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The Name Game: Toward a Sociology of Diagnosis

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Although diagnosis is integral to the theory and practice of psychiatry, social scientists have not developed a comprehensive approach to diagnosis. This paper presents a preliminary outline of the issues which a sociology of diagnosis should integrate. These include bias and social control in psychiatric diagnosis, diagnosis as part of a new extension of the biopsychiatric medical model, and flaws in contemporary diagnostic categorization. These issues are then viewed in terms of professional practice styles, diagnostic biases, psychiatry's professional dominance over the mental health field, and psychiatric hegemony over the clinical interaction with patients.

Diagnosis is integral to the theory and practice of psychiatry, yet it is loosely studied by social scientists. In this paper I lay out what I consider to be the main areas which a sociology of diagnosis should examine. The field is still new, and not all the components are well-developed. Some are more well-developed than others, for instance sex, race, and class bias, though they are not usually integrated with each other or with the other major areas of concern. There is also a small tradition of examining diagnosis in clinical interaction as a social construction. But we have not seen adequate attention to conceptual models which integrate medical sociology and sociology of science in order to understand the pivotal position which diagnosis plays in the larger professional project of biopsychiatry. Although much of the recent attention to diagnostic issues specifically addresses the DSM-III-R biopsychiatric project, it does not pay attention to other diagnostic currents. In this paper I discuss diagnosis historically, epistemologically, and sociologically, working to make links between the often disconnected components of the sociology of diagnosis.

Below I shall present an outline for a *sociology of diagnosis*. I view this as

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both an approach to the study of diagnosis, as well as an overall critique of modern psychiatry. The critical approach to psychiatry seen in the 1960s and 1970s has been fairly dissipated, owing to the general conservative trend in society, the success of organized psychiatry in promoting its new face, and the abandonment by many social scientists of their interest in this area. There are, however, signs of a renewal of interest in a critical perspective, stemming in large part from an interest in critiquing the new diagnostic project of psychiatry – a project blind to the entire past history of that profession.

In exploring the potential for a rich subject matter in the sociology of diagnosis, I can only mention briefly some of the key work already done. Many components of a sociology of diagnosis already exist in varying degrees of development. The task is to solidify those that are least developed, and to synthesize all the components into a new focus. Although I am concerned here with psychiatric diagnosis, I think that many of the issues can be extended to medical diagnosis. Indeed, a considerable body of work in medical sociology is concerned with lay-professional differences in disease and illness conception and experience, and with the social construction of disease (c.f. Freidson, 1970; Schneider and Conrad, 1983). That research directly touches on diagnostic issues, although they are not usually considered specifically as such.

I begin by discussing some historical examples of bias and social control in psychiatric diagnosis. I then situate current concern with diagnosis in the context of a new extension of the biopsychiatric medical model. That leads to a discussion of flaws in the theory and measurement of contemporary diagnostic categorization. These issues are then situated in specific phenomena of professional practice styles and their social biases. Following that, I examine psychiatry's professional dominance over the mental health field and its control over the clinical interaction with patients. Last, I take up the social gatekeeping functions of diagnosis by looking at diagnosis as an arena of struggle, and at the ahistorical nature of psychiatric diagnosis.

The Vagaries of Psychiatric Diagnosis

Benjamin Rush, a signer of the Declaration of the Independence and Physician General of the Continental Army, is considered the "founder" of American psychiatry. His cameo appears as a logo on the American Psychiatric Association's publications. In the period immediately following the American Revolution, Rush named an interesting diagnosis called "anarchia" (Szasz, 1970, pp. 138–149). Anarchia was the "form of insanity" in which people were unhappy with the new political structure of the United States (there were problems, such as black slavery and the restriction of the vote to white men

who held landed property), and sought a more democratic society. Rather than deal with these opponents on their own political terms, Rush found it easier to transform their opinions into the symptoms of a mental disease.

Rush was also an innovator in treatment. He developed the "tranquilizer," a chair which held patients immobile by straps on their limbs and a cage over their head. He originated the "gyrator," a rotating board to which patients were strapped and then spun at high speeds. And when a patient presented the delusion of feeling fragile like glass, Rush figured out that the best thing to do was to pull a chair out from under them and then to show them glass pieces in order to demonstrate the wrongness of their belief (Szasz, 1970, pp. 138–149). Evidently, there was some connection between Rush's social control ideology of diagnosis and his social control practice of treatment.

Rush's psychologization via diagnosis was by no means new. The Catholic Church had a long medieval history of "diagnosing" nonconforming women as witches who were possessed by the devil and his legions. This was a complementary phenomenon to the then current religious/demoniacal perspective on social deviance. Indeed, a central element of Szasz' (1970) critique of institutional psychiatry is the latter's functional resemblance to the Inquisition. The Catholic Church's persecution for what it identified as deviance was torture and murder. There was a clear connection between labeling and social control practices: the purpose of both was to ensure fear, division, and supernaturalism in the working population, in order to maintain the feudal solidarity of exploiting nobles and authoritarian clergy.

In 1843 Dr. Samuel Cartwright identified the disease of "drapetomania," which occurred only in black slaves and which resulted in a curious form of pathology – the victims had a compulsion to run away. Blacks also were the only people to contract "dyaesthesia Aethiopica," which caused such pathology as "pay[ing] no attention to property" (Thomas and Sillen, 1972, p. 2). The function of these diagnostic practices was to provide support for a social order based on slavery.

In the early 20th century, psychiatrists developed a new use for the diagnosis of "psychopathic." Originally used to label a variety of male deviants such as vagabonds, criminals, and revolutionaries, the diagnosis was used by Progressive Era psychiatrists to label sexually active women. Lunbeck (1987) provides a fascinating account from her research in the archives of the Boston Psychopathic Hospital covering the years 1912–1921. Typically, women committed to the hospital for "hypersexual behavior" were working class women living on their own who had chosen to forego or delay marriage, or who were widowed or divorced. Psychiatry's response to the new sexual morality of the time was to target it as a mental disease. Sexual freedom was but one manifestation of these new women's autonomy in the world of work, pleasure, and social and familial relations. However, the women were out of character with

traditional norms, and (male) psychiatrists could only see them as having a mental disease.

Psychiatrists classified sexually active women along with prostitutes, blaming them equally for enticing men into illicit sex; this diagnosis let men off the hook. The psychiatrists found further evidence of derangement in that "immoral" women would not accept money for sex. The doctors followed the general social values that proclaimed sex as a commodity: a moral woman saved her virtue as her best asset; an immoral one could only give it up for pay – otherwise she was crazy. Psychiatrists had earlier tried out the diagnosis of "feeblemindedness," but the patients scored too high on IQ tests. Some were intelligent enough to openly debate with their caretakers the sexual double standard. Hospital psychiatrists in turn warned them not to read or discuss those other social issues, since education was bad for women (Lunbeck, 1987).[1]

In the turbulent 1960s, Bettleheim (1969) told the United States Congress of his findings: student antiwar protesters who charged the University of Chicago with complicity in the war machine had no serious political agenda; they were acting out an unresolved Oedipal conflict by attacking the university as a surrogate father. Bettelheim's appellation worked well to pathologize essentially rational political protest.

The Logic of A Sociology of Diagnosis

The few examples above are manifestations of the application of psychiatry for social control, and from our current vantage point they seem very crude. I emphasize them, however, precisely because in their own time they were part of very ordinary worldviews. There are certainly other forms of social control, especially today, which are far less overt. Indeed, critiques of the "psy complex" (Castel, Castel, and Lovell, 1982) argue that it involves social control at very routine levels of socialization, labeling of behavior, and prescriptions for medical/psychiatric intervention.

The entire history of the sociological study of mental health, as well as the tradition of radical critiques of the mental health field, have revolved around this common theme of psychiatry's role in social control. What has not always been clear is that *diagnosis* has been a central component of this social control. *Giving the name* has been the starting point for social labelers. The power to give the name has been a core element in the social control nature of the mental health professionals and institutions.

In one sense the critique of diagnosis *is* the critique of psychiatry, because

[1]This was the same prescription given to Charlotte Perkins Gilman in the last decade of the 19th century, and in fact we usually associate this with the psychiatric approach to upper class women.

diagnosis is the *language of psychiatry*, which by extension defines the practice of psychiatry. Diagnosis locates the parameters of normality and abnormality, demarcates the professional and institutional boundaries of the mental health system, and authorizes psychiatry to label and deal with people on behalf of society at large (or, more appropriately, certain sectors of society). It is the legal basis for provision of benefits, and often for involuntary commitment.[2] Especially in the guise of DSM-III-R (American Psychiatric Association, 1987), psychiatric diagnosis is the social representation of psychiatric knowledge, as well as the psychiatric profession's presentation of self. Diagnosis, Blaxter (1978) notes, is "a museum of past and present concepts of the nature of disease" (p. 12).

Diagnosis thus cannot be studied on its own: it is integral to the whole of psychiatry. We are compelled to question what I term the *diagnostic project* of psychiatry in the context of the entirety of psychiatric knowledge and practice. This means, in particular, putting it in the context of all the errors and maltreatments of organized psychiatry – overreliance on drugs, abusive treatment such as psychosurgery, conscious and unconscious social control, replication and support of racism, sexism, and class bias. Put simply, if modern diagnosis is the *culmination* of psychiatry – which DSM-III proponents certainly claim it to be – then what are we to make of the history of psychiatry leading up to this modern phenomenon? I think the answer is that diagnosis reaps the sad legacy of the mental health system and mental health professions.

This is not to be read as a simplistic antipsychiatry which sees all mental health services as social control. Many people and facilities sincerely strive to help patients. However, as I discuss below, they do so mainly *without* reference to the official diagnostic framework, and often enough do so with knowing or unknowing circumvention of and opposition to official diagnosis (Brown, 1987).

Diagnosis and the Biomedical Model

The increasing faith in DSM (hereafter used in place of the cumbersome DSM-III-R) is central to the new biopsychiatry. We are in a period of "remedicalization" of psychiatry. I say "remedicalization" because the prior medicalization process was challenged by attention to social factors and the role of the mental health system in social change. The newer biopsychiatry has taken aim at the proponents of a social context, offering an assortment of new work in molecular biological studies of psychosis, with a new armamen-

[2]Committment, however, requires varying degrees of *behavioral* characteristics, such as actual or imminent violence to self or others, or in more broad-based statutes, inability to care for oneself. These characteristics cannot be read directly from diagnoses, although some diagnoses imply a greater likelihood of those characteristics.

tarium of laboratory tests and brain imaging. Apparently the proponents hope that such "hard" data will legitimate their biopsychiatry more than have descriptive neo-Kraepelinian categories and observations of the effects of psychoactive drugs. But the new molecular biological approach only offers simple correlations between biochemical states and accepted diagnostic categories. Further, it accounts for only a small fraction of categories of the official nosology.

Let me give an example of the attitude of this new biopsychiatry. In 1987 I attended the founding conference of the Commonwealth Research Center, a major research center funded by the Massachusetts Department of Mental Health. Most invited speakers were fully locked into the molecular biological levels of psychiatry, eager to show the biochemical bases of mental illness. A small minority of speakers represented a social context, though clearly from within the medical model. One was Bruce Dohrenwend, probably the most respected psychiatric epidemiologist in the United States; he has worked closely with many leaders of biopsychiatry, and has developed rating scales widely used by biopsychiatrically oriented people. The other was Courtney Harding, a psychologist who has been a principal investigator of the Vermont Longitudinal Study (Harding, Brooks, Ashikaga, Strauss, and Breier, 1987). This is a remarkable study which shows that diagnosed schizophrenics have a higher rate of recovery than previously expected, and that psychosocial rehabilitation prior to discharge plays a major role in reducing future symptoms. Despite the fact that both these respected scholars have always worked alongside psychiatrists, and adhered to a medical model (albeit with a strong social component), most conference speakers and participants sharply challenged them for arguing that social factors were significant determinants of mental illness. It was simply astounding – the biopsychiatrists went against the grain of well-established research findings about social factors, and stridently challenged these two speakers. There was no apparent need for it – the biopsychiatrists already dominated the conference. Yet, clearly, they perceived a need to demonstrate the worth of their perspective and to guard against future usurpation of their dominance.

Organized biopsychiatry has embarked on what it self-consciously styles a "neo-Kraepelinian" project. Quite literally, its adherents seek a return to Emil Kraepelin because he was such a remarkable labeler and classifier. They desire the neo-Kraepelinian model because it is hyper-empirical, easily measurable and computable. This approach states that it disregards etiology and dismisses conflicting theoretical standpoints (Andreasen, 1984; Blashfield, 1984). Early diagnostic schema of physical illness were also accumulated without reference to etiology, but when etiological knowledge later accumulated, doctors typically tried to apply it. We certainly do not expect doctors today to return to an atheoretical, descriptive framework simply to

avoid controversy. Yet this is what the current diagnostic project in psychiatry is purportedly all about. Further, despite their claims, the neo-Kraepelinians do not disregard etiology so much as history, whether personal or social. They would most likely be satisfied with some form of genetic and biochemical etiology, which is in fact what they aim for. The neo-Kraepelinians simply do not want to deal with any form of *social* etiology.

In addition, the growth of drug treatment as the intervention of choice has cemented the centrality of diagnosis. Interest in formal diagnosis was rekindled in the 1950s as a result of the introduction of psychoactive drugs (Guimon, 1989). Since medication requires a match between disease and treatment, exact diagnosis became increasingly important. Unfortunately, the advent of widespread drug prescription often led to an uncritical reliance on medication, while at the same time diminishing the importance of social and institutional contexts in generating and maintaining what we call mental disorders. In fact, unlike medicine where diagnosis typically leads the doctor to prescribe a medication with known effect, psychiatry often reverses this logic by making a diagnosis based on the patient's *response* to medication.

The forefront of social psychiatry during World War II and immediately after presented a vibrant criticism of biological reductionist thinking. Jones (1953) and later Wing and Brown (1970) noted the significance of "institutionalism" caused by hospitals rather than biological processes. This did not imply that *all* symptoms were socially and institutionally caused, but rather that *many* were. Sociological studies in the 1950s and 1960s heightened this awareness (Belknap, 1956; Caudill, 1958; Goffman, 1961; Stanton and Schwartz, 1954). Community mental health approaches grew up in this environment, leading to emphasis on non-institutional treatment and to attention to social factors.

Pseudoscience

Such changes in orientation created disagreement within psychiatry. As we know, all science is full of controversy, and claims makers are always attempting to win colleagues and the rest of society to their perspective. What becomes accepted as science is often the result of successful social organizing and claims-making (Latour, 1987). In large part the biopsychiatry project is a way of securing unity in a disunified profession. The purpose of this unity is largely to secure professional dominance over the mental health field, since psychology and social work have grown to be important mental health disciplines in the last several decades. Unity within psychiatry also solidifies the psychiatric claim that it is a "hard" medicine worthy of third party reimbursement.

The leaders of the diagnostic project claim that they are being atheoretical. While it is true that they are emphasizing symptom clusters and avoiding traditional arguments, such as those between organic and psychoanalytic perspectives, they cannot be atheoretical. As Faust and Miner (1986) point out, even the most descriptive observations in psychiatry are based on criteria of normality which are at bottom value judgements, e.g., "aggressive behavior" by five year-olds, cut-off points for IQ measurements of mental retardation. Faust and Miner, following recent work in the social studies of science, argue that facts are largely defined by prior theoretical or organizing constructs. To insist on only facts, in fact, obstructs scientific development. Natural science, upon which psychiatry unsuccessfully attempts to measure itself, typically hypothesizes abstract concepts which go beyond observable entities. But there is no way to bracket the prior organizing configuration. That configuration may just be very subtle, even unnoticeable.

Everything is based on some theory, and the theory in this case is a biopsychiatric one. The neo-Kraepelinians actually put forth a claim to a neo-Kraepelinian theory, while at the same time denying the existence of *any* theory. They thus put forth a theory in the guise of a non-theory, and at the same time command others to avoid theoretical models. The claim to be atheoretical is really a technical means to avoid a political question, namely, who should have the power to define and implement psychiatric knowledge and practice?

According to biopsychiatric nosologists, the symptom clusters and categorical entities which form the basis for DSM-III and DSM-III-R have been scientifically detected. Mirowsky and Ross (1989) describe some fundamental problems in the diagnostic project. In the absence of "gold standards" to prove disease (e.g., demonstrable lesions), psychiatry uses concepts of latent biological classes as evidence for the validity of its diagnostic system. As Mirowsky and Ross note, however, "The problem is that the categorical biological state may not cause the symptoms on which a diagnosis is based" (p. 16).

Factor analytic studies by DSM-III developers came up with symptom clusters which do not correspond to DSM groupings, although DSM diagnostic groups have distinct profiles of mean scores on the factors. This can be understood in two ways. First, each diagnostic category can be seen as a latent class, with still unknown pathophysiologic entities (this is the belief of the DSM-III developers). Second, "We can regard the factors as separable attributes of people and the diagnostic categories as subjective constellations of those attributes" (Mirowsky and Ross, 1989, p. 17). Just as stellar constellations are mythical creations of human perception, so too, Mirowsky and Ross tell us, the diagnostic groupings are "mental overlays grouping elements that seem to form something distinct, but which may have no real connection with each other" (p. 17).

In addition to these conceptual errors, the diagnostic project contains measurement flaws. By collapsing continuous metric scales into categorical assessments, certainty is increased at the expense of reliability. Mirowsky and Ross point out that if cut-points were used on bathroom scales to categorize light and heavy, almost everyone would be classified correctly, but without any reliability of measurement.

Psychiatry seeks to achieve predictive power in a situation where certainty is low. This phenomenon is common to positivist approaches to the social world – uncertainty is viewed as an interloper to be overcome rather than as a basic feature which may provide problems that cannot be surmounted. DSM proponents claim they have achieved a high degree of interrater reliability (Klerman, 1983). A careful review by Kutchins and Kirk (1986) of a number of field trials of DSM reliability, however, demonstrates otherwise. Even using their own standard of good agreement on diagnosis (Cohen's kappa of 0.7 or higher), DSM originators only reached that level on 31 kappas, while falling below that mark on 49. Further, no major diagnostic category attained that level of agreement.

The psychiatric literature is full of DSM reliability studies on countless numbers of diagnoses on all the axes. Yet hardly any research addresses validity. Anyone can achieve interrater reliability by teaching all people the "wrong" material, and getting them to all agree on it. Chang and Bidder (1985, p. 202) put the problem this way:

> At the current stage of psychiatric knowledge, grouping patients according to selected properties rather than in terms of their total phenomenology is analogous to classifying a car by observing any four of the following eight properties: wheels, motors, headlights, radio, seats, body, windshield wipers, and exhaust systems. While an object with four of these properties might well be a car, it might also be an airplane, a helicopter, a derrick, or a tunnel driller.

Put otherwise, witch trials showed a much higher degree of interrater reliability than any DSM category (Kovel, 1988), yet we would not impute any validity to those social diagnoses.

Validity requires that the variable or item be highly correlated with a known measure, such as clinical diagnosis in medical records. Biopsychiatry is satisfied to take as construct validity the fact that DSM-III and DSM-III-R have been widely accepted by courts, prisons, third party payers, and medical schools. Actually this is merely successful social hegemony, yet the neo-Kraepelinains mistakenly take it as evidence of scientific breakthrough (Kovel, 1988). Of course, from a social constructivist approach to science, such successful social hegemony is in fact a scientific breakthrough. This is because when a society's leading institutions accept the beliefs, practices, and implications of a scientific model, a form of scientific knowledge has been "created."

By criticizing the existing attempts at "objective" measurement in psychiatry, I do not mean to imply that these can be sufficiently refined to the point that they offer a very valid picture. Indeed, my point is that psychiatry is approaching the problem incorrectly by examining patients and their symptoms as discrete phenomena without context. More so than other medical fields, psychiatry faces a large gap between *signs* noticed by the doctor and *symptoms* reported by the patient. To a large degree, the attribution of mental illness is made not on the basis of characteristics of the patient in isolation, but on the interaction between patient and provider (Rosenberg, 1984). Given what we know of the disparity between medical and lay perspectives of illness, and given the many communication problems in medical interaction, we would expect psychiatry to be particularly prone to attaining distorted information. Thus, methodological and measurement refinements will not be likely to increase the validity of psychiatric diagnosis. We can understand this better by examining professional practice styles and psychiatry's social biases.

Professional Practice Styles

Not only is validation generally lacking, but when researchers study validity the results are startling – validity is very low. For a good example, let us examine the well-known data on the diagnostic differences between the United States and the United Kingdom (Kendell, Cooper, Gourlay, Copeland, Sharpe, and Gurland, 1971). Professionals were surprised to learn that depression occurred far more often in the United Kingdom than in the United States, and that schizophrenia occurred more frequently in the United States than in the United Kingdom. In researching this problem, it was found that the differences were due to practice styles and their underlying belief systems. American practitioners were simply more likely to read certain psychotic symptoms as signs of schizophrenia when they should have done otherwise. DSM-III leaders point to their diagnostic project as a way to avoid such biases (and hence to improve validity), through the use of clear checklists and decision-trees. Yet Lipton and Simon (1985) restudied the same hospital (Manhattan State) years later, examining patient charts, and found the same level of erroneous diagnosis. In particular, clinicians picked up on a single symptom (i.e., hallucinations) which is often associated with schizophrenia, yet failed to examine corroborating symptoms. In fact, hallucinations are seen in affective disorders as well, and more details are required to make the differential diagnosis.

In other research, Rubinson, Asnis, and Friedman (1988) surveyed mental health professionals and found serious misconceptions about the diagnosis

of major depression. The most common errors were erroneous beliefs that this diagnosis required vegetative signs and a distinct quality of mood, and that it could not be made if the condition was chronic. Respondents answered incorrectly on these items 48%, 41%, and 37%, respectively.

There are not enough such studies – largely because they are threatening, or at least perceived as of doubtful value – so we cannot tell how common such errors are. But there is good reason to believe that idiosyncratic use of DSM is widespread. My own field work in the psychiatric walk-in clinic of a free-standing community mental health center provides ample evidence that clinicians resisted official diagnostic classification in order to make their own work easier, to help patients, and to criticize the official nomenclature and its underlying theory (Brown, 1987). The staff used humor and sarcasm, and invented alternative diagnoses. They minimized and normalized certain behaviors by giving mild diagnoses to protect people from employers and others. They evaded formal diagnosis when possible, in order to cover their own potential errors or to protect patients from outside agencies. Clinicians also downplayed formal, accurate diagnosis when patients came from non-psychiatric agencies (homeless shelters, welfare department, prison pre-release) since they did not want to be doing the "dirty work" for those agencies (Brown, 1989).

In examining more closely one component of the diagnostic process, the Mental Status Exam (MSE), I found other curious features. The MSE was employed in a highly variable manner in patient interviews and discussions with supervisors, and this variation was not consistent with research, teaching, and theoretical models of the MSE. In addition, clinicians and patients often found the MSE to be awkward and embarrassing. As a result there was much humor, as well as clinician disclaimers (e.g., "Some of these questions may sound silly") [Brown and Drugovich, 1989].

Arising from these observations, it makes sense to think of diagnoses as involving both *diagnostic technique* and *diagnostic work*. *Diagnostic technique* involves the formalization of classification, including the specific tasks, techniques, interviews, and chart recording necessary to make the formalized classification. These elements are mostly discrete, measurable phenomena which can be taught in specific training programs. However, the discrete and measurable aspects of these elements are only *potential*, and their actual practice varies greatly across clinicians and institutions.

Diagnostic work consists of the process by which clinicians concretely proceed with their evaluation and therapeutic tasks. Many clinicians – especially young ones in training – employ short-cuts and individual practice styles. This stems in part from their awareness that their senior colleagues do not completely accept the given standardization and formalization. Most clinicians have a basic distrust of the attempt to force fit scientifically repeatable measurements into a framework which is much too "soft" for such measure-

ment. The use of short-cuts and individual styles also comes from a desire to feel more "experienced," like the elder practitioners. Diagnostic work is thus embedded in routine work, and clinicians' desire to be more advanced makes them less accepting of the rigors of routine "scut-work" of diagnostic technique.

Looking at surveys of psychiatrists' opinions on DSM-III, we see that even those who agreed that DSM contributed positively to psychiatric training and practice nevertheless believed that it emphasized signs and symptoms at the expense of overall understanding. In one survey (Kutchins and Kirk, 1988), 35% of psychiatrists sampled said they would stop using DSM if not required to use it. Clinical psychologists are more critical; 90% said their chief application was for insurance purposes (Kutchins and Kirk, 1988). A survey of social workers found that 81% saw DSM as very important for insurance purposes. Their top four categories of usefulness – insurance, agency, Medicaid, and legal requirements – all had nothing intrinsically to do with clinical practice (Kutchins and Kirk, 1988). Another survey found that psychologists prefer social-interpersonal, nondiagnostic, and behavioral analysis rather than DSM. Nearly one-half rejected the notion that a universal nosology was valuable (Smith and Kraft, 1983).

We see, then, a significant ambivalence in that clinicians both laud and criticize the official nomenclature. This stems from the diverse social functions and mixed agendas of diagnosis. Mental health institutions, government agencies, clinicians, professional groups, and third party payers all have different needs for the diagnostic project. Generally these needs are incompatible, and prone to generate conflict.

Professional practice styles regarding diagnosis are not necessarily helpful to patients. Some of the above examples about clinician avoidance of DSM classifications – such as aiding reimbursement or defending against stigma and bias – are in the patient's interest. Yet much diagnostic behavior is part and parcel of traditional professionalism. This involves professionals' social biases, professional dominance in the mental health field, and control over the clinical interaction.

Professionals' Social Biases

Neo-Kraepelinians and their allies believe that past biases were due to lack of objective criteria, and thus new "objective diagnostic criteria" will eradicate the potential for biases (Maxmen, 1985, p. 45). This grand claim is evidence of a striking problematic which drives the biopsychiatric nosologists – employing a technical means to obtain a social end. The situation is impossible on two counts. First, as I have already pointed out, professional practice styles

vary across providers and institutions – even on the ostensibly less value-laden matter of DSM classifications of schizophrenia versus affective disorder. Post-DSM-III studies have shown that misdiagnosis remains common. Clinicians often simply do not follow the codified diagnostic schema, and even when they attempt to do so, they make many errors. In addition, ongoing struggles betwen biopsychiatric, psychoanalytic, behaviorist, and community approaches lead clinicians to come up with varying diagnoses.

Second, race, sex, and class bias – which have long been central features of psychiatric diagnosis – are much more value-laden, and will undoubtedly be even harder to eradicate with technical classification. These biases are part of the overall culture, and invariably will show up in major social institutions. This is especially the case in the mental health field, since it has so much latitude for interpretation.

Sexism in diagnosis has been shown to reflect continual social attitudes, as well as historically changing patterns (Chesler, 1972; Smith and David, 1975). Continual social attitudes are usually seen in sex differences in definitions of mental health and illness. In Broverman, Broverman, Clarkson, Rosenkrantz, and Vogel's (1970) classic study, mental health professionals responded to an open-ended question on the nature of mental health for men, women, and humans in general. The mentally healthy woman was defined by her similarity to overall stereotypes of female passivity; the mentally healthy man by his similarity to acceptable male dominance; the human in general was the same as the man.

It is in the historically changing patterns that we observe drastic evidence of diagnostic sexism. I have already mentioned Lunbeck's (1987) analysis of female psychopathy. In the late 19th century, neurasthenia was widely abused as a disease category designed to keep middle and upper class women from active participation in social life. Hysteria has been another widely disputed diagnosis, now discredited, although many observers believe that the "borderline" diagnosis today serves some of the same functions.

Researchers continue to find differentials in diagnosis by sex. Women are more likely to be diagnosed with depression, phobias, and histrionic personality disorders, while men are more disposed to paranoid personality disorders and antisocial personality disorders. It is unclear to what extent these are real differences attributable to social factors, the result of professional bias, or a combination of both. What is clear is that these differences provoke considerable criticism of official diagnostic approaches, and demand our attention.

Racism in diagnosis has also been widely studied. As with sexism, we can look at both continual ideology and historically specific diagnoses. Beginning in slavery, racism led psychiatrists to conceptualize blacks as belonging to a separate race which was inferior in neurological, physiological, and emo-

tional capacity. In the 19th and early 20th centuries such eminent mental health professionals as G. Stanley Hall, William McDougall, and William Alanson White pursued this "scientific racism" which viewed blacks as a race still in a childlike social development (Thomas and Sillen, 1972, pp. 1–22).

In terms of specific diagnostic practices, perhaps the best known early epidemiological example is the exaggeration of black insanity according to the 1840 census. This data, reported as true in the *American Journal of Insanity*, claimed that blacks had higher rates of madness in free states – as high as one in 14. The data were clearly fabricated, since insane blacks were reported in counties where no blacks at all lived. Yet the data were widely used for such significant political action as President John Calhoun's 1844 extension of slavery to Texas. Despite clear disproof of this data, psychiatrists in the Reconstruction era continued to cite it as evidence of the beneficial aspects of slavery (Thomas and Sillen, 1972, pp. 16–20).

From the 19th century well into the 20th, psychiatry maintained that blacks were rarely depressed. The explanations usually centered around the idea of a happy-go-lucky personality or the notion that blacks have less to lose in terms of prestige, esteem, possessions, and relationships (Thomas and Sillen, 1972, p. 128–129). Higher black rates of schizophrenia and paranoid personality disorders, combined with lower black rates of affective disorders, were often explained in terms of innate racial differences. Critics of traditional diagnosis have argued that the prevailing diagnostic categories are largely a result of professional bias. As with sex biases, there is undoubtedly a combination of bias and real difference.

To the extent that differentials are caused by professional ideology, sex and race biases have not been altered in the post-DSM-III era. Loring and Powell (1988) were interested in whether sex and race of psychiatrist and client affected diagnosis. They used an analogue study providing two real cases, and varying four categories of race and sex and a fifth category of no information. Loring and Powell found that psychiatrists were more likely to concur on the diagnosis of case studies when no information on the client's race and sex was available. When such information was available, psychiatrists tended to come up with the correct diagnosis when the client's race and sex were the same as their own. Male psychiatrists were more likely to find depression in women clients; women were unlikely to apply that category at all. Black psychiatrists gave white males the least serious diagnoses. All psychiatrists tended to give blacks the more serious diagnoses. These findings suggest that people view more seriously the abnormality or rule-breaking of those who are different from them. In a similar vein, Rosenfield (1982) found that people were more likely to be committed to mental hospitals for behaviors incongruent with their sex roles. From evidence so far, then, DSM-III has not succeeded in its promise to eradicate diagnostic bias.

Let me offer one conceptual caution. That there are diagnostic biases does not, however, mitigate the fact that there may well be class, sex, and race differences in actual mental health status. In particular, Hollingshead and Redlich (1958) showed that the class differences in mental illness are to some extent "real," and attributable to varying stresses and living conditions in the social world. Likewise, women may have higher rates of depression as a result of their social roles which lead them to be more attuned to emotional life. And blacks may have higher rates of antisocial behavior due to living in a world hostile to them. A culturally sensitive mental health system would have to deal both with social differentials in diagnosis and diagnostic bias. Further, a research effort in the sociology of diagnosis faces a major challenge in partialling out these two phenomena.

Professional Dominance

The ascendancy of the diagnostic project reflects the elite stature of research over clinical practice. Developments within and without the mental health professions have combined to make research on diagnostic categories a valued endeavor. Diagnostic researchers also see themselves as "correcting" the errors of clinical impressionism. This is related to psychiatric defensiveness against the growth of non-medical mental health professions. A strict diagnostic schema, particularly one seeking to incorporate medical evidence, allows psychiatrists to reassert their dominance over the other professions. Diagnosis has, of course, previously been affected by the degree of professional power. Temerlin's (1968) famous experiment showed how clinicians were prone to follow the suggestion effects of experts. Psychiatrists' suggestions were most likely to be followed, leading to a more or less severe diagnosis depending upon the expert's overt cues.

This is but one example of the certainty which psychiatry holds up to safeguard its professional position. We see another case of unwarranted certainty in Rosenhan's (1973) oft-cited study, which showed that psychiatrists diagnosed and admitted to hospitals healthy "pseudopatients" who presented themselves with no other evidence than that they heard voices.

This certitude is planted in young psychiatric residents during the professional socialization of the training process. Blum and Rosenberg (1968) concluded from their study of residents that journeymen held apprentices to a higher standard of purity than would later be necessary. The purpose was to convince the residents that there is a clear set of skills which must be mastered in order to progress. Light (1980) also observed resident training, and found that diagnostic instruction was a central part of overall socialization which sought to provide certainty to a disunified profession which holds multiple needs and goals.

Control of the Clinical Interaction

The same professional desire for certainty which permeates professional hierarchies also dominates the clinical encounter. DSM-based diagnosis represents a *power-linguistic approach* to categorization, in which patient subjectivity is sacrificed to clinical objectivity. As Kovel (1988) points out, DSM allows for an "objectifying gaze" rather than an intersubjective dialogue. Although mental disorders are parts of a system of social relations, DSM makes diagnosis in the abstract by separating persons from their social world. At the same time, relying on diagnosis provides detachment. Detachment is taught as a positive form of achieving objective understanding, and is also a desired goal for clinicians who feel overburdened by their work.

Although diagnosis is so crucial to the official approach to mental illness, it is treated in a curiously secretive fashion. Psychiatrists are somewhat reluctant to inform patients and families of the diagnosis, especially for schizophrenia. A survey of 221 psychiatrists (Green and Gantt, 1987) found that 75% would always tell the patient of manic-depressive illness, 73% of unipolar depression, and 31% of schizophrenia. If the category "usually inform" is added, the figure is 91% for the two affective disorders and 58% for schizophrenia. Among the clinicians I studied, two-thirds would share the diagnosis with the patient, but only 5% would bring it up on their own. This secrecy and aversion to disclosure clearly cements professional control of the interaction.

Apart from any of the other constraints I have already addressed, the status and knowledge differentials between patient and professional are enough to produce disparate viewpoints among doctors and patients concerning the meaningfulness of certain data and how it should be used. Even if there is a generally consensual approach between client and professional, the process of decoding and interpreting information is dynamic and interactive. Certain bits of information are sought or offered, leading to decisions to ask for other bits. Opinions, attitudes, emotions, and styles are in play at each step of this process, for the diagnosis carries with it a large number of implications: future treatment, future limitations, reimbursement, stigma, potential reconstruction of identity as a chronic patient. Further, the process by which the diagnosis is arrived at contains the kernel – or even the template – of the continuing therapeutic relationship in terms of authority relations, mutual participation, comfortableness, directedness, and satisfaction. As Glaser and Strauss (1965, p. 18) argue, "From a sociological perspective, the important thing about any diagnosis, whether correctly established or not, is that it involves questions of definition."

The *goals* of diagnosis are more important for the clinician than the patient. The clinician is bound by financial, bureaucratic, and professional

pressures which demand official diagnosis. As well, the clinician wishes the certainty and control which is obtainable from naming the problem. To some degree, the patient also wants the control which comes with the name. A diagnosis seems to remove the mystery of the problem by giving it a name upon which hinge future considerations of treatment, cure, personal and social implication of the problem, and social acceptance of one's diminished abilities. Yet for patients, diagnosis is less important than a broad understanding of their problems and what can be done for them.

Patients, like clinicians, use cues as a way of recognizing the disorder. Three types of cues – symptomatological, behavioral, and communicative – disturb the taken-for-granted sense of order. This is to some degree complementary to doctors' diagnostic actions: for both parties, the naming of a diagnosis helps people in "making sense of problematic experience," since "something unknown, potentially dangerous, and worrying becomes assimilated into a familiar order" (Locker, 1981, pp. 47–50).

Twenty years ago, Levinson, Merrifield, and Berg (1967) examined the same clinic where I conducted research. They found that an ideal, objective "diagnostic model" was in fact less common than a "suitability model" that selected psychotherapy clients. Despite changes in the mandate of the clinic, twenty years later suitability remains a powerful characteristic of patient selection. Suitable candidates typically are verbal and articulate middle-class persons who staff view as "healthy neurotics." Thus the "objective" gaze of DSM is short-circuited by a more subjective approach which carries its own biases.

Thus what purports to be a *diagnostic* process is in fact a *disposition* process, since the same diagnosis can lead to different dispositions. Adjustment reactions of various types and dysthymic disorder (what pre-DSM-III nomenclautre called "depressive neurosis") are often diagnosed for persons who are not very troubled, functioning well on their own, and who are able to discuss and interpret their problems. But a middle-class college or graduate student is more likely than a working-class person or welfare recipient to be offered therapy, despite similarities in diagnosis.

One woman in her early 20s, mother of 5- and 8-year-old children, came to the clinic I studied. An AFDC (welfare) recipient with a clerical work background, she felt in a rut with trying to find work. She felt she was getting little empathy and support form others though she put herself out a lot for people. One such person struck her and broke several vertebrae. She also retained many unexamined emotions about her mother being raped in their house eight years ago. This patient was able to present herself quite clearly and was articulate about many things in her history. She took an active role in asking sensible questions about the clinic, such as whether she would see the same clinician each time and whether she would have to repeat her story over again. She engaged very much with the clinician, and responded fully

to questions. But in crucial ways, her vocabulary differed from the staff's.

This client came looking for help, but was not savvy enough to say she was looking for "therapy"; this was one example of the limitations of her "treatment vocabulary." Similarly, her "vocabulary of discomfort" (Bart, 1968) was not congruent with the clinicians' vocabulary. She said too much about concrete life experiences, rather than making abstract connections. She also said she was "lonely," but not "depressed." Staff in fact took this literally, and believed the woman needed what they term "supportive therapy," i.e., periodic contact with a social worker who would encourage her to make certain social contacts. Interestingly, one of those recommended contacts would be her minister, to whom she had spoken about her problems, and who she claimed had been of minimal help. One other noncongruent discomfort vocabulary item was that she expressed guilt about her mother's rape, but did not use the term, "guilt." Compared to most working class women who came to the clinic, particularly those who like her had been teen mothers, this patient was extremely articulate and insightful. One might think that she would be an interesting challenge to take on as a psychotherapy candidate. But a query to the clinician about this elicited no answer.

From the standpoint of Balint's (1957) concept of "organizing" the illness, the psychiatrist in this case did not interpret the client's problems broadly enough so as to "organize" it as requiring therapy. If anything, the intake clinician merely listened, without interpreting. In other cases, clinicians can provide excessive interpretation which minimizes the client's problems by recasting them as inner conflicts without any reference to social surroundings. An excellent analysis is found in Scheff's (1968) analysis of a training session, found both on a phonograph recording and in a written transcript. A woman presented herself for therapy because her alcoholic husband abused her verbally and prevented her from working outside the home. The psychiatrist was hostile to her, and reframed her problem as a personal shortcoming. Only when she accepted this new "organized" illness did he offer her treatment.

Thus either with the objective DSM gaze or the subjective suitability gaze, diagnostic reasoning is the central form by which many clients judge their patients and reframe their problems and needs. Diagnosis, then, serves a gatekeeping function, in which individual practice styles and local cultures of appropriate care are manifested. I next turn to some large social gatekeeping functions.

Diagnosis as an Arena of Struggle

A sociology of diagnosis can also point to the importance of diagnosis as an arena of struggle. Diagnosis is often the location in the psychiatric world where both lay and professional critics fight over the roles and functions of

diagnoses. These struggles are ample proof that scientific discoveries are not the result of an ongoing "march of science" as much as of political battles.

Bayer's (1981) study of the psychiatric profession's response to homosexuality presents a classic example of diagnosis as an arena of struggle. Without any change in the internal "science" of psychiatry, the American Psychiatric Association dropped homosexuality as a mental disorder, based on widespread opposition from the gay rights movement and from people sympathetic to that movement's demands concerning diagnosis. Feminists, too, have taken up struggles in this arena. Proposed DSM-III-R revision discussions included "paraphilic coercive disorder" which many felt would let child sexual abusers off the hook by calling them mentally ill rather than criminal. Women's groups fought this, and the proposed diagnosis was dropped. Feminist pressure also led the APA to change "masochistic personality disorder" to "self-defeating personality disorder" (this labels the victim of wife battering, rather than the batterer), and "premenstrual syndrome" was changed to "periluteal phase dysphoric disorder" (Kaplan, 1983).

Diagnostic struggles are sometimes directed toward the inclusion of new categories. Post-traumatic stress disorder, for example, was added to DSM-III through the concerted action of Vietnam Veterans Against the War and sympathetic mental health professionals. Supporters faced opposition from the Veterans' Administration and the American Psychiatric Association, and were able to overcome this by successful mobilization of mental health professionals, veterans groups, and by media attention (Scott, 1989).

Conclusion: The Ahistorical Nature of Diagnosis

Biopsychiatric neo-Kraepelinians lay claim to a project far grander than merely a comprehensive, objective diagnostic schema. Their goal is to lead the transformation of the mental health system. This is largely defined negatively – opposing the labeling/societal reaction perspective and the anti-institutionalist attitudes that have played such a large role. These new leaders seek to strip psychiatry of any social context. They wish to place psychiatry in a technocratic framework rather than an interpretive, humanistic one. But even if the professional project goes beyond diagnosis, the diagnostic project is at the core of a larger goal. One reason for the centrality of diagnosis is that diagnosis plays a coordinating role in laying out the terms of medico-psychiatric discourse. Professional leaders have taken the diagnostic terminology of DSM and reified it into the essential statement and rationale of biopsychiatry. Another reason is that the significant social powers to whom organized psychiatry asks for support view the diagnostic schema as the proper codification of psychiatry. Third-party payers, both private and governmental, as well as state and federal bureaucrats who run mental health agencies,

have established a diagnostic determinism. Quite literally, the mental health of a client only becomes "official" when the proper DSM code is affixed.

Psychiatry is ahistorical in many ways, especially in ignoring the history of its own traditions and errors. It is striking that there has been so little criticism of DSM. As we would expect, more criticism comes from social workers and psychologists, since they lose out to psychiatry's professional dominance. Within psychiatry there is very little criticism. Criticism is stifled by a general impression, fostered by DSM leaders, that the "old way" was merely a simplistic psychoanalysis or a radical antipsychiatry. DSM proponents argue that their system avoids the social expansionism of previous times. Earlier expansionism was marked during the rich funding of the 1950s and 1960s. In that period, many large-scale epidemiological studies employed diagnosis as a major vehicle for their work, which resulted in greater social, professional, and economic attention to mental illness. Yet there is a new expansionism today, again in the self-interest of psychiatry. We see expansionism now in general research areas of rich funding and prestige, such as AIDS, aging, and homelessness. Also, some new diagnoses have the same expansionist quality, e.g., "post-luteal phase disorder," "post-traumatic stress disorder."

Psychiatry's ahistoricity is illuminated by these new categories. Despite the inclusion of new categories which have clear social contexts, psychiatry ignores its own history and the history of society. In particular, psychiatry does not ask why certain diagnostic categories appear and disappear over time. Quite simply, psychiatry cannot explain why hysteria has declined, or why narcissistic problems and codependency have grown. These are essentially sociopolitical phenomena which are not comprehensible within the medical framework of diagnosis. Because psychiatry cannot comprehend diagnosis as a socio-political phenomena, alterations to the existing traditional diagnosis models will not lead to a greater understanding of mental disorder. For that reason, a sociology of diagnosis should be further developed in order to offer a more comprehensive perspective.

References

American Psychiatric Association. (1987). *Diagnostic and statistical manual of mental disorders* (third edition, revised). Washington, D.C.: American Psychiatric Association.

Andreasen, N.C. (1984). *The broken brain: The biological revolution in psychiatry*. New York: Harper & Row.

Balint, M. (1957). *The doctor, his patient, and the illness*. New York: International Universities Press.

Bart, P. (1968). Social structure and vocabularies of discomfort. *Journal of Health and Social Behavior, 9*, 188–193.

Bayer, R. (1981). *Homosexuality and American psychiatry: The politics of diagnosis*. New York: Basic.

Belknap, I. (1956). *Human problems of a state mental hospital*. Detroit: Wayne State University Press.

Bettleheim, B. (1969). Student revolt: The hard core [Testimony presented to the House Special Subcommittee on Education, March 20, 1969]. *Vital Speeches of the Day, 15*, 405–410.

Blashfield, R.K. (1984). *The classification of psychopathology: Neo-Kraepelinian and quantitative approaches.* New York: Plenum.

Blaxter, M. (1978). Diagnosis as a category and process: The case of alcoholism. *Social Science and Medicine, 12,* 9–17.

Blum, A.F., and Rosenberg, L. (1968). Some problems involved in professionalizing social interaction: The case of psychotherapeutic training. *Journal of Health and Social Behavior, 9,* 72–85.

Broverman, I.K., Broverman, D.M., Clarkson, F.E., Rosenkrantz, P.S., and Vogel, S.R. (1970). Sex-role stereotypes and clinical judgments of mental health. *Journal of Consulting and Clinical Psychology, 34,* 1–7.

Brown, P. (1987). Diagnostic conflict and contradiction in psychiatry. *Journal of Health and Social Behavior, 28,* 37–50.

Brown, P. (1989). Psychiatric dirty work revisited: Conflicts in servicing non-psychiatric agencies. *Journal of Contemporary Ethnography, 2,* 182–201.

Brown, P., and Drugovich, M. (1989, August 12). Some of these questions may sound silly: Empirical standardization versus clinical application of the Mental Status Examination. *Paper presented at the Annual Meeting of the American Sociological Association.* San Francisco, California.

Castel, R., Castel, F., and Lovell, A. (1982). *The psychiatric society.* New York: Columbia University Press.

Caudill, W. (1958). *The psychiatric hospital as a small society.* Cambridge, Massachusetts: Harvard University Press.

Chang, M.M., and Bidder, G. (1985). Noncomparability of research results that are related to psychiatric diagnosis. *Comprehensive Psychiatry, 26,* 195–207.

Chesler, P. (1972). *Women and madness.* New York: Doubleday.

Faust, D., and Miner, R.A. (1986). The empiricist and his new clothes. *American Journal of Psychiatry, 143,* 962–967.

Freidson, E. (1970). *Profession of medicine.* New York: Dodd, Mead.

Glaser, B.G., and Strauss, A.L. (1965). *Awareness of dying.* Chicago: Aldine.

Goffman, E. (1961). *Asylums.* Garden City: Doubleday.

Guimon, J. (1989). The biases of psychiatric diagnosis. *British Journal of Psychiatry, 36,* 366–373.

Green, R.S., and Gantt, A.B. (1987). Telling patients and families the psychiatric diagnosis: A survey of psychiatrists. *Hospital and Community Psychiatry, 38,* 33–37.

Harding, C.M., Brooks, G.W., Ashikaga, T., Strauss, J.S., and Breier, A. (1987). The Vermont longitudinal study of persons with severe mental illness, I: Methodology, study sample, and overall status 32 years later. *American Journal of Psychiatry, 144,* 718–726.

Hollingshead, A.B., and Redlich, F.C. (1958). *Social class and mental illness.* New York: Wiley.

Jones, M. (1953). *The therapeutic community.* New York: Basic.

Kaplan, M. (1983). A woman's view of DSM-III. *American Psychologist, 38,* 786–792.

Kendell, R.E. (1983). DSM-III: A major advance in psychiatric nosology. In R.L. Spitzer, J.B.W. Williams, and A.E. Skodol (Eds.), *International perspectives on DSM-III* (pp. 55–68). Washington, D.C.: American Psychiatric Press.

Kendell, R.E., Cooper, J.E., Gourlay, A.J., Copeland, J.R.M., Sharpe, L., and Gurland, B.J. (1971). Diagnostic criteria of American and British psychiatrists. *Archives of General Psychiatry, 25,* 123–130.

Klerman, G.L. (1983). The significance of DSM-III in American psychiatry. In R.L. Spitzer, J.B.W. Williams, and A.E. Skodol (Eds.), *International perspectives on DSM-III* (pp. 3–25). Washington, D.C.: American Psychiatric Press.

Kovel, J. (1988). A critique of DSM-III. *Research in Law, Deviance, and Social Control, 9,* 127–146.

Kutchins, H., and Kirk, S.A. (1986). The reliability of DSM-III: A critical review. *Social Work Research and Abstracts, Winter,* 3–12.

Kutchins, H., and Kirk, S.A. (1988). The business of diagnosis: DSM-III and clinical social work. *Social Work, 33,* 215–220.

Latour, B. (1987). *Science in action: How to follow scientists and engineers through society.* Cambridge, Massachusetts: Harvard University Press.

Levinson, D.J., Merrifield, J., and Berg, K. (1967). Becoming a patient. *Archives of General Psychiatry, 17,* 385–406.

Light, D. (1980). *Becoming psychiatrists: The professional transformation of self.* New York: Norton.

Lipton, A., and Simon, F. (1985). Psychiatric diagnosis in a state hospital: Manhattan State revisited. *Hospital and Community Psychiatry, 36,* 368–373.
Locker, D. (1981). *Symptoms and illness: The cognitive organization of disorder.* London: Tavistock.
Loring, M., and Powell, B. (1988). Gender, race, and DSM-III: A study of the objectivity of psychiatric diagnostic behavior. *Journal of Health and Social Behavior, 29,* 1–22.
Lunbeck, E. (1987). A new generation of women: Progressive psychiatrists and the hypersexual female. *Feminist Studies, 13,* 513–543.
Maxmen, J.S. (1985). *The new psychiatry.* New York: Morrow.
Mirowsky, J., and Ross, C.E. (1989). Psychiatric diagnosis as reified measurement. *Journal of Health and Social Behavior, 30,* 11–25.
Rosenberg, M. (1984). A symbolic-interactionist view of psychosis. *Journal of Health and Social Behavior, 25,* 289–302.
Rosenfield, S. (1982). Sex roles and societal reactions to mental illness: The labeling of "deviant" deviance. *Journal of Health and Social Behavior, 23,* 18–24.
Rosenhan, D.L. (1973). On being sane in insane places. *Science, 179,* 250–258.
Rubinson, E., Asnis, G.M., and Friedman, J.M. (1988). Knowledge of the diagnostic criteria for major depression: A survey of mental health professionals. *Journal of Nervous and Mental Disease, 176,* 480–484.
Scheff, T.J. (1968). Negotiating reality: Notes on power in the assessment of responsibility. *Social Problems, 16,* 3–17.
Schneider, J.W., and Conrad, P. (1983). *Having epilepsy: The experience and control of illness.* Philadelphia: Temple University Press.
Scott, W.J. (1989, August 6). PTSD in DSM-III. A case study in the politics of readjustment from combat. *Paper presented at the Annual Meeting of the Society for the Study of Social Problems.* San Francisco, California.
Sharma, S.L. (1970). A historical background for the development of nosology in psychiatry and psychology. *American Psychologist, 23,* 248–253.
Smith, D., and Kraft, W.A. (1983). DSM-III: Do psychologists really want an alternative? *American Psychologist, 38,* 777–785.
Smith, D.E., and David, S.J. (Eds.). (1975). *Women look at psychiatry.* Vancouver, British Columbia: Press Gang Publishers.
Stanton, A., and Schwartz, M. (1954). *The mental hospital.* New York: Basic.
Szasz, T.S. (1970). *The manufacture of madness: A comparative study of the Inquisition and the mental health movement.* New York: Delta.
Temerlin, M.K. (1968). Suggestion effects in psychiatric diagnosis. *Journal of Nervous and Mental Diseases, 147,* 349–353.
Thomas, A., and Sillen, S. (1972). *Racism and psychiatry.* New York: Brunner/Mazel.
Wing, J.K., and Brown, G.W. (1970). *Institutionalism and schizophrenia.* London: Cambridge University Press.