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Editors
Joel Paris, MD
Jewish General Hospital
Department of Psychiatry
McGill University
Montreal, QC, Canada

James Phillips, MD
Department of Psychiatry
Yale School of Medicine
New Haven, CT, USA

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Preface

In 2013, the American Psychiatric Association is publishing the fifth edition of its Diagnostic and Statistical Manual of Mental Disorders (DSM-5). This book examines some of the conceptual and pragmatic issues raised by the new manual.

DSM has sometimes been called "the bible of psychiatry." This seems a strange term to describe a manual that only classifies mental disorders, but does not explain them or guide their treatment. Yet while earlier editions of DSM had little impact on clinical practice, DSM-III, published in 1980, was a kind of "paradigm shift," reflecting the shift of focus in American psychiatry from psychodynamics to phenomenology and neuroscience. Moreover, DSM-III introduced algorithms for diagnosis that proved popular, even if they were not followed very strictly. This edition of the manual became influential all over the world, and also became a standard for almost all research.

The controversy over DSM-III eventually blew over. Biological psychiatry won the day, and was accepted as the primary paradigm for the field. DSM-IV, published in 1994, made only minor changes in the manual. Thirty odd years later, few could remember a psychiatry that did not follow the DSM. However flawed the system was, the pace of research was slow, and most mental disorders remained poorly understood.

Nonetheless, the American Psychiatric Association felt it was time for a revision. To this end, they appointed David Kupfer, a prominent biological researcher, and Darryl Regier, their own research director, to head a task force to prepare DSM-5. This process took quite a few years, with work groups of experts asked to propose revisions based on the most recent research findings. Originally, APA hoped to introduce another paradigm shift, in which psychiatric diagnosis would be in greater harmony with neuroscience. When it became clear the data supporting these changes was too fragmentary for radical changes, it backed off from major revisions.

The final document that constitutes DSM-5 is a compromise. It is not dramatically different from DSM-IV, but reflects a tendency to see mental disorders as lying on a continuum with normality, and supports the view that half of the population can be labeled as having some kind of mental disorder. It is hoped that this model will eventually be supported by the discovery of biological markers and endophenotypes.

The chapters in this book examine DSM-5 from the point of view of these conceptual principles, and also assess the implications of its approach for clinical practice.

Several chapters consider the problem of over-diagnosis and false positives. Psychiatry has long been criticized for medicalizing and pathologizing normal variations, and over-diagnosis means over-treatment, with all the attendant side-effects of psychopharmacological interventions. At the same time, some conditions listed in DSM-5 may be underdiagnosed. This "dialectic" can best be resolved by a combination of conservatism and pragmatism. Diagnostic epidemics could discredit psychiatry by claiming that there is no essential difference between mental disorder and normality, and by forcing clinicians to treat normal people with drugs that they do not need.

One must also consider the political and economic context in which over-diagnosis occurs. The history and politics of American psychiatry is marked by a need to stand equal to other medical specialties. The creation of the new manual is seen as an attempt to create a system that is consistent with neuroscience, but that goes beyond existing data. At the same time, psychiatry hopes to legitimate itself with a scientific diagnostic system. But in DSM-5, the overall definition of mental disorder in the manual is weak, failing to distinguish psychopathology from normality. Moreover, there are powerful interests, both corporate and, public, that could profit from a highly inclusive diagnostic system.

Finally, we have to address the question of whether the vision of psychiatry guiding DSM-5 is valid. Its scientific theory corresponds to a medical approach, but does not distinguish "disease" from "illness." Thus diagnoses in psychiatry may not be "natural kinds." DSM-5 raises both conceptual and pragmatic problems that will affect the future of psychiatry. In the years to come, it will be subjected to detailed empirical testing. At the same time, the diagnostic system needs to adopt a broader model that does not reduce all of psychopathology to neuroscience. These developments could eventually lead to a better system for DSM-6.

Montreal, QC, Canada Joel Paris

Contents

Contributors

Allen Frances Department of Psychiatry, Duke University, Durham, NC, USA

Allan V. Horwitz Department of Sociology and Institute for Health, Health Care Policy, and Aging Research, Rutgers University, New Brunswick, NJ, USA

Warren Kinghorn Department of Psychiatry and Behavioral Sciences, Duke University Medical Center and Duke Divinity School, Durham, NC, USA

Aaron L. Mishara Department of Clinical Psychology, Sofia University, Palo Alto, CA, USA

Joel Paris Jewish General Hospital, Department of Psychiatry, McGill University, Montreal, QC, Canada

James Phillips Department of Psychiatry, Yale School of Medicine, New Haven, CT, USA

Joseph M. Pierre Department of Psychiatry and Biobehavioral Sciences, David Geffen School of Medicine at UCLA, Los Angeles, CA, USA

VA Greater Los Angeles Healthcare System, Los Angeles, CA, USA

Douglas Porter Central City Behavioral Health Center, New Orleans, LA, USA

John Z. Sadler Department of Psychiatry, The University of Texas, Southwestern Medical Center, Dallas, TX, USA

Michael A. Schwartz Departments of Psychiatry and Humanities in Medicine Texas A & M Health Science Center College of Medicine, Round Rock, TX, USA

Edward Shorter Faculty of Medicine, University of Toronto, ON, Canada

Owen Whooley Department of Sociology, University of New Mexico, Albuquerque, NM, USA

Part I
Historical/Ideological Perspectives

Chapter 1
The History of DSM

Edward Shorter

> *At Ohio's Academy GP meeting one year, I gave a paper on the [new] drugs, and in the discussion afterwards, a man got up and said: 'Very erudite paper, but it isn't worth a damn to me, because when you say don't give this drug to an obsessive compulsive, this drug is good in an endogenous depression, you are talking way over my head. The doctor sitting next to me might be schizophrenic or he may have an endogenous depression, I wouldn't know this.*'
>
> —Frank Ayd, one of the pioneering psychopharmacologists, at the founding meeting of the American College of Neuropsychopharmacology, 1960 [1].

Psychiatric diagnosis turns out to be complicated, probably far more so than anyone thought 50 years ago in the heyday of psychoanalysis when diagnosis didn't really count. And the story of the Diagnostic and Statistical Manual of the American Psychiatric Association is, at one level, a tale of steady progress in getting things right. At another level, it is the story of a nosological process that has, to some extent, run off the rails. Despite enormous investments of time, thought, and academic firepower, the means of establishing a reliable nosology of psychiatric illness continues to slip from our grasp.

Psychiatry has always had a nosology, or roster of classifying diseases according to some basic principle. The motto of no treatment without diagnosis is as valid in psychiatry as in any other specialty. And modern systems of classification, detached from the humoralism of the Ancients, go back to such seminal writers as Philippe Pinel in Paris [2] and August Heinroth in Leipzig [3]. Yet how reluctant nature has

E. Shorter (✉)
Faculty of Medicine, University of Toronto, 150 College St, #83D, Toronto, ON M5S 3E2, Canada
e-mail: edwardshorter@gmail.com

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been to give up her secrets! In presenting the new diagnosis delirious mania—later seen as a form of malignant catatonia—to the profession in 1849, Luther V Bell, chief physician at the McLean Asylum for the Insane in a suburb of Boston, lamented the difficulty of digging a new disease entity "from the mass of rubbish—of confused, irregular conglomerations of amorphous appearance, to separate it from the encumbrance of incidental matters, and so present it, that others may be able to satisfy themselves of its genuine individuality" [4].

Anticipating DSM

As medicine established itself increasingly as a science rather than an art in the course of the nineteenth century, the demand became loud within psychiatry for a system of classification that went beyond the rough categories of Pinel and Heinroth. In 1851 Louis Delasiauve, a veteran psychiatrist at Bicêtre mental hospital in Paris, scorned his colleagues for their uninterest in diagnosis, leading to anarchy in treatment. "I have been preoccupied over almost the entire course of my career with ways of putting an end to this. And it seems to me that the comparative study of different kinds of types, and of the analogies they have in common as well as the differences that separate them, is calculated to lead to more satisfactory data on which a nomenclature might be based" [5]. But how to derive such data?

There are three approaches to creating a nosology: reliance on authority, on consensus, or, the third, by identifying a disease by the "medical model," a well-defined process that depends on more than "consensus" in opinion or symptoms alone. At the origins of twentieth-century classifications of psychiatric illness was the principle of authority, namely the authority of Emil Kraepelin, the great German nosologist who taught in Heidelberg and in Munich. Kraepelin simply sat in the quiet of his study, deliberated, then communicated to the profession his views about disease classifications, which thereupon were almost universally adopted. (He was, of course, a very active clinician as well.) This process began with the first edition of Kraepelin's textbook in 1883 [6] and reached its maximum influence with the massive eighth edition, the last one he was to create himself [7]. The innovative aspect of the Kraepelinian system was its intention of predicting prognosis. Not the phenomenology as such determined illness classification, but "how things are going to progress," as Kraepelin's colleague Robert Gaupp put it in 1926, the year Kraepelin died. "The prognosis is the touchstone of all of our science" [8]. In an epoch that lacked effective treatments, the ability to foretell a patient's future was the very rationale of nosology.

With the sixth edition in 1899, Kraepelin made several distinctions that are still with us. He had already originated in earlier editions the diagnosis dementia praecox, which became schizophrenia in 1908 under Eugen Bleuler's pen [9]. But in 1899 Kraepelin erected a firewall between the psychosis of dementia praecox and the affective troubles of manic-depressive illness [10]. Thus the two great illnesses of psychiatry became schizophrenia and "MDI," as different from each other as chalk and cheese and, for the most part, never destined to meet, or converge.

Yet authoritarian as he was in imposing his own concepts, in a sense, on the entire world, Kraepelin was also quite thoughtful about the requirements of successful nosology: the purpose was, as he explained in 1894, to create small, homogeneous groups of patients whose illnesses had "the same etiology, course, duration, and outcome." (He gave the presentation verbally in 1892 at a psychiatric meeting but the abstract was published only in 1894 [11].) Indeed, this is the holy grail of nosology, with differential responsiveness to medication added in today.

At an international level, the tradition of determining nosology by eminent experts rather than committees continued with Aubrey Lewis, professor of psychiatry at the Maudsley Hospital after the Second World War. Lewis angled towards the view that it was not useful to distinguish between "endogenous" and "exogenous" forms of depressive illness [12]. Yet Lewis never wrote a textbook and failed to have the same comprehensive impact on nosology that Kraepelin did. In these years the continent fell silent as a source of innovative thought because of war and the Holocaust (with a few exceptions [13]), and the baton passed across the ocean to the United States and the DSM series of the American Psychiatric Association.

The DSM series began with a document much in the tradition of authoritarian pronunciamentos rather than consensus. On October 19, 1945, psychoanalyst William Menninger, in charge of psychiatric services for the US Army during World War II, promulgated on his own a diagnostic roster, called Technical Medical Bulletin no. 203, which became the immediate ancestor of the DSM series [14]. (One recalls that in these years Army psychiatry was permeated with psychoanalysis. Max Fink describes attending the Army School of Military Neuropsychiatry at Fort Sam Houston in 1946, where the curriculum was one third general psychiatry, one third neurology, and one third psychoanalysis [15].) "Medical 203," as Menninger's creation came to be called, bore an immediate Freudian flavor, dwelling at length upon "psychoneurotic disorders... resulting from the exclusion from the consciousness (i.e., repression) of powerful emotional charges, usually attached to certain infantile and childhood developmental experiences." Chief of these disorders was "anxiety," always the vaulting stone of the Freudian edifice. Menninger spoke of "anxiety reactions... unconsciously and automatically controlled by the utilization of various psychological defense mechanisms (repression, conversion, displacement, etc.)" [14].

Yet Medical 203 also bore the Kraepelinian imprint that would spill over 7 years later into the DSM series. "Psychotic disorders," meaning serious illness, constituted a separate category. And they were separated into watertight compartments: First were "schizophrenic disorders," also called, in the tradition of Adolf Meyer at Johns Hopkins University, "reactions." Kraepelin's three schizophrenic subtypes—hebephrenic, catatonic, and paranoid—were in attendance, and chronic "paranoia," without deterioration of the personality, was, as in the Kraepelinian system, singled out as separate. Then came "affective disorders," led by "manic-depressive reaction" and quite distinct from schizophrenia. This was the firewall.

Menninger distinguished among manic-depressive illness, psychotic depression, and Kraepelin's involutional melancholia. (Curious that Menninger should have retained involutional melancholia, the serious depression of midlife, after Kraepelin himself had rejected the diagnosis and made it part of MDI.) All these nosological decisions would shortly reappear in DSM-I.

DSM-I and DSM-II

In 1951 the US Public Health Service organized a working party under George Raines, who was the representative of the American Psychiatric Association, to consider revising the sixth edition of World Health Organization's International Classification of Diseases (ICD-6) [16] to bring it into correspondence with American usage. It was the output of that group that eventuated in 1952 in the first edition of the DSM series, later known as "DSM-I." Led by Raines, 44 in 1952, a former Navy neuropsychiatrist and then professor of psychiatry at Georgetown University Medical Center, DSM-I hewed fairly closely to Medical 203. It was, of course much longer and more comprehensive, yet the same psychoneuroses were laid out in detail, as were the same psychoses, which included manic-depressive illness (in the Kraepelinian sense, meaning mania and all forms of depression except neurotic depression) and schizophrenia. Medical 203 had spoken of psychotic disorders "without known organic etiology." DSM-I attributed these psychoses to "disturbance of metabolism, growth, nutrition or endocrine function" [17]. The main intellectual differences between the two documents were actually trivial, and DSM-I carried on the Meyerian tradition of labeling psychiatric disorders "reactions." Interestingly, of the six other members of the drafting committee, only one—Moses Frohlich—was an analyst, and several others had backgrounds that were military or in neuropsychiatry, or were colleagues at Georgetown.

DSM-I was virtually without influence on the international scene, although by 1967 it had reached 20 printings in the United States. Yet, with the possible exception of the WHO's own classification, promulgated in 1957, none of the other nosologies current at the time had been influential either [13]. It was the explosion of new psychopharmacologic agents in the 1950s that made the field sit up and take notice of nosology. Yet this did not have an undilutedly favorable influence on psychiatry's ability to make the kind of fine diagnostic differentiations that nosology calls for, which entails a sense of differential responsiveness.

What are the diagnoses that respond differentially to different agents? The conventional assumption was that the new drugs encouraged diagnostic differentiation, because it made a difference in prescribing whether your patient had an affective illness or schizophrenia. This may have applied in combatting the influence of psychoanalysis in the United States, where the new drugs reinforced the firewall between manic-depression and psychosis. In the US Max Fink and Donald Klein used the new drugs as a kind of "pharmacological torch" for distinguishing one disease from another [18]. But in Europe the new psychopharmacology, if anything, discouraged old traditions of fine psychopathologic differentiation. The Germans once made elaborate refinements among the different kinds of psychotic illness, and Christian Müller's Psychiatric Dictionary goes on for nine pages about the different courses of the variant forms of psychosis [19]. Yet with the new antipsychotic medications none of this differentiation mattered: all forms responded equally to chlorpromazine. As pioneering French psychopharmacologist Pierre Lambert lamented to the Collegium Internationale Neuro-Psychopharmacologicum (CINP) at its

founding meeting in Rome in 1958, "The classification of the patients, and their assignment to a more elaborated clinical entity according to a minute description of their symptoms, is a task that has been practically abandoned," an unfortunate consequence, he said, of the new psychopharmacology and loss of interest in "psychiatric nosology" [20]. So in at least part of the Atlantic community, the most thrilling development in psychiatry for years—the eruption of successful drug treatments—was working not in favor of sophisticated nosology but against it.

Meanwhile, in the United States the APA published DSM-II in 1968. It was, again, a desire to bring US nosology into accordance with the WHO's ICD series (this time ICD-8) that gave rise to DSM-II, and throughout the 1960s several international committees coordinated the drafting of the two documents [21]. (Why the United States wanted its own classification, that eventuated in ICD-9-CM in 1978, is an interesting question: the APA seems to have clung to psychoanalysis and feared the Europeans would impose concepts alien to the US psychiatric culture.)

Unlike its predecessor, DSM-II featured psychoanalysis on the bowsprit. Jointly led by Ernest Gruenberg, a Columbia professor who was not a psychoanalyst, and analyst Lawrence C. Kolb, director of the New York State Psychiatric Institute, the five other members of the committee included only one further analyst, Henriette Klein. But the document had a Freudian ring.

The meat and drink were the sections on "psychoses" and what had been called "psychoneurotic disorders" but that by 1968 had become "neuroses." "Schizophrenic reactions" had become in DSM-II "schizophrenia," a single disease (and, in psychoanalysis, little more than a defense against anxiety). Reactions in general were gone, and among the new neuroses introduced in 1968 was the classic psychoanalytic chestnut "hysterical neurosis." (The 1952 Manual had known only "conversion reaction" and "dissociative reaction.") What was hysterical neurosis? "An involuntary psychogenic loss or disorder of function. Symptoms characteristically begin and end suddenly in emotionally charged situations and are symbolic of the underlying conflicts" [21]. Neurasthenia, also once a favorite of Freud's, had been revived as "neurasthenic neurosis." Commented Henry Davidson, superintendent of a psychiatric hospital in New Jersey, "If we are going to take hysteria out of storage, polish it up and reinstate it, why then 'hysterical neurosis?' Why not just plain hysteria?" As well, "The dreadfully outmoded word 'neurasthenia' is back at the old stand. We are really better off without it. It is too easy a waste-basket for almost anything we can't explain and it has a wretchedly 1910 flavor about it. Better let it go with the horse-cars" (Davidson HA to Gruenberg EM, 1967 Mar 30; APA Archives (Arlington VA), Medical Director's Office, Range 37, box E-2, DSM II: "Comments on the new nomenclature.")

To recap: In the early DSMs, depression had been handled in two ways:

1. Kraepelin's manic-depressive illness had been considered a major affective disorder, part of the "psychoses" that also included schizophrenia; this meant that depression of both polarities, bipolar and melancholic unipolar, were lumped together.
2. Neurotic depression was part of the "neuroses," along with phobic neurosis, obsessive-compulsive neurosis, and so forth.

This division was much in keeping with the traditional psychiatric view that there were two very different depressions, different diseases really, the one melancholic, the other nonmelancholic, or neurasthenic, reactive, neurotic, characterological, or whatever was the adjective of the day.

A young biometrician at the New York State Psychiatric Institute named Robert Spitzer had been named "consultant" to DSM-II. He couldn't wait to get rid of hysteria, neurasthenia, and the rest of the psychoanalytic baggage. He would shortly have his chance.

DSM-III

After DSM-II, psychiatric diagnosis in the United States began to seem increasingly unsatisfactory. For one thing, the diagnosis "schizophrenia" was vastly overused, manic-depressive illness by contrast much ignored [22]. This was because the analysts had a tropism towards what they called schizophrenia as something they could work with. As Jerome Frank at Johns Hopkins University explained at a meeting of the American Psychopathological Association in 1971 (published in 1972), "The depressed patient is a poor candidate for psychotherapy. He interacts sparsely with others, is dull and unproductive, sees the world in an impoverished and stereotyped way, and really wants to be left alone." As well, said Frank, the depressed patient responded readily to such non-psychotherapeutic treatments as electroshock and antidepressants. "Young schizophrenics, on the other hand, are considered in the United States to be ideal candidates for psychotherapy—at least, psychotherapy with them is always a rewarding and challenging experience for the therapist. They have a rich inner life, are very sensitive to nuances in interpersonal behavior, and the therapeutic relationship is a lively and eventful one with constant shifts and challenges" [23]. Yet this happy state of affairs gave American diagnosis a peculiar cast in international perspective and was unacceptable in a discipline with increasingly scientific pretensions.

The powder train that led to DSM-III in 1980 began in April 1969 when Martin Katz, chief of the clinical research branch in the extramural program of the National Institute of Mental Health (NIMH), convened in Williamsburg, Virginia, a conference on "the psychobiology of the depressive illnesses." After decades of psychoanalysis, it was finally time to hear about depression and biology, and a who's who of big names in the biological side of the field, among them Eli Robins, head of psychiatry at the country's premier biological department, Washington University in St Louis, came together at the College of William and Mary to talk about such issues as "electrolyte changes in the affective disorders" [24]. At the meeting the idea germinated that it was time to take a closer look at the classification of psychiatric illness in light of the new biological learning. This was also the beginning of NIMH's major "Collaborative Study" in the biology of depression, which in the view of psychiatric epidemiologist Myrna Weissman "brought depression to the forefront" [25].

In 1972 the first step on the road to DSM-III was trod when the Washington University group, led by Eli Robins and Samuel Guze, proposed an innovative new nosology that would be guided by such Wash U principles as careful description, verification, and validation. It was mainly the doing of the residents, inspired by the teachings of Guze and Robins, who met in Robins's office every Wednesday for months, as Paula Clayton, then a resident herself though not involved in these discussions, remembers it [26]. Robins himself was increasingly ill. Senior resident John Feighner took the initiative of writing up the diagnoses. Fritz Henn, also a resident at the time, later said, "We all sat around a table and simply made these criteria up from the old Kraepelin stuff. The idea was to be able to communicate with each other and form homogeneous groups" [27]. The residents' work—together with input from department members—appeared in 1972 as the "Washington University diagnostic criteria."

The Feighner group boiled down diagnoses quite radically. Gone were Kraepelin's manic-depressive illness and the psychoanalysts' neurotic depression. In their place arose simply "primary affective disorders: depression." Mania was another primary affective disorder. Then came schizophrenia and four of Freud's neuroses, and that was it for the main psychiatric diagnoses.

Highly innovative was the introduction of operational criteria required to get a patient into any particular diagnosis. For depression, for example, were required dysphoric mood plus at least five of eight specific criteria (e.g., loss of energy, sleep difficulty), plus an illness duration of at least a month and not caused by some other preexisting psychiatric condition [28]. This kind of fine attention to symptoms was a radical break with the psychoanalytic tradition of uninterest in symptoms and concentration on supposed intrapsychic conflict.

Shortly after the appearance of Feighner's diagnostic criteria, Martin Katz contacted Endicott, Spitzer, and Robins to create the Research Diagnostic Criteria (RDC) [29]. Spitzer and Endicott were at the New York State Psychiatric Institute, often called "PI." Spitzer, 40 years old in 1972, had trained in psychiatry at PI and was a member of the biometrics unit under Joseph Zubin. His contact with clinical psychiatry had been minimal. But he was a veritable font of enthusiasm and charismatic charm, and if anyone were equipped to overturn the massive psychoanalytic enterprise, it would be he. Endicott, 36, was a psychologist at PI and contributed sound common sense throughout the entire exercise. Spitzer and Endicott made several trips to St. Louis, staying at Robins's, and a collaborative effort began to evolve between the two groups.

These efforts reached initial fruition in 1975 with the RDC being tried on psychiatric case records. Authored principally by Spitzer, Endicott, and Robins, the RDC introduced the fateful distinction into American psychiatry between "bipolar disorder" and unipolar depression, which latter the RDC divided into "major depressive disorder" and "minor depressive disorder." Given that bipolar disorder (as distinct from unipolar depression) had originated in 1957 from German nosologist Karl Leonard, its American beachhead was led by the Wash U group, especially Winokur and Clayton [30]. But the major depression concept was refined into ten subtypes [31]. And minor depression included anxiety, giving American psychiatry

definitively a mixed depression-anxiety conception to take the place of "nervous disease" of yore [32]. The definitive version of RDC published in 1978 was essentially the nosology of 1975, with the addition of splitting bipolar disorder into types I (with mania) and II (with hypomania), but this would shortly vanish [33]. In a way, with its many finely differentiated diagnoses, the RDC represented the apex of postwar American nosology; so much of this was to disappear from DSM-III, a testimonial to the political pressure Spitzer was under in dealing with the American Psychiatric Association but freed from in RDC.

But let's not get ahead of our story. In 1974, keen to keep American diagnostics in step with the new draft of the World Health Organization's ever evolving ICD series, the APA appointed Spitzer head of a task force to rejig American psychiatric nosology. Why Spitzer, a relatively junior and unknown figure? Donald Klein later said, "Bob Spitzer got the job after they offered it to Henry Brill [former deputy commissioner of the New York State Department of Mental Hygiene], who turned it down, saying he wasn't interested. Spitzer got the job because it was unimportant. The whole notion of diagnosis was just a nuisance and not really central to anybody's concern" [34].

The APA leadership had no idea what they had let themselves in for. Spitzer intended a fundamental re-creation of psychiatry's diagnoses. In keeping with the emerging alliance between Washington University and the New York State Psychiatric Institute, Spitzer appointed a Task Force with heavy representations from both camps: Robert Woodruff, Donald Goodwin, and Nancy Andreasen from the Wash U camp, and then Paula Clayton after Woodruff committed suicide. From PI came Donald Klein, who along with Max Fink was then the single most powerful voice in American nosology, Rachel Gittelman, who was married to Klein, and Endicott. Interestingly, at heated moments in the discussion, key corridor decisions were made by PI staffers such as Edward Sachar, the director, who were not even members of the Task Force! Although several members, including Spitzer and Don Klein, had been trained as analysts, by 1968 they had turned their backs on Freud and were reaching out to the new biology.

In the background of these events were Spitzer's boss Joe Zubin, and Gerald Klerman, who might be considered the fixer of American psychiatry. In 1980 52 years old and professor of psychiatry at Harvard, Klerman's fingerprint appears nowhere on the printed text yet his views were given great weight, and some feel that DSM basically gave up on classifying depressions after Klerman made it seem of such complexity [35]. As Thomas Ban observes, "It is true that Bob Spitzer did much of the work, but it was really Zubin and in some way Klerman who were trying to pursue the line that began with Kraepelin using his Zählkarten [one-page case summaries], in developing a diagnostic classification in mental illness by using psychometrics. If the DSM-III people had pursued it clean without mixing it with a consensus-based approach for identifying syndromes, they would have created something to build on" (Ban TA, personal communication, 2012 Jul 15).

In September 1974 as the Task Force was getting organized, a number of key decisions were made (Task Force on Nomenclature and Statistics, 1974 Sep 4–5; APA Archives, Professional Affairs, box 17, folder 188). First of all, so much for the