

Psychiatric Diagnosis: Lessons from the DSM-IV Past and Cautions for the DSM-5 Future

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Abstract

The American Psychiatric Association's *Diagnostic and Statistical Manual of Mental Disorders* provides the authoritative list of what are considered to be mental disorders. This list has a tremendous impact on research, funding, and treatment, as well as a variety of civil and forensic decisions. The development of this diagnostic manual is an enormous responsibility. Provided herein are lessons learned during the course of the development of the fourth edition. Noted in particular is the importance of obtaining and publishing critical reviews, restraining the unbridled creativity of experts, conducting field trials that address key issues and concerns, and conducting forthright risk-benefit analyses. It is suggested that future editions of the diagnostic manual be developed under the auspices of the Institute of Medicine. The goal would be broad representation, an evidence-based approach, disinterested recommendations, and a careful attention to the risks and benefits of each suggestion for change to the individual patient, to public policy, and to forensic applications.

Contents	
INTRODUCTION	110
PURPOSES OF THE DSM SYSTEM.....	110
CONCEPTUAL ISSUES	110
The Elusive Definition of Mental Disorder	111
The Limits of Neuroscience Understanding.....	112
Limits of Descriptive Psychiatry	112
The Epistemology of Mental Disorder	112
Why Is the DSM Classification So Messy and Atheoretical?.....	114
Is Pragmatism Practical?	114
Psychiatric Fads and Diagnostic Inflation.....	115
LESSONS FROM DSM-IV	117
Critical Review	117
Reign in the Experts	118
Field Test Proposals	118
Risk-Benefit Analysis.....	120
Diagnostic Conservatism	121
Writing Must Be Clean	122
PROBLEMS WITH DSM-5.....	122
Diagnostic Inflation.....	122
Inadequate Consultation with the Field.....	123
Inadequate Empirical Documentation	123
Inadequate Field Trials.....	124
THE FUTURE	124

INTRODUCTION

We were invited to provide a kind of retrospective summary of what we have learned about psychiatric diagnosis during our years of work together on the fourth edition of the American Psychiatric Association’s (APA) *Diagnostic and Statistical Manual of Mental Disorders* (DSM-IV; APA 1994; hereafter the acronym “DSM” is used to refer in general to the diagnostic manual rather than any particular edition) and how this experience might inform our understanding of the process that will culminate in DSM-5

in 2013. Most of this review highlights the general conceptual issues that inform any revision of the APA’s diagnostic manual. We end with specific lessons learned from DSM-IV, our concerns about the direction taken by DSM-5, and a suggestion for a radical change in how the DSM is revised in the future.

PURPOSES OF THE DSM SYSTEM

The DSM has many uses and serves many masters (Frances et al. 1995, 1989; Kendell 1975; Regier et al. 2010; Sartorius et al. 1993). It provides a common language for clinicians, a tool for researchers, and a bridge across the clinical/research interface (APA 2000). It offers a textbook of information for educators and students (First & Pincus 2002). It contains the coding system for statistical, insurance, and administrative purposes. DSM diagnoses can also often play an important role in both civil and criminal legal proceedings (Schwartz & Wiggins 2002).

For the most part, the varied goals of the DSM system are compatible, even seamless. But there are inevitable difficulties caused by having one manual attempting simultaneously to do so many different things. The criteria are somewhat too detailed to be completely convenient to clinicians; not quite detailed enough for the taste of researchers; too dull for teachers and students; and not nearly precise enough for lawyers (Frances et al. 1989). The DSM is a common denominator that can often adequately meet all of these needs but meets none perfectly.

There is one purpose the DSM does not aspire to and cannot possibly achieve. It has never claimed to be and is not any sort of “bible” of psychiatry. It is a guide to psychiatric diagnosis—no more, no less.

CONCEPTUAL ISSUES

A number of fundamental problems and issues bedevil the authors of a diagnostic manual, including (but not limited to) an elusive definition of mental disorder, the limits of neuroscience,

American Psychiatric Association (APA): a professional society of American psychiatrists, which currently has authoritative control over the development of the DSM

the limits of descriptive psychiatry, an unclear epistemology, the absence of a unified theoretical model, pragmatism, and fads. Each is discussed briefly in turn.

The Elusive Definition of Mental Disorder

Many crucial problems in psychiatric diagnosis would be much less problematic if only it were possible to frame an operational definition of mental disorder that really worked. Nosologists could use it to guide decisions on which aspects of human distress and malfunction should be considered psychiatric and which should not (Spitzer & Williams 1982, Stein et al. 2010). Clinicians could use it when deciding whether to diagnose and treat a patient on the border with normality. A meaningful definition would clear up ugly confusions in the legal system where matters of great consequence often rest on whether a mental disorder is present or absent.

Alas, we have reviewed dozens of definitions of mental disorder (and helped to write the one in DSM-IV) and must admit that none have much practical value (Wakefield 1992, Wakefield & First 2003). Historically, conditions have become mental disorders by accretion and practical necessity, not because they met some independent set of abstract and operationalized definitional criteria. Indeed, the concept of mental disorder is so amorphous, protean, and heterogeneous that it inherently defies definition—creating a hole at the center of psychiatric classification.

The current list of mental disorders certainly constitutes a hodgepodge collection. Some describe short-term states, others life-long personality. Some reflect inner misery, others bad behavior. Some represent problems rarely or never seen in normals, others are just slight accentuations of the everyday. Some reflect too little self-control, others too much. Some are quite intrinsic to the individual; others are defined against varying and changing cultural mores and stressors. Some begin in infancy, others in old age. Some affect primarily

thought; others emotions, behaviors, or interpersonal relations; and there are complex combinations of all of these. Some seem more biological, others more psychological or social.

The common themes in the definition of mental disorder are distress, disability, dyscontrol, and dysfunction (Bergner 1997, Klein 1999, Widiger & Sankis 2000), but these are very imprecise and nonspecific markers with little practical value. Ironically, the definition of mental disorder that does have abiding practical meaning is never given formal status because it is tautological and potentially highly self-serving. It would go something like, “Mental disorder is what clinicians treat and researchers research and educators teach and insurance companies pay for” (Maddux et al. 2008). Historically, this is how the individual mental disorders appear to make their way into the system (Kirk 2005) rather than being logically, rationally derived from any particular definition of mental disorder (Wakefield 2001).

The definition of mental disorder has been elastic and follows practice rather than guides it. The greater the number of mental health clinicians, the greater the number of life conditions that work their way into becoming disorders. Only six disorders were listed in the initial census of mental patients in the mid-nineteenth century; now there are close to 300. Society also has a seemingly insatiable capacity (even hunger) to accept and endorse newly minted mental disorders that help to define and explain away its emerging concerns (Kirk 2005).

We must accept that our diagnostic classification is the result of historical accretion and at times even accident without a sufficient underlying system or scientific necessity. The rules for entry have varied over time and have rarely been very rigorous. Our mental disorders are no more than fallible (if undeniably very useful) social constructs (Borsboom 2008, Borsboom et al. 2003, Maddux et al. 2008). And, despite all these limitations, the definitions of individual mental disorders contained in the DSMs achieve great practical utility in the ways described above. The DSM system is imperfect, but it is indispensable.

Diagnostic and Statistical Manual of Mental Disorders

(DSM): provides the official list of acts, behaviors, feelings, and thoughts considered by the APA to be mental disorders

Mental disorders: syndromes of acts, behaviors, feelings, and/or thoughts that result in significant distress, social impairment, and/or occupational impairment

The Limits of Neuroscience Understanding

A gaping disconnect exists between the brilliant discoveries informing genetics and neuroscience and their almost complete failure to elucidate the causes (and guide the treatment) of mental illness (Insel 2009). We have learned a great deal about how the normal brain works but not what causes psychopathology.

The basic problem is that the body is extremely complicated, and most diseases don't arise from anything resembling simple genetic causes (Kendler 2005, Rutter 2003). We are the miraculous result of exquisitely wrought DNA engineering that has to get trillions and trillions of steps just right in order to produce normal, everyday development and functioning. But any super-complicated system will have its occasional chaotic glitch. Things can and do go wrong in many different ways to produce each disease. There will likely be not one but rather hundreds of different pathways producing each illness. Figuring things out will be a very gradual process taking decades—with small steady discoveries, not breathtaking breakthroughs. If the body is fantastically complicated and difficult to unravel, imagine the challenge neuroscience faces in figuring out the many things that can go wrong in brain functioning.

Limits of Descriptive Psychiatry

Descriptive psychiatry just passed its 200th birthday, if we measure it from Pinel's creation of the first modern psychiatric classification system (Kendell 1975, Zilboorg 1941). His work was born of the Enlightenment belief that some underlying order could be imposed even on the obvious irrationality of mental illness. The premise was that any domain receiving systematic observation and classification would eventually display causal patterns (Kraepelin 1917).

This approach paralleled the major paradigm shifts in science—a careful description preceded a causal model. Kepler's

astronomical observations led to Newton's gravity. Linnaeus's classification of plants and animals led to Darwin's evolution. Mendeleev's periodic table led to Bohr's structure of the atom.

Classification in psychiatry has so far been singularly unsuccessful in promoting a breakthrough discovery of the causes of mental disorder (Kupfer et al. 2002). Where does that leave the descriptive system of psychiatry? Fairly high and dry. Nature has obviously chosen to deprive us of clear joints, ripe for carving (Kendell & Jablensky 2003, Widiger & Edmundson 2011, Zachar & Kendler 2007). There is little indication of any imminent and sweeping etiological breakthrough. Everything points toward a slow accumulation of explanatory power.

Our descriptive classification of disorders is old and tired. It has worked hard for us and continues to have many valuable and irreplaceable functions. But a paradigm shift in psychiatry awaits a deep understanding of the causes of psychopathology. Fiddling needlessly with the descriptive labels will not advance science and may actually do more harm than good in its effect on clinical care.

The Epistemology of Mental Disorder

There are four ways of depicting how we understand mental disorder:

First umpire: "There are balls and there are strikes and I call them as they are."

Second umpire: "There are balls and there are strikes and I call them as I see them."

Third umpire: "There are no balls and there are no strikes until I call them."

Fourth umpire: "I call balls and strikes according to how I need to use them."

The first umpire believes that mental disorders are real things existing "out there" that will soon reveal their secrets through scientific study. This was the predominant view among biological psychiatrists until about ten years ago, but it is now widely recognized to be a misleading and reductionistic simplification

(Kendler 2005, Paris 2011). Although the revolution in neuroscience has provided a wonderful understanding of brain functioning, the more we learn about the brain the more ineluctably complicated it proves to be.

Umpire 2 has a firmer grasp on the epistemology. Biological psychiatry has failed to produce quick, convincing explanations for any of the mental disorders because it faces a fundamental flaw in the “realist” approach of umpire 1—that mental disorders don’t really live “out there,” waiting to be explained. The umpire 2 “nominalist” position is that mental disorders are no more than useful heuristic constructs. This is now the consensus of most serious students of mental illness, even the most fervent biological psychiatrists (Charney et al. 2002, Hyman 2010, Insel 2009).

As one illustration, it has become crystal clear that there is no one prototype “schizophrenia” waiting to be explained with one incisive and sweeping biological model (Craddock & Owen 2005). Instead, a painful process of promising false starts has taught us there is no single gene, or small subset of genes, to account for schizophrenia. As Bleuler (1951) intuited, schizophrenia is rather a group of disorders, or perhaps better, a mob. There may eventually turn out to be 20 or 50 or 200 kinds of schizophrenia, and its definitions are necessarily arbitrary constructs. There is no clear right way to diagnose this gang or even much agreement on what the validators should be and how they should be applied (Kendler 1990).

Simple-minded biological reductionism has bitten the dust, and the first umpire was called out on strikes.

The third umpire takes the skepticism about our ability to know things (and our goodwill in labeling them) one step further. He denies even the possibility of fully understanding mental disorder and worries that mental disorders are social constructs very much subject to arbitrariness and misuse (Szasz 1974). The third umpire warns that the power to label can be the power to destroy (Maddux et al. 2008). Indeed, psychiatric diagnoses have served as powerful tools to exert a noxious social control in ways

that are a great embarrassment; for example, in our country, the abuse of psychiatric diagnosis to involuntarily incarcerate sexually violent predators (First & Halon 2008), and in China, to stifle political and civil discontent (Kirk 2005).

The fourth umpire takes a more optimistic and trusting view. He is a pragmatist who believes that the definition of mental disorder should be influenced by the useful purposes it is meant to serve. Understanding the limitations of psychiatric epistemology helps resolve the widespread concern about the frequent occurrence of comorbidity across disorders (Krueger & Markon 2006). Mental disorders are no more than useful constructs—they are not real and independent psychiatric illnesses with clear boundaries (Borsboom et al. 2003). Because of the need to achieve reliability (i.e., to create distinct, clearly demarcated, homogeneous constructs), DSM is a splitter’s system, with the diagnostic pie divided into many small pieces. Each of these is just description (not an independent disease). When a patient meets criteria for more than one disorder, this very often doesn’t mean that the two diagnoses are independent of one another. The multiple diagnoses are better conceived as modular building blocks, each of which adds precision and information.

The concerns about heterogeneity within diagnoses also reflect a longing for well-defined psychiatric “illnesses” (Robins & Guze 1970). Instead, we are dealing with descriptive prototypes (“schizophrenia,” “panic disorder,” “mood disorder,” etc.) that are inherently heterogeneous and will hopefully with time be divided into many true etiologically defined illnesses (Regier 2008).

Our classification of mental disorders is no more than a collection of fallible and limited constructs that seek but never find an elusive truth—but this is our best current way of communicating about mental disorders (Frances et al. 1990). And despite all its epistemological, scientific, and even clinical failings, the DSM does its job reasonably well if it is applied properly and its limitations are understood.

Why Is the DSM Classification So Messy and Atheoretical?

Psychiatric classification is necessarily a sloppy business. The desirable goal of having a classification consisting of mutually exhaustive, nonoverlapping mental disorders is simply impossible to meet.

Many people are disturbed by the fact that the DSM is not based on a neat conceptual model and instead seems to be an amalgam more influenced by history than by any clear rationale. There are alternatives to the jumbled, pedestrian, atheoretical, and purely descriptive method used in the DSM; proposals call for more unified, rational, and etiological systems that are theory driven, clever, neat, and plausible.

The new systems come in three primary types: (a) brain biology—these used to be based on correlates with neurotransmitters, but recently neural networks of various kinds are much more popular (Garvey et al. 2010, Insel 2009, Insel et al. 2010, Sanislow et al. 2010); (b) psychological dimensions—hundreds of scales have been developed and carefully tested (Krueger & Markon 2006, Widiger & Mullins-Sweatt 2009); and (c) systems based on psychodynamic, ethological, and developmental models—less popular now than they once were (Psychodynamic Diagnostic Manual Task Force 2006).

Unfortunately, none of these approaches, however elegant, might be ready for the official system of psychiatric nomenclature. The DSM must by its very nature be a conservative document that follows rather than leads the field. The problem with all of the suggestions to replace the admitted DSM jumble is that there are so many contenders, none of which has been proven or has attained wide acceptance from the field. It is not possible to choose one from among so many plausible, but necessarily parochial, systems when most clinicians have little interest in many of them, and the proponents of rival systems can make equally valid claims for their respective pet methods.

Consider, for example, the classification of personality disorders, for which both authors of

this review have been long involved (Widiger & Frances 1985). Although the second author of the current review is considerably more sanguine about the success and eventual acceptance of a dimensional classification of personality disorder (Widiger & Mullins-Sweatt 2009), the first author is considerably more pessimistic (Frances 1993). It currently appears to be the case that the classification of personality disorders is shifting toward a trait-dimensional taxonomy (Siever 2011), but the authors of DSM-5 may not be adequately appreciating how much opposition, disruption, and turmoil this may in fact generate (Shedler et al. 2010).

Our descriptive classification may not be the only, or even the optimal, way to sort things for future research. But a DSM remains necessary to carry forth the current, everyday, practical clinical and administrative work that is its first priority (Frances et al. 1990). Once we have attained a widely accepted, etiological understanding of at least some forms of psychopathology, the new insights will gradually replace our clumsy but nonetheless currently still useful system.

Is Pragmatism Practical?

Many people are troubled by the fact that practical consequences are so important in making DSM decisions and would prefer these to be settled more scientifically. They fail to appreciate that the scientific data underlying descriptive psychiatry never provide a clear and unique right answer about where to set diagnostic boundaries (Kendler 1990). All else being more or less equal scientifically (which it almost always is), by far the most important deciding factor should always be whether this change (as it will be applied in the average expectable environments) is more likely to help or hurt patients (Kendler et al. 2009).

Of course, this practical question is subject to differing interpretations of the available data and the possible extrapolations from these data (Frances et al. 1989, Kendler 1990, Widiger & Trull 1993), but this is what a thorough risk/benefit analysis is all about. We have to

live with the fact that here, as in so much of medical decision-making, the science can only take us so far and never jumps off the statistical tables to provide us with the single, right answer.

Pragmatic concerns must play so central a role because the DSM is an official system of classification that has a huge (perhaps excessive) influence on how everything works in the mental health world: who gets diagnosed, how they are treated, who pays for it, whether disability is appropriate, and whether someone can be involuntarily committed, released from legal responsibility, or sue for damages (Kirk 2005, Sadler 2005, Schwartz & Wiggins 2002). The DSM also has a diverse influence on public policy—directly or indirectly influencing things as varied as the way scarce treatment and school resources are allocated, the impact of medication on the obesity/diabetes epidemic, and how sexual offenders are (mis)handled in the legal system.

Because it has such a powerful influence on real-life (and occasionally even life-or-death) decisions, the DSM can't ignore practical consequences—intended or unintended. It has to be workaday—trying very hard not to make mistakes that will hurt people, rather than having fancy but untested paradigm-shifting ideas that can very easily wind up doing more harm than good.

There is no scientifically proven, single right way to diagnose any mental disorder—and don't let any expert tell you that there is. Clinical judgment is required in diagnosing any individual patient, no matter how fallible it may be (Garb 2005), and do-no-harm common sense is always necessary in establishing any of the DSM thresholds. Those working on DSM-5 must take responsibility for the practical consequences that their decisions will have on people's lives (Kendler et al. 2009).

Psychiatric Fads and Diagnostic Inflation

Fads in diagnosis come and go and have been with us as long as there has been psychiatry.

The fads meet a deeply felt need to explain, or at least to label, what would otherwise be unexplainable human suffering and deviance. In recent years the pace has picked up, with false epidemics coming in bunches that involve an ever-increasing proportion of the population. We are now in the midst of at least four such epidemics: autism (Eyal et al. 2010, Steuernagel 2005), attention deficit (Baughman 2008, Stolzer 2007), childhood bipolar disorder (Moreno et al. 2007), and paraphilia not otherwise specified (First & Frances 2008). And unless it comes to its senses, DSM-5 threatens to provoke several more epidemics (e.g., disruptive mood dysregulation, psychosis risk, binge eating, mixed anxiety depression, and minor neurocognitive).

Fads punctuate what has become a basic background of overdiagnosis. Normality is an endangered species. The National Institute of Mental Health (NIMH) estimates that, in any given year, 25% of the population (that's nearly 60 million people) has a diagnosable mental disorder (Kessler & Wang 2008). A prospective study found that, by age 32, 50% of the general population had qualified for an anxiety disorder, 40% for a depression, and 30% for alcohol abuse or dependence (Moffitt et al. 2010). Imagine what the rates will be like by the time these people hit age 50, or 65, or 80. In this brave new world of psychiatric overdiagnosis, few will get through life without a mental disorder.

The sharply increasing rate of mental disorder classification might have a reasonable explanation. It is perhaps not surprising to find that scientific research and increased knowledge have led to the recognition of more instances of psychopathology (Wakefield 2001). Perhaps the assumption that only a small minority of the population currently has, or will ever have, a mental disorder (Regier & Narrow 2002) is questionable. Very few persons fail to have at least some physical disorders, and all persons suffer from quite a few physical disorders throughout their lifetime. It is unclear why it should be different for mental disorders, as if most persons have been fortunate to

have obtained no problematic genetic dispositions or vulnerabilities and have never sustained any psychological injuries or never have experienced significant economic, environmental, or interpersonal stress, pressure, or conflict that would tax or strain psychological functioning (Widiger 2011b).

However, it is also reasonable to consider another possibility: that the epidemics in psychiatry are caused by changing diagnostic fashions (Batstra & Frances 2011). There are no objective tests in psychiatry; no X-ray, laboratory, or exam finding that says definitively that someone does or does not have a mental disorder (Steffens & Krishnan 2003). What is diagnosed as mental disorder is very sensitive to professional and social contextual forces. Rates of disorder rise easily because mental disorder has such fluid boundaries with normality (Horwitz & Wakefield 2007).

What are the most important contextual forces?

- 1) DSM-III (APA 1980) made psychiatric diagnosis interesting and accessible to the general public. More than one million copies of each edition have been sold—more to laypersons than to mental health professionals. The widespread appeal of the DSM is in its clear definitions, which allow people to diagnose themselves and family members.
- 2) This interacts with the fact that it is fairly easy to meet criteria for one or another DSM diagnosis. The definitional thresholds may be set too low, and the DSM system has included many new diagnoses that are very common in the general population (Sommers & Satel 2005).
- 3) The pharmaceutical industry has proven to be fairly unsuccessful in developing new and improved medications. But it is wonderfully effective at marketing existing wares and is an important engine in overdiagnosis and the spread of psychiatric epidemics. The drug companies are skilled at mounting a full-court press that includes educating doctors,

supporting advocacy groups and professional associations, controlling research, and direct-to-consumer advertising (Baughman 2008, Lane 2007, Mayes & Horwitz 2005).

- 4) Patient and family advocacy groups have played an important role in calling attention to neglected needs; in lobbying for clinical, school, and research programs; and in reducing stigma and promoting group and community support. There are times, however, when advocating for those with a disorder can spill over and promote the spread of the disorder to others who are mislabeled.
- 5) It is no accident that the recent epidemics have mostly occurred in the childhood disorders. There are two contributing factors. The first is the push by drug companies into this new market. The second is that the provision of special educational services often requires that there be a DSM diagnosis.
- 6) The Internet is a wonderful communication tool that provides a wealth of information and creates a social network of informed consumers. But it can also contribute to the spread of epidemics. Disorder-focused Websites (often run by patients and families) provide a powerfully attractive forum and support system that draws people who may inaccurately self-overdiagnose in order to be part of the Internet community.
- 7) The media feed off of and feed into the public interest in mental disorders. This happens in two ways. Periodically, the media become obsessed with one or another celebrity whose public meltdown seems related to a real or imagined mental disorder. The mental disorder is then endlessly dissected by the media.
- 8) We live in a society that is perfectionistic in its expectations and intolerant of what were previously considered to be normal and expectable distress and individual differences.

LESSONS FROM DSM-IV

In the development of DSM-IV (APA 1994), we suggested that “the major innovation of DSM-IV will not be in its having surprising new content but rather will reside in the systematic and explicit method by which DSM-IV will be constructed and documented” (Frances et al. 1989, p. 375). The DSM-IV committee aspired to use a conservative threshold for the inclusion of new diagnoses and to have decisions that were guided more explicitly by the scientific literature. Proposals for additions, deletions, or revisions were guided by literature reviews that were required to use a specific meta-analytic format that maximized the potential for informative critical review, containing (for example) a method section that documented explicitly the criteria for including and excluding studies and the process by which the literature had been reviewed (Frances et al. 1989). The purpose of this structure was to make it easier to discover whether the author was confining his/her review only to studies that were consistent with a particular proposal and failing to acknowledge opposing perspectives. The reviews were published within the three-volume DSM-IV *Sourcebook* (e.g., Widiger et al. 1994). Testable questions that could be addressed with existing data sets were also explored in additional studies, which emphasized the aggregation of multiple data sets from independent researchers, and 12 field trials were conducted to provide reliability and validity data on proposed revisions. The primary purposes of the field trials were to address fundamental questions or concerns with regard to a particular proposal as well as to compare and contrast alternative proposals. Perhaps most importantly of all, critical reviews of these projects were obtained by sending initial drafts to advisors or consultants to a respective work group, by presenting drafts at relevant conferences, and by submitting drafts to peer-reviewed journals (Widiger et al. 1991). We felt that this process was reasonably successful in providing and documenting a more scientific foundation for the diagnostic manual, albeit a number of lessons clearly

were learned. It could have been done much better.

Critical Review

Journals have editorial boards for one obvious reason: No scientist can really be trusted to provide a fair and balanced review of the scientific literature. Similarly, we could not trust the authors of proposed revisions to the diagnostic manual to provide an objective, accurate, or trustworthy presentation of the science, problems, benefits, or risks of a particular proposal. No diagnostic manual can be constructed without a group of fallible persons interpreting the results of existing research. These persons ideally would be consensus scholars with no preconceptions and with an adequate understanding of the research and issues, but “participants are [in fact] rarely neutral with respect to the issues they are addressing, and it can be difficult for them to provide a dispassionate, balanced, and objective review and interpretation of the research” (Widiger & Trull 1993, p. 73). Therefore, each proposal was subjected to a critical review.

Nevertheless, the process of critical review was inadequately systematic, comprehensive, and documented. The authors of DSM-IV indicated that “the methods and results of each stage of review were shared with advisors who evaluated initial drafts of the literature reviews for inaccuracies, gaps in coverage, and biased interpretations of the research” (Davis et al. 1998, p. 6), and there are indeed indications that considerable effort was made to obtain critical review and to maintain quality control. However, the credibility and success of the process of review would be facilitated by a more systematic solicitation and documentation of critiques (Widiger & Clark 2000). Informed persons outside the decision-making process, especially persons likely to be critical of a proposal, should have been requested to provide written critiques, with the understanding that these would be published alongside the proponents’ reviews in the archival documents.

Reign in the Experts

Experts are absolutely necessary to the development of a diagnostic system but are also a serious threat to its generalizability and safety. We depend heavily on expert recommendations on how to change the diagnostic system, but we must not follow the recommendations blindly, without a careful consideration of the many risks that may be outside the expert's expertise and experience.

People are chosen to work on the DSM because they have specialized in one or another psychiatric diagnosis. To become an expert, you must devote yourself to extensive research on your given diagnosis or proposal. But the expert's research and clinical experiences occur in a rarified, hothouse environment that does not always translate very well to the exigencies of real-world practice. Diagnostic criteria (and consequent treatment recommendations) that work well in the expert's own hands may create havoc in everyday clinical settings because clinicians have much less expertise and time to apply them and a less-selected (and therefore much more difficult to diagnose) patient population.

Most importantly, experts tend to be biased in the same direction. They always worry themselves greatly about false negatives—the missed diagnosis or patient who doesn't fit neatly into the existing criterion sets. In contrast, experts are relatively indifferent to the much more serious problem of false positive patients who receive unnecessary diagnosis, treatment, and stigma and incur needless expense. In the first author's work on three DSMs over a period of 20 years, never once did he recall an expert make a suggestion that would reduce the boundary of his pet disorder. In contrast, they very often clamored for expansions.

Experts usually have considerable confidence in their views and state them assertively, in a way that seems to close the argument. They often have little awareness of how much they don't know about average clinical practice, drug company marketing effects, resource allocation, health economics, forensic misuse, and

all the practical influences of a DSM. Experts are absolutely essential to the creation of a DSM, but their recommendations always need balancing from a much broader perspective.

How to reign in the experts is not entirely clear, but it does require a firm hand and close supervision. In addition, it would be useful to consider widening membership or at least making the process of membership selection more open to scrutiny. Sadler (2005) suggests that much of the controversy and disgruntlement with each edition of the APA's diagnostic manual has been due to the absence of adequate opportunity for persons with divergent viewpoints to participate within the decision-making process. The optimal decision may at times be politically incorrect. The authors of the diagnostic manual should have the authority to make innovative decisions that are scientifically justified even when they are contrary to general clinical consensus (Widiger & Clark 2000). DSM-III (APA 1980) was innovative in large part because its chairperson, Dr. Spitzer, was willing to go against the stream. However, even the most innovative of decisions should be informed by a fair hearing of the diversity of perspectives, and these viewpoints and perspectives should be systematically, comprehensively, and enthusiastically solicited.

Perhaps the most important decision to be made in the development of the diagnostic manual is whom will serve on the task force and work groups, as these persons will have the primary authority and power in constructing the manual. Yet, this is the one aspect of the process of the development of the diagnostic manual that is least open to public scrutiny or review.

Field Test Proposals

Existing research will always be inadequate in documenting the likely effects of proposed revisions, and it is beyond the expertise of the authors of a nomenclature to fully anticipate them (Blashfield et al. 1992). The fact that DSM-III-R (APA 1987) was constructed largely to correct the many unanticipated problems and errors contained within DSM-III

(APA 1980) is itself testament for the importance of conducting adequate pilot testing (Widiger & Clark 2000). The failure to conduct pilot studies of a criterion set is uncomfortably comparable to releasing a psychological test for publication in the absence of validation data.

It is remarkable that no field testing was conducted for many of the diagnostic criterion sets that received final approval for inclusion within DSM-IV, given the substantial significance of this official nomenclature for many important social, forensic, and clinical decisions (Frances et al. 1990). The rationale for diagnostic criteria may be well intended and even compelling, but how they will in fact be used or understood by clinicians and researchers, and how they will relate empirically to other diagnostic criteria within typical clinical settings, will often be surprisingly problematic (Blashfield et al. 1992).

The field testing conducted for DSM-IV was much more substantial than had been conducted for prior editions of the manual (Davis et al. 1998), but the pilot research still fell far short of being comprehensive, due in part to the absence of adequate funding. The DSM-IV field trials were funded largely by the NIMH. The entire budget was generous, but when distributed across the numerous sites of the 12 studies, the funding was grossly inadequate. The budget for a sufficient number of field trials to cover all of the proposed revisions would be substantial and well beyond a reasonable support by NIMH, but the costs may in fact be well within the range of the profits that have been generated by the sales of the book (Zimmerman 1988).

The target of DSM-III and DSM III-R field testing was to establish the reliability of psychiatric diagnosis. This was a crucial goal because the proven low reliability of DSM-II (APA 1968) was an existential threat to psychiatric diagnosis and a challenge to the credibility of all mental health work (Spitzer et al. 1980). Thirty years of reliability testing have demonstrated that excellent reliability can generally be achieved in research settings, but that reliability in more typical practice is at best only

fair. DSM-IV field testing also included reliability checks, but this was no longer the primary or most interesting question. The real importance of field testing must now be to determine the impact of different options on rates and false positives. Otherwise, there is a strong chance that new proposals will unwittingly further what is already a rampant diagnostic inflation.

Minimally, field trials need to be focused on the precise concerns or issues that will bedevil a particular proposal rather than simply demonstrate that clinicians are able to apply the diagnostic criterion set with the help of structured interviews. The ideal field test would study how the diagnostic manual will eventually perform under conditions most closely approximating its future everyday use. The goal is to avoid unpleasant surprises in translation from what has been written on paper to what is practiced in real life.

No field test can ever approach the ideal. The very act of studying the DSM's performance always improves it beyond what can be achieved when no one is looking. Field tests are almost always conducted on samples of convenience gathered in academic settings. The diagnoses are made by expert interviewers, on their best behavior, using detailed scripted interviews, on selected fairly easy patients. This contrasts with real life, especially in primary care, where often-inexpert interviewers make almost casual diagnoses on the most difficult-to-evaluate patients.

Field tests will also fail to account for the pressures that will lead to systematic, future misuse—especially the drug company marketing of mental disorders that leads to overdiagnosis. It is thus wise always to regard field trial results as the best possible case and to assume that the DSM will perform worse, possibly much worse, in actual practice. If a change in the DSM can possibly be misused, it will be misused, and usually in ways that could never be predicted in the field trials. The DSM-IV field trials were meticulously conducted but completely failed to predict the later false epidemics in attention deficit, bipolar, and autistic

ADD: attention deficit disorder

disorders. Field trials can smoke out some problems but will inevitably miss others.

Risk-Benefit Analysis

Much has been written about the validators of psychiatric diagnosis and how they should influence the DSM (Kendler 1990, Kendler et al. 2009). A problem is that available information on the validators for most diagnoses is usually equivocal and inconsistent. In addition, proponents of a respective proposal (the persons typically placed in charge of designing a field trial) will select validators that will most likely yield positive findings. They are unlikely to put their proposal to a critical, severe test. Minimally, field trials should include validators selected by critics of a respective proposal. These persons will provide the most useful validators for identifying the likely problems, flaws, and limitations of a respective proposal.

Perhaps the most important validator is how any decision will help or harm patient care, given the foreseeable circumstances under which it will be used. A balanced risk/benefit assessment provides a full appraisal of the risks of each proposal, not just its presumed benefits. Questions regarding the types of risks that should be considered include:

- 1) What is the rate of false positive diagnosis in research studies? This sets a lower limit, since research is performed by the most skilled diagnosticians working with a group of highly selected, relatively easy-to-diagnose patients.
- 2) Is the diagnosis likely to be made frequently in primary care practice? If so, the false positive rate will undoubtedly be much higher because clinicians will have less time and expertise and patients will be at the boundary with normal, where accurate diagnosis is most difficult.
- 3) Are powerful outside forces likely to turn this proposal into a fad diagnosis, causing a false epidemic? One should try to predict the possible role of drug companies, advocacy groups, and the media; the effects of celebrity contagion; the

requirements for special school services; and the needs of the correctional system. Of the nearly 100 new diagnoses suggested for DSM-IV, we rejected all but a few—making calculated gambles by including Asperger's and bipolar II. Both of these were reasonable decisions that filled an important vacuum in the diagnostic classification. But each has also been wildly overused in ways that were never intended.

We made only small cosmetic changes in the definition of attention deficit disorder (ADD) and expected an increased rate of less than 15% (Frances et al. 1995). Several years later, the convergence of two anticipated events led to the doubling of rates: (*a*) the Food and Drug Administration approved new drugs for ADD, and (*b*) drug companies were given permission to conduct direct-to-consumer advertising campaigns. These instigated aggressive drug marketing of the ADD diagnosis targeted to parents, teachers, therapists, and clinicians.

Once the DSM is published, its creators have no control over how it will be used or misused. If a change can possibly serve to encourage a diagnostic fad and exploding rates, then it probably will do so—particularly if some powerful interest pushes the diagnosis. Because field tests are usually done in expert settings, they are poor predictors of the future use of criteria and the rates of psychiatric disorder. Although it is never possible to quantify accurately the risk of triggering an epidemic, it would seem irresponsible not to consider this risk.

- 4) Is there a treatment for the proposed disorder that has proven its efficacy? If not, given all the risks, what is the remaining benefit?
- 5) What are the risks of treatment? The way the world works, it must be assumed that the treatment will usually be a medication (whenever one is available, however unproven its benefits). What are the side effects, complications, and costs of the medication? How long will be the likely duration of treatment? What is the

risk/benefit for true positives? What are the unopposed risks, costs, and complications for the false positives?

- 6) What are the potential forensic problems and the effects on insurance and disability?
- 7) What will be the impact on the new patient's experience of stigma and on his or her sense of personal control and responsibility?
- 8) Will adding this diagnosis trivialize the concept of mental disorder? It was for this reason alone that caffeine dependence was excluded in DSM-IV, although it certainly sometimes exists as a clinical problem.

Diagnostic Conservatism

DSM-IV had a decidedly conservative bias and strict ground rules for making changes (Frances et al. 1989). We developed this policy for a number of reasons. One was that the diagnostic system had previously been in great flux, with the rapid-fire appearance within seven years of DSM-III and DSM-III-R. It needed a period of stability (Zimmerman 1988). The two previous DSMs were the product of an innovative and charismatic figure who single-handedly moved the field by dint of his energy, determination, and grit. After his accomplishments were realized, it was time for a less personalized leadership and for the field at large to reclaim responsibility for its diagnostic system.

Most decisions made for DSM-III and DSM-III-R were fairly arbitrary, with plausible supporting arguments that could have gone either way. Making additional equally arbitrary changes didn't make much sense. The scientific evidence supporting proposed changes was usually meager. Requiring that all changes be based on substantial evidence usually silenced even the most passionate advocates. The literatures are not only thin but also mostly derived from highly specialized research settings that have questionable generalizability to the real world. As Spitzer (1991) cogently suggested for DSM-IV, "My own prediction is that when final

decisions are made about DSM-IV, they will still be based primarily on expert consensus, rather than on data, as was the case with the DSM-III and DSM-III-R" (p. 294). Spitzer (1985) even further acknowledged, "Because appeals to objective data for resolving nosologic controversies were relatively rare, speaking and writing skills (rhetoric) played an important role in resolving controversies" (p. 523). The data sets supporting proposed changes for DSM-IV were remarkably thin, and those for DSM-5 are no more convincing. The data rarely reach off the table, grab you by the throat, and cry out for any one specific change. DSM definitions could plausibly be done many different ways, and the data rarely speak with one voice (Kendler 1990, Widiger & Trull 1993). The practical value of the science has been greatly oversold.

Every change made by DSM-IV could have enormous practical consequences: in determining who got medicines that could greatly help or greatly harm; deciding insurance and disability claims; and influencing life-or-death forensic issues. The conservative view is "do no harm"; revise the system with a light and cautious touch only when you are sure of what you are doing, after a thorough risk/benefit analysis. This approach assumes that things are there for a reason and are imbricated in a complex set of relations. Support for the value of our conservative approach came later, when we realized the unintended impact of some small and seemingly inconsequential changes we did make. We had the painful experience of changing a word or two in a seemingly harmless way and then later learning that we had helped trigger an epidemic of false positives (as in ADD) or a forensic nightmare (e.g., the misuse of paraphilia not otherwise specified in the extended civil commitment of sexual offenders; First & Frances 2008, Frances 2010).

DSM-IV made the conservative policy decision to require the same degree of empirical evidence to remove a diagnosis as to add one (Frances et al. 1989). Only one existing diagnosis was deleted (i.e., idiosyncratic alcohol intoxication). This was intended to stabilize the

diagnostic system and avoid arbitrary changes in either direction. When in doubt and without clear empirical evidence, we chose to stand pat thinking this would do least harm. This resulted in the DSM-IV grandfathering in a number of diagnoses whose tenure in the system was based on historical tradition rather than the level of empirical evidence that would have been required to gain entry as a new diagnosis in DSM-IV.

A strong case can be made that these weak diagnoses should not have received a free ride, as they had through previous revisions of DSM. Perhaps we should have had less respect for tenure. It has been suggested that standing pat can actually do a lot of harm and that all diagnoses—new and old—should be subjected to the same level of risk/benefit analysis. Some of these grandfathered diagnoses may have no clear benefit to compensate for the harm they might do.

Writing Must Be Clean

It is very difficult to write good, clean, trouble-free sets of diagnostic criteria. This arcane craft requires an unusual combination of clinical skills in differential diagnosis; computer skills in algorithmic logic; and legal skills of precision and vigilance about later possible misinterpretation. The closest thing to it in psychiatry is writing rating scales (and these are much easier because each scale does not interact with every other). We have known only three masters of criteria writing—Robert Spitzer and Janet Williams, who honed the DSM-III and DSM-III-R diagnostic criteria, and Michael First, who did the same for DSM-IV.

In all our DSM-IV experience, there was never a criterion set written by a work group that was remotely ready for publishing and did not need extensive editing. This did not surprise us. The work group members were selected for their clinical and research expertise, not for skills in writing criteria, which they clearly did not have. One obvious mistake we made in DSM-IV was the seemingly trivial wording changes in the paraphilia section that have led to a catastrophic misuse of psychiatric diagnosis

in the questionably constitutional involuntary commitment of sexually violent predators (First & Frances 2008, First & Halon 2008). The writing of diagnostic criteria has to be precise enough to withstand the rigors of future unanticipated forensic misunderstanding; every word should be vetted by forensic experts; if something can possibly be misunderstood, it will be misunderstood; even small changes can cause big, bad consequences (Frances 2010).

PROBLEMS WITH DSM-5

The construction of a diagnostic manual is a difficult task. We recognize the sincere efforts of the people working on DSM-5 to develop an improved diagnostic system. Our most fundamental disagreements are on the current feasibility of the DSM-5 ambitions and on the level of research support required before making changes. We discuss more specifically here diagnostic inflation, consultation, empirical documentation, and field testing.

Diagnostic Inflation

The unrealistic DSM-5 goal was to produce a “paradigm shift” in psychiatric diagnosis (Regier 2008, Regier et al. 2010). Work groups were instructed to think creatively since everything was on the table. Accordingly, they came up with numerous pet suggestions that had in common a wide expansion of the diagnostic system (e.g., hypersexual disorder, binge eating, paraphilic coercive, skin picking, and Internet sex addiction) that together would redefine tens of millions of people previously considered normal and thousands previously considered criminal, delinquent, or irresponsible. Several of the new diagnoses (particularly mixed anxiety depression, binge eating, and minor neurocognitive) would go from not currently recognized as mental disorders to become among the most common of the psychiatric disorders, potentially creating false epidemics of misidentified pseudopatients. These proposed diagnoses are at times informed by a considerable body of research (Barkow et al. 2004, Liebenluft & Rich 2008, Striegel-Moore & Franko 2008,

Wonderlich et al. 2009), but not the type of research that is really necessary and important before they would be included within the diagnostic manual (e.g., risk-benefit analyses; First 2011). Some of the proposed new categories (e.g., attenuated psychotic symptoms, mood dysregulation) will further promote what is already an alarming overuse of antipsychotic medication in children—sometimes causing obesity, diabetes, cardiovascular complications, and possible reduced life expectancy. Lowered thresholds for existing categories would increase the already high rates of ADD, generalized anxiety disorder, posttraumatic stress disorder, and substance dependence.

All of the suggested new diagnoses and lowered thresholds would expand psychiatric diagnosis at its fuzzy and hard-to-define border with normality. In our opinion, they are not supported by sufficient empirical documentation or confirmed by a careful risk-benefit analysis (we encourage readers to consider for themselves the empirical support for the proposals at <http://www.dsm5.org>). The result will be an unintended medicalization of normality with consequent overtreatment, stigma, and misallocation of scarce mental health resources.

Inadequate Consultation with the Field

Concerns with respect to the process with which DSM-5 was being constructed were perhaps first raised in 2008 by Robert Spitzer, chair of DSM-III and DSM-III-R, after having been denied access to the minutes of DSM-5 Work Group meetings (Decker 2010, Spitzer 2008). Frances and Spitzer eventually submitted a joint letter to the American Psychiatric Association's Board of Trustees on July 7, 2009, expressing a variety of concerns with respect to the process with which DSM-5 was being constructed (Frances 2009). This letter was initiated because Kupfer and colleagues (2009) had indicated in May of 2009 that the field trial would soon start in order to be completed in 2010. Yet, none of the proposals to be considered in this field trial had yet received any critical

review or even been revealed to the field for input and review. In response to this letter the field trial was delayed and most of the proposals were eventually posted on a Website in February of 2010 (Decker 2010). Persons were then able to submit reactions, concerns, and suggestions with respect to the proposals, albeit none of this commentary is posted.

One of the concerns raised by Spitzer and Frances was the fact that work group members had to sign legally binding confidentiality agreements (Decker 2010, Frances 2009). The purpose of these agreements has not been entirely clear and has likely been misunderstood (Schatzberg et al. 2009). Some have understood it to mean that work group members were not free to discuss openly the content of work group meetings. Livesley (2010), a member of the DSM-5 Personality and Personality Disorders Work Group, stated in an article on DSM-5 that his commentary was “based on information in the public domain and does not rely on information to which I have privileged access as a member of the DSM-5 Work Group on Personality and Personality Disorder” (p. 313). Minimally, the confidentiality agreements do not help to convey a spirit of free and open disclosure.

Inadequate Empirical Documentation

The process of the construction of DSM-5 appears to be largely decentralized, with little to no oversight supervision or quality control. Kendler et al. (2009) developed a manual for DSM-5 decision-making that is far superior to what was developed for DSM-IV, but there does not appear to be any appreciable effort to ensure that work group members adhere to these guidelines.

Some of the reviews are excellent, but others are sorely lacking. The variability in quality and depth is striking. We again encourage readers of this article to review for themselves the research literature posted on the DSM-5 Website (<http://www.dsm5.org>). It would appear that it is largely up to respective work group members whether or not they will provide systematic, comprehensive, and objective

reviews of the relevant literature. For example, one of the likely changes to the diagnostic manual will be the creation of a new class of addiction disorders that will subsume both the substance use disorders and pathological gambling and will allow for the diagnosis of additional behavioral addictions, such as Internet and shopping addiction. The posted literature review that provides the rationale and empirical support for this major revision consists of just two sentences indicating that pathological gambling has commonalities with substance dependence, followed by a list of references in which various commonalities can be gleaned (see <http://www.dsm5.org>). Only one of the articles listed actually addresses directly the question of whether pathological gambling is an addiction syndrome, and it is a review paper by Petry (2006) that is in opposition to the proposal. None of the concerns raised by Petry are addressed.

Kendler et al. (2009) state that any change to the diagnostic manual should be accompanied by “a discussion of possible unintended negative effects of this proposed change, if it is made, and a consideration of arguments against making this change should also be included” (p. 2). Kendler et al. further state that “the larger and more significant the change, the stronger should be the required level of support” (p. 2). Creating a new class of behavioral addiction disorders would appear to be a major change, but there is no posted review of potential negative effects of a new class of disorders within which there would be diagnoses such as Internet addiction.

The proposals for the personality disorders have been among the most radical, including the deletion of half of the diagnoses, the abandonment of diagnostic criterion sets, and the inclusion of an alternative dimensional trait model (Clark & Krueger 2010, Skodol 2010). Not surprisingly, perhaps, these proposals were met with considerable external criticism, the common theme being that the posted reviews were sorely lacking in objectivity or comprehensiveness, emphasizing instead the research by work group members and failing to give

due consideration to alternative perspectives (e.g., Gunderson 2010, Pilkonis et al. 2011, Ronningstam 2011, Shedler et al. 2010, Widiger 2011a, Zimmerman 2011). Even one of the work group members published his own critique of the proposals, stating that “the reformulation is a confusing mixture of innovation and a return to previous ways of representing diagnostic constructs that is inconsistent, incoherent, impractical, and frequently incompatible with empirical facts” (Livesley 2010, p. 304).

Inadequate Field Trials

The DSM-5 field trials are intensive, extensive, expensive, complicated, and time-consuming but will likely be largely uninformative (First 2011). They are designed to measure only feasibility, perceived clinical utility, and reliability (see <http://www.dsm5.org>). These were important questions 30 years ago when DSM-III was first introduced. They are stale and irrelevant now.

Surprisingly, there is little to no effort in the field trial to compare alternative proposals. There is no effort to obtain data on the likely shift in prevalence rates (and/or validity) through the administration of DSM-IV criterion sets, and there is no effort to obtain information concerning the risk, costs, or any other concern that will be raised with respect to the more radical of new proposals (First 2011). There is apparently no effort to obtain data in the primary care settings in which these diagnoses will most likely be used. At the end of the DSM-5 field trials, we will have no idea whether its suggestions will create false epidemics of misidentified pseudopatients.

THE FUTURE

The first edition of the DSM was a joint effort of a number of different professional organizations, not just the APA (Blashfield 1984, Kendell 1975, Zilboorg 1941). Nor was there really much interest in the DSM until the third edition, which made psychiatric diagnosis an important societal issue and a best-selling, financial bonanza. The previous implicit trust

in the wisdom and skill of the APA in having sole responsibility for this manual is no longer tenable. The high-risk proposals and sorry methodological performance of DSM-5 compel a reconsideration of how future DSMs should be done.

A new auspice must be found for its future development. New diagnoses can be fully as dangerous as new drugs. They influence decisions that determine whether millions of people get treated with what may be ineffective and unsafe medications. Paradoxically, we mount a fairly careful process of regulatory approval for new drugs through the Food and Drug Administration, but simultaneously we perform a perfunctory vetting of new diagnoses and allow the primary reviews to be done by small and parochial panels of experts who have a narrow experience, a vested interest, and a lack of appropriate skills.

The DSM-5 is being prepared with little or no attention to the methods of evidence-based medicine and risk analysis; to its public health and public policy impacts; to how its suggestions will play in average mental health settings and in primary care; to its effects on health economics; and to its misuses in forensic settings. It seems clear that the responsibility for future DSMs should be pulled from the APA and exercised instead by a broader-based and more skilled institutional setting. But it is much less clear where this future responsibility should lie. The NIMH has the needed institutional skills, budget, and scientific heft but is developing its own quite different diagnostic approach and would likely lack any broad understanding of (or sympathy for) the exigencies of psychiatric diagnosis in daily clinical practice (Garvey et al. 2010, Insel 2009, Sanislow et al. 2010).

Perhaps the best choice would be to create a new institutional auspice working under the supervision of the Institute of Medicine. The goal would be broad representation, an evidence-based approach, disinterested recommendations, and a careful attention to the risks and benefits of each suggestion for change to the individual patient, to public policy, and to forensic applications.

The future hope is that psychiatric diagnosis will gradually incorporate biological tests based on pathogenetic understanding. The experience of the past 30 years suggests that this welcome breakthrough will be painstakingly slow in coming and will usually apply to only a small percentage of the individuals diagnosed within the existing descriptive categories. With all its many limitations, descriptive diagnosis will necessarily remain our most valued standby for many, many decades. Our classification of mental disorders is no more than a collection of fallible and limited constructs that seek but never find an elusive truth. Nevertheless, this is our best current way of defining and communicating about mental disorders.

Despite all its epistemological, scientific, and even clinical failings, the DSM incorporates a great deal of practical knowledge in a convenient and useful format. It does its job reasonably well when it is applied properly and when its limitations are understood. One must strike a proper balance.

To not know the DSM well casts one outside the community of common language speakers, but to follow it slavishly reduces the flexibility needed to deal with the complexities of clinical practice. The DSM should always be used with pragmatism and clinical common sense.

SUMMARY POINTS

1. The development of the DSM is an enormous responsibility, as it has a tremendous impact on research, funding, and treatment as well as a variety of civil and forensic decisions.

2. Proposals for the DSM should be subjected to severe critical review to identify potential flaws, problems, limitations, and costs. These critical reviews should be forthrightly published, along with the reviews by advocates of each respective proposal.
3. Each proposal should be subjected to a thorough risk-benefit analysis that considers the potential costs of false positive diagnosis, including the potential misuse and misapplication of a respective proposal.
4. Field trials that compare and contrast alternative proposals should be conducted. These field trials should include the primary care settings in which the diagnoses will be routinely used in general clinical practice.
5. Future editions of the diagnostic manual will perhaps be optimally developed under the authority of the Institute of Medicine, with a broad representation of participants, an evidence-based approach, disinterested recommendations, and a careful attention to the risks and benefits of each suggestion for change to the individual patient, to public policy, and to forensic applications.

FUTURE ISSUES

1. Research is needed concerning the process with which the diagnostic manual is constructed (e.g., sociological research on how decisions are made and whether they are in fact evidence based).
2. Further research is needed on the impact of the appearance of a new diagnosis within the DSM and the extent to which it results in improved care and a decrease in suffering, relative to the potential costs of false positive diagnoses.

DISCLOSURE STATEMENT

A.J.F. receives royalties from authored handbooks concerning DSM-IV. T.W. is unaware of any affiliation, funding, or financial holding that might be perceived as affecting the objectivity of this review.

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This Internet posting provides a good history of the controversies surrounding the development of DSM-5.

Discusses the issues concerning and the rationale for the major decisions that were made for DSM-IV.

Discusses the limitations of existing diagnoses and their problematic impact on existing research and grant funding.

Provides a forthright discussion of the limitations of the existing diagnostic nomenclature along with a call to shift psychiatric diagnoses toward a neurobiological orientation.

This unpublished manuscript provides the guidelines for making decisions for DSM-5, placing considerable emphasis on empirical support and risk-benefit analysis.

Provides a rather scathing critique of the DSM-5 proposals for the personality disorders section, by a member of the DSM-5 Personality and Personality Disorders Work Group.

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Contents

On the History and Future Study of Personality and Its Disorders
Theodore Millon 1

A “SMART” Design for Building Individualized Treatment Sequences
H. Lei, I. Nabum-Shani, K. Lynch, D. Oslin, and S.A. Murphy 21

Default Mode Network Activity and Connectivity in Psychopathology
Susan Whitfield-Gabrieli and Judith M. Ford 49

Current Issues in the Diagnosis of Attention Deficit Hyperactivity
Disorder, Oppositional Defiant Disorder, and Conduct Disorder
Paul J. Frick and Joel T. Nigg 77

Psychiatric Diagnosis: Lessons from the DSM-IV Past
and Cautions for the DSM-5 Future
Allen J. Frances and Thomas Widiger 109

American Indian and Alaska Native Mental Health:
Diverse Perspectives on Enduring Disparities
Joseph P. Gone and Joseph E. Trimble 131

Emotion Regulation and Psychopathology: The Role of Gender
Susan Nolen-Hoeksema 161

Cognitive Bias Modification Approaches to Anxiety
Colin MacLeod and Andrew Mathews 189

Diagnosis and Assessment of Hoarding Disorder
Randy O. Frost, Gail Steketee, and David F. Tolin 219

The Behavioral Activation System and Mania
*Sheri L. Johnson, Michael D. Edge, M. Kathleen Holmes,
and Charles S. Carver* 243

Prediction and Prevention of Psychosis in Youth at Clinical High Risk
Jean Addington and Robert Heinssen 269

From Dysfunction to Adaptation: An Interactionist
Model of Dependency
Robert F. Bornstein 291

Personality Disorders in DSM-5 <i>Andrew E. Skodol</i>	317
Development, Evaluation, and Multinational Dissemination of the Triple P-Positive Parenting Program <i>Matthew R. Sanders</i>	345
Empirical Classification of Eating Disorders <i>Pamela K. Keel, Tiffany A. Brown, Lauren A. Holland, and Lindsay P. Bodell</i>	381
Obesity and Public Policy <i>Ashley N. Gearhardt, Marie A. Bragg, Rebecca L. Pearl, Natasha A. Schvey, Christina A. Roberto, and Kelly D. Brownell</i>	405
Cognition in the Vegetative State <i>Martin M. Monti</i>	431
Coping with Chronic Illness in Childhood and Adolescence <i>Bruce E. Compas, Sarah S. Jaser, Madeleine J. Dunn, and Erin M. Rodriguez</i>	455
Indexes	
Cumulative Index of Contributing Authors, Volumes 1–8	481
Cumulative Index of Chapter Titles, Volumes 1–8	484

Errata

An online log of corrections to *Annual Review of Clinical Psychology* articles may be found at <http://clinpsy.annualreviews.org>