Theor Soc (2011) 40:273–313
DOI 10.1007/s11186-011-9142-8

Where do classifications come from? The DSM-III, the transformation of American psychiatry, and the problem of origins in the sociology of knowledge

Michael Strand

Published online: 8 March 2011

Abstract When something serves a function, it is easy to overlook its origins. The tendency is to proceed directly to function and retroactively construct a story about origin based on the function it fills. In this article, I address this problem of origins as it appears in the sociology of knowledge, using a case study of the publication of the 3rd edition of the *Diagnostic and Statistical Manual of Mental Disorders* (DSM-III) in 1980. The manual revolutionized American psychiatry and the treatment of mental illness, because it served the function of classification that had become critical to the field of mental health by this time. But this function must be bracketed in order to reveal the "extra-functional" origins of the DSM-III. Using field theory, I argue that the manual was necessary for reasons other than the function it filled as a classification. Specifically, its origin lies in a series of conflicts among psychiatrists, psychoanalysts, and clinical psychologists within the field of mental health, which followed in the wake of the collapse of psychoanalysis as the dominant treatment type for mental illness. I reveal the generative formula behind the production of the DSM-III, capturing a variety of social processes that influenced the format of the manual and made it a useful classification, but which are not reducible to function. In this way, I reproduce its *raison d'etre* in a manner similar to how the DSM-III appeared for the people who produced it. This focus on generative formulas offers the sociology of knowledge a way to capture the epistemic importance of a range of different social processes. Most importantly, it avoids the functional fallacy of reducing origin to function, and ignoring the idea that innovations might appear necessary even without clear recognition of their functional consequences.

Keywords Psychiatry · Knowledge · Bourdieu · Technology · Professions

M. Strand (✉)
Sociology Department, University of Notre, 810 Flanner Hall, Notre Dame, IN 46556, USA
e-mail: mstrand@nd.edu

When knowledge is functional, it is difficult to distinguish between the knowledge and its function. This is particularly true in questions concerning the origins of knowledge. Why was it produced? Was it produced in order to serve its function? Classification is a form of knowledge, representing an ordering of symbolic information. It also serves a clear function, playing the role of "boundary object" that establishes a controlled consensus on meaning, which in turn synchronizes action across several different social domains (Bowker and Star 1999, pp. 297–298; Lakoff 2005, p. 86). In the case of classification, the function is particularly clear—not only the organization of action, but what things are and mean is determined relative to the *vi formae*, or "force of the form" latent to the classification itself. For *intrusive* practices, which are often susceptible to conflict—like those characterizing medical or legal roles—classifications serve the enviable function of "ontologically transmuting" otherwise contentious objects by publicizing and objectifying them, authenticating their existence in this particular form by enabling their public recognition (Bourdiu 1990, p. 85). In this capacity, classification is a "way of worldmaking," marking objects and practices as symbols that carry meaning, placing them inside a coherent system of categories, furnishing the concepts that allow different people to have similar perceptions, avoiding the skepticism about words because they now possess a literal meaning (Goodman 1978).

But the obvious function of forms of knowledge like classifications often leads analysts to overlook their origins. The tendency is to proceed directly to function and retroactively construct a "just-so" story or series of "in order to" motives that explain why classifications were necessary from the start. However, in their study of "primitive classification" (1963 [1903]), Durkheim and Mauss draw a clear distinction between logic and "extra-logical" origins. Classification, or grouping into hierarchies, separating and distinguishing between like and unlike objects, is necessary for logical thought; but, for Durkheim and Mauss, it is misleading to assume that because it is logically necessary, classification also has a *logical origin*. For them, classification has "extra-logical origins" because it corresponds to forms of social organization and the "sentimental distinctions" that enable the systematic arrangement of ideas (1963 [1903], p. 85).

In this article, I draw a similar distinction. Knowledge might appear necessary because it is functional; but it is misleading to assume that because it is functionally necessary it has a *functional origin*. If classifications are necessary because they synchronize action and meaning across diverse domains, it is possible they appeared necessary for *different* reasons for the people who produced them. This point does not dismiss the fact that functional origin and functional purpose often do match. But this relationship must be justified empirically. If Durkheim and Mauss (and later Bloor 1981) focused on "socio-centric" factors to explain the existence of classification without relying on its significance to logic, my concern in this article is even more local, focusing on a social configuration of relations and positions in a field (Bourdieu 1996), and how this made classification necessary for reasons other than the function it serves.

The classification in question is the 3rd edition of the *Diagnostic and Statistical Manual of Mental Disorders* (DSM-III). Published in the 1980, the manual offered the first extensive classification of mental illnesses and

revolutionized both the practice of psychiatry and subsequent views of mental disorder. Accounts of its origin tend to commit the *functional fallacy* outlined above, arguing that it was drafted either to allow the kind of *diagnostic specificity* useful for insurance companies seeking less ambiguity in illness classification and more efficiency in coverage of mental health treatment (Sabshin 1990; Wilson 1993; Cooksey and Brown 1998; Mayes and Horwitz 2005), or the *antibiotic specificity* useful for pharmaceutical companies seeking to target drug therapies for specific mental illnesses (Wilson 1993; Rogler 1997; Galatzer-Levy and Galatzer-Levy 2009).

In this article, I agree with these views to the extent that the DSM-III did provide both diagnostic and antibiotic specificity, and that these functions were primary reasons behind its success. But I argue that the manual emerged for different reasons, which were more proximate to the time and place of the people creating it. Specifically, psychiatrists struggled with psychoanalysts and clinical psychologists to control the field of mental health, and this struggle ultimately made it necessary for psychiatrists to produce a classification like the DSM-III. In other words, the DSM-III emerged for reasons *endogenous* to the field of mental health, but only by coupling these to *exogenous* changes related insurance and medical reimbursement, the deinstitutionalization of long-term treatment facilities, the implementation of community mental health policy, and public criticism over mental health treatment—which together allowed external interests to have more control over mental health and thus made the field more *heteronomous* (Benson 1999, p. 464)—can we understand why it was successful in prompting a "revolution" in mental health treatment (Horwitz 2002; Healy 2004; Mayes and Horwitz 2005). So while I find "extra-functional" origins for the DSM-III, I also argue that it succeeded precisely because it functioned as a classification, fulfilling each of the purposes outlined above, which became central to the field of mental health, and subsequently came to characterize the practice of a new, *diagnostic* form of psychiatry.

In what follows, I structure this argument by focusing on two trajectories: first, the trajectory of the field of mental health that saw the emergence of heteronomous conditions and resultant collapse of psychoanalysis as the dominant treatment type for mental illness. In this instance, heteronomy means that mental health became important for a more diverse collection of interests (like insurance, pharmaceutical, legal, educational) external to the field, which gained more control over the "content of work" of mental health professionals (Freidson 1970; Abbott 1988). For the reasons outlined above, this proved conducive to a classification like the DSM-III. Next, I recover the origins of the manual, arguing that a specific configuration of relations among psychiatrists, psychoanalysts, and clinical psychologists explain the development and publication of the manual, and why it was possible only among psychiatrists. Then, I review evidence that challenges functional arguments that the manual was developed *in order to* fulfill the functions of diagnostic and antibiotic specificity it would eventually serve. Finally, I link up the trajectories of the field of mental health and the relations among professional groups, arguing that the DSM-III, although its origin finds "extra-functional" reasons, fell into the role of classification that was highly valued in the heteronomous field by the early 1980s. Psychiatrists subsequently came to *speak for* the classification (Latour 1988; Shapin

1995; Pels 2000), which meant they controlled the form of capital that was dominant under these conditions in mental health treatment (Bourdieu 1986; Martin 2003). This sustained their control in the field of mental health, dominant over both psychoanalysts and clinical psychologists, even though psychiatrists had lost jurisdiction over psychotherapy.

But these are outcomes of the DSM-III—its origins find alternative reasons. As Bourdieu argues, the goal of field theory is to reveal a "*generative formula* whose knowledge allows one to reproduce in another mode the very production of the work, to feel *necessity* accomplish itself, even outside any empathetic experience" (1996, p. 303; emphasis mine). In other words, to understand something is to make it necessary. The DSM-III might seem necessary because it is functional, and identifying this function is a way of understanding it *by making it necessary*. But the historical evidence challenges this judgment, particularly when it comes to content of the DSM-III's "generative formula," where function *didn't* play a role. Instead, I argue that the generative formula behind the DSM-III, which made it necessary outside the function it fulfilled (and which subsequent editions continue to fulfill), draws largely from issues that emerge from the patterns of conflict between professional groups. In this way, I use field theory to understand the DSM-III by grasping the *raison d'etre*, or reasons why it was necessary, resting at its origin. As noted, my approach draws from Durkheim and Mauss (1963 [1903]), who introduce the difference between origin and function, posing it as a question to the analysis of knowledge and technologies that serve clear functions, but with origins that are neither clear nor reducible to those functions. In this way, I use field theory to develop a new approach to the problem of origins in the sociology of knowledge.

Psychoanalysis in American psychiatry

Psychoanalytic hegemony

The five lectures Freud delivered at Clark University in September 1909 were crucial to the history of psychiatry and mental health in the United States. Not only were they a "strategic moment" in the history of psychoanalysis, marking the most ambitious step in its proselytizing mission to date, they also marked a turning point in the struggle among various professional groups vying for control of the treatment of mental illness (Hale 1995, p. 5). It might seem extraordinary now to suggest that "one of the appeals of psychoanalysis for psychiatry was that it seemed to offer a chance for psychiatry to join the mainstream of medicine" (Robert Michels cited in Gliman 1987, p. 311). But the growing profession saw in psychoanalysis's *etiological* view of mental illness the key to establishing its own autonomous discipline and medical practice (Burnham 1967; Leveille 2002). This facilitated the kind of reception that would lead psychiatry, previously charged with the lowly duty of managing the insane, to redefine itself using psychoanalysis on the basis of clinical treatment, thereby capturing the jurisdiction over mental health treatment and establishing autonomous control over the content of the work.

By 1953, a conference of the leadership of the APA held at Cornell Medical School and organized by the newly-established National Institute of Mental Health (NIMH, est. 1949) to address the issue of psychiatric education could declare that

> A necessary part of the preparation of a competent psychiatrist is the development of an understanding of principles of psychodynamics, and it seems obvious that an understanding of psychodynamics presupposes indeed, necessitates … knowledge of Freudian concepts and of psychoanalytic theory and practice (John Whitehorn cited in Gabbard 2000, p. 103).

Although, at this time, they numbered only 500 among the approximately 7,000 psychiatrists in the United States (Shorter 1997, p. 173), psychoanalysts had moved into a position of dominance in the profession and the field. A 1954 study of major psychiatry training centers reported that "the orientation of most of the centers is described as Freudian or neo-Freudian; however, the resident's own orientation is apt to be in this direction regardless of his center's orientation" (Ward 1954, p. 123). By the mid-1960s, 58% of psychiatry departments were chaired by psychoanalysts (Hale 2000, p. 82), which was likely a slight decline from peak numbers in the 1950s. Over 70% of the 340 members of the APsaA (American Psychoanalytic Association) held positions in education in 1949, and psychoanalysts continued to focus heavily on training curriculum (Hale 1995, p. 246). Because analysts also wrote the textbooks and popularized works (among them August Aichorn's *Wayward Youth* and Gregory Zilboorg's *History of Medical Psychology* [Shorter 1997, p. 174]) that introduced psychiatry to aspiring undergraduates and medical students, it was their vision of the profession that assumed the symbolic capital of being institutionalized, among practitioners and public alike, as the way to be a practicing psychiatrist.

In this context, psychoanalysis thrived as psychiatry expanded after the war. The NIMH offered unprecedented support for psychiatric training and education. In 1951, 77% of medical schools offered psychiatric training for the full 4 years of medical education in departments of psychiatry that had carved out their own separate and powerful niche in medical education (Scully et al. 2000, p. 125). Psychiatric residency programs were also expanding at this time. From a paltry 758 slots open to hospital residents in psychiatry in 1946, the number jumped to 2,983 by 1956 (Grob 1991a, p. 98). Psychiatry was also attracting more medical graduates who would be paid better for their services as the profession's earning capacity began to climb.[1] This was a corollary of their continuing move out of the institutions and "into the office" of private practice (Grob 1991a, p. 98; Hale 1995, p. 247). All of this indicates a profession that, by the 1950s, was more independent and more important within the American medical community, and certainly the most powerful force in the field of mental health.

While psychiatry enjoyed, at this time, the largest growth-period in its history, it nurtured a generation of psychiatrists on a form of practice derived primarily from

[1] As Allan Hobson, professor of psychiatry at Harvard, and a 1959 graduate of Harvard Medical School, later recalled: "The reason twenty-five people from my medical school class went into psychiatry—*twenty-five people in a class of one hundred twenty-five*, where today it would be more like three or four—was because we all thought that psychoanalysis was the greatest thing since sliced bread. We were completely hooked" (cited in Dolnick 1998, p. 65).

psychoanalysis. Significantly, in 1946, the certifying body for psychiatry, the American Board of Psychiatry and Neurology, recommended that "psychodynamic" psychiatry be the standard of practice, and basis for board certification, that determined the qualification of training psychiatrists (Grob 1991a, p. 99). Officially, this was an important step in the move of psychoanalysis into psychiatry, but it only formalized what had been occurring on a practical level since the early 1930s.

Based on principles derived from analytic theory, psychodynamic psychiatry was adapted to meet the circumstances faced by private psychiatrists, which were much more urgent than those confronting the more protracted analytic treatment, and its much-maligned, in later years, "Young, Attractive, Verbal, Intelligent, and Successful," or YAVIS, clientele (as documented in August Hollingshed and Fredrick Redlich's 1959 study, *Social Class and Mental Illness*).[2] A 1959 APA commissioned survey of private psychiatrists found that, on average, they saw seven patients per day for 45 to 50 minutes each, with the length of treatment ranging from 3 weeks to 18 months of weekly or bi-weekly visits. The methods used by these psychiatrists were "psychological and non-directive." Unlike analysis, patients sat face-to-face with psychiatrists during therapy; and instead of free association, the patient was guided in a conversation that often included suggestions and advice. Despite these differences, the intention was still to probe a patient's psychodynamic or "primary thought" process. If this did not encourage transference, then it might provoke the kind of rational insight that, according to analytic theory, suppressed the "psychic tension" at the heart of behavioral disorder (Hale 1995, pp. 246–251).

The training required of psychiatrists also reflected the influence of psychoanalysis. Where earlier in the century, and even through much of the 1930s, the focus of psychiatric training had been on "administrative psychiatry" that emphasized techniques to manage the severely mentally ill, by 1953 psychotherapy had become the focus of residency training, and teaching sessions emphasized psychodynamic concepts derived from analytic theory (Scully et al. 2000, pp. 126–127). The most influential textbooks used in training bore a distinct psychoanalytic tone, including Lawrence Kolb's and Alfred Noyes's 1963 edition of *Modern Clinical Psychiatry* and Edward Strecker and Franklin Ebaugh's *Practical Clinical Psychiatry for Students and Practitioners,* published in 1957 (Hale 1995, p. 254). The influential Group for the Advancement of Psychiatry (GAP), many of whose members had both APsaA and APA memberships, led the official charge of psychoanalysis into psychiatric education, and by 1960 "virtually every chairperson of a department of psychiatry stated unequivocally that the psychodynamic frame of reference (as contrasted with the descriptive or organic) was dominant" in training and education (Grob 1991a, p. 100).

The structure of the field: alternative forms of practice

A number of different practitioners occupied the field of mental health at the time of psychoanalytic dominance. Some fulfilled functions allotted to them by psycho-

[2] For American psychiatrists, these principles were probably translated most into the practice of psychotherapy by Kurt Eissler, particularly his influential 1953 paper "The effect of the structure of ego on psychoanalytic technique," and Karl Menninger, particularly his 1958 *Theory of Psychoanalytic Technique*.

analysis, while others openly vied with it for control of the field. Psychiatric nurses and clinical social workers, for example, were subordinate in a division of labor that found psychoanalysts at the top (Grob 1991a, pp. 117–118). However, competition came from psychopharmacology, clinical psychologists, and a group of researchers focusing on diagnosis and nosology.

A 1959 survey published in the *American Journal of Psychiatry* (Maciver and Redlich 1959) distinguished between two camps of practitioners in psychiatry at the time: on the one hand, the much more numerous "analytic-psychological" practitioners and, on the other, a smaller group of "directive-organic" practitioners. These latter addressed patients afflicted with severe psychoses by applying "biological tools," including psychosurgery and psychotropic medication. However, with the development of meprobamate in 1950, later marketed as "Milltown" and "Equanil," the "cosmetic psychopharamacology" movement could begin in earnest (Shorter 1997, pp. 317–319; Grob 1991a, pp. 146–154). Prior to this point, the use of psychotropic medication focused largely on the treatment of psychotic patients. But as the psychiatrist Mitchell Wilson argues: it was the "*potential* [to develop] more effective treatment that might target discrete symptoms that held great promise" for psychopharmacology (1993, p. 403; emphasis original). The discovery of these more moderate applications expanded the clinical potential of psychopharmacology, and it increasingly became a viable treatment option, even for psychoanalysts.

Meanwhile, by the late 1950s, psychology had expanded out of academia and into the practical world of treating mental illness. A 1962 survey showed that, of the roughly 17,000 psychologists in the United States at the time, almost 70% classified their activity as "related to the field of mental health," with 41% of these devoting half their time to treatment using psychotherapy (Lubin 1962). This was the beginning of a movement that would eventually unseat psychiatry for jurisdiction over psychotherapy.

Finally, research into the nosology of mental illness, and the reliability and validity of clinical diagnosis, began to take shape in the mid-1950s based at the department of psychiatry at Washington University in St. Louis. This new "diagnostic" focus defined itself as an "empirical approach to psychotherapy" (Guze and Murphy 1963) and would eventually reorganize clinical practice on the basis of diagnosis. However, if psychiatric classification began in earnest here around the time of psychoanalytic hegemony, then it was primarily an aid to research, with few clinical implications (Blashfield 1984). As Mandel Cohen, among the earliest psychiatrists to use symptoms to classify mental illness (in the 1940s and 1950s), would later argue: "What we did in our research was to define precisely what symptoms you had to have for the condition—for example, it might be 6 out of 10 features. All we were saying was: if you want to repeat our work, this is what you've got to do" (Cohen and Healy 2002, p. 210). These research models marked an early precursor to the kind of diagnostic psychiatry that would eventually push into clinical practice, and to the head of psychiatry, during the 1970s.

While these actors foreshadowed changes to come, psychoanalysis and psychodynamic psychiatry were dominant in a period that saw a relative equilibrium between the 1953 recommendation by the APA leadership that psychodynamics and psychoanalysis be the basis of psychiatry to the 1972 publication of the "Feighner

criteria," which remained research-oriented, but was among the first, and certainly most influential (see Blashfield 1984, pp. 39–40), of such articles that used clinical observations to produce diagnostic criteria (Feighner et al. 1972). But psychoanalysis was secure in providing the model of practice for the rest of the field. Competitors made little headway when the field of mental health remained largely autonomous. These conditions allowed psychoanalysis to dictate the terms of work, and the expertise of mental health professionals was recognized on a more implicit and clinical basis. This coupling of autonomy and symbolic capital was effective while the field maintained relationships to exogenous actors that allowed for the lengthy therapy, selectivity of patients and practitioner-based standard of treatment characteristic of psychoanalysis. In the following section, I examine how the field of mental health changed, becoming more heteronomous, leading to the decline of psychoanalytic dominance.

From field autonomy to field heteronomy

In what follows, I outline four factors that changed this state of the field and challenged the authority of psychoanalysis. While the "community mental health" and "deinstitutionalization" policies initiated by federal and state governments beginning in the 1960s opened the field to more patients and different treatment demands, the development of third-party payment methods and the effect of a host of cultural and professional challenges, changed the standard of practice acceptable to the field. Collectively, these four factors moved the field away from the autonomous conditions where psychoanalysis could thrive and into a more heteronomous arena, where the classification of mental illness, and with it, the objectification of professional practice, developed by diagnostic psychiatry led this form of practice to take precedence.

Changes in state policy: deinstitutionalization and community mental health

As Table 1 shows, the type, frequency, and scope of patient care offered by mental health professionals changed dramatically between 1955 and 1971. In 1955, well over half the patient care episodes occurred in inpatient facilities. In 1971, only 40% did. Moreover, the number of patients seeking services nearly doubled over this time.[3] This suggests that, for private and public facilities alike, not only did the demand for mental health services rise dramatically over the span of psychoanalytic dominance in the field, but the kind of treatment required by patients also shifted—from a small population of institutionalized patients with severe disorders at the beginning of the period, they came to be applied more broadly, to patients with less serious disorders, by the end.

While many factors were responsible for this transition, the most important were the community mental health and deinstitutionalization policies instituted by federal

[3] Here, "patient care episodes" indicate the total number of residents admitted to inpatient facilities at the beginning of the year or those listed on outpatient clinics' "active" roster and admissions into both types of treatment as they occurred over the same year (Grob 1991a, p. 258).

Table 1 Number and rate per 100,000 of inpatient and outpatient care episodes at selected mental health facilities, 1955–1971

Year	Total (thousands)	Inpatient	Outpatient
1955	1,675,352	1,296,352	379,000
1965	2,636,525	1,565,525	1,071,000
1968	3,380,818	1,602,238	1,778,590
1971	4,038,143	1,721,389	2,316,754
Rate	Total (per 100,000)	Inpatient	Outpatient
1955	1,028	795	233
1965	1,376	817	559
1968	1,713	812	901
1971	1,977	843	1,134

Source: Adapted from Grob 1991a; b: 258

and state governments beginning in the early 1960s. Led by the "social psychiatry" treatment focus advocated by the GAP group, the APA had lobbied Congress since the late 1940s to institute community-based mental health policies on the federal level, building upon community-based projects already implemented by states like New York and California (Grob 1987, pp. 429–430).[4] Their efforts were rewarded in 1955 when the Joint Commission on Mental Illness and Health (JCMIH) was created to assess the nation's mental health care service provider system. In their final report, *Action for Mental Health*, published in 1961, the JCMIH advocated a host of initiatives to revamp the provision of mental health services, including two "community mental health" recommendations with important consequences for the field. First, the report noted the abysmal conditions in long-term treatment facilities and recommended "deinstitutionalization" policies that would cut the number of these institutions and the proportion of patients they treated. Second, they recommended establishing community mental health treatment clinics—approximately one for every 50,000 people—to take their place (Grob 1991a, pp. 204–205; Lamb 2000; Gronfein 1985). These clinics would focus on outpatient management of the severely mentally ill and prevention and treatment of minor mental disorder among the general public. The report anticipated a shortage of qualified psychiatrists, and so non-medical mental health workers, trained to apply basic psychotherapeutic treatment, would staff the new clinics, though the directors would be psychiatrists trained and qualified in psychoanalysis.

In 1963, many of these policies were enacted when President Kennedy signed the Community Mental Health Centers Act. Initially, the act was effective in realizing the policy recommendations. By 1977, over 650 CMHCs had been established

[4] As Robert Felix, director of the NIMH from 1949 to 1964 urged, psychiatrists must "go out and find people who need help and—that means, in their local communities" (cited in Decker 2007, p. 342). This was especially a concern for schizophrenia, which was thought to be preventable if treated during initial stages and would only exhibit full effects after a period of prolonged non-intervention. The "social psychiatry" treatment focus is perhaps best indicated by the fact that only 15% of the extramural research the NIMH sponsored was done by "biological scientists." In fact, the majority of research funding given by the NIMH during this time went to social scientists (Grob 1987, p. 425).

(Grob 2000, p. 236) and, as Table 2 shows, the "deinstitutionalization" of long-term facility populations increased dramatically in the following years, as did the number of psychiatrists employed in private practice. However, as staff numbers indicate, it quickly became clear how distant psychoanalysis and psychodynamic psychiatry had become from the demands of the field. Initially, the CMHCs were to be directed by psychiatrists approved by the APsaA. In practice, however, only 56% of them were by 1973, and this number dropped to a paltry 22% by 1977. Moreover, while the average number of full-time psychiatrists on-staff in CHMCs was 6.8 in 1970; it dropped to 4.2 only 7 years later, with much of the decrease coming in the loss of directorship positions (Grob 1991a, p. 256; Winslow 1979). Psychoanalytic expertise, it seems, could no longer so easily satisfy the new demands on mental health professionals practicing in these new conditions.

Clinical psychologists and social workers soon gained control of the CHMCs, and while they applied similar therapeutic techniques, the transition still suggests a growing *mismatch* between the pressures on mental health treatment and the treatment capabilities that psychoanalysis was able to provide. Medical schools couldn't produce enough new psychiatrists to maintain its position of dominance in a field of growing demand and urgency. The psychoanalytic training institutes continued to restrict the number of practicing analysts by making qualification longer, more difficult, and more expensive (Hale 1995, pp. 213–214, 276; Kirsner 2009). Furthermore, the psychiatrists and analysts who did begin to practice at this time did not employ techniques that could adequately address the problems they now confronted. Consequently, when "the psychoanalytic domain came to include a wider gamut of patients, the need for briefer therapy became acute and led to renewed criticism of the classical technique" (Hale 1995, p. 272).

Because psychoanalysis and psychodynamic psychiatry demanded relatively protracted and lengthy treatment, practitioners simply could not cope with the growing treatment pressures or meet the needs of new patients, whether inside the CMHCs or in private practice. Finally, with the deinstitutionalized population, the relative ineffectiveness of analytic techniques became increasingly evident and the use of psychotropic medication continued to be the treatment of choice for severe cases. Medication gained prominence as the difference in treatment outcomes between it and psychoanalysis became more visible.

The position of psychoanalysis—and the emphasis behind the Community Mental Health movement—at this time is most evident in the writings of Karl Menninger, probably the most influential American psychoanalyst in psychiatry and also founder and president of the GAP group. Suggesting that even Freud did not have enough

Table 2 Percentage of psychiatrists in private practice and percent change of inpatient population in mental health institutions for selected years

Year	1910	1921	1933	1941	1962	1970
% PRIVATE	3.2	7.3	31	38	50	66
% CHANGE	+ 19.3	−2.9	−9.9	−25.3	−37.4	−19.2
YEARS	1946–1955	1956–1960	1961–1965	1966–1970	1971–1975	1976–1980

Sources: Shorter 1997; Mayes and Horwitz 2005; Gronfein 1985

faith in the power of psychoanalysis, he argued that "the old point of view assumed that mental illness was progressive and refractory. The new point of view is that most mental illness serves its purpose and disappears, and does so more rapidly and completely when skillfully understood and dealt with" (1963, p. 2). Furthermore, it is a mistake to assume a strict distinction between the normal and the pathological. Psychoanalysis reveals that, in fact, "most people have some degree of mental illness at some time, and many of them have a degree of mental illness most of the time" (Menninger 1963, p. 33). For Menninger, psychoanalysis not only revealed the mass presence of mental illness, it also provided medical solutions to cure it—the most important being the "hope" provided to patients by psychoanalysts who alone understood "men's motives and inner resources … and the formidable power each of us holds to determine whether he lives or dies" (Menninger 1959a, p. 490).

These passages express the social ambitions, and obdurate faith in the healing powers of analytic treatment, characteristic of psychoanalysts at this time. But with the subsequent limitations revealed by the implementation of CHMC policy, which reflected this thinking, psychoanalysts like Menninger and the GAP group, who hoped to change the face of American mental health (indeed, American society) in the early 1960s, overextended their ambitions for psychoanalysis and, as it turns out, made promises they couldn't keep. As Alan Stone, APA president in 1976, put it: the intention to bring psychoanalysis to the masses had "carried psychiatrists on a mission to change the world, but had brought the profession to the edge of extinction" (cited in Wilson 1993, p. 402). As I argue below, when psychoanalysis became questionable, psychiatrists lost control over the ability to determine the effectiveness of therapeutic treatment and how it should be applied. The field of mental health became more heteronomous, in this instance, as a result of state policies that revealed the limitations of analytic treatment. This changed the definition of what made for effective therapy and challenged the perception of psychoanalysis as the ideal model for psychiatry.

Insurance and culture critique

The pressures on analytic treatment under these new conditions resulted from two other factors that also contributed to the transformation of the field: (1) growth of third-party payment for mental health services and (2) a host of cultural and professional challenges to the authority of psychoanalysis and the status of psychiatry more generally. Payment for mental health services changed considerably over the period of psychoanalytic dominance, and these changes are understood best in the context of the financial arrangement that characterized medicine as a whole during this period. Between 1950 and 1970, the medical work force expanded from 1.2 to 3.9 million people. Over this same period, national health care expenditures grew from $12.7 billion to $71.6 billion, or from 4.5% to 7.3% of GNP. By 1980, these figures were $230 billion, or 9.4% of GNP (Starr 1982, p. 335, 380). The establishment of national insurance coverage (albeit limited) with Medicare and Medicaid in 1966, and the emergence of Health Maintenance Organizations in the 1970s, marked crucial points in a trend Paul Starr has described as the "coming of the corporation" and the "waning of independent professionalism" in medicine over the course of this period (1982, p. 420).

These developments had an enormous impact on mental health, as public and private insurers became more responsible for payment. Between 1965 and 1980, the number of mental patients with private insurance coverage nearly doubled, growing from 38% to 68% (Hale 1995, p. 341). Federal and state responsibility for mental health services also grew over this period, reaching a high point in 1986 when, according to one NIMH study, almost 70% of the revenue generated by mental health service providers came from public coffers (Stoline et al. 2000, p. 351). Thus, as psychoanalysis retained tenuous a control over the field, a more instrumental third-party opinion was introduced into the relationship between psychiatrist and client that, because it was responsible for payment, gained more control over the type of services offered. The resulting antagonism between economic interests and the autonomous professional field was unmistakable. The growing influence of public and private insurers challenged the autonomous basis of mental health treatment.

As expected, private and public insurers focused much of their criticism on psychoanalysis. Most viewed analytic (indeed, all psychotherapeutic) treatment as a "financial bottomless pit" that drained resources while practitioners remained unable to demonstrate or explain the effectiveness of their techniques (Mayes and Horwitz 2005, p. 253). In 1975, a Blue Cross vice-president expressed the private insurers' concerns over psychoanalysis well when he stated that

> compared to other types of [medical] services there is less clarity and uniformity of terminology concerning mental diagnoses, treatment modalities, and types of facilities providing care.… One dimension of this problem arises from the *latent or private nature of many services*; only the patient and the therapist have direct knowledge of what services were provided and why (cited in Luhrmann 2000: p. 225; italics mine).

In 1977, Senator Jacob Javits expressed similar views when he confessed that "unfortunately, I share a congressional consensus that our existing mental health care delivery system does not provide clear lines of *clinical accountability*" (cited in Wilson 1993, p. 403; italics mine). Both statements reveal third-party dissatisfaction with the autonomous credibility of psychoanalysis. In the federal case, it culminated in the 1980 recommendation by the Senate Finance Committee that insurance support for mental health care be limited to those treatments determined by the Food and Drug Administration to be "safe and effective on the basis of controlled clinical studies which are conducted and evaluated under generally accepted principles of scientific research" (Marshall 1980, p. 35). This was perhaps the best indication of federal reluctance to support psychoanalytically-inspired treatment methods, in addition to the relatively stagnant federal grant monies allocated for mental health research throughout the 1960s and 1970s (Baldessarini 2000). Private insurers, meanwhile, continued to impose restrictions on coverage for psychotherapy and psychiatric inpatient or outpatient clinic care, becoming even more critical of psychodynamic psychiatry with the rise of the "managed care" paradigm and Health Maintenance Organizations (Luhrmann 2000; Scott et al. 2000).

Much of this skepticism mirrored the conclusions of the studies conducted during this period that failed to identify the superior effectiveness of psychoanalysis over other forms of treatment. The first of these studies, conducted in 1952 by the British behaviorist H. J. Eysenck, argued that psychoanalysis, or all "insight treatments" for

that matter, "were no more effective than no treatment at all" (Hale 1995. p. 309). As such treatments focused on neurotic patients, it was difficult to determine whether they were effective or not. Eysenck argued that most neurotic patients saw marked improvement in symptoms 2 years after their initial onset regardless of whether they received therapy or not. His study also raised concerns about the outcome criteria used by psychoanalysts and psychodynamic psychiatrists. As D.H. Malan of London's Tavistock Clinic asked: "What defines a successful treatment?" (Malan 1973). Under conditions of autonomy in the field, psychoanalysts determined the answer—with appropriately ambiguous results. Psychoanalysts insisted on more than the alleviation of symptoms, but researchers found little agreement on reliable criteria for treatment beyond this minimal standard. Such inconsistencies and ambiguities mattered little when the field was autonomous. However, they mattered more when supported by the pressures on treatment that emerged as part of the growing heteronomy of the field.

In 1961, Jerome Frank, a psychiatrist at Johns Hopkins Medical School and an open critic of psychoanalysis, took a somewhat different, but no less unfavorable view in his study of therapeutic treatments, *Persuasion and Healing* (1961). Here, he attempted to diminish the distinctions between different therapy types by arguing that all methods of psychotherapy (whether analytic or not) "heal" patients through persuasion. Since the therapeutic process was everywhere identical, with a respected "healer" arousing hope in a demoralized patient, restoring the sense of mastery, while decreasing the sense of isolation, one type of therapy could not be better than any other. This echoed the findings of the many "controlled comparative studies" of different treatment types, first conducted by the clinical psychologist Carl Rogers in the early 1950s. By the mid-1970s, these studies produced a "Dodo bird's verdict" concerning psychotherapeutic treatments: "everybody … won and all must have prizes" (Hale 1995, p. 314). In other words, there was no significant difference in outcome between different therapies. All of them were "effective" (according to the empirical measures used in these studies) and no single method was more "effective" than any other. A 1980 study corroborated this finding and fueled further skepticism regarding the diffuse power of psychoanalysis to determine mental health treatment (Smith et al. 1980).

Alongside these critiques came a host of cultural critiques of psychoanalysis and psychiatry more generally. Beginning with Foucault's *Madness and Civilization* in 1961, a series of philosophical and sociological studies followed, each attacking the symbolic control psychiatrists exercised when they defined and treated behavioral and mental abnormality. Important works here include Thomas Scheff's *Being Mentally Ill* (1966), R.D. Laing's *The Politics of Experience* (1967; among many others), and Thomas Szasz's *The Myth of Mental Illness* (1961).[5] While they all took critical views of psychiatry, the latter two fit best with the "antipsychiatry

[5] This list is, of course, far from exhaustive, and we should not underestimate the effect of other critiques of psychoanalysis at this time, particularly those that concentrate on Freud himself. As Nathan Hale suggests, "Freud-bashers" advanced "four major criticisms: first, that Freud was no scientist and never devised a reliable way to test or prove his conclusions; second, that he lacked personal integrity and fudged his data to fit his theories; third, that he was a poor, inconsistent theorist; fourth, that he established a cult, not an empirical discipline" (1999, p. 236).

movement" that emerged in many domains, including law, religion, and among ex-patients, beginning in the late 1950s (Dain 2000).

The movement led a vocal attack on the way psychiatry defined mental illness. This was clear in Szasz's influential argument that without a somatic basis for abnormal behavior, psychiatrists did not actually deal with "medical problems." Laing added to this "mental illness does not exist" view by arguing that insanity, specifically schizophrenia, was a normal reaction to intolerable social conditions, especially those found within the "emotional crucible" of the family. Outside the "antipsychiatry" movement, a similar critique was vividly depicted on the pages of *Science* when David Rosenhan's critical portrayal of psychiatric diagnosis, "On Being Sane in Insane Places," was published in 1973. More publicly, psychoanalysis took a hit when Daniel Osheroff, a successful kidney specialist, sued the Chestnut Lodge sanatorium near Washington D.C.—long one of the jewels of the psychoanalytic movement—for medical malpractice after it kept him interned for 18 months between 1978 and 1980. During this time, he underwent extensive analytic treatment that had little effect on his severe depression. After being transferred to a "drug therapy" hospital, however, he was given medication and his symptoms quickly disappeared (Klerman 1990; Healy 1997, pp. 246–247). His out-of-court settlement with the APsaA made national headlines and added to the media scrutiny of psychiatry that had been brewing since the 1973 vote by the APA membership to remove homosexuality from its official list of mental illnesses and surrounding John Hinckley's 1981 insanity plea and acquittal for the attempted assassination of President Reagan.[6]

Thus, the rise of third-party payers and the growing professional and cultural scrutiny of psychoanalysis and psychiatry indicate changes to the field in much the same sense as the community mental health and deinstitutionalization policies. Third-party payers, in particular, contributed to the field's heteronomy by introducing powerful actors and instrumental criteria that threatened practitioner's self-defined content of work. Meanwhile, the professional and cultural critiques challenged the symbolic capital of psychoanalysis as the most legitimate form of practice in the field. Given the limitations and disappointments revealed by the CHMC policy, psychoanalysis was vulnerable to criticism, and perceptions of psychoanalysis among mental health practitioners began to shift. From institutionalized "doxa," increasingly it seemed like an unaccountable "orthodoxy" (Bourdieu 1988). Its high position in the field became more inconsistent with its low-value expertise and rapidly declining symbolic capital. That the field became more heteronomous as a result of these factors is perhaps no better indicated than by the reaction of third-party payment plans, which immediately challenged the value of psychoanalysis that autonomous field conditions, and field-specific criteria for capital conversion, had made possible.

The rise of diagnostic psychiatry

So far, I have described how the field of mental health changed and why this ended the dominance of psychoanalysis. But I have not explained what took its place. The

[6] I discuss both the Rosenhan experiment and the homosexuality debate in further detail below.

discussion illustrates changes in the field of mental health that gave rise to conditions propitious for diagnostic psychiatry. But how did this form of practice emerge?

As a corollary of these changes, the rise of clinical psychologists and their growing control over psychotherapy might suggest that they took control of the field, assuming the necessary symbolic capital and reordering all of mental health treatment on the basis of their practice (Buchanan 2003). Clinical psychology had become a significant force in mental health by mid-1970s, with "behavior" and, in particular, "cognitive-learning" theories of therapeutic treatment exercising growing control over a splintered "psychotherapeutic jungle" (Mahoney 1977; Healy 2004, pp. 286–291).[7] This was part of a trend within the field of mental health that saw competing groups surpass psychiatry's strictly numerical advantage over other treatment practitioners by the 1970s.[8] However, as I argue below, it was from psychiatry that the form of practice eventually institutionalized in the field would emerge. From the relations between psychoanalysts and psychiatrists, and between psychiatrists and clinical psychologists, came a group uniquely suited to satisfy the heteronomous conditions in the field that caused the collapsing authority of psychoanalysis. But why did the struggle turn out this way?

The publication of the DSM-III provides answers. What diagnostic psychiatry offered was a form of clinical practice that, significantly, remained medical and also rendered psychoanalysis obsolete. As I argue, important for this transition was a different definition of mental illness—one that separated "phenomenology from syndrome" and pathology from patient (and their personality). But it was the structure of relations among competing professional groups that led psychiatrists to these conclusions and toward diagnosis and classification more generally.

As the above discussion suggests, the field of mental health was heteronomous at this point. More parties were implicated and interested in the field than ever before. In these circumstances, classification was of premium value. However, in what follows, I show that the classification and objectification of professional practice represented by the DSM-III was not created in response to these circumstances. Rather, by focusing on specific contests between professional groups—specifically the homosexuality debate and the Rosenhan experiment—I show how it was from relational influences, and the conflicts they engendered, that the DSM-III, and the innovative view of mental illness and professional practice behind it, ultimately emerged. In the new, heteronomous circumstances, constructing classifications that could yoke together the profession and various external actors became the capital with the highest value (i.e., most "convertible"). This meant that diagnostic

[7] With over 130 varieties of "psychosocial" therapy by the late 1970s (Hale 1995. p. 355).

[8] In 1975, there were approximately 26,000 psychiatrists practicing in the United States and 15,000 clinical psychologists. By 1985, clinical psychologists surpassed psychiatrists, numbering approximately 33,000 to their 32,000. By 1990, the gap grew even more with 36,000 psychiatrists practicing in the United States compared to over 42,000 clinical psychologists. While this transformation is significant enough, over the same period the number of clinical social workers grew enormously, from 25,000 in 1975 to over 80,000 by 1990. Certainly this reflects the impact of the community mental health initiatives begun in the early 1960s. Moreover, marriage and family counselors, a little known specialty in 1975 with only 6,000 practitioners, grew to over 40,000 by 1990, indicative of the emerging "psychotherapeutic jungle" and the increasingly specialist and directed application of psychotherapy (Goleman 1990; Shorter 1997; Kirk and Kutchins 1992).

psychiatrists would take control of the field of mental health. However, as I argue, this *consequence* was not also the *cause* of the innovations that made it possible.

A brief history of psychiatric classification

First, it is useful to review what classification was like before the DSM-III. The first edition of the DSM followed the wake of World War II. The frequency of mental illnesses common among soldiers revealed the inadequacy of the prior *Statistical Manual for the Use of Institutions for the Insane*, which was created in 1918 to meet Census Bureau requirements and had few clinical implications. The War Department, assisted by the GAP group, thus formed its own classification system during the war, and it bore the heavy influence of the analytic perspective (Plant 2005; Grob 1991b; Spitzer and Williams 1985).

After the war, the APA drafted the DSM-I. As a nosological manual, it objectified psychiatry's otherwise implicit knowledge base. However, with the analytic influence strongest in the field, the DSM-I was minimally concerned with diagnosis and had few clinical implications. The DSM-II was much similar. Written to meet World Health Organization (WHO) requirements, it featured more diagnoses and more diagnostic categories than the DSM-I. However, it reflected the influence of psychoanalysis even more than the prior manual. Disease descriptions were minimal, and more definitional than diagnostic. Much like its predecessor, the DSM-II's diagnostic categories usually recapitulated analytic theory and "made little effort to provide elaborate classification schemes, because overt symptoms did not reveal disease entities but disguised underlying conflicts that could not be expressed directly" (Mayes and Horwitz 2005, p. 250; Spitzer and Williams 1985).

Much like the DSM-II, the development of the DSM-III was initiated by a mundane bureaucratic requirement that came with US membership in the WHO, which meant that the APA's classification of mental illness needed to be compatible with the taxonomy produced in the *International Classification of Diseases*, at the time in its 9th edition (ICD-9). In 1974, Robert Spitzer, a 1957 graduate of NYU medical school who received analytic training at the Columbia University Institute, was appointed to head the "Task Force on Nomenclature and Statistics" that would draft the new manual. To psychiatrists and practitioners in mental health as a whole, the publication of a new diagnostic manual was of little concern, at least for clinicians (Sabshin 1990; Spiegel 2005; Healy 1997). That Spitzer would start from the beginning, ignore the precedent of prior DSM editions and the ICD-9, and include hundreds of experts on over 14 committees for 6 years to produce the 500-page manual indicates the magnitude that he, the Task Force, and eventually the rest of psychiatry, associated with it. Between 1977 and 1979, he conducted field trials sponsored by the NIMH in which approximately 400 psychiatrists used drafts of the DSM-III, developed in structured interviews with clinicians, but mostly by Spitzer and the members of the Task Force themselves, to diagnose the symptoms of more than 12,000 patients. The results were analyzed for reliability between practitioners using a "kappa" statistic that was initially developed to construct research diagnostic criteria, and which produces reliability scores (from 0 to 1) by testing observed diagnoses of a specific disease by several clinicians against the frequency of the same diagnosis expected at random (Blashfield 1984, p. 96; Kirk and Kutchins

1992). During the field trials, the Task Force continually adjusted the operational criteria that defined the diagnoses included in manual in order to achieve higher kappa-scores, and thus greater uniformity and reliability in the clinical setting.

That Spitzer was able to conduct such an extensive project in a profession dominated by clinicians who cared little for classification indicates, to some extent, organizational changes that recently occurred within the APA itself. Spitzer's appointment to the Task Force came from APA president-elect Judd Marmor, a British-born psychoanalyst who received a medical degree from Columbia University in 1933 but subsequently moved into the heterodox Los Angeles community for his analytic training, and who, at the time, was also president of the GAP group. Marmor broke with the analytic community over the homosexuality issue, as he publicly supported its removal from the APA's listing of mental disorders (Bayer 1978, pp. 110–111). In this, he found a ready ally in Spitzer who, as consultant to the task force that drafted the DSM-II, led the effort within the APA to remove homosexuality as a mental illness (Decker 2007, p. 351).

While the issue remained contentious, Spitzer emerged with a reputation as a skillful compromiser, and endeared himself both to Marmor and, perhaps more importantly, to Melvin Sabshin—a graduate of Tulane University Medical School who received analytic training at the Chicago Institute—who, in 1974, was appointed medical director of the APA, a post he would hold until 1997 (Kirk and Kutchins 1992, p. 98). While critical of psychoanalysis, Sabshin's primary concern was the "ideological divisions"[9] he saw plaguing psychiatry, which had erupted over the homosexuality issue. To address these concerns, he focused on the DSM-III. After campaigning to get Spitzer appointed to the chair of the Task Force that would produce the manual, Sabshin used his senior post in the APA to support a project that, according to Task Force member Theodore Millon, he saw as "constructing an empirically grounded and functional classification system … a scientifically sound and clinically useful instrument …" that would unify the profession (1986, p. 29).

The NIMH also offered much assistance to the project. Its support for the failing community mental health policies had discredited the organization, and by the early 1970s, it bounced from agency to agency within the federal bureaucracy before finally finding a home, much reduced, within the newly-formed Alcohol, Drug Abuse and Mental Health Administration (ADAMHA) in 1974 (Kolb et al. 2000, p. 219). The Nixon and Ford administrations had, by this time, removed much of its mandate by turning most policy-making power on these issues over to state governments. All of this left the NIMH "in search of a new agenda." It latched onto the "mental health policy dominated by the treatment of specific illnesses"

[9] As he would later call them: "By the middle of the 1960s, American psychiatry was characterized by multiple ideological divisions in which little communication occurred between or among the various groups. While the biological psychiatrists tried to cling to their medical roots, they often became rigid in this defense and were not very persuasive. Limited as they were to unspecific medications and such treatments as ECT, insulin, and even lobotomy, they often found themselves regarded poorly within their profession and by the public at large. The psychoanalysts and social psychiatrists on the other hand … moved away from the medical model. Their demonstration of only minimal interest in epidemiology and in development of treatments based on nosology marked the nadir of psychiatrists' demedicalization in the United States … it also marked a period of massive public confusion about the differences among clinical psychologists, psychiatric social workers, and psychiatrists" (Sabshin 1990, p. 1270).

(indicative, of course, of the DSM-III) in an effort to redeem itself within the federal bureaucracy (Horwitz 2002, pp. 76–77).[10]

Drafting the DSM-III

With resources and support from both the APA and NIMH, Spitzer's task force moved forward. Indicating the low regard that a new version of the DSM held in the profession, Spitzer was allowed to appoint anyone to the Task Force he wanted, and he did, selecting a majority of those with connections to the "neo-Kraepelinian" school at Washington University and at his own institute in New York to the central Task Force, and including many more in outside consultations and on the advisory committees overseeing the 12 major areas of classification (Healy 2004, p. 302). Predictably, the disproportionate membership on the Task Force prompted criticism from within the profession and from occupational groups in the field as a whole. Psychoanalysts and clinical psychologists in particular voiced concern over the magnitude of the project and what crucial contents it seemed the resulting manual would not include. For instance, "neurosis" was a potential omission fought over by psychoanalysts and clinical psychologists alike. The term would eventually be included in the DSM-III, but only by *fiat* of the APA leadership, and then only minimally, in parentheses and never far from the word "disorder" (Bayer and Spitzer 1985). This is only one example of many similar nosological and terminological compromises that saw the DSM-III through to publication in 1980. As Spitzer would later recall: "the entire process … seemed more appropriate to the encounter of political rivals than to the orderly pursuit of scientific knowledge" (cited in Mayes and Horwitz 2005, p. 262).

The DSM-III's impact was unusual for a profession and field apparently so little concerned with nosology. However, it can be understood given the state of the field and the struggles in psychiatry occurring at this time. The diagnostic view of clinical practice reflected a researcher's view of psychiatry. The two articles that set the groundwork for the clinically-oriented DSM-III—entitled "Diagnostic criteria for use in psychiatric research" (Feighner et al. 1972) and "Research diagnostic criteria" (Spitzer et al. 1978)—were proposals introducing methods to researchers interested in the codification of mental illness. In this instance, the papers emphasized the *reliability* of the inferences used by psychiatrists in diagnosis.[11]

Meanwhile, the manual deliberately neglected an etiological focus that tried to identify the sources of mental illness, or just the kind of theory at the heart of psychoanalysis (Kirk and Kutchins 1992). Eventually, this would be justified by the slogan "diagnosis is prognosis" and various somatic sources credited for the origins of disorder (Horwitz 2002, p. 108). But initially the diagnostic group was reluctant to speculate on the origins of the symptoms they classified, even "organic" origins.[12]

[10] The strategy worked for the NIMH, as the 1980s saw rapid increases in funding (Baldessarini 2000). In 1992, it once again became a branch of the National Institutes of Health.

[11] Among other reasons, this was to prevent a similar embarrassment as the one that occurred with the US-UK Diagnostic Project (Kendell et al. 1971) in which the unreliability of diagnosis was revealed when US psychiatrists were shown to be far more likely to diagnose schizophrenia than their British counterparts (85% to 7% in one case).

[12] As the introduction to the DSM-III stated: the "DSM-III is generally atheoretical with regard to etiology … and only rarely attempts to account for how the disturbances come about" (APA 1980, p. 7).

Instead, it sought to "eliminate the disarray that [had] characterized psychiatric diagnosis" (Bayer and Spitzer 1985, p. 187) up to that point by linking a set of distinctive symptom criteria to a set of fixed and discrete disorder categories. In this regard, such intentions to objectify and clarify professional practice shared strong affinities with exactly what the stakeholders confronting the field wanted mental health professionals to do (Bowker and Star 1999).[13]

But if the organizational conditions propitious for diagnostic psychiatry were set, who was the carrier group for the new practice? In this case, the "neo-Kraepelinian" school led the development of diagnostic psychiatry (Blashfield 1984; Hudgins 1993; Decker 2007).[14] Named for the nineteenth-century German psychiatrist Emil Kraepelin, who was born the same year as Freud (1856) but whose diagnostic work was a precursor to their own, the school was based at the Department of Psychiatry at Washington University in St. Louis, and had strong ties to Spitzer's institute in New York—together forming what Roger Blashfield early on identified as an "invisible college" (1982). The department always had a strong neurological basis but began to expand into psychiatry at the same time psychoanalysis took control of the profession. It became a haven for contrarian psychiatrists, among them Eli Robins and George Winokur who were instrumental in extending diagnosis, based on similar "operational criteria" used in research, into clinical practice. Rising from isolation in the 1950s to prominence in the 1970s, the department cultivated the view that psychiatry needed a model of practice "that limited itself to the description of the mentally ill and avoided speculation about etiology because [for them] it was unknown for almost all psychiatric diseases" (Decker 2007, p. 345). The research articles published by the group laid the groundwork for the kind of intrusion into clinical practice represented by the DSM-III.

Classification: from homosexuality to David Rosenhan

To explain the appeal of classification, we need to understand psychiatry's role in specific struggles with clinical psychology and psychoanalysis. As mentioned above,

[13] As Lakoff (2005) argues, much of the appeal of something like the *DSM-III* for interests external to the field lies in its ability to render something like mental illness "liquid." The clarity and explicitness of the document enables a range of activities to develop around the practice of mental health, perhaps in much the same way as maps of the new world set the stage for the global expansion of economic activities and political control or what the human genome project might make possible in the future (see Wood 2004).

[14] Kraepelin was known for devising the first clinically-oriented nosology system, dividing functional psychotic disorders into three major groups, each with its own subcategories: dementia praecox, manic-depressive illness, and paranoia. Though Kraepelin was masterful at describing symptoms, from which he created his nosology, he also speculated on the etiological basis of the diseases he classified, though maintaining the view that little could be known in this area until knowledge of the "normal" working brain became more comprehensive. This fed into his critique of psychoanalysis, which almost parallels later critiques developed by the neo-Kraepelinians: "We meet everywhere the characteristic fundamental features of the Freudian trend of investigation, the representation of arbitrary assumptions and conjectures as assured facts, which are used without hesitation for the building up of always new castles in the air ever towering higher, and the tendency to generalization beyond measure from single observations. … As I am accustomed to walk on the sure foundation of direct experience, my Philistine conscience of natural science stumbles at every step on objections, considerations, and doubts, over which the likely soaring tower of Freud's disciples carries them without difficulty" (cited in Decker 2007, p. 340).

the significance of these conflicts is evident in a changing view of mental illness and a changing object for the expertise of psychiatrists. This perspective would guide the production of the DSM-III and forward the appeal of classification that made diagnostic psychiatry so powerful in the field. The conflicts in question are, first, the debate over homosexuality that occurred in the early 1970s and resulted in the 1974 referendum by the APA general assembly to remove homosexuality's status as a mental illness; and, second, the clinical psychologist David Rosenhan's 1973 experiment that tested, and lampooned, the validity of psychiatric diagnosis. In both instances, the reaction by psychiatrists—in particular, Robert Spitzer—focused on the possibility of diagnosis and operational criteria defining pathologies and ordering them in a classification, a problem once limited to research, but now seeming more applicable to clinical practice. This set the stage for the project implemented by the DSM-III.

The status of homosexuality as a mental disorder reflected the control of psychoanalysis in psychiatry. The DSM-I classified homosexuality as a "sociopathic personality disorder," while the DSM-II listed it alongside other sexual deviations—like fetishism, pedophilia, and transvestitism—as a "non-psychotic mental disorder." Behind these classifications was the psychoanalytic theory of sexual development, which saw homosexuality, in Karl Menninger's terms, as "evidences of immature sexuality and either arrested psychological development or regression … there is no question in the minds of psychiatrists regarding the abnormality of such behavior" (cited in Bayer 1978, p. 39). This fit within the framework of a psychoanalytic view of mental illness—influential among clinicians—summarized by Menninger's "unitary view" of illness types:

> Suppose that instead of putting so much emphasis on different kinds of illness we tried to think of all mental illness as being essentially the same in quality and different, rather, quantitatively … Mental illness, then, is an impairment in self-regulation whereby comfort, production and growth are temporarily surrendered for the sake of survival at the best level possible, and at the cost of emergency coping devices. Psychiatrists [then] are apt to look upon mental illness as an indication of ego failure … The emphasis is on the degree of disorganization and its course or trend of development, and the factors determining this trend (Menninger 1959b, pp. 526–527).

From this perspective, identifying a mental illness presumes a developmental (and etiological) argument that implicates the patient. Mental illness is a symptom of ego failure that indicates psychodynamic impairment or "regression." Psychiatrists treat mental illness by correcting the deviation and restoring the ego to proper functioning—in this instance, by eliminating the coping device and helping patients "adjust" by restoring their capacity for self-regulation. Based on this view, psychoanalysts were strongly in favor of retaining the mental illness status of homosexuality, stating their position in terms that reiterated Menninger's central claim: homosexuality was a symptom of ego failure, constituted by an "impairment" (or, as Charles Socarides put it, a "quiescence") in the development of a "natural" sexual object-choice. As psychoanalysts spoke for the unconscious, which outlined the terms of what was pathological, homosexuality remained a mental illness regardless of what homosexuals said about it.

Gay and lesbian activists had long opposed the classification, but only gained headway starting with the 1970 APA convention in San Francisco. Notably, they were allowed to conduct a panel on homosexuality at upcoming conventions (Bayer 1978, p. 104). Consequently, access to the profession became the focus of subsequent demonstrations, employing tactics similar to those Steven Epstein (1995; 1997) calls the exercise of "lay expertise" to advocate for removal of homosexuality's illness status. It was at this kind of demonstration (referred to as a "zap" but akin to a protest/seminar) in New York in October 1972 that Spitzer first became aware of the opposition to the classification. Significantly, he was attending a discussion on therapeutic techniques where activists challenged the psychoanalysts on the panel to acknowledge their latent anti-homosexual bias (Bayer 1978, p. 116).

For Spitzer, most remarkable in this confrontation was the difference (and apparent paradox) between the official classification as "mentally ill" and the absence of *subjective distress* among the diagnosed.[15] Homosexuals emphasized their well-being, even as psychoanalysts, and nosology in the DSM-II, insisted on their impairment. As Spitzer would later put it: such instances revealed the difference between "a syndrome and a phenomenology [that clinicians should judge] independent of disability …" (2000: 420). Subsequently, he advocated for removal of homosexuality's illness status by arguing that while homosexuality might be a "form of irregular sexual development" to classify it as a psychiatric disorder applies only to "individuals whose sexual interests are directed primarily toward people of the same sex *and who are bothered by, in conflict with or wish to change their sexual orientation*" (cited in Bayer 1978, p. 128; emphasis added). Spitzer reiterated this argument in a position paper published in the *American Journal of Psychiatry* immediately before the APA's Board of Trustees voted in approval of the amendment in December 1973: "For a mental or psychiatric condition to be considered a psychiatric disorder, it must either regularly cause distress, or regularly be associated with some generalized impairment in social effectiveness or functioning … The only way that homosexuality could therefore be considered a psychiatric disorder would be the criteria of failure to function heterosexually …" (1973, p. 1215). The eventual change in classification status reflected this argument as it consisted of the replacement "sexual orientation disturbance"—or a patient's subjective disturbance with her own sexual orientation—for homosexuality as the official disease category. The amendment was approved by referendum of the APA general assembly in May 1974, with 58% of APA members in favor and 37% opposed (Bayer 1978, p. 148).

Compared with the psychoanalytic perspective outlined above, Spitzer's new definition bypassed the paradox created by the official presence of mental disorder but absence of subjective distress or impaired functioning by decoupling the psychoanalytic "unity" of mental illness and *separating the patient from the pathology.* As he would later argue, among the virtues of the DSM-III was that it allowed room for the diagnosis of controversial or ambiguous conditions like homosexuality, by confirming that "it is the *patient* who judges" (Spitzer et al. 1980: 154; emphasis original). The pathology of psychiatric conditions would not be

[15] Important for Spitzer's view on this matter was Marcel Saghir and Eli Robins's 1973 study *Male and Female Homosexuality: A Comprehensive Investigation,* which specifically tested the alleged pathology in terms of subjective impairment, and not according to a theory of etiology.

decided by an etiological theory that projects a level of normative functioning. Instead, clinicians would cross-list illness-types with reported symptoms and the patient's phenomenology of them. Psychiatrists would decide on the appropriate placement among possible categories, but *patients* would determine the pathological status of their own condition. In this instance, separating the patient from the pathology not only undermined the analysts' position of speaking for the unconscious, it also articulated the basis for a new object that psychiatrists could speak for: instead of patients, now disease classifications, which clinicians *attach* to patients who now speak for themselves.[16]

Spitzer's response to the "Rosenhan Experiment" was indicative in much the same way. To summarize the experiment: David Rosenhan, a clinical psychologist at Stanford at the time, sent 8 colleagues, none of whom had a history of mental illness, to 12 different mental hospitals around the country. At an agreed time, the researchers presented themselves to the hospital staff on duty and reported that they heard a recurring voice saying the word "empty," "hollow" or "thud." Of the 12 separate diagnoses, 11 found schizophrenia. Once admitted to the hospital, the "pseudo-patients" acted normally, evincing no other symptoms of the diagnosis. However, the average time spent interned by the experimenters extended to 19 days, with the longest lasting a full 52 days. During this time, their normal behavior was often recorded as symptoms of their disease. For instance, the hospital staff took note of several experimenters' proclivity for "writing behavior," branding as pathological pseudo-patients taking notes on their experience in the hospital. Even family history, reported accurately, was recorded as a cause of the present illness. Other patients tended to recognize the "pseudo-patients' normality" almost immediately, while hospital staff never did. As Rosenhan and the others learned, the only way to convince the staff of their "normality" was to agree with the diagnosis, submit to treatment, and then act as if they were making progress toward overcoming the disorder. Eventually, the experimenters were released, but only once staff psychiatrists believed their schizophrenia was "in remission" (Rosenhan 1973).

For Rosenhan, the study demonstrated the danger of diagnostic labels for the treatment of mental illness. Once the hospital staff branded patients with a disorder type, everything confirmed it. Pseudo-patients were insane only for the presence, in the hospital environment, of "global diagnoses" (1973, p. 257). In this regard, the experiment revealed the tendency for diagnostic labels to create "false positives," or "type 2 errors" whereby clinicians, armed with a diagnostic category, brand normal behavior pathological (Rosenhan 1973, p. 252). From Rosenhan's perspective, the solution to these problems was clear: drop diagnostic labels and focus—like clinical psychologists do—on the patient's behavior (1973, p. 257). Instead of treating mental illness as a medical problem that needs to be classified and "cured," psychiatrists should concentrate on managing behavior, finding "the stimuli that

[16] For example, consider the following statements: first, "He is a schizophrenic"; second, "He has schizophrenia." The first reflects the analytic point of view; the second reflects the diagnostic. In the first, the patient is subsumed by the disease; in the second, he is attached to a disease. In the first, the patient is coupled to a diseased identity-type; in the second, the disease is an object definable on its own, which the patient possesses. It is detachable from him and not implicated in all of his behavior (like a broken leg). This illustrates Spitzer's position. Here, we also see traces of what made diagnostic psychiatry seem more medical.

provoke [disturbances] … their correlates" (1973, p. 254) and recommending lifestyle changes that break the associations that cause the problems.

Rosenhan's study, published at the height of the "anti-psychiatry" movement and in the high-profile venue *Science*, caused a storm of controversy. Among the most pointed critiques came from Sptizer (1975), and here he reveals a second principle at the basis of diagnostic psychiatry. Spitzer restates Rosenhan's conclusions as the following: "Rosenhan's study [only] proves that pseudo-patients are not detected by psychiatrists as having simulated signs of mental illness" (1975, p. 451). In other words, Rosenhan did not find that diagnostic categories create mental illness; instead, he found that psychiatrists couldn't distinguish phony patients who *were* actually faking it. If this just restates the initial finding, then Spitzer claims Rosenhan could go no further. His conclusions could not follow from his data. If the concern is diagnosis, then Rosenhan doesn't actually challenge the validity of diagnostic categories, because his experimental design allows for only one conclusion: The patients presented themselves to a hospital, they seemed in distress even though they could not point to a precipitating event, and they were hearing voices. Given these criteria, "there is only one remaining diagnosis … and that is schizophrenia" (Sptizer 1975, p. 446).

If Rosenhan's intention was to expose diagnostic categories as invalid because they do not correspond to reality, then, for Spitzer, the study fails because it does not ever challenge diagnostic validity. In fact, the study just reveals that the only way to produce valid diagnoses of mental illness is by testing *clinicians* for the uniformity of their judgments, not patients for the presence or absence of a disease. Beyond the initial finding, the study is irrelevant to diagnostic validity, because, for Spitzer, the most interesting possibility did not take place: Patients presented the same symptoms to different clinicians, and those clinicians made the same diagnoses.[17] Had they made different diagnoses, this would raise a problem of validity. But since their judgments were the same, Rosenhan's study merely verifies the operational criteria for schizophrenia. In terms of diagnostic validity, it did not matter that he and the other pseudo-patients were, in reality, normal; what mattered were clinicians forming the same impressions of them. Here, Spitzer confronts a direct assault on the value of diagnostic categories, and defends them by *separating diagnosis from correspondence*, or from patients actually having mental illnesses. Valid diagnoses did not have to correspond to reality in order to be valid. They only had to apply diagnostic categories assembled from clinical agreement.

In these two cases, then, Spitzer takes positions against psychoanalysis and clinical psychology—his opposition consisting of principles at the basis of diagnostic psychiatry. For instance, psychoanalysis accounted for the etiology of mental illness by speaking for the patient's unconscious; but its position on homosexuality revealed the limitations of this perspective. Spitzer responded by separating patients from pathologies, and making clinical expertise a matter of connecting illness-types to reported symptoms. Clinical psychology, meanwhile, argued against the validity of diagnoses by demonstrating that they could not reveal (and likely produced) patient illness. Spitzer responded by redefining psychiatric diagnosis, breaking the connection between diagnosis and correspon-

[17] Except for the pseudo-patient diagnosed as manic-depressive, who Spitzer does not mention.

dence and making reliability, or uniformities in clinical judgment, the standard for validity.

Thus situated between psychoanalysis and clinical psychology, or between *theory* and *empiricism*, psychiatry would concentrate on *form*. Rather than speak for a universal process underlying mental illness or document the "correlates" that associate with problem behaviors, psychiatry would distill a schematic representation from symptoms and clinical criteria, reconstructing illnesses as illness-types. When coupled to a clinical emphasis on diagnosis, this makes a thorough and extensive classification system, mapping the universe of mental illness and thus codifying clinical practice, a possibility. But only because the field was constituted of these positions could diagnostic psychiatry emerge with these characteristics. From his critiques of psychoanalysis and clinical psychology, Spitzer *bootstrapped* diagnostic psychiatry into a position on clinical practice, finding an interest in a specific kind of classification, now revealed as the ideal model for mental health treatment.

The DSM-III

As noted above, this positioning against clinical psychology and psychoanalysis is evident in the DSM-III's most distinctive, and consequential, characteristics. As the introduction to the DSM-III states, the purpose of the manual is to "provide clear descriptions of diagnostic categories in order to enable clinicians and investigators to diagnose, communicate about, study and treat various mental disorders" (APA 1980, p. 12). Organized around 13 primary disorder groupings, the DSM-III lists specific diseases and the operational criteria that diagnose them.[18] As Gerald Klerman (1978) suggests, the information structure is reminiscent of a "Chinese menu" approach to diagnosis: the DSM-III requires that an illness be diagnosed by identifying (for instance) at least three of six symptoms in Category A, at least two of four in Category B, four of seven in category C, and so on.[19] Furthermore, while the 13 primary diagnostic classes are largely discrete, they are also hierarchical: "the order in which diagnostic classes are listed represents, to some extent, a hierarchy in which a disorder high in the hierarchy may have features found in disorders lower in the hierarchy, but not the reverse" (APA 1980, pp. 8–9). So organic mental disorders share criteria with psychosexual disorders, but not vice versa. The former should be referenced until anomalous symptoms indicate the latter.

The DSM-III also includes a multiaxial system of evaluation "to ensure that certain information that may be of value in planning treatment and predicting outcomes for each individual is recorded …" (1980, p. 9). So a complete diagnosis might look like:

Axis I: Clinical Syndrome (attention of treatment): *Academic Problems* [V62.30]
Axis II: Personality Disorder (adults) or Developmental Disorder (Children): *Attention Deficit Disorder with Hyperactivity* [314.01]

[18] These include: Disorder usually first evident in infancy, childhood or adolescence; organic mental disorders; substance use disorders; schizophrenic disorders; paranoid disorders; affective disorders; anxiety disorders; somatoform disorders; psychosexual disorders; factitious disorders; adjustment disorder; personality disorders.

[19] Like a placing an order from a Chinese menu: two of three items in Category A, one of four in Category B, one of three in Category C, and so on.

Axis III: Physical Disorders and Conditions: *Type I Diabetes*
Axis IV: Severity of Psychosocial Stressor: *Severe* [code 1]: *divorce of parents.*
Axis V: Highest Level of Adaptive Functioning: *Level 5: Poor: fails a grade-level.*

At the end, a complex picture of the patient emerges, including diagnosis (ADHD), initial syndrome, stressful events preceding the illness (Divorce), and how well the patient functions with the disorder. The intention is for the operational criteria and information structure to lead clinicians toward the same diagnosis, and thus the same prognosis and treatment method. In this regard, the DSM-III is structured to assure a reliable *placement* of patients in the classification.

As Spitzer later indicated, "information variance" was the most critical problem the Task Force confronted in developing the DSM-III (Spitzer et al. 1980). This variance concerned the information clinicians elicited from patients, their clinical interpretations of symptoms, and the characteristics they associated with specific diagnoses. Defining reliability as "agreement among clinicians on assigning diagnoses to patients," the DSM-III attempted to ensure that a uniformity of information be involved in each clinical exchange (Spitzer et al. 1980, p. 154). Part of the solution included the "diagnostic interview schedule," structured to elicit classifiable information from patients (Endicott and Spitzer 1978). This was among the principal means of codifying clinical practice and making it more compatible with the classification.[20]

But the principal issue raised by information variance concerned the problem of "coverage," or the certainty that the system contained enough complexity to classify patients with adequate specificity, and prevent such skewed diagnoses as all (or none) being "schizophrenic" or "depressed." The Chinese Menu system helped address this problem, enforcing symptom proportions (two of five, three of six, etc.) to discriminate between possible classifications. But the primary solution to the coverage problem came with the proliferation of new diseases.

As Samuel Guze later recalled, the problem of coverage became a problem of avoiding "the alternative [which] was to have a lot of undiagnosed cases" (2000, p. 407). The issue concerned the validity of the DSM-III. Undiagnosed cases meant non-coverage and non-knowledge. If the validity of a diagnosis rested solely on the ability of the classification system to place patients into categories, then undiagnosed cases presented the sort of *structural* lacunae that challenged the validity of every category. If cases went undiagnosed, it meant anomalous symptoms went unaccounted for. Their presence might alter the relative adequacy between categories in covering symptoms, which determined the placement of patients. Consequently, the Task Force introduced many new diseases in order to secure the validity of diagnoses, now by ensuring that potential symptoms be included under at least one illness-type.

This marked a significant departure from the research origins of the neo-Kraepelinians. For instance, the Feighner criteria (1972) identified 16 diagnostic

[20] The significance of the structured "interview schedule" for clinical practice using the DSM classification would eventually lead Spitzer to argue that the burden was on clinicians to show that they weren't "superfluous in the task of diagnostic assessment" in comparison to computers (Spitzer 1983).

categories; and the book that provides the most immediate precursor to the DSM-III, Donald Goodwin and Samuel Guze's *Psychiatric Diagnosis* (1974), used a thorough review of existing research and clinical literature on mental illness to produce 12 diagnoses. The DSM-III, meanwhile, yielded 265. If validity for the neo-Kraepelinians meant something akin to proving the *existence* of a mental illness through clinical work and "laboratory studies" that verified the boundaries and symptoms associated with a potential disease category, then, for the DSM-III, validity became strictly a matter of use. As noted above, Spitzer positioned diagnostic psychiatry against empiricist concerns first by denying that accounting for the causes of mental illness was necessary for diagnosis, and second by redefining validity against the behavioral concerns of clinical psychology. In this regard, the move toward classification restricted questions about validity to the clinical domain. It did not matter whether diagnostic categories matched mental illnesses actually in the world. For diagnostic psychiatry, what mattered was a classification system that *covered* all clinical possibilities. With classification the principal object, the DSM-III had to be extensive, even if most of the diseases were unknown outside of clinical opinion.

Meanwhile, the DSM-III's definition of mental illness reveals the diagnostic attempt to remain medical, even though disease was understood strictly according to symptoms, subjective distress, and demonstrated disability:

> In DSM-III each of the mental disorders is conceptualized as a clinically significant behavioral or psychological syndrome or pattern that occurs in an individual and that is typically associated with either a painful symptom (distress) or impairment in one or more important areas of functioning (disability). In addition, 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 society. (When the disturbance is *limited* to a conflict between the individual and society, this may represent social deviance, which may or may not be commendable, but is not by itself a mental disorder) (APA 1980, p. 6, emphasis original).

Here, mental illness is associated with an inference toward a primary dysfunction, and thereby consistent with psychoanalysis as a branch of medicine. However, accounting for this dysfunction is not necessary for the validity of a diagnosis. By attaching the syndrome (Axis I) to an enduring psychological disorder (Axis II) that supercedes possible triggering "stressors" (Axis IV)—moving the disturbance beyond merely "the relationship between the individual and society"—the DSM-III keeps psychiatry medical despite the absence of a clear etiology. But the validity of attaching a syndrome to an illness is still decided by reliability in clinical judgment, or by the frequencies (kappa-scores) among diagnoses of the same symptoms. Although the DSM-III incorporates medical "inferences" into the diagnostic framework, the connections are limited to *bundles of symptoms* that provide the operational criteria that collectively define the illness.

Consequently, the DSM-III keeps psychiatry medical by connecting illness-types to syndromes and inferring that the syndrome results from the illness-type, even though illnesses are understood according to symptoms whose grouping also

constitutes the syndrome. In other words, *everything* becomes a matter of symptoms: the syndrome, the diagnostic criteria, the illness itself. This keeps the validity of diagnoses within the domain of clinical opinion, because it bypasses the necessity of an etiological inference that would identify a *non-symptomatic* source of pathology. As a result, medical treatment for the DSM-III becomes the control and elimination of symptoms, which now means the management and cure of diseases. Predictably, clinical psychologists criticized the DSM-III on these grounds for making diseases out of "problems in living" and what they perceived to be diagnostic categories *posing as diseases* (McReynolds 1979; Smith and Kraft 1983).

The "distress test" is also contained in this definition, as impairment or subjective distress is necessary for diagnosis. While reporting distress clearly demonstrates the role of the patient in deciding pathology, even disability is redefined according to the patient's lifestyle, as a composite "previous level of adaptive functioning" before the symptoms began (APA 1980, p. 28). This drew criticism from psychoanalysts, who argued that the DSM-III leaves illnesses as mere "descriptive groupings" (Frances and Cooper 1981; Cooper and Michels 1981), thus conflating symptomatology with diagnosis. Predictably, psychoanalysts saw the overemphasis on description as resulting from the DSM-III's dismissal of unconscious process. Without primary process, diagnostic categories only "freeze" illnesses and misrecognize disturbances, now from an inability to look "behind the symptoms."

In this regard, the manual elicited much the same response from psychoanalysts as the reclassification of homosexuality did several years before. In both instances, eliminating primary process meant separating the patient from the pathology.[21] If homosexuality was a mental illness only for those disturbed by the orientation, then the DSM-III similarly relies on the patient to decide the pathology status of symptoms, in this case by voicing distress or demonstrating disability. In place of a psychoanalytic understanding of unconscious process, the DSM-III introduced a relationship among diseases, symptoms, and patients who signal pathology. This bypassed a theory of psychopathology like the developmental perspective of psychoanalysis by placing the emphasis instead on the patient's judgment of "distress" in the phenomenology of symptoms. The normative, or the contrast for pathology, was now defined according to patients, referencing a lifestyle before the illness and not a theory of unconscious process. Mental illness now *interfered* with patients, rather than indicate their culpability in not attaining some optimum level of functioning. Consequently, patients were implicated in the illness only through *their* identification of symptoms as "pathological."

Thus, we see the significance of diagnostic psychiatry's positioning against clinical psychology and psychoanalysis for the *format* of the DSM-III. What these relationships yielded was a diagnostic perspective on clinical practice, and a view of mental illness that separated pathology from patients and diagnosis from correspondence. The outcome is a form of psychiatry that derives illnesses from descriptive groupings and diagnoses them by classifying symptoms. As I have argued, these principles—meaningful only from within the context of relationships

[21] For example, Task Force member Theodore Millon described the resolution of the dispute over "neurosis" as an issue "cleverly finessed by separating the concept 'neurotic disorder' from that of 'neurotic process' … or [separating it from] a sequence of intrapsychic conflicts" (1983, p. 807).

between these different groups—are evident in the most distinctive features of the DSM-III, and with their emphasis on coverage and symptoms, also proved most consequential to its impact on the field.

Furthermore, the DSM-III represented what Andrew Lakoff (2005) calls a "solidification" of psychiatry's objects. Spitzer later recalled that discrepancy in diagnosis between clinicians "was a great source of embarrassment to American psychiatry" at this time. Furthermore, the extension of the classification to cover "categories people were using but were not officially accepted [helped] legitimize" the diagnostic approach. Defining discrete and explicit diagnostic categories also made credible the claim that mental illnesses were medical diseases. Psychiatry "looked more like a medical specialty" after the DSM-III (2000, pp. 429–430). From this, Spitzer concludes that the DSM-III was most significant because "it defines what is the *reality*. It's the thing that says *this is our professional responsibility*, this is what we deal with" (2000, p. 427; emphasis mine). In other words, the DSM-III solidified what were once *moving* objects when it furnished a new domain of *classifying* objects that psychiatrists could speak for.

Whither psychology and psychopharmacology?

As noted above, psychiatry retained control of mental health with the emergence of the diagnostic approach and the publication of the DSM-III. With the entrance of multiple and varied stakeholders into the field of mental health, the DSM-III produced the classifications that sufficed for communication, for bureaucratic work, and for yoking government, insurance, and patient interests together with the profession (Bowker and Star 1999). These circumstances raised the value of this kind of expertise, and the professional capital of classifiers, once so marginal in the field, now became paramount in treating mental illness.

But a few questions still remain. First, with numerical superiority and control over psychotherapy by the late 1970s, why didn't clinical psychology take control of the field of mental health? Second, what role did psychopharmacology play in the development of the DSM-III? Finally, how were third-party payers involved in the DSM-III?

Clinical psychology and psychotherapy

As noted above, clinical psychology made inroads into psychiatry's control of psychotherapy as early as the 1950s. With psychiatrists overextended by the community mental health initiatives, clinical psychologists moved into these openings. When studies of treatment techniques produced inconclusive results about the effectiveness of psychotherapy, clinical psychologists became more legitimate in providing the service. While psychiatry attempted to "medicalize" psychotherapy during this period, and legally limit the practice by nonmedical psychologists, they were never able to succeed, mostly because they could never adequately define psychoanalytic treatment or explain how it worked (Buchanan 2003, p. 234). A good indication of the waning skirmish over psychotherapy in favor of clinical psychology

came in 1980, when a landmark case in the US District Court of Appeals of Virginia found that therapeutic services rendered by clinical psychologists, but billed to insurers via "supervising" psychiatrists, was unnecessary and anticompetitive (Greenberg 1980). All of this meant that, by the 1980s, clinical psychologists controlled the practice of psychotherapy.[22]

However, based on my account, it would appear that, by this time, controlling psychotherapy was a moot point in terms of controlling the field of mental health. The most valued professional capital no longer concerned the practice of psychotherapy, but the classification of illness. In other words, clinical psychology captured psychotherapy too late. In field-theoretic terms, their interests were long overdue: they focused on psychotherapy, or what *used* to be dominant in the field of mental health. They could not *find an interest* in classification, because they were not positioned to do so.

This becomes clear in the differences between psychiatry's and clinical psychology's reaction against psychoanalysis. If diagnostic psychiatry rejected psychoanalysis by separating the patient from the pathology and producing illness-types, clinical psychology did so by becoming more empirical. This followed the pattern set forth by early critiques of psychoanalysis by psychologists like H.J. Eysenck and Carl Rogers whose objections to the clinical benefits of analytic treatment focused on the way psychoanalysts subsumed mental illness and successful treatment entirely into models and terms derived from analytic theory. This was behind the observation that analytic treatment actually made little difference to patients when outcome measures were removed of psychoanalytic concepts and more closely associated with *observable* behavioral differences created by the therapy (Hale 1995, pp. 309–312).

Consequently, their position relative to psychoanalysis led clinical psychologists to focus on behavior, not classification. An indication of this is found in clinical psychologists' critique of the DSM-III, which tended to reiterate the problems—association of behavior with "illness," diagnostic categories that were unreliable because they defined illness-types that lie beyond observable behavior—found earlier with psychoanalysis. This suggests clinical psychology's aversion to forms of treatment that focus on classifying or subsuming behaviors under categories.[23]

In its move against psychoanalysis, then, clinical psychologists started down a path that led them toward a close attunement with behavior—observed directly and not derivative as an indicator of a process identified by analytic theory—and how

[22] Perhaps indicative of concessions on the part of psychiatrists in the jurisdiction over psychotherapy, Thomas Hackett, professor of psychiatry at Harvard Medical School, suggested in 1977 that "apart from their training in medicine, psychiatrists have nothing unique to offer that cannot be provided by psychologists, the clergy, or lay psychotherapists. Our bread and butter—the practice of psychotherapy—has fragmented into multiple schools, all with uncertain boundaries" (1977, p. 434).

[23] This is even despite the fact that the American Psychological Association initiated its own Task Force on Descriptive Behavioral Classification in response to the DSM-III (Smith and Kraft 1983, pp. 777–778). The Task Force was charged with exploring the possibility of developing an alternative manual for clinical, teaching, and research purposes. Significantly, only a third of clinical psychologists reported hearing of the initiative shortly after it began in 1981, and an even smaller percentage reported they would use the alternative manual if it were ever published (Miller et al. 1981, p. 388). The Task Force was terminated shortly thereafter.

psychotherapy could create behavioral differences.[24] This focus on behavior followed an opposite path from the one that led psychiatrists toward classification. If they dropped the etiological and developmental theory that implicated the patient, psychiatrists did retain a medical focus from psychoanalysis. This led them to speak for *more than behavior* when they subsumed behaviors and symptoms under the canopy of illness-types and subsequently recommended treatments aimed at a *cure*—understood now as eliminating the symptoms constituting the illness.

Thus, if classification was valued most in the field, then clinical psychology's aversion to it, and contrasting taste for psychotherapy, prevented them from taking control. Alternatively, psychiatrists' interest in classification allowed them to retain control, despite losing psychotherapy. In this respect, "[psychiatrists] could be seen to have lost many battles yet, paradoxically, never lost the war" (Buchanan 2003, p. 246). This paradox becomes less puzzling when we understand the *match* between the demands of the heteronomous field and the classifications produced by diagnostic psychiatry.

Psychopharmacology and antibiotic specificity

Second, what role did psychopharmacology play in the transformation? As many suggest (Wilson 1993; Buchanan 2003; Healy 1997, 2004), the development of psychotropic medication for treatment of less serious disorders was a crucial step in the transformation of psychiatry. Eventually, it became tightly coupled to the diagnostic approach. But I have argued that the emphasis on diagnosis and classification emerged from other sources.

As David Healy (2004, pp. 283–284) shows, "randomized controlled trials" (RCTs) became the foundation for psychopharmacological research during the 1950s. Essential to this process was "antibiotic specificity," or the "magic bullet" correlation between specific illnesses and the pharmacological agents that target them (Galatzer-Levy and Galatzer-Levy 2009, p. 166). Finding this correlation required homogenous patient populations, which were necessary as "test" populations to isolate a potential drug's effect. Such homogeneity was already evident in patients with serious psychoses like schizophrenia and severe mania, and the oldest antipsychotic medication, chlorpromazine (Thorazine), was introduced after demonstrating drastic effects in controlling the distinctive symptoms associated with them. But for more subtle illnesses, a classification like the DSM-III—with its discrete illness-types and coverage of clinical problems—served the purpose of homogenizing and *specifying* otherwise ambiguous cases. This need for specification became more important following the 1962 Kefauver-Harris amendments to the Food, Drugs and Cosmetics Act that changed the requirements of drug licensing, shifting the

[24] Perhaps this explains clinical psychology's sustained fascination with psychotherapy, and initiation of original therapy types when treatment is not contained within a holistic theory comparable to psychoanalysis, but concerned instead with realizing ("tweaking") behavioral differences: "behavioral," "cognitive-learning," "cognitive-behavioral," "humanistic," "feminist," "interpersonal," "systems," "existential," even "psychodynamic" are among just a few among the toolkit of theoretical orientations (for therapeutic practice) available for clinical psychologists (Bloch 1996). Meanwhile, a 1988 lawsuit brought by clinical psychologists against the APsaA finally opened the doors of the most restrictive analytic training institutes to non-medical personnel. This was perhaps the last step in ending the "medical monopoly" over psychotherapy (Buchanan 2003: 244).

criteria from demonstrations of safety to demonstrations of effect, which now had to be specified (Healy 2004, p. 323).

Certainly these circumstances made a classification system that creates the possibility of antibiotic specificity in mental illness attractive to pharmaceutical companies developing pharmacological agents that targeted mental illness. There is much truth to the argument that the growing influence of pharmaceuticals in mental health was essential to the transformation of psychiatry that occurred around the publication of the DSM-III. But did pharmaceuticals pressure psychiatrists to produce a classification that provided the kind of antibiotic specificity that made drug development more efficient? There are reasons to doubt this claim.

For instance, consider a specific illness-type: the introduction of "Major Depressive Disorder" by the DSM-III, likely the most profitable category ever defined by psychiatrists. As a result of Spitzer's break with psychoanalysis, the term neurosis—perhaps the thickest term in the analytic lexicon with regard to personality and etiology—became problematic for the new classification and was completely absent from initial drafts. This provoked a furor among psychoanalysts on the Task Force and the analytic community at large who threatened to remove crucial pockets of support for the DSM-III (whose approval rested on a vote by the APA general assembly) if the concept wasn't included.[25] So Spitzer split the "Depressive" category in two, introducing, on the one hand, "Major Depressive Disorder," and, on the other, "Dysthymia" (a term he introduced) or "(Depressive Neurosis)." The latter was dubbed "less severe" than the former, but the operational criteria were so similar, and so ambiguous in the case of Dysthymia, that while the difference was officially a difference "in degree," it was meaningful only as a political move meant to ensure passage of the DSM-III by placating psychoanalysts, even though as we have seen the manual constituted a fundamental opposition to analytic principles (Bayer and Spitzer 1985).

Consequently, "Major Depressive Disorder" became overwhelmingly favored in diagnosis of "depressive" symptoms, which were already among the most inclusive in the DSM, making the disorder among the most frequently diagnosed. Subsequently, a variety of psychotropic treatments targeted Major Depressive Disorder, exploiting what Edward Shorter identifies as a "drug versus psychotherapy" opposition encoded into the split between the two types (2009, p. 163). But this was possible only after the category emerged, and for reasons only meaningful within circumstances endogenous to the profession itself. If this applies to the illness-type that has likely been the most significant for the interests of pharmaceutical companies, then similar scenarios likely played out involving other categories, in each case endogenous reasons prompting the emergence of classifications that pharmaceutical companies only later exploited. Indeed, similar stories have been observed involving Risperidal ("schizophreniform disorder") and Xanax ("panic disorder") (see Shorter 1997; Healy 1997).

While this indicates the importance of the DSM-III and diagnostic psychiatry for *indirectly* creating the context favorable to psychopharmacology, it is also significant

[25] Most important was the Washington, D.C. metro area, which supported an unusually high number of psychoanalysts due to federal employee health benefits with generous mental health provisions, including ample support for analytic therapy (Bayer and Spitzer 1985, pp. 190–191).

that psychopharmacology was a popular treatment option *before* the DSM-III was introduced. As noted above, the use of psychotropic treatment became more prevalent among psychiatrists, even during the period of psychoanalytic dominance. It became especially popular after the community mental health and deinstitutionalization initiatives introduced new patients with different treatment demands into outpatient care. This is reflected in a survey of hospital and outpatient practitioners published in 1978, which found that "the psychiatrist who did not prescribe drugs … had become the exception." The study also found that "psychiatrists almost routinely prescribed drugs for patients who were treated by other mental health professionals not licensed to administer drugs" (Redlich and Kellert 1978, p. 26).[26] While this suggests the division of labor between psychiatrists and allied mental health professionals that persists to the present-day, it also disputes the claim that psychopharmacology needed the DSM-III in order to become a viable treatment option. While the DSM-III had a significant impact on the growth of psychopharmacology—in particular, by allowing illness target "markets" (Healy 2004; Shorter 2009)—the fact that it could suffice without the classification challenges the idea that the pharmaceutical industry would manipulate the development of the DSM-III to ensure antibiotic specificity, rather than take advantage of psychiatry's interest in classification as it emerged from other sources.[27]

Insurance and diagnostic specificity

Finally, although psychiatry depended on third-party payment by the time the DSM-III was published, there is reason to question the significance of insurance interests for the emergence of the classification. As noted above, among the most difficult aspects of third-party payment for mental health treatment were insurance provider fears of "moral hazard." Patients were frequently suspected of abusing the "insurance concept" by receiving treatment for voluntary or "enjoyable" reasons, seeking to realize "improvements" like self-actualization or, in particular, psychoanalysis, instead of the "named perils" that insurance protected against (Hall 1974; Sharfstein 1978). Thus, mental health benefits were typically more restrictive compared with insurance policies for other medical disorders.

The DSM-III was subsequently attractive to insurance providers because it contributed the kind of diagnostic specificity that could address the inefficiencies that arose from situations conducive to moral hazard. By specifying disease categories, it demarcated medical from non-medical cases. It also made "yes/no"

[26] Among the most prominent drugs used prior to publication of the DSM-III, and before the subsequent emergence of Selective Serotonin Reuptake Inhibitors (SSRIs) like Prozac, Paxil, and Zoloft, were the meprobomates Milltown and Equanil, the benzodiazepines Valium and Librium. This was the beginning of a trend that would eventually make psychiatrists the most frequent prescribers of medication among all medical practitioners (Shorter 2009).

[27] Spitzer later recalled, in response to the question of whether pharmaceuticals influenced the DSM-III: "Absolutely no. I mean in what specific way? Some people would have said that with panic disorder we were trying to make [the pharmaceutical company] Upjohn happy. That's not why we had panic. We had panic disorder because [Task Force member] Don Klein convinced us that it was different from generalized anxiety disorder. Alprazolam [i.e., Xanax] was never part of our discussion and I don't see how psychopharmacology would have influenced the DSM. I know it was never part of any discussions" (2000, p. 426)

answers possible to the question of whether patients actually had mental illnesses. This gave third-party payers reason to restrict coverage to medical disorders only, allowing greater protection from what they perceived as insurance fraud committed by the "worried well" or patients seeking improvements in lifestyle.

But like psychopharmacology, third-party payer arrangements, both public and private, were already established by the time the manual was in development. Significantly, the APA had addressed the problem of insurance claims *before* the publication of the DSM-III. A task force consisting of APA members and insurance representatives, including medical directors from several of the largest medical insurance companies in the United States, developed the *Psychiatric Coding System for Insurance Claims Reporting* in the years prior to the DSM-III.[28] While addressing problems of confidentiality voiced by patient groups, the coding system also specified the kind of disorders that qualified as medical problems and thus were applicable to insurance policies. Distinguishing between six categories of reportable conditions, the coding system classified illness-types by severity of impaired functioning. This demarcated medical from non-medical in mental disorders and thus clarified the limitations of medical insurance coverage. As third-party payers sought a clear indication of "medical necessity" in mental health treatment, categorical distinctions were starkest between those categories that qualified patients for insurance coverage and those that did not. If disputes arose concerning the correct illness classification and coverage responsibilities, the system also provided a basis for peer review to decide how to place ambiguous cases (Sharfstein 1987, p. 534; Wallerstein and Peltz 1987, pp. 701–702).[29]

While the DSM-III expanded the number and specification of illness-types, this other coding system, developed independently and before the DSM-III was published, satisfied many of the concerns held by third-party payers that might have led them to pressure psychiatrists for a classification. Certainly the DSM-III helped insurance companies restrict coverage and address the issue of moral hazard—probably more than the coding system itself. But with the major problems addressed in collaboration with the APA, and written into a coding system that was legally-binding and more directly consequential for insurance obligations than the DSM-III—all taking place *before* it was published—the focus seems removed from the DSM-III itself. Thus, it seems reasonable to consider whether insurance providers merely took advantage of psychiatry's newfound interest in classification, after it had emerged from within the profession itself.

The reception of the DSM-III

So far, I have demonstrated why clinical psychology did not find classification a favored strategy in its professional struggles with psychiatry and psychoanalysis, but instead tended toward psychotherapy. I have also pointed to shortcomings in alternative explanations for the emergence of the DSM-III that focus on the

[28] The coding system is now included as part of the CPT (or Current Procedural Terminology) Handbook.
[29] For his part, Spitzer contests that "insurance issues had no effect. They never came up in discussion. The insurance people were always happy with DSM-II, they never complained about it. I suppose they like DSM-III, maybe more, but there was never any way in which we tried to fashion the manual to make it more acceptable for insurers" (2000, p. 426).

determination of powerful actors outside psychiatry to see the manual published, and why these explanatory problems should lead us to consider the significance of factors endogenous to psychiatry in order to account for the innovation in mental health treatment represented by the DSM-III. But a final question remains: How was the manual received?

Predictably, the DSM-III proved favorable to insurance providers and pharmaceutical companies. This is expected given the function of classifications, which allow actors outside a profession to benefit from the profession's work (Bowker and Star 1999). Previously, the absence of a real classification system in American psychiatry had limited important aspects of insurance and pharmaceutical involvement in mental health treatment, although they still remained influential in the field. But when the DSM-III was published it realized a *converging interest* between these stakeholders and diagnostic psychiatrists now armed with a comprehensive classification. If this made plausible previously impossible insurance and pharmaceutical designs for mental health, it did so by tying psychiatry together with the most powerful stakeholders in the field, thus catapulting the classifiers to the head of the profession.[30] Furthermore, with more interest in mental health, and more potential ties to develop through a classification than ever before, the pieces were in place for the DSM-III to have a revolutionary impact. Based on the manual's reception, and the impact of diagnostic psychiatry more generally, this appears to be exactly what happened.

As Rick Mayes and Allan Horwitz describe it: "The change it wrought was quick, thorough and irreversible" (2005, p. 263). In the 6 months following the DSM-III's publication, order requests for the manual amounted to more than the total requests received by the APA for the prior two DSM editions combined, even including the 30-plus reprintings for each. By the mid-1980s, the following could be observed:

> American medical schools and residency programs routinely expected students and physicians to pass examinations based on DSM-III criteria. Both referees and journal editors expected manuscripts submitted to scholarly journals to be written in its language, and it was simply assumed that psychiatric research proposals would conform to its conventions. Researchers and clinicians who resisted these conventions could assume that they would be excluded from these arenas and their resources (Young 1995, p. 102).[31]

[30] This process can be conceptualized in terms of Andrew Abbott's (1988) systems model of the professions as a new "jurisdiction" or "form of work" opening up, and psychiatrists filling it. However, while this provides a way to understand the success of diagnostic psychiatry, it still does not account for why diagnostic psychiatry or, in particular, why the DSM-III emerged, apart from crediting the outcome—or they emerged to "fill a jurisdiction." In this respect, Abbott's model falls victim to the same problems as explanations that rely on the power of pharmaceuticals and insurance companies to explain the case.

[31] In his review of the changing role of psychotherapy in mental health care, the veteran psychoanalyst Robert Wallerstein reports that, in the 1940s and 1950s, at the brink of psychoanalysis's dominance over the field, 40% to 50% of the time spent in psychiatry residency programs was devoted to training in the application of psychodynamic psychotherapy. This number would, of course, grow over the coming decades as the influence of psychoanalysis in psychiatry grew. But it would begin to decline in the mid-1970s and early 1980s, reaching the point, by the early 1990s, when psychotherapy training took up only 200 h of the total time spent in psychiatric residency, or a paltry 2.5% in comparison with the earlier numbers (Wallerstein 1991). Clearly this was a move away from the old concerns of the profession, pressed by the influence of the DSM-III and subsequent manuals, which, as one of the psychiatry resident informers in T.M. Luhrmann's recent ethnography suggests, made therapeutic training during residency seem a waste of time: "[If] we don't go into research we've failed somehow" (2000, p. 158).

Meanwhile, outside the field, the DSM-III (and its subsequent editions) came to be used by courts, corporate researchers, prisons, educational institutions, and governments at all levels. Its influence ranged far beyond the field of mental health and psychiatry. As Mayes and Horwitz conclude: "Few professional documents compare to the DSM in terms of affecting the welfare of so many people" (2005, p. 265).

For a sense of how the DSM-III extended the control of psychiatrists over the field of mental health, consider the following two studies, drawn from surveys of allied professional groups and their use of the DSM-III immediately after it was published. A study of clinical psychologists (Miller et al. 1981) found that 90% of the sample used the DSM-III to some extent, while 86% used it because third-party payers required it. A significant proportion of the sample (43%) agreed that they used the DSM-III because it was the only "classification system presently available" and that, subsequently, they had no choice but to use it. Meanwhile, a study of clinical social workers around the same time (Kutchins and Kirk 1988) found that 84% used the DSM-III more frequently than any other reference book. Insurance purposes were cited by 81% of the sample as a "very important" reason. They also reported using the manual despite a majority of respondents finding significant problems with the DSM-III, particularly for addressing the issues of marriage and family that constituted the bulk of their caseload.[32]

These studies indicate the extent of usage of the DSM-III that followed rapidly upon publication in 1980, carried by the heteronomous interests that were now a significant part of the environment in which all mental health professionals practiced. Significantly, usage of the DSM-III is tied to insurance claims reporting, which reveals a primary reason why the classification was so valuable in these conditions, and why diagnostic psychiatry was subsequently so powerful in the field. The responses also suggest the interference, or unaccountability, represented by DSM-III usage, which both clinical psychologists and clinical social workers saw as having little direct application to their practice outside of meeting requirements to third-party payers. If this further reveals the significance of heteronomous pressures on the field, it also indicates the authority exercised by psychiatrists *and their classifications* over allied professional groups.[33]

That classification became the dominant capital in the field is perhaps best indicated by what occurred in subsequent DSM editions (Table 3). Further codification of the practice of mental health professionals and illness-types expanded the manual in a process some have called psychiatry's "securing of diagnostic turf" (Kirk and Kutchins 1992, chap. 8; Shorter 1997, p. 291). Psychiatrists subsequently tweaked and modified categories, each time affecting the fortunes of the many interests now tied to the manual. Indeed, based on the success of subsequent DSM

[32] These responses also suggest practices indicative of what Bowker and Star (2000: 159) call "work-arounds" or the way practitioners adapt to formal standards like classifications by devising informal ways of meeting the requirements they impose, while still retaining autonomy of practice. They offer the example of a psychoanalyst arbitrarily classifying a patient as receiving treatment for "obsessive-compulsive disorder" in order to meet health insurance reporting requirements and thus retain benefits that include psychotherapy treatment (2000, p. 47).

[33] This is in addition to the persistence of significant income differences between psychiatrists and other mental health professionals after publication of the DSM-III (Dodosh 1981).

Table 3 Summary of DSM versions I–IV, 1952–1994

DSM Version	Year	Total diagnoses	Total pages
I	1952	106	130
II	1968	182	134
III	1980	265	494
III-R	1987	292	567
IV	1994	297	886

editions and the extensive preparations that surround the development of a new edition, managing the classification is likely the most important professional work that psychiatrists now do (Healy 2004; Spiegel 2005). We can appreciate this significance as stemming from a convergence first realized by the DSM-III.

Conclusion

It might seem remarkable for Spitzer to claim that "he hadn't thought through anything" before beginning work on the DSM-III, and "didn't know it would become important in terms of American and world psychiatry" (2000, p. 418). Those who were involved in drafting the DSM-III shared this view (Guze 2000), as did many in the profession at large (Sabshin 1990). And yet they still believed the DSM-III to be *necessary*, even though their belief was not contingent on the manual's functional significance. As I argued, the DSM-III did inspire a revolution in mental health treatment. But this revolution was the result of *matching trajectories*: the growing heteronomy of the field of mental health, on the one hand, and a classification that emerged from conflicts between professional groups, on the other. Psychiatrists alone could develop the DSM-III because only they were in the *position* to do so. And because they were its spokespersons, psychiatrists controlled the dominant form of capital in the field, remaining the dominant profession, even though they lost jurisdiction over psychotherapy to clinical psychology.

As noted at the beginning, this article used field theory to develop an approach to innovation and the production of knowledge that does not rely on function. Following Durkheim and Mauss (1963[1903]), classification is given an "extra-functional" origin, which for the DSM-III emerged from conflicts among psychiatrists, psychoanalysts, and clinical psychologists that occurred in the wake of the collapse of psychoanalytic hegemony over the field. I used the *pattern* of these conflicts to reveal the "generative formula" for the production of the DSM-III, demonstrating why it seemed *necessary* to Spitzer and the Task Force, outside any concern with its eventual function (Bourdieu 1996, p. 303). As I argued, to understand the generative formula is to understand the classification by grasping the *raison d'etre* resting at its origin. Functions can be primary features of the generative formula behind the production of knowledge or technologies like classifications, but this needs to be established empirically. In this case, I have demonstrated why function was not an important part of the generative formula for the DSM-III.

Instead, the "lay expertise" exercised by gay and lesbian activists, critiques of the intrusive characteristics of psychoanalytical treatment, and reactions to the critique of diagnosis waged by the clinical psychologists were the formula's central elements. On the one hand, they separated the patient from the pathology; on the other, they separated diagnosis from correspondence. As I argued, these are positions taken by psychiatrists on issues that emerged from conflicts with psychoanalysts and clinical psychologists. Together, they led psychiatrists toward an *interest in form*, or what situated the diagnostic approach to psychiatry in between the theory of psychoanalysts and the empiricism of clinical psychologists. By accounting for these relationships, we unpack the generative formula behind the DSM-III. Consequently, we understand it *by making it necessary*, and in the same way understand why the DSM-III seemed necessary to Spitzer and the Task Force even absent recognition of function.

This approach outlines the epistemology of field theory (Bourdieu 1996; Martin 2003). It takes into account the interface between power and knowledge as elements equal in the generative formula behind scientific and medical innovations. From this perspective, the goal is to unpack the generative formula and find the social processes behind its primary elements. This is the basis for a robust definition of *origin* in the sociology of knowledge, which builds from Durkheim and Mauss's (1963[1903]) approach to the origins of classification. By bracketing the role classification played in logic, Durkheim and Mauss revealed its "extra-logical" origins. I argue for a similar distinction, allowing room to capture empirically the generative formula behind the production of knowledge. In the sociology of science, it is a truism that knowledge can be "right" for a variety of reasons. Approaching this idea from the perspective of field theory—with its focus on necessity and generative formulas—allows the sociology of knowledge (writ large) to capture the epistemic importance of a diversity of social processes.

Acknowledgments I give special thanks to Omar Lizardo and Lyn Spillman for their advice, criticism, and encouragement since the very beginning of this project. I also thank Gene Halton, David Hachen, Ellen Childs, Sara Skiles, Isaac Reed, Nina Eliasoph, and John R. Hall for their comments on earlier versions of the article. Finally, I thank the *Theory and Society* Editors and reviewers for their insight and criticism.

References

Abbott, A. (1988). *The system of professions: An essay on the division of expert labor*. Chicago: University of Chicago Press.

American Psychiatric Association, Task Force on Nomenclature and Statistics. (1980). *Diagnostic and statistical manual of mental disorders* (3rd ed.). Washington: American Psychiatric.

Baldessarini, R. (2000). American Biological Psychiatry and Psychophrmacology. In R. Menninger & J. Nemiah (Eds.), *American psychiatry after World War II, 1944–1994* (pp. 371–413). Washington: American Psychiatric.

Bayer, R. (1978). *Homosexuality and American psychiatry: The politics of diagnosis*. New York: Basic.

Bayer, R., & Spitzer, R. (1985). Neurosis, psychodynamics and DSM-III: A history of the controversy. *Archives of General Psychiatry, 42*, 187–196.

Benson, R. (1999). Field theory in comparative context: A new paradigm for media studies. *Theory and Society, 28*, 463–498.

Blashfield, R. (1982). Feighner et al., invisible colleges and the Matthew effect. *Schizophrenia Bulletin, 8*, 1–12.
Blashfield, R. (1984). *The classification of psychopathology: Neo-Kraepelinian and quantitative approaches*. New York: Plenum.
Bloch, S. (1996). *An Introduction to psychotherapies*. Oxford: Oxford University Press.
Bloor, D. (1981). Durkheim and Mauss revisited: Classification and the sociology of knowledge. *Studies in History and Philosophy of Science, 13*, 267–297.
Bourdieu, P. (1986). The forms of capital. In J. Richardson (Ed.), *The handbook of theory and research for the sociology of education* (pp. 241–258). New York: Greenwood.
Bourdieu, P. (1988). *Homo academicus*. Stanford: Stanford University Press.
Bourdieu, P. (1996). *The rules of art: Genesis and structure of the literary field*. Stanford: Stanford University Press.
Bourdiu, P. (1990). Codification. In *In Other Words,* pp. (76–8). Stanford: Stanford University Press
Bowker, G., & Star, S. (1999). *Sorting things out: Classification and its consequences*. Cambridge: MIT.
Buchanan, R. (2003). Legislative warriors: American psychiatrists, psychologists, and competing claims over psychotherapy in the 1950s. *Journal of the History of the Behavioral Sciences, 39*, 225–249.
Burnham, J. (1967). *Psychoanalysis and American medicine, 1894–1918: Medicine, science, and culture*. New York: International University Press.
Cohen, M., & Healy, D. (2002). Mandel Cohen and the origins of the *diagnostic and statistical manual of mental disorders, Third Edition: DSM-III. History of Psychiatry* 13: 209–230.
Cooksey, E., & Brown, P. (1998). Spinning on its axes: DSM and the social construction of psychiatric diagnosis. *International Journal of Health Services, 28*, 525–554.
Cooper, A., & Michels, R. (1981). DSM-III: an American view. *American Journal of Psychiatry, 138*, 128–129.
Dain, N. (2000). Antipsychiatry. In R. Menninger & J. Nemiah (Eds.), *American psychiatry after World War II, 1944–1994* (pp. 277–299). Washington: American Psychiatric Press.
Decker, H. (2007). How Kraepelinian was Kraepelin? How Kraepelinian are the Neo-Kraepelinians?—From Emil Kraepelin to DSM-III. *History of Psychiatry, 18*, 337–360.
Dodosh, M. (1981). Psychiatrist Fight against Further Inroads by Psychologists into Mental-Health Market. *Wall Street Journal* (20 August 1981): 29.
Dolnick, E. (1998). *Madness on the couch: Blaming the victim in the heyday of psychoanalysis*. New York: Simon and Schuster.
Durkheim, E., & Mauss, M. (1963[1903]). *Primitive Classification*. Chicago: University of Chicago Press.
Endicott, J., & Spitzer, R. (1978). A diagnostic interview: The schedule for affective disorders and schizophrenia. *Archives of General Psychiatry, 35*, 837–844.
Epstein, S. (1995). The construction of lay expertise: AIDS activism and the forging of credibility in the reform of clinical trials. *Science, Technology and Human Values, 20*, 408–437.
Epstein, S. (1997). Activism, drug regulation, and the politics of therapeutic evaluation in the AIDS era: a case study of ddC and the 'Surrogate Markers' debate. *Social Studies of Science, 27*, 691–726.
Feighner, J., Robins, E., Guze, S., Woodruff, R., Winokur, G., & Munoz, R. (1972). Diagnostic criteria for use in psychiatric research. *Archives of General Psychiatry, 26*, 57–63.
Frances, A., & Cooper, A. (1981). Descriptive and dynamic psychiatry: a perspective on DSM-III. *American Journal of Psychiatry, 1981*, 1198–1202.
Frank, J. (1961). *Persuasion and healing: A comparative study of psychotherapy*. Baltimore: Johns Hopkins University Press.
Freidson, E. (1970). *Professional dominance: The structure of medical care*. New York: Atherton.
Gabbard, G. (2000). The evolving role of the psychiatrist from the perspective of psychotherapy. In R. Menninger & J. Nemiah (Eds.), *American psychiatry after World War II, 1944–1994* (pp. 103–124). Washington: American Psychiatric.
Galatzer-Levy, I., & Galatzer-Levy, R. (2009). The revolution in psychiatric diagnosis. *Perspectives in Biology and Medicine, 50*, 161–80.
Gliman, S. (1987). The struggle of psychiatry with psychoanalysis: who won? *Critical Inquiry, 13*, 293–313.
Goleman, D. (1990). Psychiatrists Under Pressure: A Special Report. *New York Times* (17 May 1990): A1-B12.
Goodman, N. (1978). *Ways of world-making*. Indianapolis: Hackett.
Goodwin, D., & Guze, S. (1974). *Psychiatric diagnosis*. New York: Oxford University Press.

Greenberg, D. (1980). Reimbursement wars. *New England Journal of Medicine, 303*, 538–540.
Grob, G. (1987). The forging of mental health policy in American: World War II to new frontier. *The Journal of the History of Medicine and Allied Sciences, 42*, 410–446.
Grob, G. (1991a). *From asylum to community: Mental health policy in modern America*. Princeton: Princeton University Press.
Grob, G. (1991b). Origins of DSM-I: A study in appearance and reality. *American Journal of Psychiatry, 148*, 421–431.
Grob, G. (2000). Mental health policy in late twentieth-century America. In R. Menninger & J. Nemiah (Eds.), *American psychiatry after World War II, 1944–1994* (pp. 232–259). Washington: American Psychiatric.
Gronfein, W. (1985). Psychotropic drugs and the origins of deinstitutionalization. *Social Problems, 32*, 437–454.
Guze, S. (2000). The Neo-Kraepelinian revolution. In D. Healy (Ed.), *The psychopharmacologists: III* (pp. 395–414). London: Hodder Arnold.
Guze, S., & Murphy, G. (1963). An empirical approach to psychotherapy: the agnostic position. *American Journal of Psychiatry, 120*, 53–57.
Hackett, T. (1977). The psychiatrist: in the mainstream or on the banks of medicine? *American Journal of Psychiatry, 134*, 432–434.
Hale, N. (1995). *The rise and crisis of psychoanalysis in the United States: Freud and the Americans, 1917–1985*. New York: Oxford University Press.
Hale, N. (1999). Freud's critics: a second look. *Partisan Review, 66*, 235–254.
Hale, N. (2000). American psychoanalysis after World War II. In R. Menninger & J. Nemiah (Eds.), *American psychiatry after World War II, 1944–1994* (pp. 77–103). Washington: American Psychiatric.
Hall, R. (1974). Financing mental health services through insurance. *American Journal of Psychiatry, 131*, 1079–1088.
Healy, D. (1997). *The antidepressant era*. Cambridge: Harvard University Press.
Healy, D. (2004). *The creation of psychopharmacology*. Cambridge: Harvard University Press.
Horwitz, A. (2002). *Creating mental illness*. Chicago: University of Chicago Press.
Hudgins, R. (1993). The turning of American psychiatry. *Missouri Medicine, 90*, 283–291.
Kendell, R., Cooper, J., Gourley, A., Copeland, J., Sharpe, L., & Gurland, B. (1971). Diagnostic criteria of American and British psychiatrists. *Archives of General Psychiatry, 25*, 123–130.
Kirk, S., & Kutchins, H. (1992). *The selling of DSM: The rhetoric of science in psychiatry*. Hawthorne: Aldine de Gruyter.
Kirsner, D. (2009). *Unfree associations: Inside psychoanalytic institutes*. Lanham: Jason Aronson.
Klerman, G. (1978). The evolution of a scientific nosology. In J. Shershow (Ed.), *Schizophrenia: Science and practice* (pp. 99–122). Cambridge: Harvard University Press.
Klerman, G. (1990). The psychiatric patient's right to effective treatment: implications of Osheroff v. Chestnut Lodge. *American Journal of Psychiatry, 147*, 409–418.
Kolb, L., Frazier, S., & Sirovatka, P. (2000). The national institute of mental health: Its influence on psychiatry and the nation's mental health. In R. Menninger & J. Nemiah (Eds.), *American psychiatry after World War II, 1944–1994* (pp. 207–232). Washington: American Psychiatric.
Kutchins, H., & Kirk, S. (1988). The business of diagnosis: DSM-III and clinical social work. *Social Work, 33*, 215–220.
Lakoff, A. (2005). Diagnostic liquidity: mental illness and the global trade in DNA. *Theory and Society, 34*, 63–92.
Lamb, R. (2000). Deinstitutionalization and public policy. In R. Menninger & J. Nemiah (Eds.), *American psychiatry after World War II, 1944–1994* (pp. 259–277). Washington: American Psychiatric.
Latour, B. (1988). *The pasteurization of France*. Cambridge: Harvard University Press.
Leveille, J. (2002). Jurisdictional competition and the psychoanalytic dominance of American psychiatry. *Journal of Historical Sociology, 15*, 252–280.
Lubin, B. (1962). Survey of psychotherapy training and activities of psychologists. *Journal of Clinical Psychology, 18*, 252–256.
Luhrmann, T. (2000). *Of two minds: The growing disorder in American psychiatry*. New York: Knopf.
Maciver, J., & Redlich, F. (1959). Patterns of psychiatric practice. *American Journal of Psychiatry, 115*, 692–697.
Mahoney, M. (1977). Reflections on the cognitive-learning trend in psychotherapy. *American Psychologist, 32*, 5–13.

Malan, D. (1973). The outcome problem in psychotherapy research: a historical review. *Archives of General Psychiatry, 29*, 719–729.
Marshall, E. (1980). Psychotherapy faces test of worth. *Science, 207*, 35–36.
Martin, J. (2003). What is field theory? *American Journal of Sociology, 109*, 1–49.
Mayes, R., & Horwitz, A. (2005). DSM-III and the revolution in the classification of mental illness. *Journal for the History of the Behavioral Sciences, 41*, 249–267.
McReynolds, W. (1979). DSM-III and the future of applied social science. *Professional Psychology, 10*, 123–132.
Menninger, K. (1959a). Hope: the academic lecture. *American Journal of Psychiatry, 116*, 481–491.
Menninger, K. (1959b). Toward a unitary view of mental illness. In B. Hall (Ed.), *A psychiatrist's world: The selected papers of Karl Menninger, M.D* (pp. 516–531). New York: Viking.
Menninger, K. (1963). *The vital balance*. New York: Viking.
Miller, L., Bergstrom, D., Cross, H., & Grube, J. (1981). Opinions and use of the DSM system. *Professional Psychology, 12*, 385–390.
Millon, T. (1983). The DSM-III: an insider's perspective. *American Psychologist, 38*, 804–814.
Millon, T. (1986). On the past and future of the DSM-III: Personal recollections and projections. In T. Million & G. Klerman (Eds.), *Contemporary directions in psychopathology: Toward the DSM-IV* (pp. 29–70). New York: Guilford.
Pels, D. (2000). *The intellectual as stranger: Studies in spokespersonship*. London: Routledge.
Plant, R. (2005). William Menninger's campaign to reform psychoanalysis, 1946–1948. *History of Psychiatry, 16*, 181–202.
Redlich, F., & Kellert, S. (1978). Trends in American mental health. *American Journal of Psychiatry, 135*, 22–28.
Rogler, L. (1997). Making sense of historical changes in the *Diagnostic and Statistical Manual of Mental Disorders*: five propositions. *Journal of Health and Social Behavior, 38*, 9–20.
Rosenhan, D. (1973). On being sane in insane places. *Science, 179*, 250–258.
Sabshin, M. (1990). Turning points in twentieth-century American psychiatry. *American Journal of Psychiatry, 147*, 1267–1274.
Scott, R., Ruef, M., Mendel, P., & Caronna, C. (2000). *Institutional change and healthcare organizations: From professional dominance to managed care*. Chicago: University of Chicago Press.
Scully, J., Rabinowitz, C., & Shore, J. (2000). Psychiatric education after World War II. In R. Menninger & J. Nemiah (Eds.), *American psychiatry after World War II, 1944–1994* (pp. 124–152). Washington: American Psychiatric.
Shapin, S. (1995). Cordelia's Love: credibility and the social studies of science. *Perspectives on Science, 3*, 255–275.
Sharfstein, S. (1978). Third-party payers: to pay or not to pay. *American Journal of Psychiatry* 1185–1188.
Sharfstein, S. (1987). Third-party payments, cost containment and DSM-III. In G. Tischler (Ed.), *Diagnosis and classification in psychiatry: A critical appraisal of DSM-III* (pp. 530–538). New York: Cambridge University Press.
Shorter, E. (1997). *A history of psychiatry: From the era of the asylum to the age of prozac*. New York: Wiley Sons.
Shorter, E. (2009). *Before prozac: The troubled history of mood disorders in psychiatry*. New York: Oxford University Press.
Smith, D., & Kraft, W. (1983). DSM-III: Do psychologists really want an alternative? *American Psychologist, 38*, 777–785.
Smith, M., Glass, G., & Miller, T. (1980). *The benefits of psychotherapy*. Baltimore: Johns Hopkins University Press.
Spiegel, A. (2005). The dictionary of disorder: How one man revolutionized psychiatry. *The New Yorker* (3 January 2005).
Spitzer, R. (1973). A proposal about homosexuality and the APA nomenclature: homosexuality as an irregular from of sexual behavior and sexual orientation disturbance as a psychiatric disorder. *American Journal of Psychiatry, 130*, 1214–1216.
Spitzer, R. (1983). Psychiatric diagnosis: are clinicians still necessary? *Comprehensive Psychiatry, 24*, 399–411.
Spitzer, R., & Williams, J. (1985). Classification of mental disorders. In H. Kaplan & B. Sadock (Eds.), *Comprehensive textbook of psychiatry IV* (Vol. 1, pp. 591–613). Baltimore: Williams and Wilkins.
Spitzer, R., Endicott, J., & Robins, E. (1978). Research diagnostic criteria: rationale and reliability. *Archives of General Psychiatry, 35*, 773–782.

Spitzer, R., Williams, J., & Skodol, A. (1980). DSM-III: the major achievements and an overview. *American Journal of Psychiatry, 137*, 151–164.

Sptizer, R. (1975). On pseudoscience in science, logic in remission and psychiatric diagnosis: a critique of Rosenhan's 'on being sane in insane places. *Journal of Abnormal Psychology, 84*, 442–452.

Starr, P. (1982). *The social transformation of American medicine*. New York: Basic.

Stoline, A., Goldman, H., & Sharfstein, S. (2000). Managed care and other economic constraints. In R. Menninger & J. Nemiah (Eds.), *American psychiatry after World War II, 1944–1994* (pp. 343–367). Washington: American Psychiatric.

Wallerstein, R. (1991). The future of psychotherapy. *Bulletin of the Menninger Clinic, 55*, 421–443.

Wallerstein, R., & Peltz, M. (1987). Psychoanalytic contributions to psychiatric nosology. *Journal of the American Psychoanalytic Association, 35*, 693–711.

Ward, C. (1954). Psychiatric training in university centers. *American Journal of Psychiatry, 111*, 123–131.

Wilson, M. (1993). DSM-III and the transformation of American psychiatry: a history. *American Journal of Psychiatry, 150*, 399–410.

Winslow, W. (1979). The changing role of psychiatrists in community mental health centers. *American Journal of Psychiatry, 136*, 24–27.

Wood, W. (2004). (Virtual) myths. *Critical Sociology, 30*, 513–548.

Young, A. (1995). *The harmony of illusions: Inventing post-traumatic stress disorder*. Princeton: Princeton University Press.

Michael Strand is a Ph.D. Candidate in Sociology at the University of Notre Dame. His interests include theory, the sociology of knowledge, cultural sociology, and organizations. Among his current research projects are an examination of the use of models in the formulation of third-person perspectives in sociological theory and a historical analysis of the institutional origins for claims of social justice.