). It might be more accurate, therefore, to say that the very idea of medicine likely developed against a backdrop of extant alcohol consumption. Although alcohol did not fall under the purgative model that informed humoral understandings of medicine, ancient people drank alcohol to induce specific physical effects, including relaxation and pain relief, that are today associated with medicinal healing (Medawar & Hardon, 2005). In addition to producing immediate physiological effects, Angier (2007) suggests that the consumption of alcohol was probably correlated more generally with health and longevity among ancient humans. Alcohol was almost certainly safer than the drinking water available to most people in antiquity, which was commonly polluted with cholera and other microbial diseases. Alcohol itself has mild antiseptic properties, and the brewing of most forms of alcohol involves boiling and other sterilizing procedures. Angier even suggests that a biological predisposition to alcohol might be viewed as evolutionarily adaptive, as heavy drinkers were probably less likely to contract

infectious diseases than those who drank water and, ergo, more likely to survive long enough to reproduce and rear children.

### **Paracelsus and Chemical Healing**

The first major development in the evolution of modern pharmacology was the introduction of chemicals and minerals as curative agents, which came about in the early sixteenth century through the work of the Swiss physician Paracelsus. According to Healy (1997), “[a]lthough some distilled oils and metals had been used before Paracelsus, he inaugurated an era in which large numbers of chemical and metallic compounds were tried on patients for the very first time” (p. 15). Paracelsus studied a variety of sciences, including the Spagyric Art, a branch of alchemy that focused on the separation of pure and impure compounds and the purification of imperfect natural compounds. He believed that chemicals and minerals must have curative properties, just as plants do, otherwise God would not have put them there. Paracelsus rejected the Galenic doctrine of humors and its basis in deductive logic, favoring instead the doctrine of signatures, which was based in observations of nature. He also challenged the humoral view that afflictions came from imbalances within the body, arguing that illnesses were the product of poisons from without (Healy, 1997).

The doctrine of signatures held that the plants have signatures, or clues, indicating what sorts of conditions they can be used to cure, and Paracelsus expanded this to include certain chemicals and minerals as well. These could be either visual or pneumonic. Liverwort and kidneywort, for instance, were thought to have been made in the shape of the organs they were able to heal. Likewise, the doctrine of signatures held that nature had created snakeroot as an antidote for



snake venom and mercury as a cure for syphilis, because syphilis was associated with the marketplace and the market fell under the sign of the planet Mercury. One of the enduring successes of this model is acetylsalicylic acid, sold under the Bayer brand name Aspirin. Acetylsalicylic acid is derived from salicylic acid, an extract of willow tree bark, which was first discovered to cure fevers in 1763 by a man named Edward Stone. Stone discovered the curative properties of the willow tree because he knew that people tended to get fevers around swamps, and thus he reasoned that a plant capable of curing fever would likely be found growing near a swamp (Healy, 1997).

Alcohol has been used as a medicine in the modern sense since long before Paracelsus, often in the form of tonics. Paracelsus himself used laudanum, opium dissolved in alcohol, to treat melancholia. By the seventeenth century, there had developed a large market for drugs therapies. The chemists of the day could make two primary kinds of drugs. Concoctions of multiple ingredients were called patent medicines. These were usually sold directly to the end-user. Tonics, or medicinal drinks made with an alcohol base, were particularly popular forms of patent medicine. Pure ingredients were generally sold to doctors who would make their own mixtures. This practice laid the groundwork for the later distinction between ethical, or prescription, medicines, which were sold to doctors, and patent medicines. Patent medicines were sold directly to patients, but their ingredients were kept secret (Healy, 1997).

### **Medical Alienation Throughout the Ages**

Paracelsus' teachings were never fully accepted in his own time. In fact, Paracelsus held a professorship at the University of Basel for just one year before he

was violently run out of town for treating patients with chemicals and advocating the view that medical theory should be responsive to the observations of practitioners. The university stripped his title and a warrant for his arrest was issued. His colleagues saw his ideas as destructive of medical healing, and later commentators suggested that he must have conspired with the devil to introduce chemicals into medicine. The idea that medical science should look at the ends of treatment threatened the deductive foundation on which therapeutics had been based since the time of Galen. "It was this rejection of authority," says Healy (1997), "that underpins the violence of his expulsion from Basel; he had effected a breach within the contemporary world view if anything even more shocking than that of Galileo" (pp. 80-81).

Feelings of alienation from medicine, and from psychiatry in particular, are common in America today. It seems increasingly as though psychiatrists see the modern psychiatric patient not as a person, but as a collection of disarticulated symptoms waiting to be matched up to the appropriate drug, while advances in reproductive technologies, bioengineering, cybernetics, cloning, genomics, and the like continually present new challenges to the foundations of religious faith and our most deeply held ideas about what makes us unique. The sense of medical alienation in modern America is widespread and palpable. One can literally feel it, and it feels distinctly modern.

The specifics of modern medical alienation are no doubt historically unique, but these sentiments are not all that different from the feeling of alienation harbored by Paracelsus' contemporaries. This sense of medical alienation is in fact as old as biopower itself. Chapter three examined the development of disease

specificity, made possible in part by the role that statistics played in helping to secure support for the idea of outcome-based medicine. Each of these ideas was seen in its own time to be source of a new and dangerous alienation, a dehumanizing influence that corrupted medicine's healing purpose.

The first inoculation procedures were developed in France in the 1720s to protect people from smallpox. Doctors discovered that they could use fluid taken from the wounds of a smallpox victim to produce a mild case of the disease that seemed to have future prophylactic effects. This practice was called variolation. The variolated patient normally developed mild smallpox and recovered quickly, but on occasion the procedure could produce a full, potentially lethal case of the illness. Using statistics, advocates of this practice were able to demonstrate that only one in fifty variolated individuals who later contracted smallpox would die from the disease, whereas smallpox usually killed about one person in five. Far from welcoming this evidence though, the French public widely criticized and rejected variolation. Many people saw it as a fundamental betrayal of the doctor's role as healer: variolation required the doctor to do harm in the name of helping people, to gamble with the well-being of the individual patient in order to produce better outcomes across a population (Healy, 1997).

One fierce critic of variolation was Julien Offray de La Mettrie, a physician who became widely vilified himself for advocating a mechanical view of the human mind several decades later. La Mettrie argued that mental functions were the product of organic processes in the nervous system and brain. This idea was so threatening to prevailing understandings about what it means to be human that he received death threats and was eventually forced to flee from France to the more

politically tolerant Netherlands. In subsequent writings he compared the human organism to a machine and suggested that happiness might one day become a matter of medical engineering. This was too radical even for the Netherlands. The ensuing public outcry led to more death threats, and he was again forced to flee, eventually finding political sanctuary in Prussia (Wellman, 1992).

By the early nineteenth century there was growing support for the idea of using numbers to judge therapeutic effectiveness, leading Pierre Louis to suggest the first precursor to the modern clinical trial: the *méthode numérique*. Louis proposed evaluating treatments during an epidemic by taking a thousand patients, indiscriminately separating them into two groups, and then administering a different treatment to each group. If fewer patients died in one group than the other, that treatment would be judged the superior one. Louis was widely criticized by his contemporaries for reducing patients to abstract numbers. This was seen as both an affront to the dignity of the patient and a retreat from medical objectivity. Today, quantitative measures are viewed as a critical component of objective science, but in Louis' time, doctors considered the unique characteristics and circumstances of each patient indispensable to an objective evaluation of medical outcomes. To abandon individual differences was not only to do a great injustice to the patient, it was an effacement of scientific objectivity (Healy, 1997).

Louis tested his numerical method during an epidemic of typhoid in 1836 by comparing purgatives to bleeding. Contrary to the conventional wisdom of the day, he found that bleeding was no more effective at curing typhoid than purging and that tremendous numbers of people died either way. These results were so shocking that Louis repeated the experiment, believing he must have made a mistake. Only

after the second experiment confirmed the first did he report his results to the medical community, and then only with great hesitation. This mathematically based reproach of bleeding created a crisis among the French medical community that lasted for decades. It was not until the 1860s – a full quarter century after Louis first performed a statistical test of the effectiveness of bleeding – that some the most respected medical thinkers of the day finally helped to secure medical opinion against the *méthode numérique*. Not only did Louis' method challenge the very foundations of nineteenth century therapeutics, but statistics, they felt, were an alienating influence and a failure of medical purpose. Statistical measures could tell a doctor how many people had died, but they did little to aid the doctor in understanding why. Opposition to the numerical method, founded on an intuitive conviction in the therapeutic effectiveness of bleeding and a critical view of statistics, would set back the cause of controlled trials in France by nearly a century (Healy, 1997).

Mary Shelley's *Frankenstein; or, The Modern Prometheus*, first published in 1818, gave voice to the concerns of an age. Most of *Frankenstein* is told in retrospective, when the narrator, a ship captain traveling to the North Pole in search of scientific knowledge, comes across the beleaguered Dr. Victor Frankenstein, who tells his story as a warning to the good captain about the consequences of scientific hubris: "You seek for knowledge and wisdom as I once did; and I ardently hope that the gratification of your wishes may not be a serpent to sting you, as mine has been" (p. 17). According to Shelley, the story was influenced by a dream she had following discussions about galvanism, the discovery of the electrical stimulation of muscles, and popular rumors that the physician Erasmus Darwin had reanimated dead

matter. Written in a period of tumultuous scientific change, *Frankenstein* is a popular warning about medical science run amok and the dangers that arise when technical medical knowledge begins to encroach on the domain of God.

Just five years later, in 1823, the reflex arc was discovered, calling into question nearly universal beliefs about the nature of human life. We saw in chapter four how this discovery laid the way for new understandings of madness by undermining the idea that the soul was an indivisible entity that resides in the brain. The discovery of nervous reflexivity challenged not only prevailing understandings of the soul, but also the idea of human autonomy, that the human body is guided exclusively by conscious directions from the mind and the soul. These developments sparked great public anxiety and concerns that science was destroying the very thing that made human beings unique (Healy, 1997).

Similar ideas about human uniqueness played a large role in the critical reception of Charles Darwin's *On the Origin of Species*, published in 1859. Darwin's most well known book provoked a broad spectrum of critical responses from religious quarters, ranging from acceptance and synthesis to vicious hostility. Although he intentionally refrained from extending his theory to human beings, Darwin's arguments were understood as a challenge to the central idea that humans were specially created in God's image, the religious foundation of human uniqueness. It is telling that despite the numerous scientific advances of the past century and a half and the many forms of scientific alienation in the modern world, the idea of evolution is still a source of significant distress and political contestation in the year 2010.

Scientific discoveries continued to construct an increasingly atomized, mechanical picture of the human organism throughout the nineteenth and twentieth centuries, exacerbating feelings of anxiety and alienation from medical science. As psychiatry grew into a unique discipline, so too did public concern about its alienating effects. Many of the ideas on which modern psychiatry and neurology are founded, such as nervous reflexivity and the concept of neurotransmission, were sources of significant public angst in their own times. Aldous Huxley's 1932 novel *Brave New World* voiced fears of psychiatric alienation and social control that also found later articulation in George Orwell's novel *1984*. By the 1960s, public concerns about psychiatry coalesced into a full-blown social movement. The anti-psychiatry movement often drew on themes of social control and even mind control in their characterization of psychiatry. Electroconvulsive therapy and psychosurgery were understood as technologies for pacifying social deviancy, icons of psychiatry's brutality and dehumanization. At the height of the anti-psychiatric period a leading expert in psychopharmacology named Herman van Praag received death threats, targeting both Praag and his family, because of his research in biological psychiatry. In an echo of La Mettrie's experience two centuries prior, the Dutchman found that his views on the biological basis of thought and mood were too radical even for the politically tolerant Netherlands, eventually leading Praag to move his family to the United States.

Modern medical alienation then, though unique in its historical expression, is clearly not a product of our modern age. Throughout history, developments that have suggested a mechanical or particulate view of the person have often been seen to undermine the prevailing sense of what it means to be human, provoking fierce,

sometimes violent resistance (Healy, 1997). From Paracelsus' time to our own, medical alienation might better be understood as a feature of life in a biopolitical world rather than a unique response to historically specific scientific developments.

### **The Birth of the Pharmaceutical Industry**

The beginnings of an industry devoted to pharmaceutical production can be traced to two developments in the early nineteenth century. In 1806 Friedrich Sertürner isolated the active principle of opium, producing morphium. It was widely known that some samples of opium relieved pain more effectively than others. Sertürner realized that the active principle of the drug must be distributed unevenly to produce such disparate effects, and so he sought a way to purify it by isolating the active principle. This discovery made possible the standardization of drugs, and it laid the groundwork for the isolation of other active substances including codeine, caffeine, and strychnine (Healy, 1997). While the primary significance of Sertürner's experiment undoubtedly lay in the pharmaceutical processes it made possible, the new drug itself also played a significant role in nineteenth century medicine. Morphine quickly came to be seen as a safer and more effective drug than opium. This was due in part to its relative novelty. As we will see throughout this section, new drugs are often thought to be safer or more effective simply because they are new. Opium was known as a drug to which people could develop addictions and morphine was not. Morphine was also preferred at least partly due to its ability to be injected directly into the bloodstream. Following the invention of the hypodermic syringe, physicians were able to inject drugs like morphine directly into the blood, bypassing the stomach and avoiding the digestive problems sometimes associated with opium and its derivatives. As the nineteenth century wore on, morphine



eventually replaced opium as the drug of choice for medicinal treatment (Medawar & Hardon, 2005).

A second important development in the establishment of the pharmaceutical industry was the isolation of quinine. Healers had long known that the bark of the cinchona tree, called Jesuit's bark, could help treat malaria fever. Following Sertürner's isolation of morphine, the active principle in Jesuit's bark, quinine, was isolated separately by Carl Runge in 1819 and by Pierre-Joseph Pelletier and Joseph-Bienaimé Caventou in 1820. Pelletier and Caventou published their findings and urged doctors to use this new salt for the treatment of malaria. Shortly thereafter an outbreak of malaria in Barcelona motivated Pelletier to establish the first factory dedicated to the production of quinine (Healy, 1997).

### **The Golden Age of Inebriation**

Set against these tremendous scientific advances, the bulk of everyday medical treatment in the early nineteenth century relied on alcohol and opiates, a period that has been called "the golden age of inebriation" (Langer, 1969, p. 14). Alcohol stimulation therapy became a popular medical cure, developed in opposition to the prevailing therapy of bloodletting. In time, alcohol stimulation gave way to different curatives, but alcohol would continue to be "regarded as a serious medicine at least until World War II... and is still the most widely used remedy for mild-to-moderate mental distress" (Medawar & Hardon, 2005, p. 12). The implication of nineteenth century alcohol therapies for the history of psychiatric medicine has less to do with the problems it treated though, than with the ones it caused. By the late nineteenth century alcohol-related symptoms had become one of the foremost reasons people were admitted to asylums. Shorter

(1997) refers to this as “insanity related to alcohol” without any apparent irony: “The drug itself can cause hallucinations. Withdrawal from alcohol may produce psychosis, fits, and delirium tremens. And the overconsumption of alcohol may... [produce] a chronic psychosis and memory loss... Liver disease from alcohol may cause its own psychiatric morbidity” (p. 59).

Opium was in wide therapeutic use by the time that excessive alcohol consumption came to be seen as a problem. Opium had been used recreationally for hundreds of years and was frequently given by doctors to treat mental distress, as was morphine. These could be taken alone or used as the basis of patent medicines. Laudanum was especially popular. As the effects of alcoholism came to be seen as a problem to be addressed by medicine, it was therefore quite natural that doctors would turn to opium and morphine as calming remedies. When morphine eventually supplanted opium as a curative, it became the favored cure for both alcohol and opium addiction (Medawar & Hardon, 2005).

### **Of Addiction, Withdrawal, and Degeneration**

These problems, to say nothing of the numerous sub-psychotic social and behavioral consequences of excessive drinking or opiate use, were understood as personal failings; evidence of bad breeding, constitutional weakness, or underlying psychiatric illness. Drug addiction and withdrawal were poorly understood at the time, and doctors were loath to acknowledge that their own therapies might be contributing to the problems they sought to cure (Medawar & Hardon, 2005). These views owe both to the nascent state of scientific knowledge about addiction and withdrawal at the time, and to the powerful influence of Social Darwinism on the intellectual life of the late nineteenth century.

In the landmark *On the Origin of Species*, Darwin famously proposed natural selection as a mechanism of action to account for transformism, or the evolution of species. Following the demographic theories of Thomas Malthus, Darwin suggested that the natural evolution of organisms in directed ways (as when breeders would select for certain traits) could be accounted for through competition for survival: those organisms possessed of traits that aid in survival are more likely to live and pass along the characteristics in question than are those whose traits do not aid them in the struggle for survival. Although Darwin was careful not to encroach on the realm of philosophy, his theory was taken up by others and applied indiscriminately to numerous other disciplines, sometimes by people who had heard about it only through hearsay. Social Darwinism is probably the most pernicious of these applications, positing that human society is guided by the same process as the natural selection of traits in animal populations. The ideology of social Darwinism was taken by its supporters to suggest both that extant hierarchies and differences are naturally ordained, and that weakness should be culled (Ellenberger, 1970).

One psychiatric offshoot of this ideology is the doctrine of degeneracy. The doctrine of degeneracy held that psychiatric illnesses are inheritable and get worse from generation to generation, causing progressive degeneration of the gene pool over successive generations. This idea is not wholly without scientific foundation. It had long been believed that mental disorders were heritable, and by the middle of the nineteenth century there was emerging clinical and statistical evidence to support the view that certain conditions, like schizophrenia, are based at least partly on heredity. Modern science also holds that there are genetically based conditions

with neurological or psychiatric symptoms that do, in fact, get worse from generation to generation. These are called trinucleotide repeat disorders. These result from mutations that cause the unstable repetition of DNA triplets (trinucleotides) in certain genes. Both Fragile X Syndrome and Huntington's Disease are examples of trinucleotide repeat disorders with psychiatric or neurological presentations. Advocates of the doctrine of psychiatric degeneracy went much farther than this, however, claiming that any measure of deviance, from epilepsy to masturbation, was a threat to the health of the social body, destined to result in full blown psychosis or dementia further along the genetic line (Shorter, 1997). Degeneracy fell out of style among psychologists long before the beginnings of Nazi eugenics,

But the genie was out of the bottle. In the last quarter of the nineteenth century, degeneration passed from an object of discussion within the closed world of academic psychiatry to the boulevard press. In a demonstration that ideas have consequences (and that physicians must be very careful about the correctness of what they believe before they make announcements to the public), the public seized with horror on the concept of degeneration. The educated middle classes began believing implicitly that European society was doomed unless action was taken to stem the poisoning of the well of heredity... the fateful notion of degeneracy was picked up by the eugenicists, by social hygienists intent on combating mental retardation with sterilization, and by antidemocratic political forces with a deep hatred of "degenerate" groups such as homosexuals and Jews... After 1933, degeneration became an official part of Nazi ideology. Hitler's

machinery of death singled out Jews, people with mental retardation, and other supposedly biological degenerates for campaigns of destruction (Shorter, 1997, pp. 98-99).

These associations with Nazi racial hygiene would taint psychiatry for decades, rendering the very suggestion of a possible genetic basis to mental disorders taboo, and fueling anti-psychiatric sentiment to this day.

As for substance dependence and withdrawal, the ideology of social Darwinism helped shape the idea these were individual failings, something indicative of the fitness of the patient's character or constitution rather than something brought about by external factors. "Intoxication," in the parlance of the day, meant a condition that users brought on themselves. The first scientific evidence of withdrawal symptoms only began to emerge toward the end of century from systematic experiments in the asylums (Medawar & Hardon, 2005). Increasing recognition of addiction as a social problem led to the passage of the Harrison Act of 1914. This made cocaine and opiates available by prescription only, but physicians largely refused to cooperate with the new laws (Healy, 2002). Although an estimated one in ten doctors was addicted to cocaine or opiates at the turn of the century (Medawar & Hardon, 2005), there persisted a very powerful perception among physicians that addiction was a matter of personal failure. Underlying this perception were linkages between addiction and crime or social deviancy. "The problem was not seen as being caused by a drug," says Healy (2002). "Addiction was associated with low-life elements, with illegal trafficking in drugs, and with opium dens" (p. 166). It is not difficult to locate the root of these perceptions. Addicts "almost certainly became manipulative and engaged in crime to finance their habits,

and their behavior was then interpreted... as evidence of a personality disorder. Psychopathy was the real problem; addiction was just one more symptom” (Healy, 2002, p. 166). As with degeneration, the idea of substance addiction as a personality fault would persist long after the ideology of social Darwinism had begun to wane. It was not until the 1950s that researchers began to demonstrate evidence of what we now call drug dependence, the idea that specific withdrawal symptoms might encourage users to feed their addictions, keeping them trapped in a cycle of negative drug effects (Healy, 2002).

Although the concept of drug addiction is now taken seriously by most people, the pharmaceutical industry has continued to promote more nuanced versions of the idea that addiction is a failure of personality, a view that has been gaining currency on the level of formal policies for the past thirty years or so. *DSM-IV* modified the definition of dependence to all but define medical dependence out of existence. Under the *DSM-IV* criteria, dependence means withdrawal symptoms along with two or more symptoms of drug abuse, such as drug seeking behaviors. Simply suffering debilitating physical or psychological effects when going off one’s medications no longer qualifies as dependence. The new dependence concept set the bar so high that some international regulators do not even require pharmaceuticals to be tested specifically for evidence of dependence. As Medawar and Hardon (2005) note, “[t]he authorities failed to explain, or even to acknowledge, that this enormous shift in meaning had taken place. The Pharmas zealously promoted the new definition, but the medical establishment welcomed it too” (p. 65). In the year 2010, it is still not universally accepted among policymakers or

regulators that drugs used for healing can cause dependence (Medawar & Hardon, 2005).

### **The Development of Organic Chemistry**

The final factor that made possible the birth of the modern pharmaceutical industry was the development of organic chemistry. In 1828, Frederick Wöhler synthesized urea, an organic molecule excreted in urine. For the first time, a molecule associated with living things had been created artificially. Wöhler's experiment marked the beginning of the end of vitalism, the theory that living organisms are comprised of something vital that cannot be reproduced by chemistry or physics. This was a critical first step toward the development of organic chemistry (Healy, 2002).

Organic chemistry was born some thirty years later, from a practice known as coal tar chemistry. This involves heating coal in the absence of oxygen to produce a variety of chemicals known as coal tars. Coal tar chemistry began in 1856 when William Perkins, trying to synthesize quinine, accidentally produced an organic dye called aniline purple, or Perkin's mauve. Textile manufacturing was a lucrative European market in the wake of the Industrial Revolution, so Perkins developed a textile dye company to take advantage of his discovery. Other organic dye companies quickly developed in the same mold, all employing chemists to aid in the synthesis of new coal tar compounds. This new organic chemistry made possible, for the first time, the creation of entirely new compounds that did not occur in nature. Drawing on recent discoveries into the nature of organic structures and chemical bonding, the organic chemists could systematically manipulate the structure of chemical compounds in a way never before imaginable (Healy, 1997).

Some of the largest pharmaceutical companies in the world began as coal tar dye companies in Germany and Switzerland at this time. J. R. Geigy created a coal tar dye company in 1859, the same year that Alexander Clavel established the company that would become Ciba. Three years later, Jean-Gasper Dolfus set up the dye company that would become Sandoz. By the turn of the century, all three companies had turned from dye production to pharmaceutical manufacturing. Geigy and Ciba merged in 1971, and Ciba-Geigy merged with Sandoz in 1996 to form Novartis, one of the largest pharmaceutical companies in the world today. Dr. Wilhelm Kalle established Kalle & Company in 1863, the same year that Meister, Lucius, and Co. was established. Meister, Lucius, and Co. would eventually become Hoechst AG, which acquired Kalle in 1953. Hoechst AG merged with Rhône-Poulenc in 1999 to form Aventis, which merged with Sanofi-Synthélabo in 2005 to form Sanofi-Aventis, another major player in the modern pharmaceutical industry. One of the few major pharmaceutical firms to survive the past twenty years with its original name is Bayer AG, originally founded in 1862 to produce dyes from coal tar (Aftalion, 2005; Healy, 1997).

The dye industry's transition to the modern pharmaceutical industry began in the 1880s, when Robert Koch and Paul Ehrlich demonstrated that some dyes acted differently on different strains of bacteria. The bacteriological model was just beginning to gain acceptance within the scientific community. Ehrlich showed that methyl green stained the nuclei of cells green but the cytoplasm of the same cells red, while methyl violet stained the bacteria in a tissue sample but not the tissue itself. This suggested that the dyes were producing reactions in the bacteria and not just staining them. Koch used methylene blue to demonstrate that the tuberculosis



bacterium was distinct from other types. This work with dyes led Ehrlich to formulate the idea of the “magic bullet,” a drug that could act selectively on one type of tissue or disease. Just as dyes could be shown to produce reactions in just one type of tissue but not others, Ehrlich envisioned healing medicines that could selectively fight a disease of the kidney while producing no effects in other parts of the body. This was a groundbreaking idea at a time when many people did not believe in disease specificity, and even today Ehrlich’s magic bullet remains the holy grail of pharmacology (Healy, 2002).

### **Medicating Mental Disorders After the Opiates**

In 1832 Frederick Wöhler and Justus Liebig synthesized chloral hydrate, a drug that was discovered to have powerful sedative and hypnotic effects about forty years later. Chloral’s powerful sedative properties made it a natural fit for the needs of asylum doctors. It rapidly replaced the opiates in asylum use. Outside of the asylums, physicians prescribed chloral in place of opium because it was less toxic in overdose and less popular for suicide (Healy, 2002; Medawar & Hardon, 2005).

Potassium bromide first came into psychiatric use in the 1850s as a treatment for epilepsy. Epilepsy has long been thought to have supernatural origins. Hippocrates himself wrote a treatise on epilepsy, which was then known as “the sacred disease.” By the nineteenth century it was still commonly believed that epilepsy was a curse from the devil. The hard-minded scientists of the day knew that this was just superstition, of course – it was quite obvious that epilepsy was caused by sexual deviance, not satanic intervention. Potassium bromide was known to have anti-aphrodisiac properties, and it was on this basis that bromides were first introduced as a treatment for epilepsy. Given current scientific understandings

about the etiology of epilepsy, it would appear to be a happy coincidence that bromides actually functioned to reduce the symptoms of epilepsy. Bromides worked so well, in fact, that they eventually became a popular treatment for any and all mental or behavioral disorders (Medawar & Hardon, 2005).

Bromides build up in the body and eventually cause poisoning, but these effects were poorly recognized because the symptoms of bromide intoxication (bromism) mimic the symptoms of psychosis, including disorientation, paranoia, and hallucinations. Bromide users found themselves trapped in a vicious cycle, taking increasingly toxic levels of potassium bromide to cure the effects of previous bromide use. The first test to identify levels of bromide toxicity in the body was developed in 1927, but these were rarely used because doctors did not believe that their own cures could be responsible for producing their patients' symptoms. Physicians continued to poison patients by treating the symptoms of bromism with more bromides well into the latter half of the twentieth century. By mid-century, more than a century after bromide toxicity was first recognized and two full decades after the first test for bromide poisoning appeared, an estimated 10% of all admissions to UK mental health institutions owed to poisoning from an overdose of therapeutic bromides (Medawar & Hardon, 2005).

### **Freud, Cocaine, and Heroin**

Cocaine was first isolated in 1855, and was quickly taken up as a component of tonics and patent medicines including, most famously, Coca-Cola. Like opium and morphine before it, this new drug was initially embraced by doctors as a safer, more effective alternative to existing medications. Cocaine's popularity as a curative was aided in particular by the boundless enthusiasm of Sigmund Freud, an avid cocaine

user who hoped to capitalize on his discovery that cocaine could be used to treat morphine addiction (Medawar & Hardon, 2004). Ernest Jones, a member of Freud's inner circle and his official biographer, notes that Freud not only gave cocaine to his patients, but also "pressed it on his friends and colleagues, both for themselves and their patients; he gave it to his sisters. In short, looked at from the vantage point of our present knowledge, he was rapidly becoming a public menace" (Jones, 1953, p. 81). In 1884, Freud published *On Coca*, a self-described "song of praise" extolling the virtues of the drug. According to Jones (1953):

There is... in this essay a tone that never recurred in Freud's writings, a remarkable combination of objectivity with a personal warmth, as if he were in love with the content itself. He used expressions uncommon in a scientific paper such as 'the most gorgeous excitement' that animals display after an injection of cocaine, and administering an 'offering' of it rather than a 'dose'; he heatedly rebuffed the 'slander' that had been published about this precious drug. This artistic presentation must have contributed much to the interest the essay aroused in Viennese and other medical circles (p. 82).

Freud advocated cocaine as a cure for morphine addiction, indigestion, and neurasthenia, the reason he used it himself. Prevailing theories of addiction held that only depressants could produce addictive reactions, like alcoholic delirium tremens or opiate withdrawal, so it was logical for advocates to initially believe that cocaine was non-addictive. But much as proponents of morphine had claimed that morphine could only be addictive to those with addiction-prone constitutions or a previous addiction to alcohol or opium, proponents of cocaine explained away early evidence of cocaine addiction as something that could only afflict those with a

constitutional weakness. Freud himself steadfastly denied that cocaine could cause addiction. Only years after the addictive properties of cocaine were generally accepted did he finally relent, conceding that cocaine addiction might be possible – but only among those who were previously addicted to morphine (Medawar & Hardon, 2005).

Chemists at the German pharmaceutical company Bayer synthesized diamorphine in 1898. This was a more potent, refined form of morphine. Marketed under the brand name Heroin, diamorphine was initially sold as a non-addictive cough suppressant for infants. Like all the drugs before it, Heroin was enthusiastically embraced by doctors as a non-addictive alternative to older drugs and used liberally to treat symptoms of the various addictions that preceded it. Heroin was used as a panacea for both mental and somatic illnesses until it was banned by congress in 1924 (Medawar & Hardon, 2005).

### **Lithium**

Lithium was first introduced into medicine in the nineteenth century in order to break down urate stones. Physicians of the time had little way to measure the internal functioning of the body except by studying bodily secretions. Accordingly, when scientists discovered a technique to measure the discharge of uric acid in urine, a wide variety of theories began to appear to explain the observed correlations between uric acid and medical ailments. The prevailing theory held that an unnatural buildup of uric acid in the body or the brain could cause disorders, ranging from gout to heart problems and mental illness. It was even hypothesized that some manic disorders might be a form of gout, different clusters of symptoms sharing a common pathology. Uric acid diathesis became a driving philosophy in

nineteenth century medicine. When it was discovered that lithium could break down urate stones, all sorts of tonics, concoctions, and medicinal treatments were developed to make use of lithium's healing potential. 7UP famously began as a one of these lithium drinks. By the 1880s, the widespread use of lithium led psychiatrists to observe that it could treat the symptoms of some mood disorders. Lithium was used as a successful treatment for mood disorders for several decades, but it fell out of use when the theory of uric acid diathesis became scientifically untenable (Healy, 2002).

Lithium was rediscovered in the middle of the twentieth century, owing primarily to the efforts of two researchers. In 1949 John Cade, the superintendent of a mental hospital in Bundoora, Australia, began injecting urine from manic patients into guinea pigs in an effort to see whether mania was caused by toxic products in the body. When the entire group of guinea pigs died, he decided to put the experiment on hold and read up on the components of urine. Cade discovered that he would need to make the uric acid soluble before he could inject it into guinea pigs without killing them. To do this, he would need to mix it with lithium. Leery of accidentally killing another batch of guinea pigs, Cade decided to inject a few with lithium alone and monitor the results. To his great surprise they not only survived the lithium injections, but exhibited marked behavior changes as well. Guinea pigs are normally frenetic and excitable. Under the effects of lithium they became extremely placid and lethargic. Eventually, Cade tested lithium in his manic patients. The results were remarkable. He managed to fully cure patients who had been manic for decades. Cade published his results, but the paper generated little attention. As he later said, the work of "an unknown psychiatrist, working alone in a

small chronic hospital with no research training, primitive techniques, and negligible equipment was hardly likely to be compellingly persuasive, especially in the United States” (Shorter, 1997).

Lithium only came to the attention of the wider psychiatric community a few years later when a Danish researcher named Morgens Shou decided to conduct a placebo-controlled, double-blinded study to test Cade’s findings. Shou had been searching for a research project when a friend pointed him to Cade’s article. After replicating Cade’s results, Shou went on to promote lithium as a wonder drug. Shou’s family had a history of manic-depressive illness. He prescribed lithium for several family members and took it himself. Lithium was slow to be adopted, however, owing largely to the fact that it is an abundant natural substance that was competing with the treatments invented by pharmaceutical companies. It took two decades for the FDA to approve lithium, and then only after a psychiatrist threatened to prescribe it without approval as an act of civil disobedience (Shorter, 1997). Despite a history of success as a clinical treatment for symptoms of mania, Healy (2002) suggests that modern lithium use will probably not long outlive the last of its major advocates:

There was then and still is now no theoretical basis for lithium’s use, no rationale that could then or can now be used to sell it. As a result, the use of lithium will almost certainly end when Shou dies. Another agent, probably of lesser efficacy, will displace it by virtue of a marketing strategy that depends on offering a “biological rationale.” Commercial support is often attracted to the artistic verisimilitude of a theoretical rationale, particularly when this is accompanied by patent possibilities (p. 49).

The lithium story speaks to Kuhn's observation that the validity of scientific evidence is often determined by its ability to meet the right explanatory needs. Despite clinical evidence suggesting that lithium is an effective mood stabilizer, a treatment that cannot be explained is unlikely to be embraced widely by the scientific community. The story of lithium also sheds light on the resilience of the serotonin hypothesis of depression. Successful treatments require theoretical explanations, and even a deeply flawed theory is likely to be accepted when no alternative explanation can be found (Healy, 2002).

### **Barbiturates and Benzodiazepines**

The first barbiturates were developed shortly after the turn of the twentieth century, but they only came into widespread therapeutic use alongside the decline of the bromides several decades later. The barbiturates are an extremely homogenous group of tranquilizers that have sedative properties at low doses and induce sleep at higher doses. These were primarily used as a replacement for potassium bromide, once the problems of bromide addiction became known in the 1920s and 1930s. Like bromides, barbiturates produce withdrawal symptoms similar to the very conditions they are often prescribed to counteract, but barbiturates produce few symptoms of addiction. Chronic barbiturate users develop intractable insomnia when withdrawn from their drugs, but they do not develop the same sorts of addiction symptoms that habitual opium or cocaine users do. As a result, the problem of barbiturate withdrawal took decades to come to light. Even when doctors understood that a patient was addicted to barbiturates, they would often advise the patient just to keep taking their drugs to avoid withdrawal. Well into the 1980s, regulatory bodies such as the Institute of Medicine continued to

dismiss barbiturate dependence as “nightly reliance on drugs to sleep,” not an addiction. To this day barbiturates are prescribed almost exclusively for “severe intractable insomnia,” a polite euphemism for severe addiction (Medawar & Hardon, 2005).

The first benzodiazepine, chlordiazepoxide (Librium), was developed by accident at Hoffman La Roche. A research chemist created the compound and got distracted, so the substance sat on his desk for several years. While cleaning up his desk a few years later, he came across the aging compound and decided to send it out for testing as a precaution before throwing it away. These tests revealed that it had extraordinary sedative properties – the first promotional video for Librium showed a tranquilized lion lying next to a lamb. Benzodiazepines are named for their structure, which combines a benzene ring and a diazepine ring. Hoffman La Roche developed the second benzodiazepine as well, which it called diazepam (Valium). The company promoted these as tranquilizers, playing up the difference between these new drugs and the older sedatives. In reality, they all did basically the same thing. They calmed people, reducing anxiety and tension, and in higher doses they put people to sleep. The third benzodiazepine, nitrazepam (Mogadon), was launched as a sleeping pill in 1965. Like bromides, barbiturates, and all the narcotics before them, the tranquilizers were used as a generic treatment for nervous symptoms and mood disorders. Originally marketed as anxiolytics, the benzodiazepines mostly became antidepressants or treatments for nerves when diagnoses of anxiety went out of fashion (Medawar & Hardon, 2005).

As with most of the drugs used to treat psychiatric conditions, recognition that tranquilizers could cause withdrawal and dependence came slowly. It was



estimated that 10-20% of ambulatory adults in the United States regularly took benzodiazepines by 1975. Despite scattered reports of tranquilizer withdrawal, the first conclusive evidence of withdrawal problems came to light with Wyeth's lorazepam (Ativan). The benzodiazepines are primarily distinguished from each other based on the half-life of the drug, or the amount of time it takes for the drug to clear out of the body. Ativan had a short half-life, which meant that withdrawal symptoms from the drug began earlier and were generally more severe than those caused by comparable drugs with longer half-lives. A short half-life also makes it easier for physicians and patients to connect withdrawal symptoms to discontinuation. In addition to its short half-life, Ativan was promoted at extremely high doses. For unknown reasons, the suggested dose was doubled in some countries; the dose that was recommended for Americans with severe anxiety was the same as the dose recommended for Brits with mild anxiety. By 1979, shortly before the first of the SSRIs was about to come to market, the first strong evidence began to emerge that Ativan withdrawal could induce seizures, among other unpleasant consequences. Upjohn marketed alprazolam (Xanax) and triazolam (Halcion) in the early 1980s amidst a growing realization that benzodiazepines could cause dependence. Xanax was received well, but Halcion was found to produce profound confusion and amnesia, and by 1991 Halcion-related cognitive impairment had been successfully invoked as a defense in murder trials. The Upjohn Company settled several large liability lawsuits.

The negative publicity surrounding Halcion was the final straw in America's growing disenchantment with the benzodiazepines, but it was far from the only reason that doctors stopped prescribing tranquilizers. Throughout the 1980s there

was increasing evidence that the benzodiazepines, originally touted as being completely safe and non-addictive, were just as habit-forming as all the drugs to come before. At the same time, the new SSRIs offered new patent opportunities and, unlike the benzodiazepines the barbiturates, or any of the drugs before them, it appeared as though the SSRIs might be completely safe and non-addicting, the panacea that psychiatrists had been seeking for all these years (Medawar & Hardon, 2005).

### **Pharmaceutical Regulation**

The first formal pharmaceutical regulation appeared shortly after the turn of the twentieth century. A 1901 outbreak of tetanus in St. Louis and Camden, New Jersey appeared to be connected to contaminated samples of smallpox vaccine. This led to public outcry and a congressional debate that resulted in the 1902 passage of a congressional act aimed at regulating pharmaceutical manufacturing. Shortly thereafter Upton Sinclair's novel *The Jungle* raised a great deal of public concern about the safety of food production, and a series of *Collier's* magazine articles written by Samuel Hopkins Adams exposed glaring problems with the nascent drug industry. Because the ingredients in patent compounds were unlabeled, consumers had no way of knowing when the compounds they purchased were safe or that they even contained active drugs at all. Adams informed readers that Radam's Microbe Killer, for instance, was made of 99% water, while other compounds contained toxic levels of cocaine or acetanilid. Following these developments, congress passed the 1906 Pure Food and Drug Act, which allowed the government to inspect meat and regulate the labeling of pharmaceuticals compounds. Under the terms of the new law, the main ingredient of a food or drug had to be truthfully labeled. The Bureau of

Chemistry, which would later become the Food and Drug Administration (FDA), had very minimal power during these years; it was mostly just allowed to seize medicine that had been fraudulently labeled (Healy, 1997).

The 1938 Food, Drug, and Cosmetics act significantly expanded the regulatory power of the FDA. In that year, over a hundred people died after taking an elixir made with sulfanilamide. The FDA traced the problem to a manufacturer in Tennessee who had dissolved the sulfanilamide in diethylene glycol rather than alcohol, but the agency was unable to intervene to stop the manufacturer from selling a product that was killing people. The FDA had little regulatory authority outside of inspecting labels, so it was forced to intervene on a technicality: elixirs were required to contain alcohol, and the manufacturer had used diethylene glycol instead of ethanol. The FDA seized the lethal medication, but only on the grounds that it was mislabeled. Following the sulfanilamide incident, congress passed the 1938 act, giving the agency increased regulatory authority. This law also banned all false or misleading statements in product labels, not just intentionally fraudulent ones, and it prohibited manufacturers from marketing any compound that had not been proven safe (Healy, 1997)

The 1951 Humphrey-Durham amendment to the 1938 Food, Drug, and Cosmetics act expanded the power of the FDA even more, giving it the authority to designate prescription-only drugs. The 1914 Harrison Narcotics Tax Act made narcotics available by prescription only, but before 1951 non-narcotic medicines had been available for anyone to purchase freely. Two factors came together during this time to raise awareness about the dangers of free drug markets: the discovery

of antibiotic resistant bacteria and the growing realization that medicinal drugs could cause addiction and withdrawal (Healy, 1997)

The most pivotal moment in the history of American pharmaceutical regulation was the 1962 passage of the Kefauver-Harris amendment to the 1938 Food and Drug Act. By the late 1950s, there were growing concerns about the lack of pharmaceutical regulation in America. It appeared as though the ethical companies were more invested in marketing their drugs than developing new ones. The industry was playing a greater role in funding research and sponsoring academic publications, leading physicians to complain that they had nowhere to get objective information about the drugs they were supposed to be prescribing. There were concerns about the regulatory acumen of the FDA and whether the organization was helping drug companies or the public. This led to a series of congressional hearings presided over by Senator Estes Kefauver in 1959 and 1960. Then, in 1961, the drug thalidomide was linked to birth defects in European and Australian children. Roughly six thousand children are estimated to have been born with deformities related to thalidomide, which raised tremendous public anxiety about the ways that drugs are tested and screened. The 1962 Kefauver-Harris amendment was born in response to that concern. It required that drug manufacturers prove both the safety and efficacy of their drugs through systematic scientific testing. It also expanded the FDA's authority to include the regulation of over-the-counter medicines in addition to prescription medicines. One immediate consequence of the act was to rid drugstore shelves of large numbers of ineffective over-the-counter psychiatric medicines (Healy, 1997).

The 1962 amendment also helped secure the prescription drug model that still defines both traditional health care and psychiatric care in this country. Before the thalidomide disaster there had been considerable public unease about the new model whereby doctors were neither paying for nor using the drugs that they alone had the authority to prescribe. This created a situation where consumers had no way to protect themselves from monopolization and pharmaceutical companies could artificially distort demand by influencing a handful of gatekeepers who were themselves neither consumers nor payers. This issue still lies at the center of modern healthcare debates. Despite the efforts of pharmaceutical manufacturers, the political climate in the late 1950s was such that congress seemed ready to reverse the 1951 amendment and abolish the professional monopoly on prescription drugs. After the thalidomide scandal, however, there were few people calling for easier access to drugs or a minimized professional role in making health decisions (Healy, 1997).

Another lasting effect of the 1962 amendment was to define randomized, placebo-controlled, double-blinded studies as the gold standard for scientifically proving efficacy and safety in the United States. One consequence of this has been to significantly complicate the matter of proving safety and effectiveness, or lack thereof, using any other form of scientific evidence. This problem is particularly acute in situations where problems are known to develop over a period of many years, as with cancer. Tobacco companies in cancer liability litigation frequently claim that epidemiological studies are insufficient to scientifically establish causation. Nothing less than randomized, placebo-controlled, double-blinded studied can genuinely establish a scientifically sound causal link between tobacco

and cancer. That is, after all, the standard for establishing drug safety, enshrined by congress in the 1962 amendment. A second, related consequence of this has been to make research prohibitively expensive, which has resulted in generally increased industry control over the direction of scientific research.

These matters lie at the heart of some of the most pressing medical and legal questions about depression and antidepressants today. One of these is the question of SSRI dependence. The most indisputable evidence that SSRIs cause dependence is the existence of withdrawal effects among newborns exposed to SSRIs in the womb. None of the traditional arguments used by drug companies to blame the patient are applicable in the case of newborns. Newborns who suffer from withdrawal symptoms are clearly not experiencing the relapse of an underlying psychiatric condition. Their experiences cannot be blamed on psychological dependence, nor are their symptoms evidence of a dependence prone personality. And yet this evidence of chemical dependence and withdrawal resulting from SSRI exposure does not meet the accepted scientific standards for establishing causation, which requires nothing less than randomized, placebo-controlled, double-blinded trials (Medawar & Hardon, 2004). Pharmaceutical companies employ this same tactic as a legal defense in SSRI wrongful death litigation. The standard industry line is that even when a particular drug is known to cause suicidal ideation or violent behavior, and even when a patient's history points toward the drug as the causal factor in his or her behavior, it is impossible to ever establish causation on a case by case basis. Scientific causality can only be established across a large sample population through randomized, placebo-controlled, double-blinded trials (Healy, 2004).

Two more regulatory developments warrant inclusion here. The first of these is the 1984 Drug Price Competition and Patent Term Restoration Act, commonly referred to as the Hatch-Waxman Act. One of the forces working against the safety procedures in the 1962 amendment was the structural incentive for pharmaceutical companies to get drugs to market as quickly as possible in order to get the most from each drug's patent life. The Hatch-Waxman Act adjusted the patent protection for drugs to account for time they spend in testing and regulatory approval. The goal of this change was to motivate drug companies to fully test their products and follow regulatory protocol. Because the drug testing process is incredibly expensive, Hatch-Waxman also allowed drug companies to establish safety and efficacy simply by proving bioequivalence with a medication that has already been approved. This is widely recognized as the regulation that essentially created the generic drug industry.

The final regulatory development of note here is the 1997 FDA ruling to allow pharmaceutical companies to market their products directly to consumers. Although direct-to-consumer advertising had technically been legal since the 1980s, it previously required unwieldy full risk statements to accompany all advertisements directed at end users. The 1997 ruling allowed pharmaceutical companies to substitute an abbreviated warning for a full statement of risk, making direct-to-consumer advertising feasible within the constraints of a television commercial or magazine ad (Angell, 2005; Medawar and Hardon, 2005).

The regulatory authority of the FDA has increased significantly over the past hundred years, but the agency still has only minimal formal power to promote drug safety. As a 2006 Institute of Medicine report concludes, "FDA lacks the clear,

unambiguous authority needed to enforce sponsor compliance with regulatory requirements and instead relies on the prospect of productive negotiations with industry.” FDA safety decisions usually take the form of a request rather than a pronouncement. In essence, the agency relies upon the threat of private litigation to enforce compliance with its postmarket safety decisions; pharmaceutical manufacturers comply not because the FDA forces them to, but because non-compliance more or less ensures that courts will hold them liable for any harm resulting from the continued sale of the drugs in question (Healy, 2004).

### **Chlorpromazine: The First Drug that Worked**

The final part of our story is the discovery of chlorpromazine. Henri Laborit was a surgeon in the French navy when, in 1949, he started researching whether antihistamines could be used to increase the effectiveness of anesthetics. Using compounds synthesized by Rhône-Poulenc, he managed to produce remarkable calm in some of his more excitable patients. After transferring to a military hospital in 1951, Laborit began working with a new phenothiazine compound called chlorpromazine. In addition to helping anesthetize patients, chlorpromazine produced “disinterest” among them. Laborit convinced some of his psychiatric colleagues to try the drug on their patients. One of these used chlorpromazine in combination with an analgesic, a barbiturate, and electroconvulsive therapy to cure a young man who had been hypo-manic only a few weeks before. Word of the success spread around the Parisian psychiatric community. In 1952, Jean Delay and Pierre Deniker treated a series of patients with chlorpromazine alone, effectively eliminating the psychiatric symptoms of each one. These were unheard of successes. Later that year, Rhône-Poulenc licensed chlorpromazine to Smith Kline & French,



then a small American firm, who sold it under the brand name Thorazine. Within a few years, chlorpromazine could be found in psychiatric wards around the world (Shorter, 1997).

Chlorpromazine revolutionized psychiatry. For the first time, psychiatrists had access to a drug that worked reliably to eliminate the symptoms of psychiatric illness. Most texts on the history of psychiatry or psychopharmacology refer to it as “the first drug that worked.” Shorter (1997) writes that the impact of chlorpromazine on psychiatry is “comparable to the introduction of penicillin in general medicine” (p.255). Healy (2002) calls the discovery of chlorpromazine “one of the seminal events of human history,” writing that it “completely eliminated the original form of insanity – delirium – while at the same time bringing into the psychiatric ambit all the neuroses and personality disorders that lie at the core of psychiatric practice today” (p. 4).

This story ends with the discovery of chlorpromazine because chlorpromazine ushered in the age of modern psychopharmacology. There is a very real sense in which the story of psychopharmacology is the story of chlorpromazine. But this is not a story about the steady march of scientific progress, when psychiatry emerged from the shadows of its tawdry, narcotic-laden past and stepped into the warm glow of enlightened modern medicine. The discovery of chlorpromazine was momentous, but it was not the end of psychopharmacology. As we have seen over the course of the last two chapters, the medication of medical disorders remains as confused today as it ever was.

## Chapter 6

### THE DIAGNOSTIC AND STATISTICAL MANUAL AND THE TRANSFORMATION OF AMERICAN PSYCHIATRY

Our genealogy has progressed from the abstract to the concrete, teasing out the strands of the extraordinarily complex relationships that exist between ideologies and institutions. In the final chapter of our genealogy we will see how the development of the *Diagnostic and Statistical Manual* has shaped the evolution of psychiatry and the concrete institutional relationships that exist between modern biological psychiatry and the pharmaceutical industry. Chapter six demonstrates most clearly the wisdom of Foucault's decision to use psychiatry as a departure point for investigating power/knowledge. This chapter will examine some of the manifold connections between psychiatry and other social institutions through the structures of power and knowledge, and it will begin to unpack the ways that a simple psychiatric label can facilitate the spread of power by rendering human beings as objects of knowledge.

#### ***DSM in the Psychoanalytic Period***

The *Diagnostic and Statistical Manual* was first published in 1952, at a time when psychoanalytic thinking dominated American psychiatry. Fueled in part by the emigration of prominent European analysts during the Second World War, psychoanalysis became quickly ingrained in the institutional structures of American psychiatry. According to Shorter (1997), during the post-war period "psychoanalysts and analysis-sympathizers took over much of the apparatus of the American Psychiatric Association... analysts wrote the textbooks, staffed the university departments, and sat on the examination boards" (pp. 172-173). The

prevailing psychoanalytic sentiment was reflected in *DSM-I*, resulting in a manual that reads more like a primer on psychiatric theory than a diagnostic guide. *DSM-I* contains detailed descriptions of the psychogenic mechanisms theorized to cause different disorders, but few operational criteria or guidelines to help practitioners decide on a diagnosis. "Psychophysiologic autonomic and visceral disorders" (in common parlance, psychosomatic disorders), for instance, are described as

reactions represent[ing] the visceral expression of affect which may be thereby largely prevented from being conscious. The symptoms are due to a chronic and exaggerated state of the normal physiological expression of emotion, with the feeling, or subjective part, repressed. Such long continued visceral states may eventually lead to structural changes (p. 29).

Likewise, the personality disorders in *DSM-I* are schematized according to psychoanalytic theories of personality development:

The personality disorders are divided into three main groups... Although the groupings are largely descriptive, the division has been made partially on the basis of the dynamics of personality development... Personality trait disturbances and Sociopathic personality disturbances under stress may at times regress to a lower level of personality organization and function without development of psychosis. Personality pattern disturbances... are more or less cardinal personality types, which can rarely if ever be altered in their inherent structures by any form of therapy. Their functioning may be improved by prolonged therapy, but basic change is seldom accomplished... The depth of the psychopathology here allows these individuals little room

to maneuver under conditions of stress, except into actual psychosis (pp. 35-36).

There is an additional section for “transient situational personality disorders,” “reactions which are more or less transient in character and which appear to be an acute symptom response... the immediate means used by the individual in his struggle to adjust to an overwhelming situation” (p. 40).

Psychoanalytic thinking continued to grow in influence in the decade following *DSM-I*. When *DSM-II* was published in 1968, over two-thirds of American psychiatrists practiced some form of analysis; “the influence of psychoanalysis reached into most of the private practices in the country. The biological psychiatrists, in contrast, had been limited to unglamorous posts in state hospitals. Analysts were being consulted by government agencies and the Congress” (Shorter, 1997, p. 180). Psychoanalysis had come to dominate psychiatric thinking and practice in America so completely that “psychiatry as a discipline became almost indistinguishable from psychoanalysis” (Paris, 2005, p. 3). These developments are manifest in *DSM-II*. The numerous “reactions” of *DSM-I* were upgraded to full neuroses in *DSM-II*. “Depressive neurosis,” for example, is defined as:

[A]n excessive reaction of depression due to an internal conflict or identifiable event such as the loss of a love object or cherished possession. It is to be distinguished from *Involuntary melancholia* (q.v.) and *Manic-depressive illness* (q.v.). *Reactive depressions* and *Depressive reactions* are to be classified here (p. 40).

Similarly, “conversion reaction” and “dissociative reaction” were replaced by

“hysterical neurosis,”

an involuntary psychogenic loss or disorder of function. Symptoms characteristically begin and end suddenly in emotionally charged situations and are symbolic of the underlying conflicts. Often they can be modified by suggestion alone (p. 39).

“Psychophysiological nervous system reaction,” one of the “psychophysiological autonomic and visceral disorders” from *DSM-I* described above, was renamed “neurasthenic neurosis.” Like its predecessor, *DSM-II* reflects a preference for definitional flexibility over categorical reliability and explication over operationalization. This trend would be reversed abruptly in *DSM-III*.

### **The Biological Revolution**

The head of the *DSM-III* task force was Robert Spitzer, a biologically oriented psychiatrist who sought to define disorders as precisely as possible, “cutting nature at the joints” to facilitate the identification of discreet biological disease entities. Spitzer put together a task force comprised overwhelmingly of individuals inclined toward the biological, rather than psychosocial, model of disorder, including psychiatrists with backgrounds in neurology and pharmacology as well as specialists in organic brain disorders (Wilson, 1993). He later acknowledge that the task force, “[w]ith its intellectual roots in St. Louis instead of Vienna, and with its intellectual inspiration derived from Kraepelin, not Freud... was viewed from the outset as unsympathetic to the interests of those whose theory and practice derived from the psychoanalytic tradition” (Bayer & Spitzer, 1985, p. 188). The architects of the *DSM-III* intended for it to situate psychiatry securely in the realm of medical science, with an acute focus on diagnostic utility, validity, and reliability.

The publication of *DSM-III* in 1980 marked a sea change in psychiatry. This was truly nothing less than a revolution (Paris, 2005; Mayes & Horwitz, 2005). As Shorter (1997) explains:

American psychiatry returned to the world of medicine, applying the medical model in diagnosis and downplaying the vague “biopsychosocial” model under which so much mischief had occurred. In the judgment of one historian of psychiatry, *DSM-III* became “the centerpiece of the knowledge base for the profession” ... The appearance of *DSM-III* was... an event of capital importance not just for America, but for world psychiatry, a turning of the page on psychodynamics, a redirection of the discipline toward a scientific course, a reembrace of the positivistic principles of the nineteenth century, a denial of the antipsychiatric doctrine of the myth of psychiatric illness (p. 302).

The broad descriptions and explanations of psychopathology that characterized the first two editions of the *DSM* had been replaced by symptomatic descriptions and lists of etiologically-neutral inclusion criteria. Far from the psychogenesis imputed by the analysts, *DSM-III* declared that:

[E]ach of the mental disorders is conceptualized as a clinically significant behavioral or psychological syndrome or pattern that occurs in an individual... there is an inference that there is a behavioral, psychological, or biological dysfunction, and that the disturbance is not only in the relationship between the individual and the society (p. 6).

Psychoanalysts protested that decades of clinical practice and case studies pointed strongly to the psychogenesis of mental disorders, noting that “unproven etiologic

concepts” really meant “not identified in the anatomy of the brain.” Although the advocates of *DSM-III* would frame it as a triumph of science, this was more accurately a conflict between two different views of what science is. Spitzer had pushed to define mental disorders “as a subset of medical disorders.” This wording was staunchly opposed by psychologists, social workers, and others without medical training who viewed it as an attempt to annex mental health as the exclusive province of medically trained psychiatrists. The task force eventually decided on the wording above instead (Mayes & Horwitz, 2005).

Neurosis was slated for completed removal in *DSM-III*, owing to the imprecision of the concept and the lack of operational criteria for identifying neuroses, but this decision sparked tremendous backlash from the analysts. The psychoanalytic community was unified in opposition to the removal of neuroses, and even threatened to secede from the APA (Paris, 2005). The task force relented and added neuroses in parentheses after the word *disorder*. According to Shorter (1997), “[t]he drafters somewhat lamely explained in the introduction that they meant neurosis ‘only descriptively,’ without implying a ‘neurotic process’” (p. 304).

At a time when diagnoses of depression were beginning to increase, the *DSM-IV* task force wrote neurotic depression out of the manual entirely, defining all depressions as bipolar, major, or chronic. Following protests from the psychoanalysts, Spitzer sought compromise in the long-dead concept of dysthymia. When that was rejected, he suggested bringing back the concept of neuroses in parentheses with “Dysthymia (neurotic depression)” as a compromise. The entry that finally received APA approval was “Dysthymia (or Neurotic Depression).”

“[B]oth the ‘or’ and the capitalized N and D of neurotic depression were felt to be critical to the passage of the entire classification” (Healy, 1997, p. 236).

Whatever the merits of the neurosis concept, these decisions compromised both views of the science involved in the name of political necessity. Nor was this the only political decision that shaped *DSM-III*. The impetus to update the *Diagnostic and Statistical Manual* had come largely from gay rights groups who wanted homosexuality removed as a disorder. A subcommittee debated about changing the diagnosis of homosexuality to “‘homodysphilia, dyshomophilia, homosexual conflict disorder, amorous relationship disorder... and finally, ego-dystonic homosexuality.’” Unable to agree, the subcommittee kicked it upstairs to the task force members, who decided to delete it entirely” (Shorter, 1997, p. 304). Like that, what had been considered a severe psychiatric disorder for centuries was now considered a normal part of human diversity. “Sexual orientation disorder” remained in the manual until the next major update.

Following the success of the gay rights groups, other interest groups began lobbying the APA. Vietnam veterans groups called for the formal recognition of lasting psychological trauma from war, long known as “shell shock.” Veterans groups and psychiatrists who specialized in treating veterans began working with the *DSM-III* subcommittee on reactive disorders, eventually coming up with a new clinical entity called “post-traumatic stress disorder.” When *DSM-III-R* declared that “late luteal phase dysphoric disorder,” “self-defeating personality disorder” (a condition affecting primarily women), and “premenstrual dysphoric disorder” were conditions needing further study, feminist groups successfully lobbied for their removal. Whether one agrees with the ends or not, these were clearly not decisions



made on the basis of science. That these conditions could be pathologized or depathologized at the behest of the majority, or even small interest groups with really good lobbyists, makes it difficult to take seriously the new biological psychiatry's claims of scientific objectivity (Shorter, 1997).

But if psychoanalysts had the political capacity to derail the *DSM-III* project, why didn't they attempt to change the manual before it had been completely redesigned in the image of biological psychiatry? Mayes and Horowitz (2005) suggest that the initial viability of the *DSM-III* owed primarily to the disinterest of psychoanalytic shareholders:

*DSM-III's* preliminary drafts triggered relatively modest interest among rank-and-file psychiatrists... Because most psychiatrists and other mental health practitioners rarely used the *DSM-II* as a diagnostic device at the time, a radical change to the manual struck few practitioners as threatening or revolutionary. The notion that the *DSM-III* would become *the* textbook for all mental health clinicians, as opposed to the "little manual" (less than 140 pages) its two predecessors had been since 1952, was slow in developing (p. 261).

By the time the significance of *DSM-III* was widely recognized, the analysts' ability to affect the process had greatly diminished. Healy (1997) attributes this primarily to bureaucratic inertia. "After four years of work and a vast expenditure on field trials," he says, "the document was not going to be thrown out lightly" (p. 236). There were other factors pushing the APA to embrace the diagnostic clarity of the biological psychiatrists over fidelity to the psychoanalytic vision as well, not the least of which was the progressive shrinking of psychiatric research funding throughout the 1960s

and 1970s, often attributed to problems stemming from the lack of clarity in the *Diagnostic and Statistical Manual* (Healy, 1997).

*DSM-III* was founded in opposition to the reigning psychiatric ideology of the time, but it was embraced, if not actively supported, by a number of influential institutional bodies including the FDA and other government agencies, medical research institutions, insurers, and pharmaceutical companies. The new criteria gave pharmaceutical companies clear research goals and definite standards to use in drug evaluation. Regulators welcomed clearer standards that could be used to determine the prevalence of disorders and the effectiveness of different therapies. Representatives from Blue Cross/Blue Shield and Aetna “virtually begged Spitzer and his task force to standardize the manual’s diagnostic criteria” so they could more easily distinguish between “legitimate mental illness” and “floundering marriages, trouble raising children, [or] the difficulties finding the meaning in life” (Mayes & Horowitz, 2005, p. 262).

It was precisely because the new diagnostic standards became embedded in the structures of so many interrelated social institutions that *DSM-III* reshaped the basic ideology and practices of psychiatry as it did. It provided insurance companies, for instance, with a cogent guide to categorizing mental disorders, which meant that mental and behavioral health specialists of all varieties had to use the classification scheme in *DSM-III* in order to claim reimbursement. Behavioral/developmental pediatrician Lawrence Diller (2006) writes of his experience with *DSM-III*:

This was the reigning ideology... when I was in medical school and residency between 1972 and 1978. Treatment for children’s problems, when treatment was undertaken, was play therapy. But a revolution was taking

place within psychiatry. That news broke through to me and attracted the public's attention with the publication of the third edition of the *Diagnostic and Statistical Manual (DSM-III)* in 1980... I could ignore the *DSM-II* because the categories were so vague. Quickly, however, the *DSM-III* became the defining tool of my profession – precisely because it was well adapted for insurance companies and billing procedures. Suddenly, I couldn't ignore the *DSM-III* (pp. 8-9).

Thus the discipline of psychiatry was set on new biological footing almost overnight.

### **The Osheroff Settlement and the End of Therapeutic Psychoanalysis**

The publication of *DSM-III* was the catalyst that sparked the professional decline of psychoanalysis, but the true end of therapeutic analysis can be dated precisely to the 1987 out-of-court settlement in the case of *Osheroff v. Chestnut Lodge*. Institutional policies, such as the revision of insurance codes or the adoption of new criteria for measuring depression, tend to effect change gradually. They create ripples that spread slowly through the social field. As we have seen throughout this history, ingrained ways of thinking can be staunchly resistant to change even when those involved have little meaningful investment in the status quo, and *DSM-III* was nothing less than a challenge to the personal and professional identity of most practicing psychiatrists. The prospect of malpractice liability, on the other hand, tends to produce change both immediate and expansive. This is precisely what happened with the *Osheroff* case.

In 1979, a forty-two year old doctor named Rafael Osheroff was admitted to Chestnut Lodge, an analytically oriented psychiatric hospital near Washington, D.C. Osheroff had long struggled with depression and he had been taking a tricyclic

antidepressant for several years. Although his doctor felt that he was getting better, Osheroff disagreed. He discontinued his medication and eventually found himself unable to function, at which time he checked into the Chestnut Lodge. Osheroff requested to be put on medication, but the hospital staff refused. They wanted him to regress to the stage of childhood when the trauma responsible for his depressive state had occurred. The doctors at Chestnut Lodge believed that drug treatments would only alleviate his symptoms while inhibiting his ability to deal with the root of the problem. Accordingly, he was treated with four sessions of intensive psychotherapy a week and denied psychiatric drugs for the duration of his stay. After seven months at Chestnut Lodge, Osheroff was able to secure a transfer to the Silver Hill Foundation in Connecticut. At Silver Hill he was diagnosed with psychotic depression and put on phenothiazine antipsychotic medications as well as tricyclic antidepressants. Within three weeks Osheroff improved noticeably. He was discharged in good health a few months later (Healy, 1997; Shorter, 1997).

Osheroff left Silver Hill to find his life in tatters. His second wife had left him. His first wife had taken custody of their two children. His business partner had kicked him out of their joint practice and his hospital accreditation had been rescinded. In 1982 Osheroff sued Chestnut Lodge for malpractice and was awarded a quarter-million dollar judgment by an arbitration panel. Both parties appealed the judgment, and the subsequent legal maneuvering involved some of the most prominent figures in American psychiatry weighing in on the legitimacy and scientific grounding of psychoanalytic therapies. In 1987, shortly before the case was set to go to trial, Chestnut Lodge paid Osheroff an undisclosed settlement. Even though the *Osheroff* settlement did not carry the force of legal precedent, it provided

a glimpse into the legal future of therapeutic psychoanalysis. When all the cards had been played, psychoanalysis was revealed to be a legally indefensible approach to the treatment of psychiatric illness. Treating psychiatric disorders with analysis alone would forevermore be understood as a malpractice risk (Healy, 1997; Shorter, 1997).

Psychiatry had begun to divest itself of psychoanalysis with the publication of *DSM-III*, but it was not until the Osheroff settlement that the analysts turned away from psychiatry. Psychoanalysts began to develop a new niche as nonmedical therapists. Within a year, psychoanalytic training programs began to accept nonmedical candidates. Psychoanalysis was soon just one form of psychotherapy among many, a tool for introspection that could help anyone be a better relational partner or live a more satisfying life. Interestingly, technical psychoanalytic explanations of the mind continued to thrive not as a tool for medical treatment, but as a method of cultural criticism. Today psychoanalysis is the province of critical theory, informing the study of philosophy, sociology, gender studies, cultural studies, literary criticism, and related disciplines. A small but growing group of rhetorical scholars use psychoanalytic methods as the basis for rhetorical criticism. “The late-twentieth-century trajectory of psychoanalysis had carried it beyond the discipline of psychiatry and into the ether of arts and letters where, however it fared, it would no longer be identified as a privileged treatment for psychiatric illness” (Shorter, 1997, p. 310).

### ***DSM in the Age of Depression***

*DSM-5* will be the fourth update to the *Diagnostic and Statistical Manual* since the 1980 publication of *DSM-III*. The last three editions of the manual added

more diagnoses, but made few substantial conceptual changes to the basic Kraepelinian model. *DSM-III* contained 265 diagnostic categories and was 494 pages long. *DSM-III-R* was published in 1987, increasing the number of diagnoses to 292 over a total of 567 pages. This version did away with “sexual orientation disturbance.” *DSM-IV* was published in 1994, containing a massive 886 pages and 297 diagnoses. *DSM-IV* abolished neuroses entirely and added a clinical significance criterion to many conditions, requiring symptoms to meet a threshold of clinical significance before qualifying as a disorder. The most recent edition was *DSM-IV-TR*, published in 2000. This was a text revision to *DSM-IV* that made few changes to diagnostic criteria but updated the text sections explaining each diagnosis. *DSM-IV-TR* is 943 pages long.

If *DSM-III* was the opening gambit in a struggle to redefine psychiatry as a biological enterprise, *DSM-5* might be considered the endgame. The *DSM-IV* definition of mental disorders noted that “no definition adequately specifies precise boundaries for the concept of ‘mental disorder’” and that “the concept of mental disorder (like many other concepts in medicine and science) lacks a consistent operational definition that covers all situations.” *DSM-5* moves psychiatry even closer to medical science by combining these into the claim that “no definition perfectly specifies precise boundaries for the concept of either ‘medical disorder’ or ‘mental/psychiatric disorder.’” Psychiatry is no longer “like many other concepts in medicine and science.” It is now linguistically parallel to medicine and conceptually indistinct. One of the *DSM-IV* criteria for a mental disorder is “a manifestation of a behavioral, psychological, or biological dysfunction in the individual.” This standard is being changed in *DSM-5*. The new version reads “that reflects an underlying

psychobiological dysfunction.” As the members of the Work Group assigned to this part of the manual explained:

There is a growing awareness of the extent to which all behavior and psychology are dependent upon brain processes, and the extent to which brain changes have complex behavioral and psychological effects. The term “psychobiological” emphasizes the extent to which these different types and levels of dysfunction are intertwined in reality (Stein et al, 2010, p. 5).

This is the view of mental disorder that underwrites the removal of the bereavement exemption from the definition of depressive disorders. It is singularly definitive of contemporary psychiatry and wholly coextensive with the increasing influence of the pharmaceutical industry on modern medical science.

#### **Pharmaceutical Influence: How Did We Come to This Point?**

The single largest difference between the psychiatry of *DSM-III* and the discipline as it will exist under *DSM-5* is the central role that pharmaceutical companies play in all aspects of modern psychiatric research and practice. As Diller (2006) explains:

The drug industry hijacked American psychiatry in the 1990s... Since the mid-1980s, drug companies have dominated funding for all medical research. This is especially true in psychiatry: the companies have financial ties with virtually every psychiatric researcher and expert in their respective fields. They also influence doctors’ education by paying for most physicians’ continuing education after they graduate from medical school (p. 13).

In many respects then, the situation in psychiatry is but a mirror of the growing influence the pharmaceutical industry exerts over medical research and practice more generally.

One of the most troubling forms of industry influence in both psychiatric and somatic medicine is a practice known as ghostwriting, wherein medical communication firms write articles, reviews, opinion statements, and even reports of original research that are submitted to professional journals under the names of academics or medical specialists uninvolved in the research or writing process. Ghostwriting is much more than just medical plagiarism, however. Ghostwriters are usually commissioned by drug companies, who request manuscripts specifically to distort the scientific record and paint their products in the best light possible. In 2009, a lawsuit alleging that Wyeth Pharmaceuticals hid the cancer risks of the hormone drug Prempro led to the discovery that much of the scientific literature about Prempro had been ghostwritten, systematically distorting the drug's safety record. Following the court-ordered release of these documents, the editors of *PLoS Medicine* (2009) wrote:

[T]he story told in these documents amounts to one of the most compelling expositions ever seen of the systematic manipulation and abuse of scholarly publishing by the pharmaceutical industry and its commercial partners in their attempt to influence the health care decisions of physicians and the general public.... Others have written about ghostwriting campaigns concerning single drugs that have led to catastrophic health effects, and how even research papers and clinical trials are affected by ghost authors. What's clear is that ghostwriting can no longer be considered one of the "dirty little



secrets” of medical publishing... While editors, medical schools, and universities have turned a blind eye to... the pervasive presence of ghostwriting, drug companies and medical education and communication companies have built a vast and profitable ghostwriting industry. Recruitment of academic “authors” appears, within some academic circles, to have come to be considered acceptable, and marketing campaigns are no longer orchestrated around paid display advertisements but instead center on “evidence” provided by seemingly respectable academic review articles, original research articles, and even reports of clinical trials. What, a cynical reader might ask, can I truly trust as being unbiased? The answer is that, sadly, for some or even many journal articles, we just don't know.

The same year, editors of the *Journal of the American Medical Association* conducted an anonymous survey of authors whose names appeared on works in *The Lancet*, *Annals of Internal Medicine*, *Nature Medicine*, *Journal of the American Medical Association*, *PLoS Medicine*, and *the New England Journal of Medicine*. The voluntary author responses identified 26% of the articles in question as having honorary authors, 8% as having ghost authors, and 2% as having both honorary and ghost authors (Grassley, 2010). Ghostwriting is viewed as a particularly acute problem in the field of psychiatry, where some estimates suggest that up to half of all published articles about pharmaceuticals may be ghostwritten at the behest of drug manufacturers (Barnett, 2003).

Set against this level of pervasive institutional influence, it should come as little surprise that the majority of psychiatric professionals responsible for shaping *DSM-IV*, *DSM-IV-TR*, and *DSM-5* have some sort of financial connection to the

pharmaceutical industry. A 2006 study by researchers from the University of Massachusetts and Tufts University looked at internet databases and publication self-disclosure statements to identify financial relationships between the pharmaceutical industry and the expert panel members who contributed to the diagnostic criteria in *DSM-IV* and *DSM-IV-TR*. The authors focused on the years 1989 to 2004, a period beginning five years before the publication of *DSM-IV* and ending four years after the publication of *DSM-IV-TR*. They found that at least 56% of the 170 expert panel members had financial ties to the pharmaceutical industry during this period, a *de minimis* figure that includes only a scattershot of publicly available or self-disclosed data. The authors note that some forms of financial interest are difficult to detect with these screening tools, such as expert witness testimony in legal cases, while others, like illicit ghostwriting honoraria, are altogether undetectable. Indirect financial ties were considered nondisclosable financial interests and excluded from the study. For example, the researchers found twenty-three people who received speaking honoraria for talks sponsored by drug companies, but who were paid out of unrestricted university grant money. Because pharmaceutical companies do not retain direct control over the use of unrestricted grant funds, the authors excluded these transactions from their analysis (Cosgrove, Krinsky, Vijayaraghavan, & Schneider, 2006).

These researchers found that the prevalence of financial ties among members of the various *DSM* Work Groups corresponds roughly to the role that drug treatment plays in the medication of each class of disorders. Fully 100% of the panel members in the Mood Disorders Work Group and the Schizophrenia and Other Psychotic Disorders Work Group – the two main categories of disorder for

which psychopharmacological treatment is standard practice – had financial ties to the pharmaceutical industry, while only 17% of the panel members in the Substance-Related Disorders Work Group – a class of disorders for which drug therapies are much rarer – were found to have financial ties to pharmaceutical companies. 81% of the Anxiety Disorders Work Group had financial ties to the drug industry, as well as 83% of the Eating Disorders Work Group, 88% of the Medication-Induced Movement Disorders Work Group, and 83% of the Pre-Menstrual Dysphoric Disorders Work Group (Cosgrove et al, 2006).

In 2008 the American Psychiatric Association instituted a limited financial interest disclosure policy for contributors to *DSM-5*, revealing that 70% of the Task Force members had financial ties to the pharmaceutical industry over a period of less than two years (the past full calendar year and the current year to date). Over three-fourths of the individuals in the Mood Disorders Work Group and the Psychotic Disorders Work Group reported financial ties to pharmaceutical companies in the same period of time. One member of the Mood Disorders Work Group disclosed forty-six significant interests and affiliations in a period of under two years, including grants, honoraria, consultation fees, and travel costs from Abbott Laboratories, AstraZeneca Pharmaceuticals, Eli Lilly Research Laboratories, GlaxoSmithKline Inc., Janssen Pharmaceutica, JDS Pharmaceuticals, Novartis Pharmaceuticals Corp., Pfizer Inc., Solvay Pharmaceuticals, and Wyeth Pharmaceuticals. This same person disclosed consultation fees, honoraria, and royalties from over a dozen medical education and communication companies.

### **Pharmaceutical Influence: Going Forward**

For better or for worse, the pharmaceutical industry has become an inextricable part of modern psychiatric research and practice. In a point/counterpoint article published in *Psychiatric Times*, Cosgrove and Bursztajn (2009) argue that the APA should set financial interest standards for those involved in drafting *DSM-5*. They write:

Even small changes in symptom criteria can have a significant impact on what new (or off-label) medications may be prescribed... to the extent that DSM is constructed as a reliable diagnostic taxonomy that emphasizes relatively short-term, acontextual symptoms that tend to be insensitive to characterological variability in expression, it encourages overuse of diagnostic checklists. Among other things, diagnostic checklists provide the basis for outcome measures in clinical trials conducted by industry-funded researchers. These researchers then maintain that the "evidence-based research" speaks for itself, that the disorder has been validated, and that the new drugs and medical devices have been proven to treat the disorders effectively and safely. Diagnostic checklists thus become established as good science and contribute legitimacy to the proliferation of new diagnoses and new medications... too often [diagnostic checklists] become self-serving industry tools and lead to inflated statistics about how many people are "suffering" from a disorder... The end result of a diagnostic manual with acceptable reliability but limited validity is that it allows pharmaceutical companies to bring what has been referred to as "me-too" drugs to market--

medications that are just different enough from existing pharmaceutical agents that they can be patent-protected.

What is exceptional about the response from Kupfer and Regier (2009), the chair and vice-chair of the *DSM-5* Task Force, is their wholesale embrace of the connection between the biological orientation in the *DSM* and the promotion of pharmaceutical solutions to psychiatric problems. “Drs Cosgrove and Bursztajn reflect biases of their own that should not go unchallenged,” they write. “The first is the assumption that something has been proved to be wrong... about financial relationships between the APA and the pharmaceutical industry.” Cosgrove and Bursztajn, they argue, “seem not to appreciate or understand how the collaborative relationships among government, academia, and industry are vital to the current and future development of pharmacological treatments for mental disorders,” noting the “massive evidence for the efficacy of... pharmacological treatments *that have been facilitated by the DSM framework*” (emphasis added). They continue:

Given the authors' now well-documented antimedication and anti-industry bias, it is clear that they would prefer to have no one on the *DSM-V* Task Force or Work Groups with any connection to industry. However, we believe that careful attention to potential conflicts of interest, including those of individuals committed to a single school of psychotherapeutic intervention or approach, will inform diagnostic revisions with a broad scientific base that has been greatly enlarged and facilitated by the definitions of mental disorders provided since the release of *DSM-III*. This includes geneticists who are looking closely at the future feasibility of having genetically informed, specific pharmacological treatments for treatment-resistant

depression in the same manner that such genetically informed pharmacological treatments are now available for certain forms of breast cancer.

The problem, as they see it, lies not in the claim that the structure of the *DSM* contributes to the proliferation of new illnesses and new drug treatments. Kupfer and Regier seem altogether quite happy to acknowledge that the framework of the *Diagnostic and Statistical Manual* has “greatly enlarged and facilitated” the “broad scientific base” of psychiatry, and that the increasing reliance on pharmacological approaches to treat mental disorders has “been facilitated by the *DSM* framework.” The problem with Cosgrove and Bursztajn’s criticism is rather the “antimedicine bias” which betrays the authors’ lack of understanding about how important these interconnections are for the development of new psychiatric drugs. As we will see in the final chapter, psychiatry’s steady progression toward the biological pole of illness and the intertwining of psychiatry and pharmacology have powerful implications for the ways we understand the stresses and trials of life as well as ourselves.

## Chapter 7

### CONCLUSION: DSM-5 AND THE SOCIAL NEGOTIATION OF POWER AND KNOWLEDGE

The removal of the bereavement exemption in the *DSM-5* definition of depressive episodes is not an isolated historical event, but the product of numerous ideological forces acting to shape the history of psychiatry. For most of history, the healing professions have sought to answer the question *why*. As recently as thirty years ago, psychiatric thinking about depression concerned itself principally with understanding *why* people become depressed, with the underlying causes of depression. The removal of the bereavement exemption in *DSM-5* evinces a profound shift in the focus of psychiatry over the past thirty years, from the *why* of mental disorders to the *what* and *how*. The removal of the bereavement exemption is not premised on a denial of the role bereavement can play in depression. Rather, the authors of *DSM-5* claim that “the evidence does not support separation of loss of loved ones from other stressors;” psychiatry purports to have turned away from the causes of depression altogether.

This abandonment of cause might be less than it appears, however. The claim that mental and behavioral health symptoms “reflect an underlying psychobiological dysfunction” in the individual does not sound like a statement of etiological neutrality. Emil Kraepelin’s system of disease classification – the basic theoretical framework at the heart of the modern *DSM* – reflects Kraepelin’s belief that psychiatric symptoms are indications of underlying organic disease entities. The importance of *why*, of cause in the sense of a situational trigger, is closely tied to the *what* of depression. If depression is a biological process or an organic disease, a

state of low serotonin, for instance, then knowing what triggered a particular episode of depression is unimportant to understanding the patient's condition. If depression is a maladaptive reaction pattern, knowing what triggered the onset of a depressive episode is absolutely critical. There is good reason to avoid "unproven etiological concepts" in the abstract, but here we are talking about a known cause: bereavement. That the loss of a loved one can bring about the onset of depressive symptoms is obvious. The operative question is whether those symptoms should be treated differently from the depressive symptoms triggered by other sorts of stress. To declare cause unimportant is to have already decided that the *what* of depression is the same in either case, that there is an underlying (biological) process common to all forms of depression.

It would seem then that the *DSM* is not truly neutral on the issue of cause, and that "avoiding unproven etiological concepts" really means "avoiding psychogenic or environmental explanations of cause." As Dana Cloud (2001) reminds us:

Affirmation bears the privilege of invisibility; it possesses the luxury of never appearing "heavy-handed"... Nonetheless, affirmation is an act of advocacy. Even when criticism claims to be descriptive of social reality rather than offering normative correctives to unethical or malign rhetorical practices, the retreat into description is profoundly ideological (p. 1).

We saw earlier that the rejection of cause in *DSM-III* functioned to redefine all depression as endogenous. This is the productivity of power. Under the guise of neutrality, the *DSM* fosters a very specific understanding of the etiology of mental disorders. By removing the bereavement exemption from the definition of



depressive episodes, *DSM-5* promotes a biological understanding of depression in which depressive symptoms are seen as surface traces of an underlying biological process. This act of production, of promotion and advocacy, is carried out in the name of neutrality, the productive nature of power hidden by its own claim to merely describe the world as it is.

Chapter five began to explore the nature of public alienation from modern psychiatry. We saw how these sentiments, though unique in their historical specificity, are in fact as old as biopower itself. One factor that contributes to modern feelings of medical alienation is psychiatry's progressive shift from a discipline focused on the *why* of illness to a discipline concerned primarily with the *what* and *how*. Modern psychiatry is no longer focused on the problems of the individual, but on treatment. Insurance policies encourage psychiatrists to meet with patients for quick diagnostic prescription visits, outsourcing talk therapy to psychologists, counselors, and social workers. The removal of the bereavement exemption in *DSM-5* can only function to exacerbate this sense of medical alienation, as it shifts the focus of treatment further away from the patient's experiences and life circumstances to a checklist of symptoms, to the sanitized, emotionless plane of clinical indistinction where patients wait in line to be matched up with the appropriate medicine.

Modern psychiatry developed alongside biopower, which brings bodies into the service of power through subtle, meticulous techniques of control that work to make them productive. Over the past two hundred years, increasingly larger portions of our lives have been brought into the fold of psychiatric knowledge. Where melancholy once meant debilitating sadness, depression is now defined so

broadly that it could include almost anyone having a bad week. As the concept of depression expands evermore, the stresses of everyday life are increasingly becoming symptoms of a disorder to be treated and fixed. The removal of the bereavement exemption expands the formal boundaries of depression further still, turning the profound grief that people often experience when they lose a loved one into a form of psychiatric disorder. The practical function of this change is to make the bereaved candidates for antidepressants, allowing them to be fixed, to be made productive in the service of power, through the wonders of psychopharmacology.

The preceding chapters have sketched out a history of biopower's spread through the institutional apparatuses of psychiatry, the ways that individual interests can be made subservient to the anonymous technologies of biopower. We saw this most clearly in chapter six, as we began to draw out the threads of the relationships that exist between the pharmaceutical industry, psychiatrists, and other social actors such as medical communications firms and academic publishers. It would be a mistake to attribute our current circumstances to the overwhelming influence of pharmaceutical companies who single-handedly drive the increasing medicalization of our society in the pursuit of ever-larger profit margins, or a grand conspiracy between pharmaceutical companies and regulators. As chapters four and five made clear, the pharmaceutical companies and regulators are caught up in a great movement that they themselves have little control over. Far from the super villains that they are sometimes made out to be, drug firms might be more like the drunk under the lamppost groping hopelessly for a way to translate our extraordinary knowledge of neurology and chemistry into a therapy that can predictably bring about changes in one's mood or disposition. The story here has

laid out the basic contours of an account of biopower's spread through the institutional apparatuses of psychiatry, the ways that a psychiatric diagnosis can function to make bodies into objects of knowledge. Once rendered objects of knowledge, these bodies can be made productive. Not only does a diagnosis of depression make people candidates for antidepressant drugs, it makes them into new kinds of legal and political subjects subject to new forms of social control.

We saw an earlier historical example of this process, an analogue to our current situation, in chapter four, in the relationship of convenience that developed between society nerve doctors and nervous patients during the nineteenth century. This relationship benefited all parties involved: the arbitrary, porous distinctions that were drawn between madness and nerves allowed physicians to escape the violence and misery of the asylums to set up comfortable, profitable practices treating the urban middle-class, and it allowed those with money or family support to get psychiatric treatment without being labeled as mad. In doing so, however, it also functioned to expand the horizons of psychiatric treatment, to blur the distinction between normality and disorder, and to begin the process of laying the institutional groundwork for the expansion of modern psychiatry into the minutest aspects of our everyday lives.

Depression is the bedrock of modern psychiatric practice, but as chapter four demonstrates, the biological understanding of depression rests on shaky scientific grounds. The bereavement exemption is a testament to this, and its removal under the guise of etiological neutrality functions to confirm the truth of the biological model. What is at stake here is not only the legitimacy of psychiatry as a healing profession, but the entire complex of power/knowledge surrounding

psychiatry: the interests of the pharmaceutical industry and insurers, the legitimacy of laws premised on psychiatric knowledge, the wellbeing of millions who rely on psychopharmacology. There is a sense in which the biological model of depression lies at the very center of this matrix of power/knowledge. As such, the removal of the bereavement exemption can be understood as nothing less than an imperative of biopower.

### **Practices of Resistance**

It would be both irresponsible and decidedly un-Foucauldian to purport to offer the reader a blueprint for resistance against the developments outlined here. Contrary to the claims of Foucault's critics (see Fraser, 1989; Habermas, 1987; Taylor, 1984), this is not because his theoretical model forecloses the possibility of resistance, dooming us to political nihilism. "It is here that Foucault implicitly takes issue with the... counter-Enlightenment rhetoric that has often laid claim to his own work... as a principal source of inspiration," argues Norris (1994). "For there is nothing more alien to Foucault's thought than the kind of ultra-relativist orthodoxy that erects its own lack of critical and ethical resources into a... terminal indifference with regard to issues of truth and falsehood" (p. 194). That truth claims are inextricably tied up in systems of power should not be understood as an injunction against epistemic coherence, nor should it be taken to imply that any given truth claim is as valid or defensible as any other. As Rouse (1994) explains, the interrelationship of power and knowledge simply "suggests a different image in which conflict and struggle are always present and inescapable. To make truth-claims is to try to strengthen some epistemic alignments, and to challenge, undermine, or evade others. To criticize power is to participate in counter-

alignments to resist or evade its effects” (p. 112). According to Haber (1994), those who claim that Foucault’s model forecloses the possibility of resistance “take him to be doing something he is not. They are trying to force him to conform to their notion of what a concerned social critic must be – but he does not want to fit that mold” (pp. 91-92). Nothing about Foucault’s investment in the contextual, contingent character of truth stopped him from making claims or taking sides. “Foucault was perfectly prepared to offer reasons for his choices of struggles and sides,” says Rouse (1994). “He was equally prepared to offer reasons and evidence for the statements he made. What Foucault was not prepared to do was to see these choices, statements, and reasons as more than a situated response to a particular political and epistemic configuration” (p. 112).

It would be irresponsible to claim to offer the reader a blueprint for resistance because biopower is indefatigably resilient, and practices of resistance can easily become co-opted by the very structures of power they seek to subvert. Jessica Kulynych (1997) notes that “[y]early Washington marches, for example, may actually diffuse discontent by providing a legitimate outlet for protest; at the same time they verify system legitimacy by focusing protest toward the formal legal structures of government” (p. 342). In a similar vein, Michael Clifford (2001) argues that our petty political squabbles, drawn along an imagined philosophical axis of liberalism-conservatism and framed in the hollow language of political right, do little to engender true freedom while functioning to mask and sustain the biopolitical processes that really and materially delimit political subjectivity:

[O]ur struggles should be “against subjection, against forms of subjectivity and submission” asserts Foucault. The source of our subjection and

subjugation is not the state (as a central governing body), not an elite group, a class, or an economic relation. Rather, it is an anonymous technology of power... it is the complex matrix of disciplinary power against which we need to mount our resistance... The possibilities for free action are directly, almost linearly, related to our identities as individuals, to how we are identified, by ourselves and others, in a relational and hierarchical complex of rank and gradation. Through power/knowledge relations, discipline “forces the individual back on himself and ties him to his own identity in a constraining way.” Our identities are to this extent the result of disciplinary mechanisms of normalization and individualization. Thus, the object of our resistance will be to “refuse what we are” – that is, to break through, to fracture, the limitations imposed on us by a disciplinary ascription of our own identit[ies] (p. 140).

Although Clifford is writing here about resistance to political subjectivication proper, the application of his argument to the question at hand is clear.

The redefinition of depression is not a scheme concocted by evil pharmaceutical companies seeking to medicalize all human experience in pursuit of profit or naïve incompetence on the part of prescribing physicians. It is a product of anonymous technologies of power that operate by making human beings into objects of knowledge, allowing bodies to be labeled and molded in the service of power. It renders the bereaved objects of medical and psychiatric knowledge, candidates for therapies to help alleviate their distress and make them productive. Resistance always takes place within the institutional realities that we live. We have seen throughout this project the ways that these institutional actors have

contributed to the development of the current situation, but resistance that is focused exclusively on the wrongdoing of drug companies or psychiatrists risks missing the ways that they too are caught up in the larger momentum of biopower. There is undoubtedly a role for practices of resistance that seek to challenge the complex interrelationship between the pharmaceutical industry and the ever-widening boundaries of depression, for instance, but such practices, like all forms of resistance, risk reifying the very processes of subjection that they seek to subvert.

The following remarks about resistance should therefore be understood as tentative and contextually bound. These examples of resistant practices are meant as a heuristic sketch to help concretize some of the abstract and rather complex ideas we have been unpacking throughout this study. They are not examples of unproblematic or wholly effective forms of resistance, nor should these be understood as a prescription or a map for future action. These are not meant as examples of ahistorical or context-free resistance to power. They are historically specific, lived practices of resistances, ways that others have chosen to orient themselves in their efforts to promote health, dignity, and freedom, to challenge the historically contingent institutions and sedimented social practices that, as a result of the genealogy we have undertaken here, we know could have been otherwise.

### **Individual and Community Resistance**

On an individual level, AIDS activists practiced very effective resistance against medical, pharmaceutical, and regulatory apparatuses during the 1980s, forcing broad changes to the conduct of clinical trials and the laws governing pharmaceutical regulation. The FDA requires that drugs undergo placebo-controlled trials to demonstrate effectiveness, which means that some trial participants will

not receive an active drug treatment. We saw in chapter five that the history of medicine is filled with examples of new drugs that were enthusiastically embraced by the medical community as safer and more effective than existing treatments, only to prove the opposite. This history alone provides ample reason for requiring proof that a new antidepressant or hair growth drug actually works by comparing it against placebo, but for patients with a terminal illness and no other options for treatment, the value of placebo testing is less clear. Early HIV/AIDS patients formed a community in a way that had never been done before, and they informed themselves about the science behind efforts to find a treatment for their condition. When pharmaceutical companies insisted that the only way they could participate in clinical trials was to accept the risk of being randomized into a placebo group, HIV/AIDS patients came together to test their medications and pool the active drugs. By systematically undermining trial designs in an effort to ensure that as many participants as possible had access to potentially life-saving therapies, HIV/AIDS activists literally forced pharmaceutical companies and regulators to give them a seat at the bargaining table: “In many places researchers, medical ethicists, and AIDS activists sat around the same tables and hammered out agreements about what needed to be done and what could be done” (Healy, 1997, p. 105).

HIV/AIDS activists also helped to bring about changes to the lengthy and cumbersome FDA approval process through large-scale organized protest activities. While meticulous safety protocols make sense for the treatment of non-lethal conditions, like blood pressure and headaches, the risk/benefit calculation is fundamentally different for a terminal condition with no existing treatment options. HIV/AIDS activists worked diligently to bring this issue to the public’s attention.



Following increasing protests and public criticism in the 1980s, the FDA developed an accelerated approval protocol for drugs that treat life-threatening conditions and allowed for increased access to drugs in phase II or phase III testing for AIDS and some forms of cancer.

Healy (1997) takes the challenge of HIV/AIDS to suggest that, despite the sanitized, technical veneer of modern medical science, all science is ultimately a community activity with a very real role for non-scientists to take part in the decisions that govern their lives:

If an activity designated as scientific is not serving the interests of the community, there comes a point when it will be rejected. I would go further and argue that the vaunted objectivity of science lies not in any method so much as in scrutiny by a community – ordinarily by a community of scientists, in the first instance, but in the final analysis by a community at large (p. 105).

There is something ennobling about this idea. As a descriptive matter it is probably correct, and it is highly consistent with the Kuhnian model that informs this study.

At the same time, these challenges to science highlights the conflicted and ultimately undecidable nature of resistance. From heliocentrism and evolution to anthropogenic climate change, there are numerous historical examples of science made subservient to religion, politics, or the beliefs of the community, and extraordinarily little reason to believe that the “democratization” of science produces either Truth or desirable policy outcomes. At an historical moment when political interests and increasing lay skepticism about the existence and causes of climate change are working to impede meaningful policy responses to a

phenomenon that threatens the very habitability of our planet, the ambivalence of resistance against legitimized science is particularly acute.

Resistance, of course, can consist in merely thinking otherwise. Medawar and Hardon (2005) suggest that a powerful step toward reclaiming control over one's own health involves a re-valuation of the placebo concept. The placebo is much maligned as a fake treatment. To be healed by placebo is thought of as being duped or tricked somehow. A more productive way to understand the placebo effect might be to think of it as the innate resiliency and healing capacity of the human body in combination with all the non-chemical effects of a therapeutic encounter. Although the placebo effect is a documented medical phenomenon, it works most powerfully on pain, mood disorders, and other afflictions that seem to have a large psychological component. That most antidepressants show little clinical benefit over placebo could be understood as a failure of psychopharmacology, but it could also be thought of as evidence of the effectiveness of all the non-chemical elements of therapeutic interactions. The fact that nearly half of all depression patients improve with placebo suggests that having a sympathetic caregiver to talk to, in combination with the desire and belief that one will get better, conspire to play a larger role in the treatment of depression than any chemical interaction in the brain.

Conventional ways of thinking and talking about placebo are profoundly disempowering. They discursively construct healing as an effect of drugs. They locate the ability to get better outside of the individual in the products sold by healing experts. A re-valuation of the placebo concept would therefore require a rethinking of the relationship between consumption and health. It would involve recognizing that the human body is miraculously resilient, and the human mind has

healing powers that modern science is as of yet largely unable to account for. As mentioned in chapter four, the target market for antidepressants is comprised entirely of people who are aware of antidepressants but do not feel that they need them, not people seeking a treatment for depression who are unaware that antidepressant drugs exist. Accordingly, antidepressant marketing is as much about selling a disease as a cure. In order to sell drugs, antidepressant marketing can do little but work to convince people that they are unable to get better without drugs. Simply rethinking the way one understands the placebo concept could go a long way toward combating these pervasive, disempowering messages and the ever-widening concept of depression.

A further example of individual and community resistance can be found in antidepressant consumers' use of the internet to both challenge the official narratives promulgated by pharmaceutical companies and to provide support for those who have come to harm as a result of them. We saw hints in chapter five of the ways that pharmaceutical companies have worked to systematically distort the safety record of SSRIs and to suppress evidence that SSRIs can have unpredictable emotional effects, including suicidal ideation and violence toward others. Before these problems could be addressed on an institutional level, first through litigation and later through regulatory changes, they were brought to light through the online community developed by antidepressant users in the early days of the internet. The first suggestions within the medical community of a possible link between SSRIs and suicide (Teicher, Glod, & Cole, 1993), based on anecdotal reports, were roundly criticized and writers friendly to the position of the pharmaceutical industry made great efforts to discredit the report's authors. It was only through the collaborative

efforts of internet users who shared their stories that a larger picture of dependence and risk began to emerge. Medawar and Hardon (2005) write:

The Internet made a profound difference on the history of Selective Serotonin Reuptake Inhibitors (SSRIs). *ADWEB* was set up in 1998, in the early days of the first explosive growth of the Internet. By this time, messages about SSRIs had begun to appear in odd chat rooms, but it was too early to imagine that this trickle would become a flood... Most of all, users reported their fears that they were going seriously ill or mad – until they found out about withdrawal symptoms and learned that many other people had them too... The immediate reaction of many who found these websites was relief and amazement that so many other people had logged on (p. 75)

This simple resistant act of sharing one's story was particularly important to the recognition of the unique problems that can arise from SSRI use among young people. Much as alcohol and narcotics can have different, less stable effects on children than adults, SSRI use can have particularly unpredictable emotional and behavioral consequences for young people. Even though children were prescribed these drugs in large numbers, these were off-label prescriptions, written for a use other than the formal indications of a drug. In the early years of the SSRI period, there was almost no official data relating to the consequences of SSRI use by children because they were designed, tested, and approved by regulators for adult use only. It was only after users began to come together through the internet to tell their stories that a collective picture began to develop of the risks that SSRI use holds for children, eventually catalyzing official investigations into the problem.

## Institutionalized Resistance

On an institutional level, one example of formal resistance to the increasing corporatization of medicine is the Public Library of Science project, which publishes the journal *PLoS Medicine*. According to [www.plos.org](http://www.plos.org), the Public Library of Science (PLoS) was established in 2000 to promote open access to scientific and medical research through free online archives like the U.S. National Library of Medicine's PubMed database. PLoS circulated an open letter signed by nearly 34,000 scientists from 180 countries encouraging journal publishers to help make the world's scientific and medical knowledge both collaborative and accessible to all. The initiative prompted many publishers to make significant changes, such as allowing open-access archiving of all research articles over six months old, but these changes fell short of what PLoS was seeking, so in 2003 the organization launched an open-access, nonprofit publishing venture. PLoS journals are freely available online with no access charges and no restrictions on use or reproduction so long as all sources are properly cited, per the Creative Commons Attribution License. *PLoS Medicine* was launched in 2004 as an open-access peer-reviewed medical journal. The first two PLoS journals, *PLoS Medicine* and *PLoS Biology*, are touted as highly respected publications in their respective fields run by professional editors that "reject a large proportion of the submitted papers and publish a great deal of value-added content. They are... representative of only the top tier of scientific journals, which includes *Nature*, *Science*, and *The New England Journal of Medicine*." PLoS charges publication fees, but these generally amount to a miniscule fraction of scientific research costs and can be waived if researchers are unable to pay. A large portion of PLoS funding comes from philanthropic foundations, as well as sponsorship, membership, and

advertising fees. Unlike nearly all other major medical journals, *PLoS Medicine* does not accept advertising for pharmaceuticals or medical devices. “Nor will it publish company-funded studies that are considered to be marketing dressed up as science. The aim, say its editors, is to break ‘the cycle of dependency’ that has formed between medical journals and the pharmaceutical industry” (Moynihan & Cassels, 2005, p. 196).

Another form of institutional resistance against the corporatization of medicine were the efforts of International Human Genome Project (IHGP), which raced to release a working draft of the human genome before Celera Genomics, and the ongoing research efforts at the National Institutes of Health’s National Human Genome Research Institute (NHGRI). In 1990, the IHGP began working to decode the human genome. Celera, a private company, began working on a competing, accelerated project in 1998. Celera sought patent applications for thousands of medically promising genes and stated its intention to release its data in proprietary form. In a highly publicized race to the finish, the publicly funded IHGP team released its sequence mere days before Celera, ensuring that the genome and the information it contains remain free and available to researchers worldwide (Center for Biomolecular Science and Engineering, 2009). Now that the human genome has been fully mapped, the NHGRI seeks to make this information a more useful medical and scientific research tool. While private companies work to develop genetic therapies for profit, the NHGRI is working to develop a new sequencing program with the long-term goal of making individual genomic screening an affordable way to screen, diagnose, and treat diseases. A related collaboration between the NHGRI and the National Cancer Institute called the Cancer Genome Atlas seeks to improve

knowledge about the genetic components of cancer to facilitate affordable, effective genomic screening, treatment, and prevention of cancer (National Institutes of Health, 2010).

These projects exemplify the profound ambivalence of biopower and the complicated nature of resistance. The IHGP was an indisputably democratizing force that helped ensure no single company owns a patent to the building blocks of life, and the NHGRI is working to develop this knowledge into an affordable tool for the betterment of all people. At the same time, decoding the human genome has rendered the very information of life an object of scientific knowledge, potentially opening up vast areas of human life to new forms of social control. These technologies make possible, for the very first time, the prospect of genetic discrimination by employers, or the possibility that a child's station in life could be dictated based on genetic indications of aptitude. Whether these forms of resistance to the corporatization of medicine are ultimately productive or desirable is a decision we each must make for ourselves.

Another instance of embodied institutionalized resistance is the Institute for OneWorld Health, the first nonprofit pharmaceutical company in the United States. We have seen throughout this story how the financial interests of pharmaceutical companies are often at odds with the promotion of health and wellness. Our analysis has been primarily confined to the United States and Europe, but one of the most insidious consequences of this tension is the systematic neglect of the health care needs of the world's poor. While the pharmaceutical industry spend billions of dollars a year developing treatments for conditions of affluence, such as hair loss, hypertension, and sexual dysfunction, often through clinical tests conducted on

impoverished populations, millions of these same people die from easily-treatable infectious diseases because it is simply not profitable for drug companies to manufacture the medicines they need to survive. OneWorld Health works with local hospitals and organizations in the non-industrialized world to research and develop cures for these diseases, and they draw upon the scientific and manufacturing capabilities of the global South to create a sustainable, affordable drug production model. OneWorld Health uses donated intellectual property and develops new drugs in combination with traditional pharmaceutical companies and academics. The organization is currently developing treatments for leishmaniasis, diarrheal disease, and soil-transmitted helminthiasis, as well as a process to synthesize artemisinin, an extract of the wormwood plant that is a critical component of the World Health Organization's recommended treatment for malaria (Institute for OneWorld Health, 2010).

### **Ambivalence and Ambiguity**

If it sounds as though there is a degree of moral ambiguity, an ethical quietude, to all of this, that is because the ethical grounding of the Foucauldian critique is radically different from the normative foundation of most traditional ethical systems. The ethical impulse that animates the Foucauldian critique is no more, and no less, than a desire to broaden the horizons that delimit our opportunities to engage the choices we face each day. Foucault once told an interviewer:

I would like to do a genealogy of problems, of *problématiques*. My point is not that everything is bad, but that everything is dangerous, which is not exactly the same as bad. If everything is dangerous, then we always have



something to do. So my position leads not to apathy but to a hyper- and pessimistic activism. I think that the ethico-political choice we have to make every day is to determine which is the main danger. Take as an example Robert Castel's analysis of the history of the antipsychiatry movement (*La Gestion des risques*). I agree completely with what Castel says, but that does not mean, as some people suppose, that the mental hospitals were better than antipsychiatry; that does not mean that we were not right to criticize those mental hospitals. I think it was good to do that, because *they* were the danger. And now it's quite clear that the danger has changed. For instance, in Italy they have closed all the mental hospitals, and there are more free clinics, and so on – and they have new problems (Dreyfus & Rabinow, 1983, pp. 231-232).

Genealogy is the activation, the putting into practice, of this hyper- and pessimistic activism. It seeks to reveal the non-essentiality of those things that are understood to be natural, essential, and unchanging, forever as they are now. Genealogy opens space for people to think otherwise. The point of this exercise is not to undermine the practices in question, but to enable people to engage the ethico-political choices of everyday life from a position of more, rather than less, options. As Clifford (2001) explains:

Nietzsche would argue to the effect that such "oughts" are, if not superfluous, then at least derivative. That is, an "ought" is always produced as a "symptom" of a situation that must be dealt with (or not), but which in itself has no moral value one way or the other... Perhaps an analogy will help to explain this. Suppose someone holds her hand directly above the flame of

a candle. Experience tells her that if she leaves her hand there, it will be burned. Is an “ought” really necessary to tell her what to do?... [W]hether she “ought” to move her hand or not will depend on the reasons her hand is here in the first place... Similarly, experience – this time referring to genealogical history – can tell us the story of dominations and subjections. No extraneous normative or rational standards are required to recognize dominations and subjections as such... One can recognize domination against the “standard” of open or closed possibilities. This recognition, which is made possible by genealogical critique, is morally neutral with regard to whether one “ought” to resist. One is faced with an arbitrary choice, but it is no more arbitrary than the domination itself... there is no transcendental basis for saying that it is better to be dominated than it is to be free. Yet one *can* recognize one’s subjection. When this subjection is judged against the immanent, neutral standard of open possibilities, is it really necessary to say that one “ought” to resist it? The choice, though arbitrary, is clear: accept your subjection, or resist it – move your hand, or let it burn... in fact, it is only the requirement of such normative standards itself that immobilizes us, since it precludes an arbitrary response to an arbitrary situation. The reality is that we choose freedom, or not, everyday (pp. 155-156).

The resistant practices of early HIV/AIDS patients discussed earlier illustrate this idea nicely. For those living under the shadow of a poorly understood terminal illness with no known treatment options, the chance of successful treatment is better than no chance at all. Whether, as a matter of policy, it is a good idea to make science subservient to the values of the community was entirely other to their

decision. From the vantage of early HIV/AIDS patients, the choice was clear: accept the chance of being randomized into a control group or challenge it, wait for new drugs to be approved for testing or protest the approval process. Viewed through the lens of traditional moral philosophy, these are arbitrary decisions in the sense that they are not derived from predefined normative standards. As Clifford reminds us though, the choice to resist, if arbitrary, is never any more arbitrary than the choice *not* to resist. These are real, lived practices of resistance to power, a desperate protest against victimization, a refusal to die quietly in the name of scientific objectivity. No matter what traditional ethical models might suggest, for those engaged in these practices of resistance the choice could not be any less arbitrary.

The Foucauldian critique is animated by an undeniable ethical impulse, but it is not one that can be reduced to the enumeration of static, transcendental normative standards. It is a critique premised not only on the rejection of such a priori standards, but the radical problematization of their possibility, on the demonstration that such things are *always* at least partially contingent, context-bound, and historically specific.

Psychiatry and psychopharmacology are morally ambiguous, ambivalent forces in the modern world, and the many questions raised here have no clear or easy answers. It is difficult not to be outraged by the appalling business practices of pharmaceutical corporations, and yet it is in no one's interest for these companies to fail. These companies have systematically undermined the fundamental purpose of our health care system, inserting their own financial interests into all levels of the decision calculi that determine patient care. They medicalize increasingly larger

portions of human experience in order to render affluent Westerners dependent, to convince us that we cannot live without consuming their products, while neglecting, if not actively harming, the world's poor. At the same time, life expectancies and quality of life throughout much of the world have grown at historically incomprehensible rates due to the efforts of these companies. They produce the medications and technologies that keep millions of people alive. It is only because of the pharmaceutical industry that so many infectious diseases have been effectively eradicated from much of the world, and it is pharmaceutical companies who are leading the way to develop new treatments for cancer and other terminal diseases.

### **The Expansion of Depression**

The complex of issues surrounding the expansion of the depression concept in *DSM-5* exemplifies the profound ambivalence of psychiatry. That the removal of the bereavement exemption amounts to an expansion of depression is undeniable, but the ethical, legal, social, and scientific questions surrounding this change have no easy answers. Where do the boundaries between normal grief and depression lie? We have seen how the depression concept has widened over time to pathologize experiences that once were considered perfectly normal, but the implications of this are less than clear. Would it really be more desirable to live in a world where it is considered normal and healthy to be unhappy all the time? By analogy, the criminalization of spousal abuse and marital rape transformed perfectly common forms of private behavior into legal matters, bringing new forms of human sociality under the gaze of the legal apparatus of the state, but few would argue that these are undesirable developments. Healy (1997) somewhat coyly notes that medicine has always tended to medicalize distress – this should not come as a great surprise to

anyone. If we are to have a productive discussion about the merits of medicalizing bereavement, it needs to take place on more specific grounds. What is it about these forms of distress that should or should not be understood as medical issues?

Ought bereavement be considered a form of psychiatric disorder? That label carries social and legal consequences that can follow someone for life. If the bereavement exemption is removed from *DSM-5*, someone diagnosed with depression after the death of a loved one will be forever unable to adopt children or work with certain populations. With a history of psychiatric disorder, she will be permanently prohibited from holding a great number of different jobs. The label of depression, however, can also serve a productive role, helping people to understand their experiences and enabling them to get pharmacological help. Should bereavement be treated differently from other stressors that are known to cause depression? We know that unemployment or bankruptcy, for instance, can cause people to experience symptoms of depression, but these people are not exempted from being diagnosed as depressed.

From a practical perspective, the primary question at the heart of the decision to remove the bereavement exemption from *DSM-5* is whether the recently bereaved should be eligible for treatment with antidepressant drugs. Is there an ethical difference between the medication of bereavement and the medication of other depressive symptoms? Our society clearly approves of self-medicating bereavement in ways that this genealogy has revealed to be much less different from modern antidepressant use than is commonly understood. If the grief of bereavement can be alleviated, allowing people to regain a semblance of normalcy

and functionality in their lives, why *not* take a pill to feel better? Is grief a desirable emotional process? Is it necessary?

These are but a few of the questions raised by the removal of the bereavement exemption from the *Diagnostic and Statistical Manual*. They speak powerfully to the ambiguity and ambivalence of our relationship with psychiatry and psychopharmacology. These are not questions that can be answered by genealogy alone. In fact, a genealogical analysis that leaves the reader with more answers than questions has failed its purpose. How to respond to the issues raised herein is an ethico-political choice that we each must make individually.

What genealogy *does* do is reveal exigencies such as this as moments of choice. It allows us to see the non-essentiality of those things that have come to seem natural and necessary. It rends open space to think otherwise. Genealogy reveals the contingency of the sedimented and naturalized structures that delimit our possibilities for action, and to see that the path we are currently on is not the only way forward. It is but one way among many, any appearance of necessity an illusion. Choose to resist the expansion of the depression concept, or not. Move your hand, or let it burn. Genealogy shows that no matter how one chooses to negotiate these issues, one is engaged in a process of choice. To be confronted with an instance of political exigency is to be propelled toward a moment of decision. In the end, genealogy reveals to us that no matter how one proceeds, the only thing one cannot do is *not* make a choice.

### **Conclusion**

Drawing on the work of Michel Foucault, this project has worked to expose some of the ways that power and knowledge function in contemporary society

through a genealogical analysis of depressive disorders in the upcoming fifth edition of the American Psychological Association's *Diagnostic and Statistical Manual*. This genealogy examined different understandings of illness and disease throughout history, tracing the development of biological psychiatry from the early asylum period. We looked at the evolution of depression as a psychiatric concept and we learned about the complex history and politics of psychiatric diagnosis. We examined the development of the pharmaceutical industry and the history of psychopharmacology, and we also explored the role that the *Diagnostic and Statistical Manual* has played in shaping the history of psychiatry. Through all of this, we have sought to create space for new ways of thinking about the removal of the bereavement exemption and its functions as a specific strategy in the workings of biopower.

Biopower, like psychiatry, is a deeply ambivalent, ubiquitous force in modern society, both dangerous and productive. How to proceed in light of the developments outlined here is a choice we each must make for ourselves. The upcoming publication of *DSM-5* is an exigency that calls out for response. It is my hope that the genealogical analysis here has enabled the reader to engage that ethico-political choice on more secure footing, "to trace new lines of making sense by taking hold of the sign whose reference has been destabilized by and through those practices of resistance" (Biesecker, 1992, p. 361).

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