

From the Editor

In these remarks let me first extend my deepest appreciation to Allen Frances for his tireless work in the production of this issue of the Bulletin. This project began many months ago with my inquiry to Allen as to whether he would be interested in carrying his critique of the DSM-5 process into the pages of the Bulletin, with commentaries and responses. He responded enthusiastically, and we were off and running. To that point a central venue for his DSM publications had been (and remains) the pages and online space of the *Psychiatric Times*. The Bulletin would add a concentrated space and format where the conceptual issues he had been addressing—a natural interest for AAPP—could be pursued further. In the Spring issue of the Bulletin he chose to reply to the commentators in one long piece, summarizing his position.

Discussion of that Bulletin issue this past Spring led to a further idea: do a second issue, invite commentaries on Allen's general response from the first issue, structure this issue with individual responses to each commentator, and cast the net wider for further commentary. Allen suggested a final idea, that commentators should be offered a final word following his responses. As you can see from the size of this issue of the Bulletin (over 60,000 words), interest in this discussion has been great.

The above remark leads me to another note of appreciation, to our commentators, who have themselves put in a lot of work on their contributions. The level of interest and amount of work put into these commentaries attest to the importance many of our colleagues attach to the ongoing DSM-5 process.

It is a well-known fact—and one alluded to in a number of the commen-

President's Column

Nancy Nyquist Potter, Ph.D.

For many years now, AAPP has been actively involved in the analysis and critique of the DSMs, including nosology in general, current classifications, diagnostic practices, and the interface between science and values. In focusing our attention on the next DSM and the many conceptual and evaluative issues raised there—both in these Bulletin issues and in next year's annual meeting—we (AAPP members, PPP readers and authors, and others) repeatedly circle back to the question of how best to serve our patients. We want the best science possible, so it was appropriate that our most recent AAPP meeting, held in New Orleans 2010, was on "Philosophical Issues in Evidence-Based Psychiatry." But we also want to give the best, most accurate, most therapeutic, and most ethical attention to those we serve—the service users—and so the President's Column this time will try to bring together some threads of these two themes for us to think about in the coming year.

Our 2010 conference, organized by Peter Zachar and David Brendel, was a huge success by all accounts. A central theme was the critical examination of just what 'evidence-based medicine' refers to, what values underpin it, and how well (or not) it applies to psychiatry. Keynote speaker Mona Gupta, for example, argued that 'good health' is a variable that, in psychiatry, requires that we grapple with the sometimes ambiguous and always messy terrain of patient values, mental diagnoses, and ethical justification of psychiatry's goals, values, and assumptions.

I would like to extend that conversation, in the President's Column, to the INPP conference in Manchester, England that took place June 28 2010. There, for the first time, service users were built into the structure and content of the conference. This is no small step: it affirms the epistemological and ethical necessity of not only taking into account, but making central the experiences, values, perspectives, and voices of patients themselves. If we truly want evidence-based medicine, we must count as evidence the contributions that patients make and the wishes they express. And if we truly want both evidence- and value-based medicine, we must take into account the values of patients themselves. Narrative psychiatry is already doing this, as are scholars who study and analyze memoirs, such as Serife Tekin, and several other presenters who offered case-based analyses.

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taries—that a proposal to appoint a Conceptual Issues Work Group in the DSM-5 process (*Am J Psychiatry* 2008;165:174-5) was declined by the DSM-5 Task Force. I think that those of us working on these two issues of the Bulletin see ourselves (somewhat grandiosely, to be sure) as the missing DSM-5 Conceptual Issues Work Group. It will left for historians of psychiatry (our Hannah Decker, for one) to describe the parallel work taking place in these months and years prior to publication in 2013—work within the official chambers of the DSM-5 Work Groups, and work going on in parallel outside the official chambers.

In the remaining space I have allotted myself, of the many topics I could comment on from the ensuing discussions—umpires, pragmatism, diagnostic conser-

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Symposium on DSM-5 Part II

In this issue of the Bulletin we continue the discussion initiated in the previous issue. In that issue commentaries were focused on Allen Frances' ongoing critique of the DSM-5 process. Dr. Frances followed the commentaries with a general response directed at all the participants. For this issue we are following a different format. Commentaries are directed to Frances' 'Response' from the previous issue. Dr. Frances then follows each commentary with an individual response. Finally, each commentator has the opportunity for a 'last word'.

The format for this issue will be the following. A Table of Contents directs the reader to specific commentaries and responses. Following the Table of Contents we are first republishing Dr. Frances' Response to Commentaries from the previous Bulletin, so that readers do not have to refer back to that issue for reference.

The Web site for accessing both issues of the Bulletin is:

<http://alien.dowling.edu/~cperring/aapp/bulletin.htm>

Individual pieces from this issue can be accessed in Word format at:

<http://sites.google.com/site/aapponline/>

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DSM in Philosophyland: Curiouser and Curiouser

Allen Frances M.D.

First off, thanks to James Phillips for inviting these stimulating commentaries. Second, a confession. My last (and only) formal training in philosophy was a freshman course in college that went well over my head. Now I have been invited to share my

(probably sophomoric) speculations on the meanings that swirl below the surface of psychiatric classification. I do so without any confidence they can survive rigorous analysis by those more expert than I in the tools of philosophic inquiry. Much of what I say below may be simple minded or simply wrong. What I do understand (perhaps better than anyone) are the practical issues of creating a psychiatric manual and the many good and bad (intended, unintended) consequences it can have. My views on deeper meanings are given, and should be taken, with a large grain of salt.

The Epistemological Game

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."

As I recall it, the three umpires are replaying a marathon epistemological game that: 1) began with Plato; 2) continued in the medieval joust between the realists and Occam's nominalists; 3) was revived in the post-renaissance debate between Descartes and Vico on the power and limits of rational thought; 4) was refined by Kant; 5) churned up by Freud; and 6) finally settled by quantum physicists who have sharply downgraded the capacity of the human mind to ever fully intuit (much less understand) reality. Closer to my turf, I like to think of Bob Spitzer as umpire #1, me as umpire #2, and Tom Szasz as umpire #3.

Spitzer's DSM-III achieved a paradigmatic revolution in psychiatric diagnosis and nosology. He introduced the method of diagnostic criteria (originally developed for research purposes) into a tool for general clinical practice. For the first time, psychiatrists could agree on diagnoses and make interpretive judgments across the research/ clinical interface. Certainly, the level of reliability achieved by DSM-III was over sold, especially when it was used by the

AAPP 23rd Annual Meeting 2011 The Future of Psychiatric Nosology

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*(in conjunction with the American
Psychiatric Association
Annual Meeting)*

In the half century since Carl Hempel addressed the World Conference on Field Studies in the Mental Disorders in 1959, the literature on the philosophy of psychiatric nosology has grown exponentially. However, it is not clear to what degree conceptual explorations of psychiatric nosology have actually influenced our systems of classification. Now, as we anticipate the publication of ICD-11 and DSM-5 in 2013, what have the lessons of the last 50 years been, and what should we anticipate for the next 50?

What shall we strive for in the future? Should future classification systems be global and intercultural in scope? Should they attempt to serve multiple purposes – clinical, research, and administrative? Should we hurry to move beyond descriptive diagnostic criteria? On what bases should we make decisions about lumping and splitting, defining 'mental disorder', utilizing dimensions versus classes, and articulating the role of theory in shaping our nomenclature? How do we duly consider the social, political, epistemic, and professional values that influence a classification and how it is used? What do developments in contemporary philosophy of science contribute to the future of psychiatric nosology?

Abstracts are welcome addressing any of these issues.

Presentations will be strictly limited to 20 minutes, followed by 10 minutes of discussion. Abstracts will be blind reviewed—attach author's identifying information on separate cover page. Abstracts should be 500-600 words and should be sent via email by November 15, 2010 to Claire Pouncey, MD, PhD, Program Chair, at pouncey@mail.med.upenn.edu.

average clinician. But DSM-III was a huge leap forward from the useless and neglected guidance offered by DSM-I and DSM-II. It gave hope that psychiatry could become scientific and join in the advances that were being made in the rest of medicine.

DSM-III resulted from and promoted the victory of biological psychiatry over the psychological and social models that until then were its serious competitors. In the early dawn of its triumph, the biological model was presented with a realist, reductionist flourish that would have done umpire #1 proud. Mental disorders were real entities that existed "out there." The process of scientific discovery would elucidate their etiology and pathogenesis using the powerful new methods of neuroscience, imaging, and genetics.

The next section will focus on the disappointing fate of this ambitious program, but one central point belongs here. Biological psychiatry has failed to produce quick, convincing explanations for any of the mental disorders. This is because it has been unable to circumvent the fundamental and inherent flaw in the biological, "realist" approach - mental disorders don't really live "out there" waiting to be explained. They are constructs we have made up - and often not very compelling ones at that. It has, for example, become clear that there is no one prototype "schizophrenia" waiting to be explained with one incisive and sweeping biological model. There is no gene, or small subset of genes, for "schizophrenia." As Bleuler intuited, "schizophrenia" is rather a group of disorders, or perhaps better a mob. There may eventually turn out to be twenty or fifty or two hundred kinds of "schizophrenia." As it stands now the definition and boundaries of "schizophrenia" are necessarily arbitrary. There is no clear right way to diagnose this gang and not even much agreement on what the validators should be and how they should be applied. The first umpire was called out on strikes when the holy grail of finding the cause of "schizophrenia" turned out to be a wild goose chase.

Szasz is the third umpire. He quickly saw through the epistemological "no clothes" of umpire #1 and led

the fight against simple minded biological reductionism (even well before the biologists had discovered their own voice and began making their overly ambitious and naïve claims). Szasz vigorously presented the view that mental illness is a medical "myth." Mental disorders were no more than social constructs that in some cases served a useful purpose, but in many others could be misused to exert a noxious social control, reducing freedom and personal responsibility. The biological "realists" reacted predictably to Szasz' "nominalist" attack. They dismissed it. If schizophrenia is a myth, they crowed, it is a myth that responds to medication and has a genetic pattern. But their triumphalism was premature and based on both weak philosophic and weak scientific grounds. It turned out that the neuroscience, genetics, and treatment response of "schizophrenia" follow anything but a simple reductionist pattern. The more we learn about "schizophrenia" the more it resembles a heuristic, the less it resembles a disease.

This brings us to me (a call'um as I see'um) second umpire. In preparing DSM-IV, I had no grand illusions of seeing reality straight on or of reconstructing it whole cloth from my own pet theories. I just wanted to get the job done - i.e., produce a useful document that would make the fewest possible mistakes, and create the fewest problems for patients. Following Vico, I accepted that much in real life (and almost everything in psychiatric classification) is overlapping, fuzzy, and heterogeneous - anything but Cartesian and amenable to overarching rationalist principles or mathematical precision. Psychiatric classification is necessarily a sloppy business. The desirable goal of having a classification consisting of mutually exhaustive, non-overlapping mental disorders is simply impossible to meet.

Instead, the second umpire follows a down-to-earth brand of Bentham utilitarian pragmatism. His umpire's eye is fixed on the end result of getting to what works best - not distracted by biological reductionism or rationalist models of how things

should be constructed. A diagnosis is a call to action with huge and unpredictable results. No decision can be right on narrow scientific grounds if it winds up hurting people.

Descriptive Psychiatry Gets Long of Tooth

The Dodo: "Everyone has run and everyone has won and all must have prizes".

Modern descriptive psychiatry just passed its 200 birthday - if we measure it from the milestone of Pinel's creation of the first psychiatric classification that resembles our own. His work was born from the Enlightenment belief in a rational world - 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.

This approach was enormously successful in each of the major paradigm shifts in science. Always a careful description preceded a causal model. Kepler's astronomical observations led to Newton's gravity. Linnaeus' classification of plants and animals led to Darwin's evolution. Mendeleev's periodic table led to Bohr's structure of the atom. There have been dozens of descriptive systems vying to describe things so brilliantly that their truth would shine forth. "All have run, but none has won prizes." Descriptive classification in psychiatry has so far been singularly unsuccessful in promoting a breakthrough discovery of the causes of mental disorder.

This is doubly disappointing given the miraculous advances in our understanding of normal brain functioning. The advances in molecular biology, brain imaging, and genetics are spectacular - their impact on understanding psychopathology almost nil. Why the disconnect? The answer lies in a paraphrase of the opening lines of Anna Karenina. All normal brain functioning is normal in more or less the same way, but any given type of pathological functioning can have many different causes.

This is also true for all the complex diseases in medicine. A genetics company using the Icelandic registry had tremendous success in finding gene markers for a dozen diseases, including schizophrenia. It recently went bankrupt because, in each instance, the particular candidate marker explained fewer than three per cent of the cases of the particular disease. There appear to be no common genes even for the common illnesses. Psychopathology is heterogeneous and overlapping not only in its presentation but also in its pathogenesis. There will likely be hundreds of paths to schizophrenia, not one or just a few and perhaps no final common pathway. 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. There is little indication of any imminent and sweeping etiological breakthrough. Everything points towards a slow and painstaking retail accumulation of explanatory power. It is not even clear that the DSM categorical approach is the best research tool. The NIMH is embarking on a project to correlate an integrated exploration of neural networks with psychopathology. They chose to study dimensions of behavior (e.g. anxiety, pleasure seeking, executive functioning) - not with the standard psychiatric disorders which are deemed too complex to have any simple relationship with a given neural network. Our DSM categories may not lead the future charge in understanding psychopathology.

Our descriptive classification of disorders is old and tired. It has worked hard for us and continues to have many valuable and irreplaceable functions (which we will discuss in the last section). Fiddling needlessly with the labels will not advance science and may actually do more harm than good in its effect on clinical care.

The Elusive Definition of Mental Disorder

Humpty Dumpty: "When I choose a word it means just what I choose it to mean."

When it comes to defining the term

"mental disorder" or figuring out which conditions qualify, we enter Humpty's world of shifting, ambiguous, and idiosyncratic word usages. This is a fundamental weakness of our field. Many crucial problems 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. Clinicians could use it when deciding whether to diagnose and treat a patient on the border with normality. A meaningful definition would clear up the great confusion in the legal system where matters of great consequence often rest on whether a mental disorder is present or absent.

Alas, I have read dozens of definitions of mental disorder (and helped to write one) and I can't say that any have the slightest value whatever. Historically, conditions have become mental disorders by accretion and practical necessity, not because they met some independent set of operationalized definitional criteria. Indeed, the concept of mental disorder is so amorphous, protean, and heterogeneous that it inherently defies definition. This is a hole at the center of psychiatric classification. And the specific mental disorders certainly constitute a hodge-podge. 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 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, yet others behaviors, others interpersonal relations, and there are complex combinations of all of these. Some seem more biological, others more psychological or social. If there is a common theme it is distress and disability, but these are very imprecise and nonspe-

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cific markers on which to hang a definition.

Ironically, the one definition of mental disorder that does have great and 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." In effect, this is historically how the individual mental disorders made their way into the system.

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. There were only five disorders listed in the initial census of mental patients in the mid nineteenth century,

now there are close to three hundred. Society also has a seemingly insatiable capacity (even hunger) to accept and endorse newly defined mental disorders that help to define and explain away its emerging concerns. As a result, psychiatry is subject to recurring diagnostic fads. Were DSM-5 to have its way we would have a wholesale medicalization of everyday incapacity (mild memory loss with aging); distress (grief, mixed anxiety depression); defects in self control (binge eating); eccentricity (psychotic risk); irresponsibility (hypersexuality); and even criminality (rape, statutory rape).

Remarkably, none of these newly proposed diagnoses even remotely pass the standard loose definition of "what clinician's treat." None of these "mental disorders" has an established treatment with proven efficacy. Each is so early in development as to be no more than "what researchers research" - a concoction of highly specialized research interests.

We must accept that our diagnostic classification is the result of historical accretion and accident without any real 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 social constructs (but nonetheless useful ones if understood and applied properly).

The Conservative/Innovation Debate or Where Have All the Normals Gone?

Alice: "But I don't want to go among mad people"

Cheshire Cat: "Oh, you can't help it, we're all mad here."

DSM-IV would have been a very different document if I had adopted Humpty Dumpty's confident attitude and used my authority to shape it to my personal taste. Bob Spitzer, who had led the efforts to create DSM-III and DSM-III-R is a "splitter" whose preference is to divide the diagnostic pie into small manageable pieces. This enhances reliability, but creates many new diagnoses and artificial comorbidity (as complex syndromes are divided into their component parts). I joke that Spitzer never met a new diagnosis he

didn't like.

I am more of a lumpner and also very wary of diagnostic fads and the unintended consequences of introducing new diagnoses. Given my druthers, DSM-IV would have had fewer, lumped categories and tighter criteria sets to make it harder to get a diagnosis. Instead, I chose not to impose this view on DSM-IV. We would apply a conservative standard for all changes - equally not add new things or take out old ones unless there was substantial evidence to support the change. Many decisions were thus grand-fathered into DSM-IV that would not have had nearly enough support to meet the new higher evidentiary standard.

I am not a particularly risk averse or conservative person in my everyday life. So why the conservative tilt in setting ground rules for DSM-5?

1) The 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;

2) 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. Now that 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;

3) My experience working on DSM-III and DSM-III-R was that most decisions were fairly arbitrary - with plausible supporting arguments that could have gone either way. Making more arbitrary changes didn't make much sense;

4) The scientific evidence supporting proposed changes was usually meager. Requiring that all changes be based on substantial evidence usually shut up even the most passionate advocates;

5) The literatures are not only thin but also mostly derived from highly specialized research settings that have questionable generalizability to the real world.

One's position on the conservative/innovation continuum is influenced by reactions to the epistemological question raised previously. If

you regard the categories in DSM as descriptions of "real entities," you will be eager to change definitions in accord with evidence that they can be better described in a way that captures their real natures. On the other hand, if you believe as I do, that the DSM is necessarily more an exercise in forging a common language than in finding a truth, you need a strong reason to change the syntax. And it turns out that such strong evidence is usually lacking. This is why the reliability and utility goals are so important (and for all the discussion about it, validation is not yet particularly meaningful).

The second divide in the conservative/liberal split relates to how worried one is by real world consequences. As a pragmatist, I was acutely conscious that every change made by DSM-IV could have enormous practical consequences: 1) determining who got medicines that could greatly help or greatly harm; 2) deciding insurance and disability claims; and 3) influencing life and death forensic issues. Those of a more pure research world, innovation orientation would argue for "following the data" and damn the consequences. In my view, data sets that are thin and selective are never sufficient support for changes that can cause considerable mischief. So there are two contrasting attitudes. Mine, 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." The conservative approach assumes that things are there for a reason and are imbricated in a complex set of relations. I have 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 Attention Deficit Disorder) or a forensic nightmare (e.g., the misuse of Paraphilia NOS in the extended civil commitment of sexual offenders).

One of the commentaries presents quite the opposite view - that the existing system is so bad that even the aggressively innovative DSM-V is suggesting far too little change, not too much. I believe this to be a naïve Cartesian rationalist view that neglects the deep roots and far flung branches of the diagnostic system. Most of the sug-

gested DSM-5 changes are such really bad ideas that they do not even represent a meaningful test of the conservative/innovator divide. I believe that most sensible people informed of their risks and benefits would veto them (this leaves out the Work Group members who are otherwise sensible but too attached to their pet suggestions to be objective about their risks).

The new suggestions all share the common problem of greatly expanding the reach of "mental disorders" at the expense of normality. Armies of millions (perhaps tens of millions) of false positive "patients" would receive unnecessary and harmful treatments. I have covered this problem extensively elsewhere and won't repeat the details here. A better, because much tougher, test case of the conservative/innovator debate comes from the DSM IV introduction of Bipolar II disorder. Here there are strong arguments on both sides and no clear right answer.

We knew that adding Bipolar II would be one of the most consequential changes in DSM-IV but went ahead (despite our conservative bias) because of what seemed to be compelling enough research evidence (descriptive, course, family history, treatment response) that it sorted better with bipolar than with unipolar mood disorders. We recognized the risks that some unipolar patients would be mislabeled and receive unnecessary and potentially harmful, mood stabilizing and antipsychotic medication. But this risk seemed more than counterbalanced by the opposing risk posed by uncovered antidepressants for those whose bipolar tendencies were previously missed by the diagnostic system.

Several facts are incontestable about trends since DSM-IV: 1) with a huge push from the pharmaceutical industry, Bipolar II has become an enormously popular diagnosis; 2) so that the ratio of bipolar to unipolar patients increased dramatically; 3) and prescriptions jumped for mood stabilizers and antipsychotics (which can cause huge and dangerous weight gains), and 4) for different reasons rates of childhood Bipolar Disorder have increased forty fold. Some patients are undoubtedly better off for being diagnosed as Bipolar II. Others have gained a lot of weight (and risk diabetes and a poten-

tially shortened lifespan) taking a medication that was unnecessary.

A conservative might prefer that such public health experiments be based on more evidence than was available to us when we made the decision to include Bipolar II. We also had no way of anticipating how aggressive and successful were the pharmaceutical industry marketing efforts to move product. Bipolar II also illustrates the exquisite and dangerous sensitivity of the diagnostic system to small changes. The hugely consequential decision regarding the need for potentially very harmful medication rests on the most fragile and unreliable of distinctions - the decision whether or not a hypomanic episode is present. If the minimum duration of the episode is set at a week (or even longer), people at risk for antidepressant worsening will be missed; if the requirement is 4 days (or even less), many people will receive unnecessary medication. The symptom thresholds for defining a hypomanic episode are similarly arbitrary and subject to wide swings in sensitivity and specificity, based on very minor adjustments. Making this even more complicated are the difficulties distinguishing hypomania from normal mood in someone who is chronically depressed or hypomania from substance induced mood elevation in someone using drugs.

The point here is that tiny changes in definition can (and often do) result in large, unpredictable (and usually unwarranted) swings in diagnostic and treatment habits, especially when amplified by drug companies, advocacy groups, and the media. Such potentially dangerous fads are enough to turn a lifelong, risk-taking liberal like me into a conservative nosologist. First, last, and always - DO NO HARM.

Afterword

The Talmud: "We don't see things as they are, We see things as we are".

Many people are troubled by the relativism implied in this penetrating insight - but I find it liberating. We will never have the perfect diagnostic

system. Our classification of mental disorders will always necessarily be 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 seeing and communicating about mental disorders. 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.

The concern about comorbidity across disorders arises from the misconception that each is a "real" and independent psychiatric illness and that clear boundaries should or could be created to separate them. If instead, one accepts that each disorder is just a description (not a disease), then the combined descriptions become 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." Instead, we are dealing with descriptive prototypes ("schizophrenia," "panic disorder," "mood disorder," etc., through the manual) that are inherently heterogeneous and will hopefully with time be divided into many true etiologically defined illnesses.

The greatest misuse of the DSM occurs in diagnosing conditions at the border of normality and criminality. Clinicians should hold themselves to the most rigorous standards when applying criteria sets in these dangerous boundary territories. The DSM incorporates a great deal of practical knowledge in a convenient and useful format.

To not know it casts one outside the community of common language speakers - the language being clinical psychiatry. But it should always be used with pragmatism and clinical common sense.

DSM: The Nosology of Nondiseases

Thomas Szasz, M.D.
SUNY Upstate Medical University

I thank Dr. James Phillips for inviting me to comment on this debate. I am pleased but hesitant to accept, lest by

engaging in a discussion of the DSM (the American Psychiatric Association's Diagnostic and Statistical Manual of Mental Disorders) I legitimize the conceptual validity of "mental disorders" as medical diseases, and of psychiatry as a medical specialty

Psychiatrists and others who engage in this and similar discussions accept psychiatry as a science and medical discipline, the American Psychiatric Association (APA) as a medical-scientific organization, and the DSM as a list of "disorders," a weasel word for "diagnoses" and "diseases," which are different phenomena, not merely different words for the same phenomenon.

In law, the APA is a legitimating organization and the DSM a legitimating document. In practice, it is the APA and the DSM that provide medical, legal and ethical justification for physicians to diagnose and treat, judges to incarcerate and excuse, insurance companies to pay, and a myriad other social exchanges to be transacted. Implicitly, if not explicitly, the debaters's task is to improve the "accuracy" of the DSM as a "diagnostic instrument" and increase its power as a document of legitimation.

Long ago, having become convinced of the fictitious character of mental disorders, the immorality of psychiatric coercions and excuses, and the frequent injuriousness of psychiatric treatments, I set myself a very different task: namely, to delegitimize the legitimating authorities and agencies and their vast powers, enforced by psychiatrists and other mental health professionals, mental health laws, mental health courts, and mental health sentences.

In *Psychiatry: The Science of Lies*, I cite the warning of John Selden, the celebrated seventeenth-century English jurist and scholar: "The reason of a thing is not to be inquired after, till you are sure the thing itself be so. We commonly are at, what's the reason for it? before we are sure of the thing." In psychiatry it is usually impossible to be sure of "what a thing itself really is," because "the thing itself" is prejudged by social convention couched in ordinary language and then translated into pseudo-medical jargon.

Seventy-five years ago, in my teens, I suspected that mental illness was a bogus entity and kept my mouth shut. Twenty-five years later, more secure in my identity, I said so in print. Fifty years later, in the tenth decade of my life, I am pleased to read Dr. Allen Frances candidly acknowledging: "Alas, I have read dozens of definitions of mental disorder (and helped to write one) and I can't say that any have the slightest value whatever. Historically, conditions have become mental disorders by accretion and practical necessity, not because they met some independent set of operationalized definitional criteria. Indeed, the concept of mental disorder is so amorphous, protean, and heterogeneous that it inherently defies definition. This is a hole at the center of psychiatric classification." This is as good as saying, "Mental illness, there ain't no such thing," and still remain loyal to one's profession.

The fallacy intrinsic to the concept of mental illness – call it mistake, mendacity, metaphor, myth, oxymoron, or what you will – constitutes a vastly larger "problem" than the phrase "a hole at the center of psychiatric classification" suggests. The "hole" – "mental illness" as medical problem – affects medicine, law, education, economics, politics, psychiatry, the mental health professions, everyday language – indeed the very fabric of contemporary Western, especially American, society. The concept of "psychiatric diagnosis," enshrined in the DSM and treated by the discussants as a "problem," is challenging because it is also a solution, albeit a false one.

Medicalization, epitomized by psychiatry, is the foundation stone of our modern, secular-statist ideology, manifested by the Therapeutic State. The DSM, though patently absurd, has become an utterly indispensable legal-social tool.

Ideologies – supported by common consent, church, state, and tradition – are social facts / "truths." As such, they are virtually impervious to criticism and possess very long lives. The DSM is here to stay and so is the intellectual and moral morass in which psychiatry has entwined itself

and the modern mind.

An Appreciation and Dissent

Allen Frances, M.D.

Fifty years ago Thomas Szasz was a lonely (and then much reviled) voice in the wilderness when he boldly challenged the simple reductionist assumptions of modern biological psychiatry. His blow was prophetic and proactive – coming as it did many years before modern biological psychiatry had matured enough to fully articulate its grand reductionist ambitions. But Szasz' target had a long past as well as its seemingly promising future – modern biological reductionism is an outgrowth of the strict materialism of nineteenth century brain science.

Most people came to accept that eventually we would discover the underlying brain dysfunctions causing many, if not every, psychiatric disorder. It was just a matter of time before the exponentially growing power of the neuroscience and genetic tools would unravel the admittedly complex relationships between brain and psychopathology.

Until fairly recently, there was no reason to believe that Szasz would be proved so right about the protean nature of psychopathology—and that the mighty engine of brain research would turn out to be so limited in explaining mental disorders. Everything seemed to favor the cause of the biologists. The astounding technical revolutions in genetics, molecular biology, brain imaging, computer and cognitive science were daily providing profound insights into normal brain functioning—more than anyone would dare predict fifty years ago. There was every reason to expect this explosion of knowledge about the normal would soon be followed by profound insights into abnormal brain functioning. The smart money was betting on NIMH, not on Szasz.

But a funny thing happened. Psychopathology refused to cooperate with the reductionist program. It turned to be

heterogeneous not only in its presentation, but also in its causes. And not just slightly or temporarily or technically baffling. To paraphrase Tolstoy, normal behavior requires brain functioning that has gone right in about the same way; but abnormal behavior can come from things going wrong in lots of different ways. All the evidence suggests that there is no low hanging fruit in understanding psychopathology. Almost certainly, there will be many pathways to each "disorder," none of which will explain more than a few percent of the cases.

Schizophrenia may not be a "myth" but neither is it a coherent disease—nor will it ever likely be seen as such. What we call "schizophrenia" is a descriptively and etiologically heterogeneous aggregate caused by problems in what may be hundreds of different pathways. Bleuler intuited this with his concept of the group of schizophrenias—but he probably never imagined how big would be the crowd. Szasz understood, decades before there could be any scientific proof to confirm his intuition, that schizophrenia was no more than a construct—and much less than a disease.

Szasz has been right and prescient on many other issues at the core of psychiatric practice. He is absolutely right that psychiatry sometimes lends itself to misuse as society's tool to contain and imprison deviance—witness our current role in the long term involuntary commitment of sexual offenders. He is absolutely right that psychiatry has gone too far in medicalizing normality and criminality. He is absolutely right that this leads to inappropriate treatment and to a reduction in personal responsibility.

Which is not to say that Szasz gets everything just right or that mental illness really is just a myth or that most diagnoses are a fiction and their treatments an imposition. I think there are two factors causing Szasz to press his views to an excessive extreme that has unfortunately reduced the acceptance of all that he has gotten right: 1) his powerful dread of the misuse of power and the infringement of personal liberties and responsibilities; and, 2) the fact that (because, on principle he would not participate in involuntary treat-

ment) his training and clinical experience has not included work with severely ill patients. Szasz started his career already convinced that most of psychiatry was self-evidently self-serving and misguided—too corrupt to participate in except on his own terms with individuals who could be totally free agents.

Szasz critique of psychiatry goes a bit overboard whenever it is based on rhetoric and politics and is uninformed by practical clinical experience. Szasz is correct that psychiatric diagnoses are no more than fallible social constructs and not true illnesses. But he greatly undervalues the clinical, practical, everyday utility of these fallible social constructs. The fact that we don't understand the pathogenesis of the disorders doesn't eliminate their value in treatment planning and prognosis. Even with all its powerful scientific tools, most treatments in modern medicine remain empiric and uninformed by any deep understanding of why they work.

Even though the term "schizophrenia" lacks explanatory power, most people who meet the criteria for the diagnosis suffer greatly and do benefit from treatment. Szasz' critiques of psychiatry stand, but he goes too far in dismissing the value and necessity of clinical psychiatry.

In my view, psychiatry is a high clinical art backed by some clinical science that helps a lot of people. Many of Szasz' criticisms of psychiatry are right on the money, but he weakens the credibility of his arguments when he is so globally dismissive. I think his views would have modified if he had spent more time in the trenches, had seen the devastation caused by the "mythical illnesses," and experienced the relief that comes with prudent diagnosis and treatment.

Mental Disorder vs Normality: Defining the Indefinable

Joseph M. Pierre, M.D.
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Yamaoka Tesshu, a young student of Zen, visited one master after another. He called upon Dokuon of Shokoku. Desiring to show his attainment, he said: "The mind, Buddha, and sentient beings, after all, do not exist. The true nature of phenomena is emptiness. There is no realization, no delusion, no sage, no mediocrity. There is no giving and nothing to be received." Dokuon, who was smoking quietly, said nothing. Suddenly, he whacked Yamaoka with his bamboo pipe. This made the youth quite angry. "If nothing exists," inquired Dokuon, "where did this anger come from?" [1]

Even if you have closely followed and largely agreed with Dr. Frances' public critique of the evolving development of DSM-5 over the past year or so as I have, it is still sobering to read these words from, if not the creator of DSM-IV *per se*, then certainly its godfather:

...mental disorders don't really live "out there" waiting to be explained. They are constructs we have made up – and often not very compelling ones at that. ...Alas, I have read dozens of definitions of mental disorder (and helped to write one) and I can't say that any have the slightest value whatsoever. ...Indeed, the concept of mental disorder is so amorphous, protean, and heterogeneous that it inherently defies definition. This is a hole at the center of psychiatric classification." [2]

The inability to establish a functional definition of mental disorder is more than a hole in psychiatric nosology, it would seem to be a foundational, ground-zero crater that threatens to render the entire DSM meaningless. How can there be a diagnostic manual that sets out to provide descriptive criteria for mental disorders if we can't agree on what a mental disorder is? In identifying with the Second Um-

pire, Dr. Frances casts himself as the Justice Stewart of psychiatric diagnosis (“I know [mental illness] when I see it,” though certainly a central purpose of the DSM is to remove that kind of subjectivity from the diagnostic equation.

Of course, Dr. Frances is well aware of this dilemma and notes that a more pragmatic definition of mental disorder may be “what clinicians treat and researchers research and educators teach and insurance companies pay for.” This almost tongue-in-cheek definition is correct to a point, but at the same time highlights how a unitary definition of mental disorder cannot possibly satisfy all of the various arenas that utilize psychiatric diagnosis. Each of those arenas has its own reasons for asking the question, “What is a mental disorder?” and many different answers may be required to address the more specific questions of what to treat, what to study, what to teach, and what to pay for. In its preface, DSM-IV specifically outlines an intent to serve the needs of both clinical and research domains, but while those domains are interdependent, each has a different aim. With the addition of third parties (such as insurance companies and the legal system) to the mix, things become impossibly entangled. This state of affairs – that different definitions of mental disorder may be required based on careful analyses of “contextual utility” – is incompatible with what for many is the simplest definition of mental disorder – “what’s in the DSM.”

Whether of not something should “count” as a mental disorder will always, in the final analysis, be based upon value judgments [3,4]. For the DSM, the threshold to guide such judgments is usually rooted in the principle of “clinical significance,” currently defined in the DSM-IV by the presence of “clinically significant distress or impairment of functioning” [5]. Although this recursive definition begs the question of how to define “clinically significant,” the answer is intentionally open to subjective interpretation, and is nonetheless, in my view, the correct approach to take. Such an emphasis on clinical utility also makes perfectly defensible Dr.

Frances’ stance that “if you believe as I do, that the DSM is more an exercise in forging a common language than in finding a truth, you need a strong reason to change the syntax.”

No doubt, there are myriad forces that are influencing the rush to publish DSM-5, though from a scientific standpoint the central motivation seems to be a desire to make progress on the establishment of construct (i.e. biologic/etiologic) validity for DSM disorders. That goal was outlined by Robins and Guze [6] in 1970, but has gone sadly unrealized despite more than 40 years of active research, technological advancement, and the evolution from DSM-II to DSM-IV. It is therefore the hope of DSM-5 architects that changing the syntax of the DSM in a fundamental way – by moving to a “dimensional” model of mental illness – might pave a new pathway towards the validation of psychiatric disorders.

Although a truly dimensional model is taking shape for the personality disorders in DSM-5, elsewhere the incorporation of “dimensional measures” is more correctly described as the modeling of disorders or symptoms as “spectra” that can be quantified along a continuum, spanning from normality to pathology. Psychiatric research, as well as a general scientific worldview, supports the notion that categorical distinctions are illusory and that the real world is made of “fuzzy boundaries.” Therefore, moving towards a spectral view of mental disorder in DSM-5 might very well point us in a better direction to ultimately achieve validity in psychiatric diagnosis (note however that establishing construct validity does not verify that something is a “disorder” – explanatory physiologies underlie both pathological and normal variants alike). But while this shift may be justified on the grounds of “research utility,” most of Dr. Frances’ concerns about “unintended consequences” pertain to the DSM-IV’s chief aim as a guide to clinical practice.

Within the clinical world, Dr. Frances is most concerned that shifting to a spectral model of psychiatric disorder – embodied in the newly

proposed “psychosis risk syndrome,” “behavioral addictions,” “mild cognitive impairment,” and “temper dysregulation disorder” – will likely occur “at the expense of normality.” Overdiagnosis, false positives, and the pathologization/medicalization of normal behavior by psychiatrists are already issues of academic debate, not to mention pervasive fears of the public at large. I share such concerns, but view the situation and the future somewhat differently.

First, I disagree when Dr. Frances suggests that these “softer” ends of psychiatric illness spectra don’t meet the definition of “what clinicians treat.” In fact, treatment for mild and sub-threshold disorders is already standard practice, just as psychotherapy of the “worried well” dates back to its invention. Many children in prodromal psychosis research centers have already been by treated with antipsychotics prior to referral; kids with mood problems are often diagnosed and medicated for conduct disorder, bipolar disorder, and/or ADHD; treatment centers abound for behavioral addictions including binge eating and sexual excess; and our society is obsessed with stemming the tide of age-related change, both physical and mental.

Second, though I agree there has not been enough advancement in psychiatric research to warrant a new DSM at this time, I see the shift towards spectral models of disorder as unavoidable in DSM-5 and beyond. This in turn will inevitably lead to etiologic discoveries that allow manipulation and intervention regardless of where a patient sits on a spectrum, thereby rendering distinctions between pathology and normality even more arbitrary than they are now. Therefore, while in current clinical practice many patients resist psychiatric diagnosis and treatment, that picture will predictable change as novel interventions open the door to not only better treatment of disorders, but enhancement of normality. It may be a far off vision, but for better or worse, if we build it, they will come. The result would be a drastically altered landscape of mental healthcare and society at large, in which, as Peter Kramer suggested in *Listening to Prozac* [7], we can make people “better

than well” through “cosmetic psychopharmacology.” This potential for “neuroenhancement” highlights the subjective relativity of “clinical significance,” where distress and impaired functioning themselves exist on a spectrum. “What clinicians treat” will ultimately be defined by the availability of interventions rather than “what’s in the DSM.”

Finally, this view of the future means that clinicians must take care to not assume that what works at one end of a spectrum will work at another. Careful research is needed to determine the most effective and safest treatment strategies along illness spectra. If spectral disorders appear in DSM-5 before such research is done, then it is likely that “treatment” will be applied in a haphazard fashion. At the same time, while concerns about the “disease mongering” interests of Big Pharma and others are valid, particularly at the soft end of illness spectra [8], there seems to be an inherent fear of pharmacologic neuroenhancement that extends beyond mere risk-benefit considerations. This warrants philosophic and ethical reflection and invites further debate about why many feel that talking to someone, eating right, exercising, and studying hard should be encouraged, but taking a psychotropic medication, anabolic steroid, or cognitive enhancer should not [9,10].

Whether or not the DSM should implicitly sanction such practice through diagnostic expansion is a further matter of debate, and immediate opinions on whether to forge ahead with DSM-5 would seem to depend on whether one believes that the main intent of DSM is to guide current clinical practice or to facilitate future scientific discoveries that might result in the eventual validation of psychiatric disorders. On the one hand, my feeling here is that scientific discoveries should precede DSM revision, not the other way around. But on the other hand, I believe that psychiatric illnesses are fuzzily-bounded states that arise from genetically-mediated and environmentally-influenced aberrancies in neural networks, and that escaping existing DSM categorical illnesses in favor of dimensional models is a necessity for scientific progress in etiologic research

and the development of therapeutic interventions. For this reason, there is reason to be excited about the NIMH’s development of the Research Domain Criteria (RDoC), intended as a “next step in a long journey” to “create a framework for research on pathophysiology, especially for genomics and neuroscience, which ultimately will inform future classification schemes” [11]. Such ongoing efforts underscore how etiologic research does not need a new DSM to proceed, and suggest that, pending validation of the RDoC and demonstration of their clinical utility, DSM-IV is, to borrow from Winnicott, a “good enough” rough guide for clinical work.

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The Psychiatric Spectrum And Chasing The End of the Rainbow

Allen Frances, M.D.

Dr Pierre and I agree on almost everything, but I think we part company in our attitudes toward the creeping expansion of the psychiatric spectrum (as it increasingly shades out normality). Dr Pierre seems to see the increasing inclusiveness of psychiatric diagnosis as: 1) an inevitable result of the fuzzy boundaries between mental disorder and normality; 2) as no great threat because treating sub threshold presentations has already become the practice norm; 3) as potentially useful in alleviating mild symptoms and even in providing performance enhancement via cosmetic psychiatry; and 4) as an inevitable trend that defies correction.

I don't have inherent moralistic concerns about expanding the boundaries of psychiatric diagnosis and treatment (although admittedly it can take on a Brave New World feel). But I believe strongly that we shouldn't attempt to extend our reach until we are much more sure of our grasp. Before we stake out our claim to the milder, spectrum presentations and to performance enhancement, we need to have convincing scientific evidence of the risks and benefits of doing so. We need to be sure that we will not wind up doing more harm than good? We also need a thorough public policy debate that goes

far beyond just the views and interests generated from within our profession in determining what are the appropriate limits of our profession.

My concern is that psychiatry is rushing pell mell to define as illness a number of milder conditions for which there are currently no treatments with proven effectiveness (other than placebo and time). This is being done on the basis of tissue-thin scientific evidence; on the recommendation of experts in each field who have an understandable bias toward expanding their special interest; and without the benefit of serious external review. There has been far too little consideration of the risks to false positives or of the societal costs of conducting what amounts to a enormous public health and public policy experiment in promoting the widespread use of medication for unproven indications.

The premature inclusion of the largely unstudied spectral diagnoses in the official nomenclature would give them a substance beyond their shadowy deserts and feed the drug company marketing beast. The medications (particularly the antipsychotics) are far from benign (even when they are clearly indicated) and have a simply dreadful risk/benefit ratio when they are prescribed for people who don't need them. The judgment on the desirability of cosmetic psychiatry or performance enhancement should be the subject of a broadly inclusive policy debate. The best example of how not to slip inadvertently into performance enhancement is the DSM 5 suggestion to reduce to a bare minimum the diagnostic threshold for adult ADD. This despite the fact that we know that there is already a large secondary market for stimulants on college campuses. If we want college students to have easy access to stimulants for performance enhancement, let's make them available over the counter and not require what may be a spurious psychiatric diagnosis for their use. Decisions that make stimulants even more widely available should be made by the FDA—not just by a small group of ADD experts.

Finally, we should touch on the issue of scarce resources. If psychiatry and the drug companies focus attention and dollars on the mildest of condi-

tions, on the worried well, and on high performing people striving for even greater perfection, there will necessarily be fewer resources for treating the patients with more severe conditions who clearly need our help and for whom we do have treatments that have been proven to make an important difference.

So what is the verdict on spectrum conditions. Before making them official, let's wait until we know we can diagnose them accurately and can prove the clinical utility of both the diagnosis and treatment. By all means, this should be a fertile area of research for reasons both practical (the value of early intervention) and theoretical (mental disorders simply don't have clear categorical boundaries). But until the research is in, let's focus our diagnostic system and our clinical work on the patients who definitely need us and let's provide them with treatments that have been proven to work better than placebo.

* * *

Final Comment

Joseph Pierre, M.D.

Since it might not be apparent that Dr. Frances and I agree far more than we disagree, I will break from my usual attempt to maintain neutrality in academic writing, and share some disclosures. First, as an inpatient psychiatrist whose work focuses on the treatment of psychosis, I have intentionally placed myself in a comfortable position that allows me to play the role of Dokuon (from the Zen koan that I quoted in my commentary), where despite fuzzy borders, it is most often impossible to deny that there are conditions that ought to be called “mental disorders” and that warrant intervention. Second, having worked extensively with patients with substance abuse disorders, I have acquired a strong skepticism regarding the indiscriminant use of pharmacotherapy (whether self-administered or prescribed), and am keenly aware that interventions intended to make people feel better can often be more harmful in the long-

run. Third, I am already disheartened by what I see as the over-diagnosis of DSM-IV conditions such as bipolar II/NOS, ADHD, and PTSD, and am therefore opposed to diagnostic expansion that creeps further into the softer ends of illness spectra. While I have worked in a “prodromal schizophrenia” research clinic and believe this area of research is important, I feel that the predictive power of “at-risk for psychosis” (at best, 40% at 2-year follow-up) and the lack of clear treatment guidelines (e.g. psychotherapy? antipsychotics? antidepressants? omega-3 fatty acids?) argue strongly against inclusion of “psychosis risk syndrome” in DSM-5. In short, I do not feel that a new DSM is warranted at this time.

With these biases, I do indeed view diagnostic expansion as a potential threat. Dr. Frances clarifies however that his concerns are for a more immediate future in which DSM-5 proposals would predictably leave us floundering with many more diagnoses, without safe and evidence-based interventions to accompany them. I share that concern, but at the same time don't view that future as too different from current clinical practice. For me, the more intriguing and dire threat, though farther off and ironically more optimistic in terms of scientific advancement, is the real potential for neuroenhancement to create a dystopian *Brave New World* (or *Gattaca*, to update the cultural reference). Either way, Dr. Frances is correct that I view diagnostic expansion as an inevitability, both because current APA/DSM-5 leadership seems to view the end-game of diagnostic validity as trumping concerns about potential consequences and because drug companies, clinicians, and patients alike will drive the market for interventions that hold the promise (if not the reality) of making people feel better. With this inevitability, it seems obvious to me that discussion and debate by not only policy-makers, but philosophers, ethicists, clinicians, researchers, patients, and the general public alike are clearly necessary. Yet I question whether this will happen to any significant degree beyond pages such as these.

The values that ultimately underlie threshold decisions in diagnosis are the

same types of values that govern opinions about where psychiatry “should” be headed. Many, if not most, of us are guilty of passivity when it comes to taking time away from our busy lives in order to articulate these values and attempt to influence this direction. We should therefore be grateful for Dr. Frances’s seemingly lone voice among his generation of leaders in the field. More than anything else though, my sense of inevitability here stems from what is no doubt a timeless observation. While I am “only” 10 years out of residency, there already seems to be a palpable division between myself and the younger generation of budding physicians, where my own dystopian musings often seem to fall on deaf ears and where the prospect of neuroenhancement is embraced outright. This makes me suspect that my own values on this particular subject might be conservative, outmoded, and in the minority, leaving me with only the truism that change, if not progress, is unavoidable.

DSM Purpose and Threshold for Revision

Donald Klein, M.D.
Columbia U. School of Medicine

A few remarks. As a member of the DSM III Task Force, my recollection is that we were neither driven by recent scientific advances nor the triumph of biologism. In fact we explicitly eschewed etiologically based categorization since both psychogenic and biogenic theories seemed so data free, contradictory, and useless. We were principally driven by the methodological discovery that psychiatric diagnostic unreliability was due to criterion variance. If (pre-DSM-III) I was told that a patient was schizophrenic, I didn't have a clue, but we could lucidly discuss the presence or absence of delusions and hallucinations. The innovation was trying to make the criteria clear enough to be clinically communicable by reasonably explicit (operational was the buzzword) inclusion and exclusion criteria, in the guise

of polythetic categories.

That some promoted these into causal entities contradicted DSM III explicit statements that these were (probably multicausal) syndromes (following Sydenham) that had some useful prognostic and hopefully treatment implications but best helped clinicians to understand each other. That this hampered some researchers (not research) is too bad, but so many things hamper some researchers.

To enter nosologic theory, it is surprising that midst some rather high-flown verbiage there is no mention of evolution, evolved functions (often to be discovered), as empirical benchmarks—or the suggestion that it is multi-causal dysfunctions—something has gone wrong—that are manifest as syndromes, which are not social constructs but repeatedly described across cultures. That neural circuitry, complexified by genomics, will do the job is possible but in my view doubtful. After all, that is the view that has led to the by now conspicuous failure of the heavily researched pharmaceutical industry to find any actually new psychotropic agents. All of our current psychotropic agents are the offspring of serendipitous observations. That should inform our efforts.

As for DSM-5 revision, it should be clear that verbal changes usually lead to more confusion than clarification. There, field trials may have a point in demonstrating if such are helpful in improving comprehension by professionals. Minor increases in reliability are not worth the concurrent confusion. Substantive changes must pass the high hurdle of demonstrated clinical utility in terms of improved prognosis or treatment outcome. I doubt if any of the projected field trials can provide such evidence.

It should be clear that epidemiological “findings” depend on the particular, often arbitrary—see agoraphobia—algorithm used by the epidemiologist. Usually, this is acting on data collected by non-clinicians from a set verbal questionnaire. Therefore, the note-taker has neither the knowledge nor the warrant to follow up the often ambiguous replies. The data base is weak, making diagnostic in-

ferences even weaker.

We would all like objective findings to increase the firmness of our diagnoses. For over the past 50 years relentless biological research has fallen afoul of artifact, with no outstanding successes except when linked to general medical conditions, e.g., infection, exogenous toxicity, vitamin deficiency, endocrinological derangement, etc. The recent brain imaging, genomic, neuroscience advances enhance optimism but remain too thin to use. Too bad.

Dimensional revision, except for the fairly trivial, often too global, severity ratings is another abstract framework rather than a demonstrated useful tool.

Second Umpires Everywhere

Allen Frances, M.D.

Dr Klein's remarks are pleasantly surprising since we seem to agree in some ways I did not anticipate.

1) I expected Dr Klein to take the part of the first umpire. He was a central member of the small circle of great pioneers in biological psychiatry and has been perhaps its most articulate, influential, and energetic spokesman. I thought Dr Klein would provide the most elegant and persuasive possible defense of the “I see them as they are” realist school of biological psychiatry. Instead, he and I seem to agree completely in sharing a constructionist view of the epistemology of psychiatric diagnosis. I am not sure whether I misread him before, am misreading him now, or (more likely, I hope) we have come to the same interpretation of the scientific evidence accumulated since DSM-III - that psychopathology defies, and probably will continue to defy, easy etiologic answers. As I read his remarks, Dr Klein and I are both “second umpires” who believe that mental disorders are no more than useful constructs, certainly not diseases.

2) Dr Klein is quite right that DSM-III was explicitly meant to improve reliability in a way that would be

atheoretical in regard to etiology. But the publication of DSM-III was also, symbolically at least, a paradigm shifting moment in psychiatric diagnosis—marking the rejection of psychodynamic etiologic models and greatly promoting the search for etiologies rooted in biological psychiatry. Its major innovation—the criteria sets—were certainly no more than a tool to improve reliability. The categories and definitions should not have been reified—but they have been. Although the DSMs have explicitly disavowed any assumption that the descriptive definitions presume anything about causality, almost all research funding has been DSM disorder driven, and in many cases driven up a blind alley. The new NIMH RDOC project is a useful departure to a less procrustean approach, but it will be many years (decades?) before we will know whether it will be any more successful.

3) We agree about the "me tooism" pharmaceutical rut. The interesting question is its cause. Do the drug companies fail to make advances because what they care about most is marketing and lobbying—and "me too" is the safe play? Or are they hampered by the stultifying DSM categorical approach which perhaps does not provide the best signposts to progress? Both are undoubtedly at least partly true. But there is a even more worrisome third potential problem—that all the low hanging fruit has already been picked by the original serendipitous pioneers (their serendipity was made possible precisely because the early fruit was so low hanging). It may be that, from here on out, the obstacles to real (not just advertising/pharmaceutical) breakthroughs will be especially difficult to surmount.

4) I agree that evolutionary factors combining in complex ways with current environmental variables (and chance) must play a crucial role in understanding how any behavior is, and is seen as, deviant. The problem is that the available models of the evolutionary psychology of normal behavior are still in their earliest, least tested (and in many cases even untestable) stage of development. We are certainly very far from having testable evolutionary models of psychopathology. Although it is

always tempting to develop appealing and plausible "just so" stories providing an evolutionary rationale for one or another mental disorder, there is no evidence supporting any of them or even a clear methodology for testing them.

5) Dr Klein and I agree completely on several other important issues. We agree on the need for high thresholds before changing our current definitions. We agree that dimensional models are not ready to make much of a contribution to DSM5. We agree that the interpretation of the results of epidemiological studies should be extremely cautious. Because of their severe and inherent methods limitations, giant inferences about prevalence rates have been made on the basis of very limited and fallible data, with no possible way of evaluating whether the self reported symptoms have clinical significance.

Final Comment

Donald Klein, M.D.

That Allen and I agree on so many practical issues is gratifying and, hopefully, useful to our field. However, we have a misunderstanding regarding my emphasis on evolution. The issue is how to get past the confusions promoted by an arbitrary, value determined, constructivism to a non-arbitrary framework for conceptualizing what has gone wrong. The stream of social life that brought about the concept of illness was due to considering the occurrence of acute, cryptogenic involuntary afflictions. This led to the reasonable inference that something had gone wrong—perhaps a curse or a demon, or bad food, etc. Evolutionary theory provided a detailed natural selection framework for understanding how functions that address problems in survival and reproduction evolve. Because these functions can objectively achieve their goals, they can also go wrong. The argument whether dysfunction is real, rather than an arbitrary value, should cease with this acknowledgment. That it is often diffi-

cult to discern is true, but irrelevant to the worth of this evolution-based concept. The heart beats to circulate blood - not to make noise, as Harvey discovered. Functions are hypotheses - open to both confirmation and disconfirmation.

However, long before Darwin, the concept of an underlying disease that led to a manifest illness was a rational hypothesis. This was enriched by Sydenham's formulation of the syndrome, thus enabling rational prognosis. Since functions may be impaired in many ways, there is rarely a simple syndromal relation to simple genetic malfunctions, as has been demonstrated by recent disappointments.

With growing understanding of pathogenic processes, the boundaries of disease have progressively sharpened. However, these are no longer restricted to polythetic symptomatic boundaries but are diagnosed by delineating pathogenic processes, and their resulting dysfunctions, that may manifest as syndromes. Unfortunately, psychiatry has not progressed to that point. However, that certain syndromes have been reliably described for centuries, across many cultures, infers a common discoverable dysfunction.

References below indicate that the combination of psychopharmacology, challenge techniques, and family studies provides grounds for inferences regarding the pathophysiology of panic disorder. This gets us closer to objective diagnosis.

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The DSM Debate: Potential Harms Related to Psychiatric Diagnosis

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Introduction

Dr. Allen Frances, by raising questions about the DSM-5, has stirred up a vigorous discussion about the weaknesses of psychiatric diagnostic classification as well as troubling elements related to the DSM in general. Dr. Szasz's critical voice has added richness and value to the discussion. Both Dr. Frances and Dr. Szasz agree on three points: first, our diagnostic system has not led to the identification of any biomarkers or biological causes for mental disorders; second, the diagnostic categories are heterogeneous within categories and often overlap with each other as well as with normalcy; third, diagnoses can cause real harm, not just to a few people, but to millions (Frances, 2010).

Many commentators express relief that the limits of our current diagnostic system have become the topic of an open conversation. Clearly, the reliability of psychiatric diagnosis has been oversold (Beutler & Malik, 2002). Moreover, the gap between research and clinical settings has not always been bridged successfully. Criteria sets that seemed to work in research settings have not provided reliable diagnostic tools for clinicians who struggle with the highly variable interpretations of patient data under the pressures of

reimbursement procedures and the vested interests of the pharmaceutical industry. Our "Decade of the Brain" has yet to produce any biomarkers of psychiatric diagnoses, despite the claims of advertising websites for pharmaceutical companies that provide colorful animations of neurotransmitters and the brain in marketing directly targeting consumers.

This passionate discussion itself, however, is a sign of the profession's health, reflecting a willingness to examine the methods and scientific data that form the foundations of our endeavors. We wish to expand the discussion on the potential for psychiatric diagnoses to cause harm in both clinical and forensic contexts. We also wish to suggest ways to mitigate these harms.

Diagnostic Harms in Clinical Settings

As the DSM creeps further into normal human experience, so does the pharmaceutical industry. One example is the growth of the attention deficit disorder category, an example used by Dr. Frances and an area that presents multiple problems. First are the potential side effects of the medications. Stimulants are Schedule II drugs, some of which carry black box warnings for cardiac arrest and psychosis, a reflection of the potential harm for the individual and anyone else who may have access to the medication. Another concern is the effect of diagnostic creep on self efficacy and personal responsibility. Receiving a psychiatric diagnosis may create a sense of illness-imposed limits and dependency on medications in order to function.

Expanding diagnostic categories by lowering the threshold (e.g., reducing the number of symptoms required) for a diagnosis, has in some cases, led to a manufactured epidemic. An expansive interpretation of the diagnostic category of Bipolar Disorder to be more inclusive of children has led to a 40-fold increase in the number of children diagnosed with the disorder over the past decade and the doubling of the use of antipsychotic medication in children aged two to five (Olfson et al., 2010).

The problems of diagnostic heterogeneity, diagnostic overlap, and expanding diagnostic categories, coupled with a tendency to rely on pharmaceutical interventions, has led many in psychiatry to use multiple psychotropic medications in treating the same patient. Polypharmacy may take the form of a different medication for each diagnosis or symptom, essentially a pill for every ill. For example, a patient may simultaneously meet criteria for Major Depression, Attention Deficit Disorder, and Generalized Anxiety Disorder. Such a patient may be prescribed an antidepressant, a stimulant, and an anxiolytic even though there are no randomized controlled trials to guide such prescribing combinations in terms of safety or efficacy. Available data suggest increased risk from combining medications. For example, systematic observations reveal increased side effects and medical risks (including risk of death) when SSRIs are combined with other medications (Dalfen & Stewart, 2001). The use of multiple antipsychotic medications is increasingly common but without safety and efficacy data to guide this practice (Mojtabai & Olfson, 2010). Children and adolescents may be particularly at risk (Jerrell & McIntyre, 2008).

A related phenomenon is the "prescribing cascade," or the prescription of medications to combat the side effects of the medications for the original ill (Rochon & Gurwitz, 1997). This prescribing practice can lead to harm from interactions that raise the risk of side effects and adverse events, particularly in the elderly or in children. For example, a depressed patient may be prescribed an antidepressant but may experience antidepressant induced agitation (e.g. Preda et al., 2001). The prescriber may then order an anxiolytic, a mood stabilizer, and even an atypical antipsychotic medication to counteract these side effects. There is evidence that these types of medication combinations are becoming increasingly common, particularly in vulnerable populations like children. For example, one study found that 74% of children seen by a psychiatrist are on a psychotropic medication; half of these children are taking two or more psychotropic medications (Staller, Wade,

& Baker, 2005). Zito and colleagues (2008a) found dramatic increases in the off label use of antidepressants in children. Foster children are especially at risk for polypharmacy (Zito et al., 2008b). Of foster children who had been dispensed psychotropic medication, 41.3% received at least 3 different classes of these drugs, and 15.9% received at least 4 different classes (Zito et al., 2008b). Controlled scientific studies evaluating more than two psychotropic medications are virtually nonexistent (Antonuccio et al. 2008).

Diagnostic Harms in Forensic Settings

In the courtroom, experts are hired to present psychological and psychiatric evidence about defendants and plaintiffs in many different types of legal situations. Even though the DSM-IV includes a cautionary statement regarding the use of the manual in legal settings, there is no accepted alternative. In most legal settings, a diagnostic label in itself is not as important as the impact of the psychiatric symptoms on an individual's functional abilities (such as in a custody situation) or ability to reason (such as in a competency to stand trial case). The diagnosis of Mental Retardation, however, is an exception. The Supreme Court determined in *Atkins v Virginia* [*Atkins v Virginia*, 536 U.S. 304 (2002)] that defendants with the diagnosis of Mental Retardation cannot be sentenced to death. The limits of psychiatric diagnosis become clear in these life and death cases—even with well validated instruments (e.g., I.Q. tests) there will be measurement error and differences in diagnosis, especially in people who are at the border of mild mental retardation and low normal I.Q.. Under the intense scrutiny of capital litigation, areas of uncertainty and variations in information gathering and interpretation of diagnostic criteria are exposed. (Footnote: The United States Supreme Court decision referred to both the DSM and the American Association on Retardation criteria in the *Atkins* opinion).

Civil commitment is another

forensic setting in which to consider the limits and potential harms of psychiatric diagnosis. Civil commitment laws vary by state, but most states have a threshold requirement that a person have a mental disorder before they can be involuntarily hospitalized. Some states specifically exclude some DSM diagnoses, such as substance use disorders, from their definition of mental disorder. However, the potential expansion of psychiatric diagnoses raises the possibility of expanded eligibility for involuntary hospitalization, with resulting risks to personal liberty.

Legal cases involving sexual crimes are another source of concern regarding psychiatric diagnosis in the courtroom. An important example is the practice of civil commitment of a sexual offender following the completion of a criminal sentence in some states as set out in *Kansas v Hendricks* (*Kansas v Hendricks*, 521 U.S. 346.). In this case, Mr. Hendricks was a sex offender who became eligible for civil commitment because he had a "mental abnormality" or "personality disorder [s]"—pedophilia. Since 1997, the lack of a psychiatric diagnosis that applied to some sexual offenders has invited creativity (Zander, 2008). Is Antisocial Personality Disorder a qualifying mental abnormality, even if it is specifically excluded from a state's insanity law? Can we look beyond the DSM and use the non-DSM diagnosis of Psychopathy for these civil commitments? Does "Paraphilia NOS" meet the requirement for a mental disorder in rapists or do the civil commitment statutes for sex offenders require creation of new diagnoses used in civil commitment of sex offenders? A recent U.S. Supreme Court decision (*U.S. v Comstock*, 650 U.S. (2010)) upholds earlier cases and references only "mental illness." Critics of the use of psychiatric diagnosis to indefinitely commit people convicted of a sex offense question whether our legal system is using the psychiatric diagnoses as a tool of social control instead of changing sentencing guidelines to allow for indeterminate sentences for sex offenders. One might expect the Not Guilty by Reason of Insanity (NGRI) defense to be another major concern in the forensic area. This defense is rare, however. Many state

statutes have narrowed the NGRI criteria to exclude many defendants with significant psychiatric disorders and the success rate varies for this plea averages far below 50% (Borum, 2003).

Strategies for Reducing Diagnostic Harm

We suggest two general protections to mitigate the potential harms of psychiatric diagnosis: the emphasis of practice standards related to prudent care (with increased emphasis on the NNTB/NNTH data) and the education of our judges and legislatures on the limits of psychiatric diagnosis.

Psychiatry has an opportunity with the DSM to participate in the evolving culture of patient safety. Psychiatrists, especially those who prescribe medications, can adopt the role of "the reasonably prudent physician," which may represent a safer standard than some of the community standards described above (Simon, 2005). What does the reasonably prudent psychiatric practitioner do? Prudent practice behaviors include watching, waiting and data gathering during a period of uncertainty. Every prescriber encounters diagnostic uncertainty and needs more time in these situations or more data to confirm or disconfirm a diagnosis or a treatment response. There are always urgent situations when patients need immediate treatment. There are also less urgent situations where an individual presents with mild to moderate symptoms that raise several diagnostic questions. Additional data in the form of laboratory tests, drug screens, collateral informants and the observation of symptoms over time should be a greater part of the diagnostic approach. The DSM supports these practices with clear rule-outs and time criteria. yet persistent questions about interclinician diagnostic agreement of DSM diagnoses suggest that clinicians have not embraced these practices (Meyer, 2002).

Another characteristic of the reasonably prudent prescriber is the rare use of polypharmacy. Patient safety calls for confirming the diagnostic indication of each medication as well as the safety and efficacy of medication combinations. Medications that do not have

clear efficacy are discontinued. In addition, a reasonably prudent physician includes areas of uncertainty, both diagnostic and therapeutic, into discussions of informed consent.

One strategy for managing information on risk and benefit of medication treatment is to calculate NNTB (number needed to treat to benefit one extra patient compared with placebo) and the NNTH (number needed to treat to harm one extra patient compared with placebo). In an example from the childhood depression literature, Whittington et al. (2004) reviewed all of the available data (published and unpublished) from controlled trials of SSRIs in depressed youth. This meta-analysis concluded that the balance of risk and benefit based on NNTB and NNTH calculations was favorable for fluoxetine, but was unfavorable for paroxetine, sertraline, citalopram, and venlafaxine. The NNTB and NNTH analyses can guide informed consent discussions so that clinicians and patients may select scientifically supported treatment choices.

The Treatment of Adolescent Depression Study (TADS, 2004) expanded the data on potential risks and benefits to non-medical interventions. The TADS authors measured the short-term relative risks of treating depressed children with psychotherapy alone, medication alone, the combination, or a placebo. Despite the fact that suicidality decreased across all four arms of this study, the fluoxetine condition had a significantly higher rate of adverse events (such as suicidal ideation), physiological side effects (diarrhea, insomnia, and sedation), and psychiatric adverse events (irritability, mania, and fatigue) compared with placebo or CBT alone. Using the global response measure from the TADS study, the NNTB is about three in the combined condition, five for fluoxetine alone, and twelve for CBT alone, all compared to placebo. In terms of harm-related adverse events, the NNTH is approximately twenty in the fluoxetine-containing conditions in comparison to non-medication conditions. When considering psychiatric-related adverse events, the NNTH is

approximately ten in the fluoxetine alone condition compared with placebo and only about five compared with CBT alone. Adding together the risk for psychiatric, physiological side effects, and harm-related events reduces the NNTH even further. Clinicians and consumers can use information like this to inform their treatment choices in a way that is consistent with their own values.

In addition to adopting prescribing practices that address ambiguity of diagnoses and risks of treatment, psychiatrists can educate others on the fallibility of psychiatric diagnoses. As psychiatry continues the self-examination and introspection stimulated by Dr. Frances' essays, we have a duty to share our concerns about the validity of psychiatric diagnosis with those who also need to understand the strengths and weaknesses of the DSM. This includes the medical students and residents who have entrusted their education to us as well as judges, legislators, and consumers.

Summary

Most people using the DSM have the intention of advancing the well-being of patients or the science of psychiatry. A few use the DSM to assist judges and juries. Dr. Frances and Dr. Szasz remind us that the road to hell is paved with good intentions. If we honestly face the risks related to diagnostic creep, validity problems, polypharmacy, and the use of psychiatric diagnoses for social control, we can begin to put into place protections that safeguard the well being of our patients and advance our science.

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Diagnoses Can Be Bad for your Health

Allen Frances, M.D.

This is a really wonderful summary that should be required reading for every practitioner and trainee in the mental health professions. I agree completely with everything said and wish I could have said it as well. I will offer a few reinforcements on the same themes covering first the overuse of medications and then the forensic risks.

I agree completely that polypharmacy has gotten out of hand- a point made also by Dr Pinsker and in my response to him. Of course, polypharmacy is often rational and necessary, particularly for bipolar disorder and for some patients who respond only to custom tailored drug regimen. But polypharmacy can never be studied well and so lends itself to extremely careless, folk practice.

Patients acquire a collection of medications in different ways: 1) To quote the author's well chosen words, "Polypharmacy may take the form of a different medication for each diagnosis or symptom, essentially a pill for every ill"; 2) Chasing non-response with an additive approach that never sunsets previous failed drugs; 3) Excessive use of adjuncts; 4) The "prescribing cascade" treating side effects with more medications rather than stopping or reducing the doses of the medications the patient is already taking; 5) Too little use of psychotherapy, too much of medication; and, 5) Mindless overprescription because there are multiple doctors or one doctor who just keeps throwing stuff in.

The diagnostic system doesn't by itself cause careless and excessive prescribing habits, but it can facilitate it. DSM-III meant to divide the pie into small slices to facilitate diagnostic agreement. Some clinicians naively assume that the presence of multiple disorders implies the presence of multiple diseases.

The drug companies have played the largest role in promoting the use of "adjunctive medication", complex drug combinations, and the excessive use of medications, recently especially in children. The drug company ads showing pretty pictures of exactly which neurotransmitter is not making it to exactly which receptor best demonstrate how pseudoscience has become a marketing tool.

Our diagnostic system can be easily misunderstood and misused by the courts in what amounts to a psychiatric hijacking of constitutional rights. The most flagrant current example is misuse of the "diagnoses" Paraphilia Not Otherwise Specified, nonconsent or hebephilia to justify long term involuntary commitment of

sexual offenders after their prison sentence has been served. DSM-5 would make the current bad situation much worse and cries out for a careful forensic review. Deciding the death sentence on the vagaries of IQ measurement is the other leading current problem at the boundary between psychiatry and the law.

The authors suggest the value of metrics to quantify the balance between benefits and harms: the NNTB (number needed to treat to benefit one extra patient compared with placebo) and the NNTH (number needed to treat to harm one extra patient compared with placebo). The "reasonably prudent physician" assesses not just the benefits but also the harms of any intervention. Watching waiting and data gathering is a good stance during a period of uncertainty-whether dealing with an individual patient or the entire nosology.

The Ideal and the Real: How Does Psychiatry Escape The DSM-5 "Fly-bottle"?

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What is your aim in philosophy? To show the fly the way out of the fly-bottle.

...Ludwig Wittgenstein
(*Philosophical Investigations*)

Introduction

We all have our fantasies, and I confess that one of mine is a tad grandiose: I imagine being charged with creating a new system of psychiatric diagnosis, starting from scratch. My strong belief is that American psychiatry has become trapped, much like Wittgenstein's famous fly, in a kind of conceptual fly-bottle, embodied in the DSM framework. Getting psychiatry out of that trap is the impetus behind my fantasy, and motivates part one

(“The Ideal”) of this essay. On the other hand, I am realistic enough to know that we are likely to be saddled with some version of the present DSM framework for many years to come. With that in mind, I offer some specific comments on the DSM-5 framework, and on some of its prominent critics, in part two of this piece.

The Ideal

So what’s my first move in creating a new diagnostic framework? I would drop the “DSM” designation—where were all those “statistics” anyway?—and call my magnum opus the *Manual of Brain-Mediated Disease*, or MBMD. I choose the term “brain-mediated disease” because I assume there is relatively little controversy in the claim that those conditions of greatest interest to psychiatrists are “mediated” by the brain. The concept of “mediation” gets me out of the fruitless Cartesian conundrum of “mind” versus “brain”, “mental” versus “physical” or somatic conditions, psychological vs. biological theories of disease, etc. My supposition is that—whatever the ultimate, ontological nature of what are now called “mental disorders”—the organ chiefly responsible for their manifestation is the brain, and not, say, the gallbladder.

Technically speaking, the particular conditions listed in the MBMD would be considered “instantiations of disease”, rather than as “disease entities” or discrete “diseases.” I avoid the term “diseases” because this term carries with it the connotation of reified entities in the physical world, with the same ontological status as stones, trees, or sodium atoms. I don’t believe disease entities sit in the same ontological category as these objects; rather, disease entities are essentially pragmatic constructs for making sense of human suffering. In this regard, my view is closely related to that of Kendell and Jablensky, who wrote in 2003 that “... the mere fact that a diagnostic concept is listed in an official nomenclature and provided with a precise, complex definition tends to encourage this insidious reification.” [1]

To be clear: my position does not deny that specific pathoanatomic le-

sions or pathophysiological dysfunctions may *underlie* many common instantiations of disease. But my position entails that such lesions or dysfunctions are not *disease itself*. The latter—derived from our subjective awareness of “dis-ease”—is a pre-scientific construct, available to men and women long before there were physicians, X-rays, or MRIs. (The English language once had the term “diseasy” to describe how people feel when experiencing dis-ease). The distinction I draw is one based on Virchow’s apparent distinction between *Krankheiten* (diseases) and *die Krankheit* (disease in general) [2]. I also draw on the important claim put forth by Kendell; namely, that “disease” is properly predicated of *persons* (“people”)—not of minds, brains, or bodies [3].

My next move would be to separate *clinical descriptions* of disease—embodied in what I call “prototypes” [see below]—from *research-oriented criteria* aimed at fostering inter-rater reliability and uniform selection of research subjects. These research criteria would be relegated to one of the MBMD appendices, and would not be of primary interest to clinicians. The research-oriented criteria would be quite similar to those now used in the DSMs, and would consist of symptom checklists that “define” a particular condition, for research purposes. These criteria sets would indeed be “conservative”, as Dr. Frances would have it, to this extent: criteria would be altered from the previous DSM only if convincing scientific data supported the change—an issue to be determined by experts in research methodology, not just by experts in a particular area of study. By separating clinical from research-based descriptions, I try, in a rudimentary way, to deal with the important distinction Dr. James Phillips makes; i.e., “...the distinction between utility in practice and utility in research.” [4]. As Dr. Phillips rightly asks of the present DSM diagnostic criteria, “Who uses them?”

The MBMD would be built upon six foundational principles, which I call the 6 “Ps”: *privilege, prototypes,*

parsimony, pragmatism, pluralism, and phenomenology. Roughly, these are defined as follows:

1. “Privilege” refers to strict limitations on what conditions are permitted into the diagnostic schema. If psychiatric nosology is conceived as a house with many rooms, only certain kinds of conditions would get through the front door; namely, only conditions that entail substantial and enduring *suffering and incapacity*. (To signal forward a bit, I believe my position is compatible with similar views advanced by Dr. Allen Frances). Furthermore, the “suffering” would need to be *intrinsic* to the condition, at least in part—not merely the result of societal disapproval, punishment, or “bad stuff” ensuing from a particular behavior [5]. In the Judaic tradition, the rabbis speak of *tiruf hada’at*—“mental anguish”. This is not a bad phenomenological starting point for the kinds of conditions we ought to let through our diagnostic “door” to disease. Accordingly, conditions like ego-syntonic “hypersexuality” or sociopathy—both presumably lacking intrinsic suffering—would not be admitted as instantiations of *disease*. This is not to say that such conditions should never be the focus of *therapeutic intervention*, or of social and legal regulation—but those thorny issues would take us far afield.

2. “Prototypes” refers to the use of *idealized models* or archetypes of disease, rather than of “categorical” or “dimensional” methods of classification. These prototypes are similar to what Nassir Ghaemi has called “ideal types”, described as “... simplified version[s] of reality” [6]. The use of disease prototypes is one way to navigate around the Scylla of “nominalism” and the Charybdis of “realism” [7]. In a sense, prototypes are the diagnostic equivalent of “fuzzy logic”, and would make up the core of the new diagnostic descriptions. Each prototype would consist of a richly-detailed, generic, clinical case history, illustrating a particular clinical condition. The prototypes would be *compatible with*, but not identical to, the research-oriented criteria. Essentially, the research cri-

teria would constitute a subset of features within the surrounding “fuzzy” construct of the prototype.

3. “Pragmatism” refers to the *instrumental* nature of the diagnostic schema; specifically, psychiatric diagnosis is seen fundamentally as a *means to an ethical and humanitarian end*—namely, the effective relief of certain kinds of human suffering and incapacity (“dis-ease”). Thus, my diagnostic categories would strive for “utility” in roughly the sense described by Kendell and Jablensky [1]. Specifically: if the net amount of psychic misery in the world were reduced more by using prototype A than prototype B, prototype A gets priority, all other factors being equal. I do *not* use the term “pragmatic” in the debased sense that Nassir Ghaemi MD rightly castigates; i.e., as a hodge-podge of “...the beliefs of clinicians, the wishes of patients, our general ignorance about many scientific facts, the limitations of our treatments, [and] the needs of insurance reimbursement.” [8]

4. “Parsimony” refers to the goal usually expressed in terms of Occam’s Razor; i.e., “entities should not be multiplied beyond what is necessary”. Ideally, this principle would reduce the total number of psychiatric diagnoses, but without eliminating essential categories. However, I would not prejudice the scientific enterprise by aiming for a particular number of diagnostic categories, or even for the goal of increasing or decreasing the total number of such categories.

5. “Pluralism” refers to the use of multiple types of evidence and levels of understanding, in answering Prof. Tim Thornton’s question; i.e., “What is it... for something to be a mental disorder?” [9] Or, in my terms, “What ought to count as an instantiation of brain-mediated (“psychiatric”) disease?” *Pluralism allows for, but does not require, biologically-based criteria for specific instantiations of brain-mediated disease.* Data on biological factors related to a particular condition would be appended to the basic prototype, as “Supporting Data.” Phenomenological data (see #6) would also “count” in identifying instantiations of brain-based disease.

6. “Phenomenology” —i.e., the *contents and structure of the patient’s felt experience*—would be an important part of the prototypical descriptions in the MBMD, following the work of Husserl, Karl Jaspers, and various “existential” philosophers. Exemplary in this regard are Arieti’s classic descriptions of the inner world of patients with schizophrenia [10].

The Real: DSM-5 and its Discontents: Areas of Agreement with Dr. Allen Frances

I find many areas of agreement with Dr. Allen Frances’s positions, regarding the DSM-5. For example, I agree with Dr. Frances that there is no “paradigm shift” involved in the ideas underlying the DSM-5. As several commentators in the AAPP Bulletin have already pointed out, Kuhn’s construct of the paradigm shift is in no way commensurate with the tactical tinkering proposed for the DSM-5, including the possible use of “dimensional” approaches to diagnosis.

I also agree with Dr. Frances that there is a “threshold” problem with several of the diagnostic categories proposed for DSM-5; i.e., too many conditions that do not reach the level of “suffering and incapacity” seem to be getting through the door. Specifically, I agree with Dr. Frances that “... the difficulties people have in meeting society’s expectations should not all be labeled as mental disorders...” [11], absent convincing evidence of *intrinsic suffering and substantial incapacity*; that is, *the presence of dis-ease* [5]. If society were suddenly to demand that we all function at the cognitive level of Steven Hawking, and began to label as “mentally disordered” those who couldn’t meet that expectation, something clearly would be amiss. On the other hand, it is pragmatically necessary to specify certain (relatively) culture-neutral, *rudimentary abilities* as defining the lower limits of functional capacity; e.g., the ability to get out of bed, feed oneself, maintain basic self-care, and perform certain essential cognitive tasks, such as remembering to turn off the stove.

Consequentialism in the DSM-5

As Dr. Alan Stone has noted (personal communication 12/24/09), medical ethics partakes of both *deontological* (duty-based) and *consequentialist* (outcome-based) elements. Dr. Frances espouses a largely “consequentialist” ethos, in his urging that psychiatrists consider the possible adverse effects that may flow from changes in the diagnostic schema. Thus, in a recent blog, he writes: “Much has been written about the “validators” of psychiatric diagnosis and how they should influence DSM...To my mind, by far the most important validator is *how will any decision help or harm patient care*, given the foreseeable circumstances under which it will be used.” (italics added) [12]

Indeed, on a purely ethical level, I agree with Dr. Frances’s position. By way of analogy: in the Talmud, the principle of *pikuach nefesh* (“danger to life”) overrides all religious laws except those involving murder, idolatry, and prohibited sexual unions. [13]. In psychiatric nosology, I would argue that demonstrable “danger to life” (e.g., “harm to patients”) should also override virtually all other concerns, *if there is strong empirical evidence of such a danger.* For example, if we had well-founded, empirical evidence—say, from actual clinical experience in Europe—that criteria set A for diagnosing ADHD inevitably leads to substantial harm to patients, whereas criteria set B does not, the latter ought to be favored, all other things being equal—even *if criteria set A were directly linked with specific biomarkers, endophenotypes, etc.*

But it is a different matter when—despite persuasive scientific evidence to the contrary—we *pre-emptively* manipulate our diagnostic categories, in order to head off some anticipated form of substandard medical care. In my view, this amounts to well-intentioned but misguided nosological gerrymandering. Thus, the fear that criteria set A will harm patients, based solely on hypothetical scenarios of slipshod medical

practice, *should not be sufficient to overcome criteria set A's scientific superiority to criteria set B.*

For this reason, I am very uneasy with Dr. Frances's speculation that certain proposed changes in the DSM-IV criteria will lead to excessive prescribing of stimulants [11] or antidepressants [14]. In this regard, both Dr. Sidney Zisook and I differ with Dr. Frances on the issue of the "bereavement exclusion" for major depressive disorder, which now appears likely to be eliminated in DSM-5 [14, 15]. Zisook and I believe that concerns regarding overzealous prescribing practices are best addressed thorough careful drug labeling information; and especially, through *intensive continuing medical education* aimed at improving prescribing practices. Indeed, I am not aware of any medical specialty that routinely determines if condition X is, or is not, a "disease"; or whether symptom X ought or ought not to be part of the criteria for a disease, based on anticipated problems with physicians' prescribing habits or the marketing strategies of pharmaceutical companies.

What is Harm Avoidance?

Similarly, the issue of harm avoidance—often expressed by the maxim, "First, do no harm"—also needs careful analysis. As Dr. Ghaemi has noted in his discussion of the Hippocratic philosophy, the complete quote attributed to Hippocrates is, "As to diseases, make a habit of two things—to help, or at least to do no harm." [16] (italics added). Note that the *first* Hippocratic principle entails actively *helping* the patient. Ghaemi goes on to observe that, for Hippocrates, "ethics grows out of science", which for Hippocrates meant *knowledge of disease*. "The ethical principle, standing by itself, is not at all what Hippocrates taught." [16]

Thus, Hippocratic "harm-avoidance" begins with *good science*: i.e., deciding whether or not the patient is actually suffering from disease; if so, determining what kind of disease; and then offering the appropriate treatment. Hippocratic harm-avoidance is not a solicitation to "gaming the system" by pre-arranging our disease criteria so as

to avoid certain feared socioeconomic or behavioral outcomes.

To be sure, Dr. Frances rightly calls attention to the risk of over-medication that might very well accompany "over-diagnosis" [11, 14]. But this is merely *one element* of an overall, clinical risk-assessment. The *inherent morbidity and mortality of a disorder* must also be weighed in the balance. For example: Dr. Frances maintains that reducing the number of days required for the diagnosis of hypomania (from the current 4 to 2 days) will result in over-diagnosis of bipolar disorder; over-prescription of atypical antipsychotic drugs; and adverse medical outcomes, such as metabolic syndrome[17]. This putative pathway to "harm" might or might not come to pass. But assuming, as Dr. Ghaemi argues, that the 2-day hypomania criterion is scientifically well-founded, its use could lead to robust societal "goods" that may well overcome the putative harm envisioned by Dr. Frances.

For example, the suicide rate in bipolar disorder is roughly 15-20 times that of the general population [18]. It is quite possible that by reducing the hypomania criteria from 4 to 2 days, we would vastly increase bipolar patients' access to lithium, which appears to reduce suicide risk substantially in bipolar populations [19]. To be sure, this is a pharmaco-epidemiological prediction that depends in part on the behavior—and proper education—of physicians. We would need several careful, medical-epidemiological studies, analyzing, say, the risk of antipsychotic-induced metabolic syndrome *compared with the risk of missed bipolar disorder and subsequent suicide*. My point is that merely positing iatrogenic harm to patients should not, by itself, overcome the scientific merits of refining our criteria for hypomania. Only well-documented, demonstrable harm to the public interest should "trump" scientific data showing that a change in criteria is justified. As Drs. Waterman and Curley persuasively argue, we must consider "...the negative consequences of leaving largely unchanged a taxonomy we know to be

inadequate at best and simply wrong at worst." [20]

As a very crude mathematical representation of these complex calculations, we can write:

$$J = \frac{S_1 B}{S_2 R}$$

Where J is the justification for changing a specific criterion; S₁ is the scientific evidence supporting the change; B is the known benefits of diagnosing and optimally treating the condition (e.g., reduced suicide rates, disability, etc.); S₂ is the scientific evidence arguing against the change, and R is the known risk of over-diagnosis and excessive or inappropriate treatment. Obviously, this theoretical calculation would be exceedingly difficult in practice. But it is equally evident that retaining a dubious DSM-IV diagnostic criterion requires much more justification than a mere prediction of iatrogenic harm to patients.

What is a "False Positive" in Psychiatry?

I also find Dr. Frances's use of the term "false positive" [14] in the psychiatric context deeply problematic. Indeed, some psychiatrists have appropriated this term from the fields of pathology or infectious disease, without thinking through its epistemological meaning in psychiatry. For example, when an infectious disease expert says, "False positive FTA-ABS tests [for syphilis] can occur in *Lyme borreliosis*,"[21] the expert is invoking a very different epistemological "deep structure" than that invoked, implicitly, by Dr. Frances. Thus, when Dr. Frances expresses concern that eliminating the bereavement exclusion will increase the rate of "false positive" diagnoses of MDD [14], it is unclear to what veridical standard he is appealing. What is our laboratory "test" for a *true positive* in MDD? Until we have a widely agreed-upon criteria set for MDD, for which a specific biomarker or endophenotype has been identified, the term "false positive" is, at best, a wishful metaphor; and at worst, a kind of Rylean "category mistake."

The term misappropriates, from the epistemic structure of the physical sciences, a term that has little if any “physical” meaning in psychiatry—at least, in the sense that an infectious disease expert would use the term “false positive.” One hopes, of course, that this situation will change as neurobiological knowledge advances [22].

That said, the term “false positive” can have a coherent conceptual meaning in psychiatry, when, for example, it describes a *problem with the application of specific diagnostic criteria*. For example, the statement, “Failure to recognize cocaine intoxication can lead to a false positive diagnosis of DSM-IV-defined mania” is at least conceptually sound, since we can point to cocaine as a confounding factor in applying our present criteria for the diagnosis of mania.

Finally, it is fallacious to assume, following a change in condition X’s diagnostic criteria, that an *increase in the total number of cases* of X necessarily indicates an increase in “false positives” for X. To assert this is merely to beg the question of what constitutes a false positive; indeed, absent a veridical biomarker for condition X, the increase in cases may simply reflect a legitimate increase in the identification of the illness. Thus, if elimination of the “bereavement exclusion” leads to an increase in the *total number of diagnosed cases* of major depressive disorder, this does not necessarily point to an increase in “false positives”[15].

Conclusion

I have tried to look at the problems of DSM-5 from the standpoints of the “ideal” and the “real”. Ideally, in my view, the field of psychiatry would scrap the basic “categorical” structure of the DSMs, and start from scratch. A new diagnostic system, in my view, would be guided by the six basic principles enumerated earlier: *privilege, prototypes, parsimony, pragmatism, pluralism, and phenomenology*. The foundational principle of this idealized system is that only those conditions that entail substantial, intrinsic suffering and incapacity “count” as instantiations of *disease*. Disease descriptions

would consist of “ideal types” (prototypes)—not symptom check-lists specifying necessary and sufficient conditions or “essential definitions.”

In reality, we are probably stuck with tinkering around the edges of our present, seriously flawed diagnostic system. Even so, our tinkering should at least be guided by the best available science. Absent convincing empirical evidence that a change in criteria will harm those we treat, our diagnostic criteria should follow the principle, “Go where the best science leads you.” That direction just might lead psychiatry out of the diagnostic fly-bottle!

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Sorry But No Easy Exit

Allen Frances, M.D.

I very much enjoyed Dr Pies' learned and graceful commentary and agree completely with him - except for the following:

1) The fly metaphor is vivid, but I think quite misleading. The implication is that, but for the procrustean bed imposed by the DSMs, descriptive psychiatry would overcome its disappointing failure to promote the deep understanding of pathogenesis. I agree wholeheartedly that the DSMs are ungainly historical accretions. If any of us were starting from scratch with no consideration for practical consequences, we would cut the (nonexistent) joints somewhat differently or try to number rather than name. But the fact that the current system is unlovely doesn't mean it bears responsibility as the trap holding us back. Our bottle is much more difficult to escape than Dr Pies suggests.

There are two reasons why replacing DSM would solve no problems, eliminate no traps. The first and lesser problem is that were we to bench DSM, there is no wonderful and universally accepted pinch hitter ready to step up to the plate and hit the ball out of the park. Many people offer alternative schemes (often passionately)—but all are about equally plausible and none has much scientific backing or offers such compelling new insights as to be clearly superior to any other. Better methods of defining things descriptively are much more likely to emerge

from a deeper understanding of psychopathology than to be the vehicle for gaining this understanding. For the most part, changing the descriptions amounts to rearranging the furniture.

The second problem is probably inherent and certainly more ominous. Our stalled effort to understand psychopathology most likely comes from its "chaotic" etiologic complexity, not from our inability to describe it properly. It is hard to predict the weather because so many variables influence it in such unpredictably complex and interacting ways, not because we have difficulty describing it. The development and maintenance of neural networks may work the same way. The fly can't escape the bottle because there may not ever be a simple and accessible way out.

2) Dr Pies accepts my pragmatic, consequentialist view on how best to make diagnostic choices, but with one important caveat—when the scientific evidence is compelling, it should trump. Fair enough. But the practical point is that for the kinds of questions being asked and answered in DSM deliberations, the scientific evidence supporting one or another position is rarely anywhere close to being compelling. On the very few occasions when the facts speak for themselves, there is no controversy—the decision is a no brainer. But most often, and whenever there is controversy, the evidence is remarkably incomplete, methodologically questionable, difficult to generalize, and subject to very different interpretations. It almost never grabs you by the throat in the way a "realist umpire" would have you believe.

Indeed it appears that Dr Pies has changed his umpire stripes. In his first section, Dr Pies is a "second umpire" constructionist who strongly decries the reification of descriptive categories. Here, we couldn't agree more. But in these comments, Dr Pies suddenly reverts to a "first umpire" who can comfortably call the balls and strikes of diagnosis "as they are" because the science tells him so. Dr Pies is here reifying what is still a very incomplete scientific base that can be plausibly interpreted in contra-

dictory ways and doesn't capture an elusive reality. The best way to diagnose grief or bipolar disorder may vary dramatically depending on whether you see the patient in a research clinic or in a busy primary care setting and on your level of expertise. This is why it is so difficult to apply the Bayesian conditional probability approach to psychiatric diagnosis—the probabilities are so setting specific. There is no one, right, real way to define each diagnosis and we know far too little how any proposed criteria will work differently in different settings. The DSM must be informed by the most balanced among the always contradictory interpretations of the available science. But it is unwise to reify an incomplete and fallible scientific base or to assume that it can or should trump possibly terrible consequences, when these are obvious.

And one must also factor in a universal and systematic bias. Experts in any given area have a blind spot that has them overvalue the research findings in their area, especially their own. They are prepared to accept conclusions from very incomplete data supporting their own pet suggestions that they would promptly shoot down were it from another area. The biggest flaws in the DSM5 process were allowing each work group to deliberate in relative isolation and then not to subject the proposals to a stringent and iterative external review.

On all the rest, I agree with Dr Pies and thank him for his comments.

The Past and the Future: What Constitutes a Mental Illness

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Like Allen Frances, I had just one philosophy course in college, so I will try to comment only as an historian of psychiatry, although like many people, (my guess), I am drawn to the philosophical issue of how to conceive of a mental disorder.

My first observation is that Dr. Frances is not only a psychiatric scholar but a sound historian as well.

Based on the fact that there were unintended negative consequences from incorporating even small revisions into DSM-IV, he is urging the leadership of DSM-5 to move slowly and carefully as they plan some significant changes for the new manual. He is arguing that we have to learn from the past, a position with which I, like any historian, would happily concur. Yet I imagine another question is—for both sides of the ongoing controversy—how large a vote should the past have in any decision. I do not intend to answer that at this point, but perhaps my discussion will throw some light on the current debate.

My second comment is that the “epistemological game” is a game of great consequence. Frances writes that as editor of DSM-IV, he “had no grand illusions of seeing reality straight on or of reconstructing it whole cloth.” There was no Truth “out there.” By contrast, I am reminded of the authors of the famous “Feighner criteria,” who seemed certain that with enough knowledge they, or their descendants, would be able to construct a totally valid classification of all mental disorders. Truth is achievable. The principal figures behind the legendary 1972 diagnostic criteria of disorders, a short list which they were sure were valid, were Eli Robins (1921-1994) and Samuel B. Guze (1923-2000) of Washington University in St. Louis. Robins and Guze clearly enunciated their position in their 1970 paper, “Establishment of Diagnostic Validity in Psychiatric Illness: Its Application to Schizophrenia” (the precursor to Feighner). They asserted that by following the procedures they outlined, one could assure that any given diagnosis was valid. Thus, before these procedures could be put in place, psychiatrists were to be limited to a very few diagnoses. This was why the Feighner criteria were restricted to only 15 diagnoses.

The “Wash. U.” psychiatrists (dubbed the “neo-Kraepelinians” by the erudite and witty psychiatrist Gerald L. Klerman (1928-1992), argued that very little was known about most mental disorders and therefore psychiatry as a medical discipline had to be rigorous in its research to assure diagnostic validity. The five methods

they declared were necessary for validity of any diagnosis were (1) description of the clinical picture, (2) laboratory tests, (3) exclusion criteria to weed out patients with other illnesses, (4) follow-up studies in order to make certain the initial diagnosis had been correct, and (5) family studies. For the time being, they pointed out, there were no laboratory tests for most mental disorders, so the other diagnostic methods were essential, never optional. Among these, follow-up studies especially were deemed crucial. The neo-Kraepelinians reiterated tirelessly the message that these studies were indispensable in the making of a valid diagnosis. In two separate publications Donald W. Goodwin (1942-1999), a junior colleague of Guze and Robins, quoted the words of Peter D. Scott, a well-known British forensic psychiatrist: “The follow-up is the great exposé of truth, the rock on which many fine theories are wrecked and upon which better ones can be built; it is to the psychiatrist what the post-mortem is to the physician” (Goodwin, Guze, and Robins 1969, 182; Woodruff, Goodwin, and Guze 1974, x).

Not surprisingly, Robert A. Woodruff (another Wash. U. psychiatrist), Goodwin, and Guze were even stricter than Feighner in their 1974 textbook, *Psychiatric Diagnosis*, where they offered only 12 diagnoses. They wrote in their Introduction: “Not every patient can be diagnosed by using the categories in this book. For them, ‘undiagnosed’ is, we feel, more appropriate than a label incorrectly implying more knowledge than exists” (1974, ix).

On this issue, Guze later recounted to David Healy an amusing anecdote of a meeting of the DSM-III Task Force, which he had attended. He had proposed “that perhaps we should urge [the APA] that, until there had been at least two long-term follow-up studies from different institutions with similar results, we shouldn’t give the entity a status in DSM-III. The alternative was to have a lot of undiagnosed cases. We could have a way of subcategorizing undiagnosed patients in which the label would indicate what the diagnostic problem was. That would put us on a

stronger scientific basis and it would constantly remind psychiatrists of our ignorance and what kinds of questions needed to be studied...I couldn’t get that group to vote in favor of my suggestions. The answer that I was given was that they said we have enough trouble getting the legitimacy of psychiatric problems accepted by our colleagues, insurance companies and other agencies. If we do what you are proposing, which makes sense to us scientifically, we think that not only will we weaken what we are trying to do but we will give the insurance companies an excuse not to pay us” (Healy 2000, 407).

Before I leave the questions of the nature of knowledge in psychiatry, diagnostic criteria, and achievable certainty, I should emphasize again the fundamental tension between Frances’ and the neo-Kraepelinians’ views of conceptualizing mental disorders. The authors of the Feighner criteria seemed convinced that once enough was known about any mental disorder, it could be completely categorized. Frances, on the other hand, argues that “almost everything in psychiatric classification is overlapping, fuzzy, and heterogeneous . . . The desirable goal of having a classification consisting of mutually exhaustive, non-overlapping mental disorders is simply impossible to meet.” Historically speaking (in addition to other aspects of the issue), I would venture the conclusion that here the rationalism of the Enlightenment and the deconstructionism of Post-Modernity confront each other.

My third comment has to do with the definition of mental disorder, an area of conflict in psychiatry. A definition of a “mental disorder” is indeed a tricky business, and many psychiatrists see no benefit in such a definition. Dr. Frances has acknowledged in his commentary that “many crucial problems would be much less problematic if only it were possible to frame an operational definition of mental disorder that really worked.” Nevertheless, he has concluded that thus far this has been impossible. Dr. Frances’ discussion of this matter

brought three things to mind which I would like to discuss: the vicissitudes of defining a mental disorder during the making of DSM-III (a DSM about which I am currently writing), the anti-psychiatry movement—an unhappy era in the history of American psychiatry—and the role of unsuspected motivation driving historical events.

When Robert Spitzer began convening meetings of the DSM-III Task Force in 1974, he emphasized that one of the things he wanted the Task Force to accomplish, as part of a revolutionary construction of DSM-III, were definitions of “medical disorder” and “mental disorder.” These were to show that mental disorders were a subset of medical disorders, so when he first raised the subject, he used the terms “medical illness” and “mental illness.” He wanted to establish that, without any doubt, psychiatry was a part of medicine. Spitzer had initially thought seriously about mental disorders even before he was appointed the head of the Task Force. In 1973, he had brokered the removal of the diagnosis of homosexuality as a mental disorder from DSM-II, and the controversy surrounding the event sensitized him to the subject of what constituted a mental disorder. He soon found impediments to his goal of establishing definitions in DSM-III. Still, at every turn he persevered because he envisioned the issuance of the new diagnostic manual as having intellectual goals far larger than its being a diagnostic classification. Spitzer wanted DSM-III to play a role in combating the anti-psychiatry movement of the 1960s and early 1970s and to refute critics such as Thomas Szasz who said mental illness was a myth.

I would like to spell out briefly the obstacles that lay in the path of an agreed-upon definition of a mental disorder. First, Spitzer encountered strenuous opposition from psychologists to the notion that mental disorders were medical disorders. This was a turf issue, with the psychologists fearing that they would lose the right to treat mental disorders if they were defined as medical. In June 1976, a conference was held in St. Louis on “Critically Examining DSM-III in Midstream.” Dr. Maurice Lorr, representing the American Psychological Association, “expressed the view that mental disor-

ders (as medical disorders) should be limited to those conditions for which a biological etiology or pathophysiology could be demonstrated.” In addition, just two months earlier, a former president of the American Psychological Association had been quite blunt in expressing his view that DSM-III was “turning every human problem into a disease, in anticipation of the shower of health plan gold that is over the horizon” (Spitzer and Endicott 1978, 36). However, even psychiatrists had complaints about Spitzer’s attempts at definitions. A month before the St. Louis meeting, at the annual meeting of the American Psychiatric Association, Spitzer and Jean Endicott, a close colleague on the Task Force, had put forth their definitions of medical and mental disorders. The reaction here too was quite negative. As Spitzer later reported: “Some questioned the need and wisdom of having any definition. Many argued that the definition proposed was too restrictive, and if officially adopted, would have the potential for limiting the appropriate activities of our profession . . . they also felt that it was out of keeping with trends in medicine that emphasize the continuity of health and illness” (Spitzer and Endicott 1978, 16). (This continues to be an important question in current debates over what diagnoses should be in DSM-5. Frances, in particular, has argued against pathologizing what he sees as aspects of normality, “everyday incapacity,” in his words.)

In spite of disagreements, Spitzer, as was his wont, did not surrender easily. He returned the next year to bolster his arguments. This was at the yearly meeting of the American Psychopathological Association, an organization of preeminent American psychiatrists dedicated to research on human behavior. In 1977 it devoted its annual conference to “Critical Issues in Psychiatric Diagnosis.” Spitzer and Endicott not only presented retooled definitions of medical and mental disorders, but Spitzer, as an editor of the 1978 published proceedings of the conference, now took the opportunity to remind his readers of the blows psychiatry had endured in the 1960s and early

1970s: “The very concept of psychiatric illness has been under considerable attack in recent years. This attack has largely depended upon studies derived from the social sciences. Some have taken the stand that what are called mental illnesses are simply those particular groups of behaviors that certain societies have considered deviant and reprehensible.” Spitzer believed that this rejection of the legitimacy of psychiatry was partly owed to the fact that “no generally agreed upon definition of mental illness has been propounded that is not open to the criticisms of cultural relativism” (Spitzer and Klein 1978, v).

In addition to his conviction that DSM-III, with its new diagnostic criteria, would bring diagnostic reliability to psychiatry, Spitzer conceived of the DSM as a weapon that could repel psychiatry’s cultural challengers. The new manual would have a potential of historical proportions. Nevertheless, although Spitzer labored mightily to develop “mental illness” as a subset of “medical illness,” he was ultimately forced to bow both to the demands of the psychologists that mental illnesses be labeled “mental disorders,” and the opinions of his psychiatric colleagues, who had philosophical and practical objections to his definition. The upshot was that mental disorders did not get to be defined as medical disorders. The attempts of Robert Spitzer—a psychiatrist of considerable accomplishment in many areas of the field—to establish a definition of a mental disorder, illustrate the complexities of arriving at one that is intellectually satisfying, clinically useful, and practically acceptable. It seems likely, however, that individuals in each generation of psychiatrists will pursue this elusive goal.

These three subjects, the role of lessons from history, the extent of possible certainty in psychiatry, and the attempts at a definition of mental disorder, have been prompted by my reading of Dr. Frances’ extensive discussion of the issues facing the authors of DSM-5.

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History Rhymes

Allen Frances, M.D.

How wonderful for our field to have Hannah Decker as our memory and our interpreter. I will respond to her three comments in turn:

1) "How large a vote should the past have in any decision." Dr Decker raises this most fundamental of human questions, but modestly refrains from voting on it. My authority is Thucydides — who inconveniently voted both ways. He wrote his history explicitly as a cautionary tale to guide us in our current decision making — since human nature is constant, the same dilemmas are inevitably recurrent and the unfolding of the past should be our best blueprint for how to proceed into the future. But Thucydides was a deep and tragic thinker who was also fully aware of the futility of his own didactic ambitions. History repeats, but there are so many interacting contingencies that it never repeats exactly or predicta-

bly or on time (instead it "rhymes" as Mark Twain so perfectly put it). Pericles could be the most visionary of Athenians and rightly predict that her strength would come from her fleet—but still miss the unpredictable point that the plague would also arrive on the very same ships. History provides no more than a rough guide, not a map.

Which brings us to what we can learn from the DSM past. I was a participant observer in DSM-III and DSM-III-R. I learned that Spitzer was a brilliant and dogged innovator. He had the insight that a criteria-based diagnostic system (developed originally as a research tool covering just a few disorders) could be adapted and broadened for wide clinical use as the official diagnostic nomenclature. And he had the determination and smarts to overcome considerable opposition both within and outside psychiatry. I also learned that, as with any extreme innovation, there were accompanying problems and excesses—too many rapid changes (DSM III-R only 7 years after DSM- III); overselling descriptive psychiatry; reifying diagnostic categories; too many unproven disorders, especially at the boundary with normal; heterogeneity within and fuzzy boundaries between categories; pseudo-precision; overestimating reliability in average clinical settings, and so forth. What I concluded from this history was that DSM-IV needed to stabilize rather than innovate—be careful, conservative, and evidence-based. The field needed time to swallow, digest, and incorporate all the radical changes wrought by DSM-III and III-R.

The work on DSM-5 has been misinformed by what is, in my view, a bad misreading of the recent history of our field. The DSM-5 leadership, responding to all the wonderful advances in neuroscience, developed an ambitious taste for a producing its own "paradigm shift." They missed the fact that despite its wondrous advances, the neuroscience revolution has not yet at all informed psychiatric diagnosis. Descriptive changes that don't follow from causal understanding are now bound to be arbitrary and

likely to do more harm than good. The DSM-5 leadership has also failed to learn from the mistakes we made in the previous 3 DSM's—that faddish, false "epidemics" can be caused by overinclusive criteria exploited by drug company marketing and that psychiatric diagnoses can be gravely misused in forensic settings. It is too soon to draw any confident conclusions about the historical meanings of DSM-5, but the early returns are not promising.

2) Regarding the Robins and Guze 1970 paper, "Establishment of Diagnostic Validity in Psychiatric Illness: Its Application to Schizophrenia." When I read this paper as a second year resident, my reaction was "these guys are so amazingly simpleminded and don't begin to understand how complicated psychiatry is." I felt the same way about Bob Spitzer, who was a favored teacher of mine, but seemed to see psychiatry in terms that were far too naïve for my sophisticated, sophomore mind. Then I grew up some and realized that perhaps my elders were right after all. The late seventies and eighties were exciting times for biological psychiatry. The genetic and molecular and brain keys to the psychiatric kingdom all seemed within reach. Well, sometimes even ignorant sophomores turn out to be right. The psychiatric "disorders" clearly are not simple "illnesses" ready to be unmasked by our powerful tools. Instead, they are remarkably heterogeneous and complex, not only in presentation, but also in etiology. The methods of validation suggested by Robins and Guze were meant to be no more than way stations on the road to biological understanding. It turns out that this understanding will be long in coming and that the suggested "validators" are very poor substitutes.

Robins and Guze did have a prescient understanding of the risks of diagnostic overreach. Their suggestion to limit the approved diagnoses to those with a strong evidentiary base was bound to collide with practical necessity. But they were

dead right to be concerned about the spawning proliferation of new diagnoses, often based only on the fact that a few clinician/researchers declare them to "exist." My tease to Bob Spitzer was that he never met a new diagnosis he didn't like. This applies even more to the DSM-5 work groups who are suggesting a plethora of unproven and potentially dangerous diagnoses that would have appalled Robins and Guze.

3) Regarding the inability to operationally define the term "mental disorder": The only consolation is that the terms "illness" and "disease" are equally elusive.

It will be fun to follow Dr Decker's ongoing chronicle so that we may eventually learn what this DSM fuss has all really been about.

The DSM and "Do No Harm:" Is a Radical Pragmatism Sufficient?

Warren Kinghorn, MD
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Dr. Frances, in a helpfully candid glimpse into the politics of psychiatric diagnostic classification, classifies himself as the second of the three umpires in the "epistemologic game." But is this borne out in his subsequent discussion? The second umpire, like the first and unlike the third, is apparently a realist, clearly holding that balls and strikes exist independently of the umpire's judgment, which itself may or may not be accurate. But Dr. Frances appears to take a much more radical stance: "mental disorders don't really live 'out there' waiting to be explained. They are constructs we have made up . . ." But if this is true – if the standard for diagnostic classification is not what exists "out there" but rather in "getting to what works best," if indeed "our mental disorders are no more than fallible social constructs (but nonetheless useful ones . . .)" – then Frances fits the type of the third umpire perhaps even more cleanly than the early Szasz, whose foundational work in *The Myth of Mental Illness* (1961) was premised

on the uncompromising nosological realism of the first umpire (the problem being, for Szasz, that mental illnesses were balls). It is the third umpire, not the second, who best exemplifies Frances' "utilitarian pragmatism."

To be sure, there are glimpses of realism in Frances' account. His observation that the NIMH Research Domain Criteria (RDoC) project rather than *DSM* might "lead the future charge in understanding psychopathology," along with his hope for a taxonomy of "true etiologically defined illnesses," implies that psychopathology is in some sense "out there" to be analyzed and understood, even if discrete mental disorders are not. But this realism plays little if any role in his account of how diagnostic revisions should be made: *DSM* should be revised not when new mental disorders "out there" are recognized (for how, after all, would we know a "mental disorder" if we saw one?) but rather when the consequences of a revision are likely to provide benefit to patients and, above all, will do no (anticipated) harm. *DSM*, for Frances, serves and should serve as a regulatory and even disciplinary document demarcating limits for the appropriate extension of psychiatric technology and for the appropriate use of psychiatry by particular interests (such as the state).

Frances is surely correct regarding the social function and power of *DSM* and regarding the need to approach potential revisions with extreme care. But his insightful account begs the important question: *who should decide?* Who should decide what "mistakes" and "problems" are, or what "mental disorder" is, or what constitutes "harm," or what would render *DSM* "useful?" Should patients decide? Should individual psychiatrists decide? Should the Task Force decide? And on what grounds? And how would we know if the judgments of any of these potential "deciders" were shaped, subtly and unconsciously, by particular forces such as pharmaceutical marketing, consumer-driven ideals of beauty and success, gender stereotyping, and so on? Can psychiatric diagnosis ever extricate itself definitively from Fou-

cauldian and Szaszian critique? It is difficult to see how Frances' pragmatism can ensure that diagnostic revisions will "do no harm" if "harm" is itself a contested category.

If, as Frances argues, efforts to establish a consensual and non-tautological account of "mental disorder" are likely to fail, there would seem to be no way around these questions. Psychiatric diagnostic classification, that is, must be understood as a pragmatic and tradition-constituted enterprise in which individuals and groups with particular interests interpret research data (itself compiled and reported by individuals with particular interests) in such a way as to shape the use of psychiatry and psychiatric technology in accord with these interests. This recognition should, at the very least, provoke humility and non-defensive soul-searching among those tasked with revising the *DSM*, since biases and moral failures in the "deciders" would very likely become manifest in their nosological decisions, and the ongoing cultural acceptance and use of the *DSM* hinges on the ongoing public credibility of these "deciders." It is no wonder that the *DSM-5* architects, in the face of much work in the contemporary philosophy of psychiatry, continue to speak in realist terms about syndromes "actually present in nature" and a nosology which "[carves] nature at its joints" (Regier et al., 2009, pp. 646, 648). In the absence of a narrative of progressive scientific discovery, would the social consensus regarding the usefulness of the *DSM* continue to hold?

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Pragmatism Meets Its Limits

Allen Frances, M.D.

Dr Kinghorn makes two extremely penetrating critiques of my position that cut straight to the heart of the matter. The first—that I am really a disguised third umpire—I can answer, at least to my own satisfaction. The second—that my pragmatism lacks normative values and a vouchsafed method—is devastatingly accurate and impossible to dispute. The two points in turn:

1) Does my statement that mental disorders are "fallible social constructs" mean there are no mental disorders til "I call them"- making me a third umpire? I don't think so. As I conceive the third umpire, he believes there is no underlying reality of balls and strikes and that construct is essentially all. In contrast, I believe there is a knowable underlying reality to what we now call mental disorders, just that this reality is so remarkably complicated and heterogeneous that it so far has successfully eluded our best efforts to "see" it at all straight on. As our scientific tools get more sophisticated, we get ever clearer windows into that reality, but also discover evermore complexity that frustrates simple causal modeling. But that complexity doesn't mean that brain/behavior relationships are "unreal" or inherently impossible to explain. And certainly there is an "out there" that needs explaining—I am just not sure that our current constructs are all that close to explaining it.

In fact, as we plod along with gradual scientific progress in the coming years, decades, and centuries, it is conceptually possible the my second umpire will gradually merge into the first and become able to call this complicated game just as it truly is. In other words, I think that the relationship between the brain and psychopathology, however complex, resides in the solid world of everyday Newtonian reality, not the goofy, solipsistic "(un)realities" of quantum physics which place inherent limits on our clumsy human capacity to measure and to know.

2) Dr Kinghorn's second critique is so telling I will quote its central portions again to provide the emphasis it deserves: "But his insightful account

begs the important question: who should decide? Who should decide what "mistakes" and "problems" are, or what "mental disorder" is, or what constitutes "harm," or what would render DSM "useful?" Should patients decide? Should individual psychiatrists decide? Should the Task Force decide? And on what grounds? And how would we know if the judgments of any of these potential "deciders" were shaped, subtly and unconsciously, by particular forces such as pharmaceutical marketing, consumer-driven ideals of beauty and success, gender stereotyping, and so on?"

The essential problem of utilitarian pragmatism is that it often lives case by case, without clear external value guidelines of the good or even the best methodologies for establishing what those guidelines should be. Suppose a drug for schizophrenia improves life, but in the process shortens it—who decides how the utilities should play out? In deciding whether to add a new diagnosis for "psychosis risk syndrome," one pragmatist may worry more about the lost benefit for false negatives of not having the diagnosis; another (I think wiser) pragmatist about the treatment burden on false positives if it is included. The Benthamite utilitarians tried to solve this conundrum with "the greatest good for the greatest number" and developing metrics for "good" is now part of behavioral economics. But as Kinghorn puts it, the basic question is often begged—who decides the values, goals, and methods of utilitarian pragmatism and how?

Back to Dr Kinghorn's telling words: "It is difficult to see how Frances' pragmatism can ensure that diagnostic revisions will 'do no harm' if 'harm' is itself a contested category. If, as Frances argues, efforts to establish a consensual and non-tautological account of 'mental disorder' are likely to fail, there would seem to be no way around these questions. Psychiatric diagnostic classification, that is, must be understood as a pragmatic and tradition-constituted enterprise in which individuals and groups with particular interests interpret research data (itself compiled

and reported by individuals with particular interests) in such a way as to shape the use of psychiatry and psychiatric technology in accord with these interests. This recognition should, at the very least, provoke humility and non-defensive soul-searching among those tasked with revising the DSM, since biases and moral failures in the 'deciders' would very likely become manifest in their nosological decisions, and the ongoing cultural acceptance and use of the DSM hinges on the ongoing public credibility of these 'deciders.'"

Right on. But to where? The DSM's have come to assume enormous (probably too much) influence in widely diverse decisions that impact greatly on public health, the distribution of scarce mental health and school resources, and even the protection of constitutional rights. The scope and strength of influence of DSM has grown far beyond what anyone could have envisioned thirty years ago. The American Psychiatric Association has sponsored the DSMs for sixty years, taking on the task originally because no one else wanted to be bothered with anything so insignificant. It seems clear now that the importance and scope of the psychiatric diagnosis has outgrown its being comfortably nested within a single professional organization. The sorting of different values and weightings in making tough pragmatic choices require much wider consensus.

If not the American Psychiatric Association, then who should be responsible for future revisions in the diagnostic system? There is no clear right answer. My best (but far from perfect) choice would be the National Institute Of Mental Health. NIMH would bring a far broader view to the task and be less burdened by publishing concerns. But NIMH also has limitations. It would tend to be too research focused, less sensitive to practice concerns, and not necessarily representative of larger public policy and forensic issues. So my choice would be NIMH supervision of a very inclusive and transparent process.

None of this really answers Dr Kinghorn's fundamental point. Nor can it be answered. Given the current state of psychiatric knowledge, there are rarely clearly right choices based on a

cut and dried science base, and the proper course of pragmatism is often in the eye of the beholder. The safe play is to be aware of risks and potential blind spots and to build in a lot of checks and balances. In an uncertain world, your worst critics are often ultimately your best friends.

Doing No Harm Redux: The Case For (Ultra) Conservatism?

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Discussions and depictions of psychiatry in the news media rarely fail to make me cringe. One scenario guaranteed to produce that result is reference to the DSM as “the Bible of psychiatry.” As is so often the case in circumstances in which one feels embarrassment, though, perhaps that characterization is uncomfortably close to the truth. Many of the elements are there: a sizeable minority of its adherents take its words literally and believe it to demarcate entities that exist independently of their descriptions, while others see it as a heuristic device that prompts productive thought and action, but is more of an approximation that necessitates interpretation and nuanced application. To the extent that the main contestants in the most recent DSM wars seem to take much of the prevailing framework of psychiatric diagnosis as a given, they may be viewed roughly as the modern analogues of the monophysite and Chalcedonian factions of early Christianity. What I hope to add to the mix is (in this extended metaphor) the atheist position: that the DSM system – a development of undeniably immense historical significance – can no longer be considered the best we can do to describe and assist in understanding the world of psychopathology, that the prospects of it being considered more favorably in the future are even bleaker, and that continued employment of it in mildly altered forms can

only impede much needed progress in all of the areas of psychiatric thought and practice on which it has profound influence.

In this follow-up commentary I will summarize very briefly the specific changes to diagnostic practices David Curley and I (2010) recommended in our previous piece, and then address what I assume to be Allen Frances’ (2010) objection to our approach. Since our initial commentary presents the opinion (in Dr. Frances’ words) “that the existing system is so bad that even the aggressively innovative DSM-V is suggesting far too little change, not too much,” it seems very likely that his diagnosis that this is “a naïve Cartesian rationalist view that neglects the deep roots and far flung branches of the diagnostic system” applies to us. That conclusion is so far wide of the mark that we must take responsibility for having failed to make our position sufficiently clear. I am, therefore, grateful for this opportunity to correct that shortcoming.

In our previous commentary in these pages, Dr. Curley and I identified several specific features of the DSM system that we believe must be eliminated: 1) multi-axial diagnosis as currently conceived, whose unmistakable but spurious implication of deep distinctions between “mental” and “general medical” conditions, and between “clinical” and “personality” disorders, is misleading and destructive; 2) definitions of some of the somatoform disorders that invoke the vague, non-specific, and misleading constructs of “psychological factors” and failure to be “fully explained by a general medical condition”; 3) adjustment disorders, which misleadingly imply that adversity in the social environment is less etiologically relevant to other, more serious forms of psychopathology; and 4) disorders “due to a general medical condition,” a construct that both unnecessarily deviates from the primary/secondary distinction that has served the rest of medicine well, and implies falsely that the etiopathogenesis of psychiatric syndromes fall into two distinct categories – “medical” and otherwise.

Beyond those particular sugges-

tions for the cutting-room floor (some of which are apparently shared by the editors of DSM-5), our general critique of the DSM project and the error of its neo-Kraepelinian underpinnings led us to recommend a radical overhaul of the diagnostic system. Given our emphasis on empiric findings that reveal myriad problems with DSM-defined phenotypes, Dr. Frances (and likely other readers as well) may be forgiven for his mistaken impression that our approach to nosology is an essentialist/naïve realist one. But one need not be an essentialist to recognize that some taxonomies accomplish the goals they should be expected to achieve better than others, and Dr. Frances’ response to our original commentaries in the *Bulletin* compellingly catalogues the DSM’s many shortcomings and disappointments. He points out, for examples, “Descriptive classification in psychiatry has so far been singularly unsuccessful in promoting a breakthrough discovery of the causes of mental disorder” and “It is not even clear that the DSM categorical approach is the best research tool.” Ironically, he parlays these accurate (and, in the latter case, understated) observations into a case for conservatism, and it is there that we part ways. I believe that the disconnection between his realistically austere view of what the DSM can accomplish for us and his strong belief that we should nevertheless stick with it is traceable to an error in what he sees as (at least my) philosophical motivation for scrapping it, and a limitation on what he imagines are its alternatives.

Dr. Frances is far from alone in his assimilation of the DSM enterprise to the expression and construct “biological psychiatry.” Although the historical ties between the two are undeniable, they are also long-since expired, and with them the misguided expectations that DSM-defined clinical entities might turn out to be caused by small numbers of genes that act autonomously from the social environment, and treated effectively with drugs that are specific to them. That sort of antiquated “biological reductionism” has thankfully given way to a more mature – and, frankly, more genuinely “biological” – framework in which phenotypes are recognized as results of

the actions and interactions of multiple genes and environments. Most relevant to psychiatric phenotypes are social environments, which exert their influences via the mechanisms of epigenetics, understandings of which are progressing rapidly. My complaint that the framework of the DSM does not comport with this emerging conceptualization of psychopathology, and that that has become a fatal flaw, is seen by Dr. Frances as a naively realist objection which is therefore not compelling. As I hint at above, however, recognition of the necessity of classification schemes both to reflect and to support scientific advance simply does not depend on the view that diagnostic categories are what Dr. Frances calls “real entities.” Our increasingly sophisticated conception of the etiopathogenesis of psychopathology, involving as it does multiple genes interacting over time with multiple environments, renders unsurprising the ubiquitous observation in clinical psychiatry that the syndromes with which patients present and those defined in the DSM often resemble one another only weakly.

From a practical standpoint, the domain of psychiatric endeavor for which changes to the diagnostic system are most urgently needed is the one in which I spend most of my professional life – that of education and training. It is widely recognized that experienced clinicians use the DSM only to the limited extent that they find it useful in any given circumstance, and clinical investigators at least sometimes have the ability to employ other taxonomies that suit their scientific purposes better. But the felt necessity of students and residents to learn the application of DSM diagnostic rules not only crowds out opportunities to learn actual science; it is creating generations of physicians who adopt either what Dr. Frances would recognize as naïve realist/essentialist misunderstandings, or cynical conclusions that psychiatric diagnosis is a mere administrative chore, akin to procedure coding, that is divorced from actual clinical medicine.

If we reject Dr. Frances’ claim that, limited and problematic as it currently is, amending our diagnostic system will bring more harm than good, how can psychiatric nosology be recon-

ceived to help us accomplish our clinical, educational, and scientific goals? I agree with him that forging ahead with new categories, or fiddling with the definitions of old ones, are not the answers. The approach I am recommending can be characterized as both a highly provocative and an ultra-conservative one that acknowledges explicitly that the DSM – despite the best of intentions and methods of the time – took the discipline down the wrong road, and that continuing on that road or one of its branches will only deepen the trouble we are in and make it ever less likely that we will get where we need to be. We must, therefore, turn around, return to a fork passed long ago, and consider our options from there. The steps I recommend contemplating include:

1. Identification for possible retention of that handful of syndromes that have performed well over time (in some instances, such as mania and a few others, millennia). That list of entities may overlap significantly with that for which the diagnostic criteria of Feighner and colleagues (1972), the forerunner of the DSM-III, were formulated.

2. Provisional elimination of the majority of the remaining DSM categories, which neither capture nor help us understand or investigate adequately the phenotypes, etiopathogeneses, prognoses, or treatment responses of the patients ostensibly described by them.

3. Study and implementation of methods of documentation of clinical findings that are reliable and systematic and thus lend themselves to standardization and to teaching and research on psychopathology.

4. Organization and funding of investigations that link clinical presentations, natural histories, potentially relevant alleles and epigenetic markers, and treatment responses that can inform proposals for bottom-up formulation of diagnostic constructs – categorical, dimensional, or both, as indicated. A similar hope was expressed in the Introduction to *A Research Agenda for DSM-V* (2002). Though more modest in scope, a highly successful model for a bottom-

up approach to the taxonomy of emotional and behavioral problems, the Achenbach (2006) System of Empirically Based Assessment, already exists.

5. Gradual adoption of official (and amendable) diagnostic nomenclature as data are judged to warrant it, without the need or expectation of encompassing soon – or maybe ever – every conceivable instance and permutation of human distress or dysfunction.

The set of recommendations briefly outlined above obviously requires far more elaboration to be considered an alternative proposal. It is instead offered only to illustrate that a radical change to our nosological approach need not – indeed, should not – lead scientific, clinical, and educational advance, but instead must both reflect and facilitate it. If the DSM system were capable of fulfilling that fundamental criterion, I would advocate its retention. I understand that my suggestions may be seen as outlandish, rhetorical, irresponsible, or simply impossible – and thus unserious. I hope that is not the case, and that psychiatry strongly considers leaving the Bible business to others.

Perhaps Dr. Frances and I agree more than either of us realized. Perhaps “doing no harm” *is* best served by conservatism – in this case, by just getting out of the way.

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Ultraconservatives Are Radicals in Sheep's Clothing

Allen Frances, M.D.

I too am an ultraconservative in my personal preference for a smaller and tighter diagnostic system. But, unlike Dr Waterman, I am a conservative in wanting to avoid radical changes and instability in the classification. (Incidentally, being ultraconservative and conservative in any way goes against my usual grain—I am a hopelessly bleeding heart liberal in most other things).

Dr Waterman and I do agree about the limitations of the current system. I am as skeptical as anyone can possibly be about DSM-IV because I know its shortcomings so up close. If I were starting from scratch, I too would insist on a higher standard for diagnosis that would eliminate some of the existing categories and raise the thresholds for many others. I felt, as I was watching it happen, that DSM-III and DSM-III-R were both puffed up with questionable new diagnoses and low thresholds. The high rates of DSM-IV diagnosis reported in community samples suggest that the system is very overinclusive (although the epidemiological study methods are also often questionable).

But Dr Waterman and I disagree in a fundamental way about whether it is desirable to radically change an admittedly flawed system. The question I faced, on assuming responsibility for DSM-IV, was whether to undo what I didn't like in what had been done previously to suit my own preferences or whether it was wiser to settle for the less ambitious goal of reducing additional puffery (by introducing high thresholds for change in DSM-IV). The

latter seemed to me then, and still does now, the smarter and safer choice. Ultraconservatives are always radical, never conservative. Whenever an ultraconservative acts upon his ideas, the actions are inevitably risky and likely to do much more harm than good. I prefer the skeptical Edmund Burke conservative approach that eyes suspiciously any grand revolutionary designs—whether to lurch progressively forward or fundamentally backward.

So I resisted any impulse to remake the diagnostic system in my own image. I believed that the system should not oscillate wildly based on the whims of any one person, who happens to be in charge at that given moment. It is very hard to know which approach is best when none (including my own pets) seems particularly more proven or promising than any other. When so little is well established, there is no reason to feel confident even on ones own best judgment.

The bottom line is that I distrust all fancy ideas on how to improve our admittedly flawed and possibly overblown system, even my own. There has to be a very good reason and strong evidence to make every change. Radical changes to the system shouldn't come from armchair reasoning and personal whim - they should follow only from compelling scientific evidence, even if this may be a long time coming. My own personal experience in the different roles has been that it is very much easier to criticize the system from the sideline than to actually find compelling ways to make it better when one is actually in the game.

Final Comment

G. Scott Waterman, M.D.

I thank Dr. Frances for his response to my most recent commentary, and I appreciate the opportunity to rebut it. This reply, however, is only partly a rebuttal, since he and I apparently agree on so much. It is largely the conclusions we draw from

our shared premises that differ, though they differ dramatically.

Dr. Frances acknowledges – as he has throughout this debate – that the DSM is “flawed” with “limitations” and “shortcomings,” and that the standards of inclusion of diagnostic categories have not been high, leading to our current “puffed up” system. That is not an auspicious starting point for a call to conservative inaction, but Dr. Frances makes the best of it. He is more fearful of the effects of “instability” than he is of the adverse consequences of retaining a diagnostic system that has failed to serve any of its purposes or constituencies well, and whose retarding influence on progress – to whatever extent the field will continue to treat it seriously – will only grow with time. He puts great stock in the fact that the current DSM system dates back 30 years, a period of time that (though a mere blink of an eye in historical perspective) he sees as having committed us to this unfortunate course for the indefinite future.

Dr. Frances and I agree on other premises as well: that “grand revolutionary designs” are to be viewed suspiciously, and that taxonomies should not be the products of “armchair reasoning and personal whim” but should instead “follow only from compelling scientific evidence, even if this may be a long time coming.” Exactly! Those principles should guide both the decision to scrap the current DSM system (which ran afoul of them) and the means of formulating its successor. My proposal explicitly urges a process that brings to bear empiric investigations on a range of parameters (clinical presentations, natural histories, potentially relevant alleles and epigenetic markers, and treatment responses) in the reformulation of psychiatric nosology. It also recommends that official diagnostic nomenclature be adopted gradually as evidence warrants it, without the expectation that all conceivable clinical problems will be codified soon, or perhaps ever.

Dr. Frances’ final point – that it is easy to “criticize the system from the sideline” – is no less valid for being *ad hominem*. Unfortunately, though, the ease with which dissatisfaction with the DSM has arisen relates far more to its attributes than to those of its numerous

critics. For what it is worth, I too confess my life-long leftward leanings, making my ultra-conservatism (if that accurately describes my position) on this topic uncharacteristic. Persistence may be a virtue in many contexts, but not in the face of evidence that the *status quo* is an unacceptably poor alternative.

About DSM in Philosophyland

André E. Haynal M.D.

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Allen Frances' all encompassing "DSM in Philosophyland..." touches fundamental problems that make us think. As this reader is leaning toward the position of the third Umpire, which has for long been defended by Thomas Szasz, it may be that a pathological mistrust concerning classifications plays a role in the leaning of this reader. A memory emerges from ancient Greek, where for Aristotle, e.g., the verb categorize meant not only to put into categories, but also to accuse. It seems that today's common words stigma and stigmatization have a much nobler origin, which is the description of people who wore the wounds (stigmata) of the Christ. In this respect, the DSM may have grown on dubious philosophical and theological bedrocks in an emotionally loaded context...

I personally don't think that DSM-III was a victory of biological psychiatry as it is stated by Allen Frances. What might be biological in it? Only the "myth" as Szasz called it. Proofs at that time: zero. *Fides quaerens intellectum*—a belief looking for understanding. I think it was historically simply a revolt against the dominance of psychoanalytic-Freudian thinking in the field at a time when Freud's influence in psychiatry was anyhow declining. It was also from the beginning on clearly stated that the work should be a-theoretical (i. e. stripped of the psychoanalytical ballast and bias).

What Allen Frances states about

the importance of classification in the history of sciences in general (Kepler-Newton, Linnaeus-Darwin, etc.) is highly convincing. Even if in psychiatry the efforts in the domain of categorization were not followed by spectacular breakthroughs, there were without doubt some fundamental definitions, such as the distinction between neuroses and psychoses and the delineation of the syndrome of schizophrenia which may prove the contrary. As in the Bible: your words should be yes-yes, no-no, what you add comes from the Devil. Big categories: yes ; minutiae: no.

For the protection of a weak population of suffering and handicapped people, it remains nevertheless important to maintain these social constructs and not to deny their usefulness, e.g., in the insurance system and perhaps also permitting to scrutinize more or less homogenous groups in research work. But their potential, especially for exclusion, should neither be denied.

Reading this text is a great pleasure and stimulation and Allen Frances' utilitarian-pragmatic point of view can be very well accepted, even by people who, themselves, suffered from political, ethnic, professional (!) or other categorizations.

The Power to Name is the Power to Harm

Allen Frances, M.D.

We agree completely.

1) Professor Haynal's classical references remind us that the power to name is not always medicinal and benign. Naming can also be misused or misunderstood to accuse or stigmatize. The egregious misuse of the concept "paraphillic rape" in legal settings to allow for the inappropriate (and often lifetime) involuntary psychiatric commitment of sexual offenders is by far the most conspicuous and shameful current example. But the unintended potential risk of stigma is fairly ubiquitous and must be calculated into the risk/benefit analysis for each new proposal.

2) I don't mean to imply that DSM-III represented some kind of victory for biological psychiatry. It was more an iconic marker of the already well advanced changing of the guard from psychodynamic to biological models and thought leaders. Although DSM-III was theoretically atheoretical, it lent itself best to, and was a culmination of, biologically oriented research methods. The provision for each diagnosis of a definitional criteria set (the major innovation of DSM-III) was a direct application of criteria based methods that were just previously being developed for (mostly) biologic research. DSM-III also helped bring American psychiatry back into the mainstream of medicine, and thus toward an emphasis on medication, not psychological, treatment.

3) The lag between description and explanation is certainly not unique to psychiatry and also applies at two of psychiatry's boundaries—e.g., most of medicine is still empiric, and the social sciences all do a lot better at describing than at explaining. Explanations are easier to come by in the somewhat simpler worlds of astronomy, physics, and chemistry than in medicine, psychiatry, or economics. The really interesting next question is whether the gap between description and explanation will be crossed soon with our powerful scientific tools or whether explaining mental disorders will be as tough as explaining the complexities of fluid dynamics (which is what makes weather forecasting so problematic). I am betting that the complexity will give the science a very long run for its money.

4) I am indebted to Professor Haynal for standing behind the third umpire. Well informed philosophical skepticism is a useful and rare commodity these days.

DSM-IV, Hippocrates, and Pragmatism: What Might Have Been

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The main conceptual critique that Allen Frances, head of DSM-IV, appears to be making about the DSM5 process is as follows: "It fails to address the most important questions concerning the impact of proposed DSM-5 changes on prevalence rates and on false-positive diagnoses." (Psychiatric Times, June 2010) He calls for external committees, beyond those on each subgroup of the task force, to review proposed criteria for these practical purposes. In our previous discussion, he becomes explicit about his two apparent primary conceptual assumptions, underlying the above critique: 1. First and foremost, that we should be guided by the principle "Do no harm." 2. That "pragmatic" judgments should be the overriding principle in the final nosological definitions for each DSM revision.

Here I will critique those two assumptions.

"First do no harm"

This is obviously derived from the famous Hippocratic teaching. (The full original quote was in the maxim of Epidemics I: "As to diseases, make a habit of two things – to help, or at least to do no harm.") It may be relevant to understand what it meant in that Hippocratic tradition.

The Hippocratic tradition is often mistakenly identified simply with a conservative approach to treatment. While partly true, this popular simplification fails to capture the deeper genius of Hippocratic thinking, for its ethical maxims were not abstract opinions but rather grew out of its theory of disease (1, 2).

The *basic* Hippocratic belief is that Nature is the source of healing, and the job of the physician is to aid nature in the healing process. A non-Hippocratic view is that Nature is the source of disease, and that the physician (and surgeon) needs to fight Nature to effect

cure. Even in ancient Greece, physicians had many potions and pills to cure ailments; Hippocrates resisted that interventionist medicine, and his treatment recommendations often involved diet, exercise, and wine – all designed to strengthen natural forces in recovery. If Nature will cure, then the job of the physician is to hasten Nature's work carefully, and at all costs to avoid adding to the burden of illness.

Based on this philosophy of disease, the Hippocratics divided diseases into three types: *curable*, *incurable*, and *self-limiting*. *Curable* diseases require intervention, aimed at aiding the natural healing process. *Incurable* diseases generally were best left untreated, since treatments did not improve illness and, due to side effects, would only add to suffering. *Self-limiting* diseases also did not require treatment, since they improved spontaneously; by the time any benefits of treatment would occur, the illness would resolve by itself, again leaving only an unnecessary side effect burden. The concept of *Primum non nocere*, thus, meant *knowing when to treat and when not to treat, based on what kind of disease one diagnosed*.

It is exactly this primacy of disease, this special appreciation for the scientific importance of understanding disease, that cannot meet due by the second claim above: the primacy of pragmatism.

"Pragmatism"

Pragmatism in its postmodernist flavor has become, in fact, the unconscious philosophy of the average 21st century American (and indeed the average Westerner). To appreciate what this means, I will describe what pragmatism originally was, what it has become, and how it has become a philosophy of mental illness that undergirds DSM-IV, and destroys chances for a beneficial evolution in future DSMs.

As a philosophy, pragmatism in the hands of its late 19th century originator, Charles Sanders Peirce, meant that the truth of a concept could be found in its results. If we have a hypothesis in science, for instance, we

do an experiment, and based on the results of that experiment, we judge the truth of that hypothesis. In this way, Peirce's pragmatism is simply equivalent to science; it has nothing to do with "being practical." Peirce knew that science involves hypothesis and that the process of science is not simple. The results of one experiment can differ from another. Seeing pragmatism as the same as science entailed seeing the limits of science. But Peirce did not draw postmodernist conclusions. His solution was to think of science, as a whole, as a process of many experiments and many experimenters, constantly subjecting the truth to the scrutiny of research (he calls this process "inquiry"). Over time, the truth would display itself through this process of inquiry. Then, the consensus of investigators would be the same as the actual truth.

So pragmatism, in its inception, not only valued science, but pragmatism was identified with science, properly conceived. Pragmatism is not a means of devaluing and ignoring science.

After Peirce, William James took a few steps toward postmodernism by famously identifying truth with the "cash-value" of an idea. This would seem to identify pragmatism with utilitarianism – the truth of a concept is what is useful. This step moves beyond Peirce and loses the connection with a real truth that would justify our beliefs. John Dewey took a few more steps than James. By equating truth with "justified belief", rather than reality, Dewey further distanced pragmatism from truth. Where Peirce's inquirers gradually moved closer and closer to the truth, Dewey's justified believers could never know where the truth really was. They could only justify what they believed at the time as best as they could. A half century later, when French postmodernism began to suffuse itself in Western culture, Richard Rorty and others took a few more leaps and jumps from Dewey, landing fully in the arms of Foucault: there should be no talk of truth, there is no objective truth, only conversations between you and me, only agreements and arguments that reflect power, not truth.

Those among our colleagues who

claim never to read philosophy suffer from unconsciously imbibing their philosophies from the larger culture. What Frances means by “pragmatism” seems to be a postmodern Rortyeian neo-pragmatism, one which denigrates science as the power-plays of experts – a view that only cares about what is practically useful – truth be damned. This is a long way from Peirce’s original attempt to explain science. This degeneration of pragmatism is reflected in the standard English usage of the word as equivalent to being practical, not engaging in theorizing, focusing on what happens in the real world, and the actual consequences of acts or decisions.

Postmodernism

It is but a short step from this simplistic neo-pragmatism to a dangerous postmodernism. All definitions become arbitrary, not in a superficial or transient way, but profoundly. All diagnoses represent cultural and professional consensus. Now, homosexuality is a mental illness; now, it is not. Neither view is right or wrong. There is no right or wrong, only our cultural preferences. If I want to define depression, I should do so based on awareness of the cultural powers that be, not just first and foremost, but solely and completely. What does society want? What are the relevant social forces (pharmaceutical and insurance companies, the professions, advocates)? What produces the most useful results? (Postmodernists do not stop much to justify their claims to utility, because their own philosophy undercuts them. Since there is no truth, what is useful is a merely a relative matter of cultural preference. Utility, as everything, is ultimately arbitrary).

Without truth, the process of arbitrariness degenerates into anarchy – a pure eclecticism of thoughts and afterthoughts, actions and reactions, consensus and debate, without end. Diagnosis is chaos, and some conclude that we should give up on diagnosing altogether. Either way – with arbitrary eclectic definitions or with the refusal to define – postmodernist approaches to mental illness are a dead end for psychiatric nosology.

Bipolar disorder

One sees this postmodernist ideology play out in some of the specific critiques made by Frances. For instance, he strongly opposes the notion that the cut-off definition for hypomania should be reduced from four days to two days, even though the original definition of four days was based on zero scientific evidence, and there is reasonable evidence to make a change to a shorter number of days. For instance, Jules Angst published a review of this literature, including his half a century of research on this topic which also includes a 40 year outcome study from Zurich, a psychiatric equivalent of the Framingham heart study. His research was an important basis for the whole distinction in DSM-III in 1980 between MDD and bipolar disorder, so Angst can hardly be criticized as someone who opposes the diagnosis of MDD. Yet in his review of a lifetime’s work in the *British Journal of Psychiatry*, he provides evidence for a shortened duration of hypomania (3). I would like to see one citation that refutes Dr. Angst’s data and supports a four day cutoff as more valid for hypomania than shorter definitions. If, as I believe is the case, such data do not exist, one might hear instead the debating point that this is just the power-play of experts; science is never definitive; there is always uncertainty; the literature can be selectively rendered, and so on. All true, but this reduction of scientific knowledge to nothing but mere opinion, no different than any other kind of opinion, is the hallmark of antiscientific postmodernism.

In his blog, Frances writes: “The ratio of bipolar diagnoses at least doubled since the introduction of Bipolar II in DSM IV and the extraordinary drug marketing campaign promoting antipsychotics and mood stabilizers. This has undoubtedly helped some people and harmed some others- the exact extent of each is unknown and perhaps unknowable. But my bet is that this is a fad that has overshot- they always do. I would assume that anyone now presenting

with anything suggesting equivocal bipolar disorder is much more likely to be overdiagnosed and over-treated than to be missed.”

Actually this matter has been studied. The last statement has been proven false. And it comes from a researcher, Mark Zimmerman, who is quite a skeptic about bipolar disorder and is critical of its overdiagnosis (and thus cannot be accused of simply having the power-motivation and bias of the “experts”). In that very recent study (long after all the purported marketing of bipolar disorder) (4), whose data I reanalyzed in the *British Medical Journal* (5), 30% (27/90) of patients with unequivocal Structured Clinical Interview for DSM-IV (SCID) diagnosed bipolar disorder *had never been previously diagnosed* with bipolar disorder by clinicians in the community.

These data directly contradict the last quoted sentence stated above. All the pragmatism in the world is unhelpful if it is based on inaccurate opinion.

To continue with that study: Evidence for overdiagnosis of bipolar disorder indicated a lower frequency than underdiagnosis. Bipolar disorder had been mistakenly diagnosed in the community in 13% (82/610) of people in whom the gold standard Structured Clinical Interview for DSM-IV (SCID) determined that they did not have bipolar disorder. This is why we can conclude that, as a matter of relative risks, bipolar disorder is more than twice more frequently underdiagnosed in those who have it than overdiagnosed in those who do not have it (30% > 13%). The absolute frequency of bipolar disorder is low though, so, ignoring the denominator, more people were misdiagnosed who did not have it, than those who had it. Yet this still does not entail generalized “overdiagnosis” if by that phrase we mean that almost all people who have the diagnosis are diagnosed with it, and many who do not have the diagnosis are diagnosed with it. This is not the case with bipolar disorder.

Obviously, science does not entail absolute knowledge, but this should not lead to the postmodernist conclusion that our science is always so limited that it is near useless in informing our judgments.

What could have been

One need not spoil for a fight between “biological reductionism” and whatever one wishes to label this apparent mish-mash of neo-pragmatism, postmodernism, and pure opinion. These are false options. There is such a thing as medical humanism: one can be reductionistic when it is correct to be so, with many diseases of the body, and non-reductionistic when it is correct to be so, with problems of living that do not represent disease. And even when one faces diseases of the body, one still always needs to be humanistic because we are always faced with human beings, who may or may not have diseases. It is a poor doctor who fails to identify a disease, and a poor profession that fails to care about disease; it is the ultimate anti-humanism to fail, as a doctor or profession, to identify and cure those diseases which can be identified and cured. And yet, there is still more to medicine, the human connection, the personal relationship, which is especially important when disease can be ruled out, and life’s vexing problems ruled in. This medical humanist model has long existed, dating back to Hippocrates, indeed, those physicians who cared deeply to know about disease – reductionistically and biologically – and also cared deeply about those persons, as persons, who have disease (or not). In the modern era, this Hippocratic tradition was identified and developed most clearly by William Osler (6, 7).

My teacher the psychotherapist Leston Havens MD (8) used to say that contemporary psychiatry exists at the same level, scientifically and clinically, as general medicine in the late 19th century, i.e., Oslerian medicine. In Osler’s era, causes of most diseases were unknown; treatments were legion, empiric, and ineffective (though widely believed effective); and diagnoses were unsystematic. In 1892, Osler wrote the first edition of his textbook, a magnificent, careful, honest, faithful depiction of the signs and symptoms and course of most medical conditions. For the next 50 years, in 16 editions ending in 1948, Osler’s text, which outlived the man by three decades, was the central descriptive nosology – the DSM – of

internal medicine. In the 1920s randomization was invented; in the 1930s antibiotics were discovered; in the 1940s the first RCTs occurred for pneumonia; in the 1950s hormone treatments saved the lives of those with diabetes and Addison’s disease. Over time, the (reductionistic) science of modern medicine made its discoveries, and Osler’s honest nosology proved quite useful as a map of how to apply and advance those biological studies. The antibiotic revolution, for instance, was effective in part because Osler’s nosology of pneumonia was mostly correct.

Imagine William Osler, sitting at his desk for the third edition of his text circa 1900, and saying to himself: Well, these doctors are rather ill-educated; and the pharmaceutical houses will fool them (he wrote about this factor back then, by the way); let me tinker with the definition of pneumonia this way and that way, so as to discourage the use of these ineffective and harmful treatments of my day.

If Osler had approached medical nosology this way, justifiably based on the poor treatments of his day, his textbook would have been out-of-date within half a decade, rather than half a century, and future antibiotic studies would have been hampered if applied to the consciously false definitions of pneumonia that our counterfactual Osler would have devised in prior decades.

But Osler took the honest approach: he described diagnoses based on the best scientific and clinical knowledge of his time. He then spent much of his career trying to convince doctors to use less drugs, and to engage in more research, in the belief that in future years such research on those honestly described diagnoses would bear fruit: causes would become known, and effective treatments developed.

Time proved Osler right; medicine’s advances in the past century can hardly be gainsaid, especially by those many of us who would have succumbed to a mere infection of childhood a century ago.

But psychiatry has not experienced similar progress, despite major

growth in neuroscience, in the past two generations. Could it be that it is not a coincidence that this stagnation in clinical progress coincides with DSM-III and DSM-IV. Instead of the Oslerian model, the leaders of psychiatric nosology apparently have focused on avoiding immediate pragmatic risks, in their estimation. It is, perhaps, not surprising that their gerrymandering of psychiatric diagnoses for contemporary pragmatic purposes does not correlate with biological research or robust treatment benefits.

Many blame the drugs, or disbelieve in biology; but it could be that we have the biological tools, and even the drugs, we need, but our “pragmatic” diagnoses blur our vision of the right connections to be made. Osler’s uncompromising scientific realism, combined with a therapeutic conservatism, hit the perfect balance for his own age, and proved successful in the future. His diagnostic realism (not pragmatism) produced spectacular practical results. In contrast, in the past two decades, few practical successes have followed from a pragmatic DSM-IV. This “pragmatism” has proven useless in practice.

Conclusion

I finished residency a generation ago, as DSM-IV was just published. My closest friend, James Hegarty, was an energetic and optimistic young man with a strong interest in research; he conducted a classic study showing that outcomes of schizophrenia were dependent on its diagnostic definitions over a century of changing nosologies (9). Jim, and I, and many others, hoped to contribute to knowledge in psychiatry, naively, perhaps, believing that this attitude was useful, and would help patients. For a generation, we and others conducted our research under the aegis of DSM-IV definitions, looking at treatments, genetics, biology. We have had some small successes, and many failures. Jim passed away recently in his forties of a terminal medical illness. His time was especially short. Now a new generation is entering our field, as DSM-5 is about to come out, and other young persons with great energy and compassion will try to advance our knowledge, and per-

haps help some patients. The current leadership in our field, and the past leadership, has an important responsibility to help, and not hinder, this process.

Even under the best circumstances, the work of research is difficult, success infrequent, progress slow, each human life inadequate. Hippocrates famously remarked that life is short, art is long. It took Oslerian medicine a century to make real advances for its major illnesses. But if our nosology is consciously gerrymandered so as to make scientific progress well-nigh impossible, we are sacrificing entire generations to wasted activity, and other generations of persons with mental illness will continue to suffer as our professional ineptitude persists.

Nosology is not just about research, I know; it has many uses: there are the lawyers and the capitalists and so on. But, at some level, we should care to know the truth, I would think. In fact, we should give primacy to getting at the truth - unless we do not believe in truth of any kind, unless we view science as just power, and its knowledge as mere opinion.

I think of how many of my colleagues in the last 20 years - an entire generation, some of whom are already gone - had hoped to build on what we were given, including DSM-IV, expecting that our leaders would care, first and foremost, not about pragmatism, not about their opinion of what treatments were harmful, not about their social and cultural opinions, but about the truth.

Let's have the right priorities, trusting that in psychiatry as in medicine, seeking out the truth will prove to be the most pragmatic course of action as well, and remembering that, millennia ago, our Greek teachers knew this was the way, and the only way, to achieve a practice based on First Do No Harm, and that, in the history of medicine, only this approach has succeeded in moving us towards that Hippocratic goal: to cure sometimes, to heal often, and to console always.

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"Do No Harm" Meets "The Rule Of Thirds"

Allen Frances, M.D.

Dr Ghaemi is a first umpire who makes the wrong calls on Hippocrates; on Mark Zimmerman; on the strength of the science supporting nosological decisions and the ease of its interpretation; and on the relation of pragmatism to reality.

Getting Hippocrates Right

When their heads weren't trapped in the Platonic clouds, the Greeks were the most remarkable of scientific observers and intuitors (e.g. witness their development of atomic theory and their accurate measurement of the circumference of the

earth). Early on, the Hippocratic school made its own great discovery - the "rule of thirds." This was the crucial triage observation that about one third of patients can't be helped, about one third get better without help, and the remaining third is the appropriate focus for active and potentially harmful medical treatment. This rule of thirds was not the result of any theory of disease; it was a prognostic tool that came from pragmatic empirical observation.

The rule of thirds has turned out to be the most astoundingly robust finding ever to inform medicine- applicable across varying epochs, places, medical specialties, types of illness, medical theories, research knowledge, and methods of treatment. It works best for patients with moderately severe illness. When the illness is especially severe, chronic, or fatal, the nonresponding group expands beyond its allotted "third." When patients are mildly or acutely ill, the spontaneous recovery group expands. In psychiatry, the modal response rates over thousands of clinical trials is 60 -70% for active treatment and 30-40% for placebo- ie yet another confirmation of the rule of thirds. For the milder conditions in psychiatry, the placebo response, spontaneous remission rate approaches 50%-so that only a small proportion of patients actually benefit from the active treatment. For schizophrenia, the placebo response rate is below 10%.

Dr Ghaemi and I both admire the Hippocratic wisdom of "As to diseases, make a habit of two things - to help, or at least to do no harm." Dr Ghaemi goes on to state correctly that "Hippocrates resisted that interventionist medicine, and his treatment recommendations often involved diet, exercise, and wine - all designed to strengthen natural forces in recovery. If Nature will cure, then the job of the physician is to hasten Nature's work carefully, and at all costs to avoid adding to the burden of illness."

It is curious that Dr Ghaemi would introduce Hippocrates into our debate since Hippocrates was so clearly the first and best advocate for caution in diagnosis and treatment. I couldn't possibly ask for a better support for my position or a clearer contradiction of Dr Ghaemi's.

Let's go deeper into the implica-

tions of Hippocratic caution. The elusive trick that has so far eluded psychiatry (and the rest of medicine) is the ability to pick out which patients are likely to be in each of the three groups and to tailor specific interventions to their different needs. It is the great disappointment of scientific medicine (and psychiatry) that we are still so far from achieving this goal. With just a few exceptions, psychiatric and medical treatment remains an empirical, trial and error endeavor with many people treated who don't need it and many others receiving treatment that does them more harm than good—just as was observed so long ago by Hippocrates.

"First, do no harm" was a brilliant recognition and a suitably humble confession of the limitations of medical art and science. It applied then, it applies equally now. Certainly, we have better medications, but haven't solved the age old problem of how to use them with best effect and least risk.

The question then becomes how to deal with the interaction of "the rule of thirds" and "do no harm" when we have no way of predicting prospectively who is in which third? In their commentary, Drs Piasecky and Antonuccio provide numerous wise counsels that bear repetition here because they are so helpful and would warm the heart of a cautious Hippocrates.

Let's start with the case for conservatism in diagnosing and treating milder cases at the border of normality. For this group of likely spontaneous remitters, the best first course is almost always tincture of time, watchful waiting, education, encouragement, and support. Let the illness declare itself more clearly before starting what might be unnecessary and harmful treatment. Medications have very little advantage in efficacy over placebo when used in mild conditions (especially those of recent onset which have especially high rates of spontaneous remission). In these situations, medications are usually more likely to cause harmful side effects than extra clinical improvement. So let nature have first crack at healing and follow the treatment sequence of time and support

first, brief psychotherapy second, and medication reserved as a third line only for those who need it. And nosologically, be cautious by keeping subthreshold diagnoses in the appendix until they have proven themselves safe and useful.

Of course, this flies in the face of the recent early interventionist dogma. But suggestions for primary prevention in psychiatry have so far been based on theory, hope, and hype rather than scientific evidence. None of the five subthreshold conditions proposed for DSM- 5 has been well studied enough to be safe. All would lead to a frenzy of drug company marketing and the use of potentially harmful treatments for conditions highly likely to remit in the natural course of events. Hippocrates would certainly not be pleased. Early treatments for subthreshold conditions need to establish themselves with scientific evidence before becoming a risky public health experiment.

At the other end of the severity spectrum, we have inflicted a scientifically unsupported, often useless, and sometimes very harmful polypharmacy on patients in third group of poor responders. Polypharmacy is inherently impossible to study systematically in the practical world because of the sample sizes required by the large number of permutations of possible treatment combinations. It is an art more equivalent to alchemy or dress designing than to science. Skillful clinicians treating the more responsive patients of the third group are sometimes able to use polypharmacy for a custom tailored perfect fit with the patient showing a much better result than would otherwise be possible. Too often, however, polypharmacy is an act of clinician and/or patient desperation egged on by drug company marketing with little rationale and much potential for harm. Hippocrates knew that less is often more.

Pragmatics, Reality, and Science

Dr Ghaemi and I have differing views on the strength of the science supporting nosological changes. Reviews of the literatures of the various DSM disorders are consistent in the relative paucity of studies and their

limited generalizability (because they are usually conducted in university hospital settings with highly selected patients and evaluators). The "validators" of descriptive diagnosis are usually disappointingly inconsistent and uninformative. The findings are almost always equivocal and refuse to submit to a single, unimpeachable interpretation. Plausible arguments can always be made on both sides. There are no "real" balls and "real" strikes here—only a fuzzy picture that requires a "call them as you see them" humility.

As an illustration of this issue, I asked Dr Mark Zimmerman to comment on Dr Ghaemi's interpretation of his research on the over and underdiagnosis of Bipolar Disorder. I will quote his reply in its entirety because it sheds a crucial light on differences in epistemology between first umpires and second umpires.

Hi Allen.,

Ghaemi has the numbers right, but I do not agree with his interpretation. Which is more important, rates or persons? He indicates that the rate of underdiagnosis is higher than the rate of overdiagnosis. A rate calculation depends on the denominator. When I think of the over-under diagnosis issue I think of persons. How many individuals are overdiagnosed or underdiagnosed? We found 3 times as many individuals were overdiagnosed compared to underdiagnosed (82 vs. 27).

Of interest, the only other paper that has examined both under and overdiagnosis, which he did not cite, found that the overdiagnosis rate was higher than the underdiagnosis rate. In summarizing this study I recently wrote: Although other studies have reported overdiagnosis of bipolar disorder, we are aware of only one other study with data on both overdiagnosis and underdiagnosis. Hirschfeld and colleagues[16] interviewed 180 depressed primary care outpatients receiving antidepressant drugs with the structured clinical interview for DSM-IV. Forty-three patients reported a prior diagnosis of bipolar disorder, and this diagnosis was not con-

firmed in 14 (33%). The overdiagnosis rate of 33% was higher than the 22% underdiagnosis rate in the 137 patients who had not had bipolar disorder previously diagnosed.

Regarding the question of the 2 day cutoff...Like you I am very concerned that lowering the threshold will increase the frequency of false positives, and subsequent overtreatment. I am not aware of any treatment studies of patients who meet the lower (but not DSM-IV) threshold. Do I treat some individuals with 2 day episodes as having bipolar disorder? Absolutely, and I diagnose them with bipolar disorder NOS. The data may well bear out the validity of the 2 day threshold. The question, though, is how to balance the 2 potential harms—the false negative problem of undertreatment due to a clinician not using the bipolar disorder NOS diagnosis if the hypomanias only last 2 days versus the false positive problem that is likely to increase if the duration requirement is lowered.

Mark.

We don't have space here to recount all the persuasive reasons not to follow Dr Ghaemi in his idee fixe for a two day duration of hypomania. The point here is that Dr Ghaemi is a first umpire—he is very sure that there is a "real" right answer to the question and that he has it. Although a vanishing breed, first umpires serve a very useful function in the world as producers of scientific data. It often takes a true believer to slog away at the frustrating, and often disappointing, day to day labor of conducting the research studies. But first umpires are very often not trustworthy guides to the interpretation of the data—they often set out selectively to prove a point and miss the contradictory interpretations and larger issues.

Dr Ghaemi also seems to believe that DSM-IV is an obstacle on the royal road toward scientific progress in psychiatry. If there is a simple and straightforward "reality" of mental disorders and we are not finding it with all our powerful neuroscience tools, it must be because of the obscuring veil of the incorrect, excessively pragmatic

diagnostic system. Just get the diagnoses right and by golly we will figure out what causes them.

As we have discussed in many of the other responses, this naïve realism puts the cart before the horse. The obstacle to progress in understanding psychopathology is the complexity of the brain realities which inherently defy simple answers - not that we lack the right descriptions. There is nothing sacred about DSM-IV—it could be different and improved in thousands of ways. But none of these would provide anything remotely like the keys to the kingdom of deeper understanding. The causes of psychopathology are obscure to us because they are so complicated, not because we have not described it well enough.

Dr Ghaemi incorrectly assumes the peculiar notion that pragmatism represents a denial of the underlying realities. In fact, pragmatism is a humble, flawed and limited—but altogether necessary—place holder when there is no clearer path to truth or action. Pragmatists believe in the reality that reality is very hard to figure to figure out and that, in the meantime, we must muddle along as best we can trying to do the most possible good and least harm.

The debate is not academic. Many of the dangerous DSM-5 suggestions are defended with the Dr Ghaemi's claim that "this is where the science has taken us". But reviewing the studies always shows (as it does for Dr Ghaemi's prized two day hypomania) that the science is weak while the risks to patient welfare are great. Reducing the required duration of a hypomanic episode from four to two days will serve little useful purpose (any patient who really needs the diagnosis can be covered by Bipolar NOS). But shortening the duration will extend the Bipolar fad with consequent overuse of harmful antipsychotics and mood stabilizers. The pragmatic verdict is a no brainer.

Utilitarian pragmatics is certainly not the best method of developing a psychiatric nosology—but it is unfortunately the only option currently available to us. For a good discussion of this see the Kinghorn commentary which provides a devastatingly accurate critique of the limita-

tions of pragmatics in nosology. I confess to the obvious dangers and limitations in my response to him. . But when all is said and done, there is no other viable approach to pragmatism given our limited scientific understanding of the psychiatric disorders and the huge practical impact the diagnostic system has on people's lives and public policy.

If we had a stronger and less equivocal science base for making nosological decisions, we would use it. But the currently available results never reach out and grab you and (as the Ghaemi/Zimmerman exchange illustrates) are always subject to different interpretations. Whenever there is a DSM controversy, the science probably cancels out or there would not be a controversy. When the science does cancel out, the best guide (however fallible and difficult to operationalize) is the practical common sense of Hippocrates—Do No Harm.

Final Comment DSM-IV-ever

S. Nassir Ghaemi MD

Discussion with Allen Frances becomes, unavoidably, disputation – long, heated, and invariably (to paraphrase Ambrose Bierce) confirmatory of the errors of others.

He engages in polemics, not inquiry. Polemics are a game; one scores debating points, and rarely admits error; like a boxing match, one dodges and weaves, hoping to make it to the last round. In such debates, truth is annihilated, as Montaigne remarked. Inquiry seeks the truth; knowing that truth is corrected error, it readily admits error when present, and it seeks to find those aspects of truth that can be seen in most any argument. Frances seems to say that I am wrong in everything that I have said; I did not find him to admit that he was wrong in anything, even when clear facts refute specific claims (such as his claim that almost all those who have bipolar disorder are diagnosed with it).

Let's try to revive what is true, or agreed upon, in all that has been said. His claim, stated most powerfully, I

believe, and most simply, is this: When our science is weak, then pragmatic considerations about utilitarian outcomes are important in psychiatric diagnosis and treatment. I agree with this view as a necessary evil, i.e., when our science is weak. (Our disagreements entail from his postmodernist rejection of science in practice.) I agree with the Hippocratic approach of caution in treatment when our diagnoses are unclear or when our treatments are ineffective. (We disagree because he extends this caution to areas where our science of diagnosis is more clear.) I agree with conservatism regarding treating mild, borderline, or subthreshold conditions. (We disagree on whether this should mean that we should not allow them to be diagnosed.) If we are referring to definitions of sexual disorders or ADHD or MDD (which he always ignores and whose diagnostic realm he has expanded greatly), I would agree with these cautions. We disagree because he extends it to cases, like bipolar disorder, where his “pragmatism” means ignoring legitimate science in favor of his personal tastes.

The crux of the conceptual discord is this: I am a scientific realist; I value facts and truths, though I know we fallibilistically approximate them, and that truth is corrected error. Having not read philosophy texts since college in the last millennium, my colleague does not explicitly state any philosophy other than a claim to “pragmatism” which I philosophically critiqued previously. I argue that his unconscious philosophy is postmodernism, a preference for tastes over truth, his opinions over scientifically solid facts. My interlocutor’s view seems to be that pragmatism is that philosophy in which one ignores scientific facts by insisting that facts are, after all, open to dispute, while the common sense opinions of retired professors emeriti are self-evident. His method is consistent – the standard postmodernist obfuscation of facts as interpretations, setting everything up as the disagreements and ideologies of experts, with himself as ultimate arbiter.

Those appear to be the general differences; since he courageously identifies four errors in my commentary, here are specific responses to

those claims:

On Hippocrates

We both agree that the Hippocratic tradition is important, but a key error – fixable by actually reading the sources I previously cited – is that he thinks that the Hippocratic tradition supports caution *in diagnosis* and treatment. This is simply wrong as a historical fact. The Hippocratic tradition was aggressive in diagnosis, and cautious in treatment. It valued diagnosis highly, and viewed itself as different from the empirical, pragmatic approaches that preceded Hippocrates exactly in the value given to technical praxis (*teknae iatrike*), scientific knowledge, applied to understanding disease. Disease comes first and foremost, and before anything else: Diagnosis matters most. Then one cautiously treats those diseases which one understands and can treat, while not treating those which one does not understand well or cannot treat. The outcome: Doing less harm than good. Since Frances likes the outcome as a slogan, he really should study more carefully how it is achieved. Then he may realize that his pragmatic gerrymandering of psychiatric diagnoses produces the opposite result. Further, the “rule of thirds” is not an immutable clinical fact, but a matter for scientific research, as all else. Certainly in the past century, major advances have been made in reducing that third which is untreatable in medicine. Lewis Thomas’ *Youngest Science* demonstrates this therapeutic revolution clearly. No such progress is possible when couch-pragmatism consciously falsifies our best scientific knowledge to date.

On Zimmerman

(Previously, to respect word limits, I refrained from extensively describing study results. But since Frances extensively cites opinions from emails in place of facts, I provide the facts here at length).

Dr. Zimmerman states the “only” other study (besides his own, which proves underdiagnosis of bipolar disorder) supports overdiagnosis. But he

ignored three other studies, one which I published a decade ago (1), and two more recently (one by our group (2), and one by a German group (3)), all of which found bipolar underdiagnosis compared to MDD. For instance, in our study, the average patient with bipolar disorder saw 3.3 psychiatrists before getting the correct diagnosis, with a delay of about 9 years. In contrast, the average patient with MDD saw 1.5 psychiatrists before getting the correct diagnosis, with a delay of about 3 years. In the German study, case vignettes of DSM-IV mania and MDD were shown to 185 mental health professionals, and 62% of bipolar cases were misdiagnosed as MDD primarily, while only 5% of MDD cases were misdiagnosed as bipolar disorder. In our recent study of 64 children in community practice (again not Frances’ ivory tower), we found that 33% of children meeting DSM-IV criteria for mania were diagnosed previously with MDD (despite past manic episodes), while only 5% of children meeting DSM-IV criteria for MDD were previously diagnosed with bipolar disorder. Here again is the postmodernist method: create an artificial controversy based on pseudoscientific hand-waving, and then claim that since there is so much controversy, the science is too limited to use.

Also, Zimmerman’s claim that we can ignore the denominator, when judging over vs underdiagnosis, is simply wrong, ignoring the impact of prevalence. (NB: I actually stated Zimmerman’s argument in my commentary; his restatement in his email was superfluous.) Most diseases are low in prevalence; most of the population is healthy. Even the broadest definitions of bipolar disorder would lead to its diagnosis in no more than 5% of the population (versus 10-20% for MDD in its current definition). Imagine if 100 out of 1000 (10%) persons are misdiagnosed with disease X when they have disease Y, but 90 out of 100 (90%) persons are misdiagnosed with disease Y when they have disease X, would we still say that there is overdiagnosis of disease X? On that definition, all diagnoses are always overdiagnosed ($n=100 > n=90$).

On the science supporting nosology:

Regarding the “*idée fixe*” of 2 days for hypomania, Frances seems wedded to his overvalued ideation for the 4 day criterion he invented in 1994. (One might call it a delusion since it has no basis in even a single study, which he has not, and cannot, cite; neither can any of his favored experts). Zimmerman says he knows of no such studies, which unfortunately suggests the inability to type www.pubmed.com. Five minutes on Medline would have shown his error. (Type “hypomania” and “duration” and you will see 88 articles. Reading the abstracts takes about half an hour, less time than it takes to send emails and opine.) There are multiple studies that show that less than four days adequately picks out bipolar disorder from MDD based on the standard diagnostic validators. For instance, here is one abstract from the late Franco Benazzi, in an obscurely titled article conducted, as Dr. Frances believes all research occurs, in the hallowed halls of academe: “The duration of hypomania in bipolar-II disorder in private practice: methodology and validation.” It reads: “DSM-IV 4-day minimum hypomania duration is not evidence-based. Epidemiologic data suggest that briefer hypomanias are prevalent in the community. We sought to find out the relative prevalence of short (2–3 days) versus long (> 4 days) hypomanias in private practice. Methods: 206 bipolar-II (BP-II) depressed outpatients (group B) and a group of 140 remitted BP-II (group R) were assessed with the DSM-IV Structured Clinical Interview, as modified by the authors. BP-II with short vs. longer hypomania were compared on such bipolar validators as early age at onset, depressive recurrence, atypical feature specifier, depressive mixed state and bipolar family history. In addition, to ascertain the bipolar status of depressed patients with brief hypomanias, we included a comparison group of 178 major depressive disorder (MDD) patients assessed when depressed. Results: 27–30% of hypomanias (depending on whether assessment occurred when patients were depressed or in remission) had 2–3-day duration; 72% lasted less than 4 weeks. Except for the atypical feature specifier, BP-II with short vs. BP-II with longer hypomania were not significantly different on bipolar valida-

tors. Moreover, BP-II with short, like its longer hypomanic counterpart, was significantly different from the comparison MDD group on all bipolar indicators.”(4)

Then there is Dr. Jules Angst, who as I said, has followed patients for over 40 years in the Zurich cohort study, and was the person whose research was key to the whole definition of MDD as separate from bipolar disorder in DSM-III in 1980. Here is what he found: “The Zurich cohort study identified a prevalence rate up to age 35 of 5.5% of DSM-IV hypomania/mania and a further 2.8% for brief hypomania (recurrent and lasting 1–3 days). The validity of DSM-IV hypomania and brief hypomania was demonstrated by a family history of mood disorders, a history of suicide attempts and treatment for depression....The study suggests that recurrent brief hypomania belongs to the bipolar spectrum.”(5) *Why were Dr. Angst’s data – from the exact same study by the exact same researcher – central to making the major changes of DSM-III in 1980, but unworthy of being considered at all for much smaller changes for DSM-5 now?*

It is perhaps superfluous to note who in this discussion does not know the scientific facts, and then claims that there are not enough scientific facts to know the answers. The two studies above are easily accessible by Medline; searching and reading them took me three minutes. Scientists are used to at least looking at data; polemicists can’t let facts get in the way of beliefs.

In sum, the method of assessing the scientific evidence expressed by my colleague appears to involve the following procedure: first, avoid looking at any scientific data; second, send an email to one who knows little about said data; third, refute said data with the uninformed opinions of said email correspondent; fourth, conclude that the scientific data for said topic are too weak to draw conclusions; fifth, return to the philosophy of pragmatism described above (especially the part where the opinion of the professor emeritus is final). Move to the next diagnosis: Repeat, wash, and rinse.

On science and expertise

I will add here a major factor in my colleague’s erroneous belief-system. He views science as messy, with experts constantly disagreeing; hence the need for his humble pragmatism. In so doing, he makes the postmodern move of equating science with opinion, and reducing data to pure interpretation, so he can continue to believe what he wants. Let’s examine his assumptions:

He acts as if all experts are equal, or as if those experts with less expertise (second umpires) are more objective than those with more expertise (first umpires), and those experts in diagnosis, like himself, who have no expertise in anything in particular, are the most objective. (Call them über-experts). In fact, he has proposed that we need a whole slew of über-experts (non-specialists in the fields being studied) to review all the criteria created by the experts. In this worldview, the less one knows, the more qualified one is to judge the views of those who know. If this is the rationale, forget the über-experts; let’s grab the first random person we see on the street and ask him to give the final word on our diagnostic criteria.

On the relation of pragmatism to reality

What should we be pragmatic about? It seems to me that some justification exists for being pragmatic about treatment, but not about diagnosis. This is what Frances continually confuses. I consulted on this matter with another “expert” (a third umpire I suppose), Dr. Jerome Kassirer, former editor of the *New England Journal of Medicine*, Tufts faculty, and co-author of the classic medical text, *Clinical Reasoning*. There he uses pragmatic concepts (Bayesian) for diagnostic tests (as is standard in medicine) and for therapeutics. We discussed the ideas of Frances and the data of Zimmerman, and what became clear to me is that they are making the mistake of applying pragmatic notions to diagnostic criteria, which is an entirely different matter. In medicine, if one is going to

use toxic chemotherapy, one wants a highly reliable diagnosis of cancer. If one is going to use aspirin, one does not need as reliable a definition of headache. Fine. But this pragmatic approach does not mean that we would alter the definitions of cancer so as to make it harder for clinicians to decide to treat it with chemotherapy. This is what Frances is doing with bipolar disorder. Cancer is cancer, and we need to be honest about it if we are to understand it better and get better treatments. The same holds for bipolar disorder.

I will briefly add that it is naïve to think that all realism is “naïve” realism. There is such a thing, in philosophical terminology, as “scientific realism”, as a philosophical concept, which I uphold and have studied in my graduate training. The postmodernist flings the accusation, however, assuming the motivations and knowledge base of others.

Nescient disregard for facts, and an umpire metaphor, is another classic postmodernist move: all is interpretation, there is no fact. He is explicit about it when he says that whenever there is controversy, then the science is weak. But there is controversy about everything in science, even when the evidence is strong (unless one wants to believe that Darwinism was accepted placidly; leeching was dropped quietly; hormone replacement therapy ended effortlessly; and homosexuality was removed from DSM without fuss). In fact, epidemiologists have shown that even after strong scientific evidence refutes certain notions, those ideas persist for a long time in the medical literature (6).

The last word on science and realism might belong to another professor emeritus, Harry Frankfurt, the Princeton philosopher, who wrote a slim best-selling book that shows that the vagaries of my colleague’s postmodernist nihilism are widespread in our culture. Frankfurt created a technical philosophical term for the problem, which he put in the title of his book; he defines it thus: “It is just this lack of connection to a concern with truth – this indifference to how things really are – that I regard as the essence of bullshit.” (*On Bullshit*, pp. 33-34)

Postmodern polemics emeritus

Here are the options: the cynically humble pragmatics of 1994 DSM-IV-ever, blocking us from getting to the truth, and disrespecting the lives that are spent and lost in the process; or the humbler, more practically successful, scientific inquiry after truth. Polemics, rather than inquiry, result in the verbal equivalent of repeating oneself and simply raising one’s voice, trying to win the argument based on the superior power of one’s lungs (Montaigne). The next generation is no longer convinced by this rococo pastiche of postmodern rhetoric.

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The Ideology behind DSM-5

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Allen Frances has performed a great service to his colleagues by highlighting the many problems with DSM-5. While far from a disinterested observer, Frances frankly reports on his own experience with DSM-IV, in which minor revisions led to major (but unexpected) consequences.

Any DSM manual has multiple constituencies. Researchers who study patients are expected by journal editors to use its criteria. Lawyers, judges, and insurance companies will consult it. The general public can look up their problems (or those of their friends and relatives). But the main purpose of a diagnostic classification is to guide clinicians. And that is where the most serious difficulties lie.

Many of the problems with DSM-5 are not particular to this edition, but go back for decades. As Frances notes, no one has ever come up with a convincing definition of mental disorder that separates it from normal experience. Over five editions, the number of diagnoses in the manual has increased, as has their range. Epidemiological research on the prevalence of disorders in the community depends entirely on inclusive DSM-based definitions. Thus it is not surprising that half of the population meet formal criteria for a mental disorder sometime during their lifetime (Kessler et al, 2005). It has been argued that mild disorders should not be excluded from DSM-5 (Kessler et al 2003). But almost every problematic human emotion or behavior can be found in this manual. Sadness becomes depression (Horwitz and Wakefield, 2007), moodiness becomes bipolarity (Goodwin and Jamieson, 2007), and uncontrolled anger becomes intermittent explosive disorder (Kessler et al, 2006). If DSM-5 goes on to describe eccentricity as autism spectrum disorder or as risk psychosis, and labels people with normal declines in cognition with age, the process will go even fur-

ther.

Like other professionals, psychiatrists believe that many, if not most, people can benefit from their services. They are happy to medicalize the human condition. But the consequences will be inappropriate treatment. Fifty years ago, many normal people were encouraged to undertake psychoanalysis (Paris, 2005). Today, every human dilemma is managed with medication. Psychiatrists have turned away from psychological theories and treatment. They have embraced neuroscience (Paris, 2008), while doing much less talking and much more prescribing (Mojitbai and Olfson, 2008). DSM-5 will accelerate this trend. Stockholders in pharmaceutical companies may rejoice, but the rest of us should be deeply concerned.

The leaders of DSM-5, as well as the leaders of psychiatric research, are committed to the vision of psychiatry as a clinical application of neuroscience (Insel and Quirion, 2002; Regier et al, 2009). This point of view emphasizes genomics and neural circuitry, and downplays life experience. It subscribes to a reductionist model in which mind is seen as reflecting brain activity, and does not allow for emergent properties of complex systems that cannot be explained at a molecular level (Gold, 2009).

The leaders of DSM-5 seem uninterested in, if not hostile to, psychological theories and therapies. This corresponds to the current zeitgeist of psychiatry. But while such ideas claim to be scientific, they do not correspond to evidence. As Frances points out, there is not a single biological marker for any diagnosis. In spite of progress in understanding the brain, we know little more about the causes of mental illness than we did 40 years ago. While drugs are essential for severe mental disorders, they are not that effective for common problems such as mild to moderate depression (Kirsch et al, 2008). A vast literature supports the efficacy of psychotherapies (Lambert, 2003), but is largely ignored by psychiatrists.

Everyone agrees that DSM-IV was, at best, a rough draft, and that the system that has endured since DSM-III remains inadequate. I have taught diag-

nosis to psychiatry residents for decades, and I tell them that DSM provides a common language, but should not be viewed as a serious scientific document. However, with time, these diagnoses have become reified, and treated as if they represent some absolute truth.

At this point in the history of psychiatry, almost any classification has to be inadequate. But even biology, which has had 200 years to describe species, still suffers from problems in defining such boundaries. So we have to be patient and humble.

Moreover, DSM-5 is much too complicated for clinical utility. Even DSM-IV was hardly ever applied, at least as directed, in clinical practice. The manual will sit on everyone's shelf, but many will go on doing what they have always done.

The crucial question is whether we should make radical changes in a flawed system, without the theoretical advances and empirical data we need to guide us to do so. To describe this dilemma as the opposition of conservative vs. liberal agendas begs the question. Societies and political systems often maintain anachronisms when the consequences of alternatives are unknown. One only has to look at the history of the 20th century to see how readily well-meaning "progressive" ideas can lead to disaster.

For this reason, while I am in favor of *some* of the proposed changes in DSM-5, I agree with Frances that radical revision will be destructive. It is not just the changes that worry me. It is the fact that psychiatrists want to categorize all of human experience. We are already making too many diagnoses, and treating too many patients who do not require our services. DSM-IV may be a mess, but DSM-5 could make a bad situation worse.

Where Frances is most clearly right on the mark is in noting that DSM-5 consistently errs on the side of expanding boundaries—out of fear of missing something. The result is that many people with normal variations in emotion, behavior, and thought will receive a formal diagno-

sis, leading to inappropriate and aggressive treatment. This is what the military calls "mission creep". And when biological processes in normality and pathology are seen as lying on a continuum, it becomes even more impossible to set any boundary that could define mental disorder. With this system in place, psychiatrists are very likely to do as much harm as good to their patients.

Frances is particularly apt in pointing out on the arrogance and hubris of modern "scientific" psychiatry. It takes a really great mind to be humble about lack of knowledge. Isaac Newton once described his own contribution to science: "I was like a boy playing on the seashore, and diverting myself now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay undiscovered before me."

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The Elusive Definition of Mental Disorder and Problems With Reification

Allen Frances, M.D.

I agree completely with Dr Paris' elegant commentary and will just elaborate on some of his points:

1) There is not now, and probably never will be, a satisfying definition of mental disorder. If the boundary with normal can't be established with abstract concepts or scientific findings, it must be set pragmatically—what does the most good and least harm. Of course, the utilitarian calculation is itself inherently uncertain because the problem comprises so many complex and interacting variables and the data are always woefully inadequate. What we can do is take into account the best available scientific evidence and attempt to extrapolate it to the real world

settings in which the manual is used. A searching risk/benefit analysis of each new proposal is crucial and has not been done for DSM-5.

2) There is an unfortunate imbalance between the use of medication vs psychotherapy in the treatment of milder conditions at the border of normality. This is precisely where psychotherapy is most likely to hold its own in efficacy and cost and have an edge in producing durable results with fewer side effects and greater generalizability to the other life problems the person may have. But there is an overwhelming pharmaceutical marketing force that pushes drugs when they are not always needed and no comparable support for psychotherapy.

3) The Introduction to DSM-IV contains many efforts to reduce the reification of the disorders covered in the manual - which efforts are, unfortunately, largely ignored.

4) Anyone interested in just how cumbersome and obscurely unreadable DSM-5 can be should review the posting of the personality disorders work group.

The Illusion of Epistemological Problems in the Definition of Mental Illness

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The recent discussion of the DSM-5 has raised interesting epistemological questions about how best to define mental illness. Within the philosophical literature of the taxonomy of illness there are two basic camps: naturalists and normativists (Boorse 1997, Nordenfelt 2007). Naturalists feel that disease is an objective fact which can be defined by a breakdown of the normal biology. Normativists contend that disease is subjective and depends on culturally relative judgments. Yet this debate is largely an academic exercise where

philosophers try to defend extreme positions declaring all disease is either completely subjective or completely objective. Outside of this small philosophical circle no one pays any attention to this debate for the obvious reason that illness is a very heterogeneous concept and medicine a very pragmatic business. Some illnesses are best defined from the naturalist or normativist perspective but the vast majority of illnesses are best understood using both perspectives.

In his discussion of how to define disease Frances provides a nice baseball analogy to illustrate the different epistemological positions available (Frances 2010). Frances describes himself as most sympathetic to the second umpire who believes there is an underlying reality to balls and strikes yet acknowledges there is also a large subjective element to being an umpire. At first glance this seems to be consistent with the common notion of admitting the importance of both the naturalist and normativist perspective. However, latter remarks by Frances seem suggest otherwise. For example, he states that:

We must accept that our diagnostic classification is the result of historical accretion and accident without any real underlying system or scientific necessity ... Our mental disorders are not more than fallible social constructs (but nonetheless useful if understood and applied properly) (Frances 2010).

This statement suggests that the naturalist perspective plays no role in how we define mental illness. Together with the earlier baseball analogy this places Frances in the uncomfortable position of being an umpire who believes there really are balls and strikes but who feels his rulings have absolutely no relationship to them whatsoever, and who is OK with this!

I believe Frances backs himself into this awkward position at least partly because of a rather extreme pessimism about the etiology of mental illness. He states that:

All normal brain functioning is normal in more or less the same way, but any given type of

pathological functioning can have many different causes. (Frances 2010).

Following this statement there is a discussion about the lack of a simple genetic explanation for any mental illness and the worry that there are potentially “hundreds of paths to schizophrenia” (Frances 2010). While the above statement may be true the conclusion that we can never understand the etiology of mental illness (or perhaps the stronger statement that there is no etiology) does not follow from this. Frances then seems to change course and acknowledges that there can be progress in understanding the etiology of mental illness but worries that it will be painstaking slow and that our current DSM disease categories may play no useful role.

While I agree that progress will most likely be slow it seems premature to give up a link between our current diagnostic criteria and etiology. As Frances admits, the DSM-III and IV definitions of many diseases have proved extremely useful in both research and clinical practice. The criteria for these illnesses evolved from decades of experience in descriptive psychiatry (just as our understanding of all physical illness started with descriptive medicine). I would surmise that, at least for the major axis one disorders (major depression, bipolar disorder, schizophrenia), the diagnostic criteria have proved useful precisely because they capture correctly some part of the underlying biological reality of mental illness.

When discussing the reality of mental illness it is often useful to step back and take a larger philosophical and pragmatic perspective. All of our mental states are ultimately a product of our brains. As neuroscience advances we will come to understand how the brain generates cognition, emotions, and moods. Thus we will come to understand how the brain causes the symptoms of what we call mental illness regardless of whether mental illness is mostly normativist or mostly naturalist. From a pragmatic standpoint the vast majority of people seeking voluntary outpatient treatment for mental illness have very objective symptoms that follow a specific illness

course. Thus there is nothing intractable or mysterious about finding the biological etiology of mental illness.

In the end Frances and I both share similar concerns about premature and disruptive changes to the DSM-IV and we agree that we are still far from understanding the etiology of any mental illness. Yet I would argue that, at least for the major diagnoses, this has nothing to do with the normativist elements of mental illness. There is a strong normativist element even in many physical illnesses but this doesn't seem to impede our colleagues in internal medicine (e.g. when is blood pressure or cholesterol too high?). When defining diseases with strong normativist elements small subjective changes in the criteria can sometimes have vast consequences. Thus I share Frances' concern about making changes to illnesses that have serious consequences in forensics or the potential to create stigma. It is better to acknowledge the normativist elements in these cases and seek outside societal input and all potential consequences should be studied and debated openly before making any changes to diagnostic criteria. This being said, I believe the vast majority of the major mental illnesses (depression, bipolar disorder, schizophrenia etc.) have only small normative elements. These mental illnesses are mostly naturalistic and are about as normative as Parkinson's disease or diabetes. For these illnesses we need to continue to study objective symptoms and their biological correlates and there is every reason to be optimistic in the long run.

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Epistemological Problems not so Easily Solved

Allen Frances, M.D.

Dr Cerullo and I have a quantitative, not qualitative, difference of opinion on what Dr Cerullo calls the naturalist/normative divide. Let's start with the qualitative ways in which we converge and then indicate the quantitative ways we part company.

Dr Cerullo and I agree that when a debate has managed to maintain itself for twenty-five hundred years, there must be truth on both sides, absolute truth on neither. We are therefore both second umpires who reject the extreme poles on the epistemology of mental disorders. We don't accept the third umpire view that mental disorders have been conjured up in some Bishop Berkeley, solipsistic sense. Certainly mental disorders are not "myths". They exist and cause suffering in ways that are all too tangible. And they all have materialistic biological underpinnings that we will some day understand. We also don't accept the opposite "realistic" extreme. Mental disorders are not preordained and simple "diseases" just waiting to be easily decoded by our increasingly sophisticated scientific tools.

Dr Cerullo and I thus agree that the best epistemological stance is to somehow split the difference. We both expect to call them as we see them as second umpires. But this brings us to our considerable quantitative disagreement. We are very different second umpires and disagree on how easy it is to make the calls separating the balls from the strikes. Dr Cerullo misstates my position to make me sound like a third umpire manqué when he suggests I might even say that "the naturalist perspective plays no role in how we define mental illness." Of course, I don't believe this. How could any sensible person? There are naturalistic underpinnings to everything that happens in our little universe—and it could not be otherwise. There are always real balls and real strikes, whether or not it is within our poor powers to see them or understand their meaning. Dr

Cerullo confuses my statements about difficulties discovering causal realities into thinking I am saying these realities don't exist.

Indeed, this is precisely where Dr Cerullo and I have our quantitative disagreement. He is much more confident than I am in the eventual explanatory value of the mental disorders described in our current diagnostic system. He is a second umpire who believes we are close to seeing things as they really are and states confidently, "I would surmise that, at least for the major axis one disorders (major depression, bipolar disorder, schizophrenia), the diagnostic criteria have proved useful precisely because they capture correctly some part of the underlying biological reality of mental illness."

In contrast, I regard our mental disorders as no more than superficial and heterogeneous constructs, of great practical utility now but eventually of quite limited explanatory value. Dr Cerullo sees our mental disorders as being much closer to reality; I see them as fragile, not necessarily capturing much the elusive, underlying biological reality. We are both second umpires, but Dr Cerullo is ever so much more confident in his calls.

Dr Cerullo titles his commentary "The Illusion of Epistemological Problems in the Definition of Mental Illness". Of course, in a limited sense this is accurate. There is no inherent reason to believe that the human race (if it lasts long enough) cannot unearth the numerous and heterogeneous pathogenetic underpinnings of most, if not all, the behaviors we now label as mental disorder. But the title obscures the practical epistemological problems that arise if my guess turns out to be right that schizophrenia (and the other major mental disorders) each have more than fifty (and perhaps hundreds of) different underlying causes. Would we still call it "schizophrenia" and be so attached to this unsatisfying and heterogeneous melange of symptoms once we knew all the things that are "really" going wrong. Like Dr Cerullo, I also treasure the practical, everyday utility of our homely system. But I think that he has an unwarranted complacency and offers false hope about its eventual explanatory power.

I do thank Dr Cerullo for providing

a better summary of my position than I did myself when he says, "Frances describes himself as most sympathetic to the second umpire who believes there is an underlying reality to balls and strikes yet acknowledges there is also a large subjective element to being an umpire".

In summary, then, Dr Cerullo and I are both second umpires, but with very different stripes. I am a very uncertain second umpire, never sure whether I am calling the right balls and strikes—nestled about equidistant from the first and third umpires. In contrast, Dr Cerullo is an extremely confident second umpire quite sure of his calls and ready to become a first umpire if only there were just a little more light. If he is right, we should have some pretty quick and dramatic findings to explain the major mental disorders. If I am right, it will be, as it has been, an inherently slow and retail slog despite the brilliant tools.

Nosology for Beginners: The Context of Psychiatric Classification and Diagnosis

Avram Mack, M.D.
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Today, as in a few other previous instances, our profession faces calls for change or movement of the field either in or through its classification. When a membership organization such as APA engages in the creation of a classification there are risks that are shared by all who care about the field. The risks of a problem-ridden official classification include:

- Forensic Misuse
- Clinical Disruption
- Reduced Research Generalizability
- Confusion for Patients
- Deterioration of the Profession's Standing
- Degradation of the professional

association's control over mental health classification systems

As our professional association continues to move toward a proposed classification, it is in all of our interests to understand the historical context of issues in psychiatric classification by focusing on several recurring problems and issues in the classification of psychiatric conditions, and considering some proposals for DSM-5 in this context.

Review of the Neo-Kraepelinian Movement:

Notwithstanding the hope of its authors that DSM-5 shall represent a "paradigm shift," and with the exception of DSM-I and DSM-II, official American psychiatric classification has followed the descriptive tenets of the Sydenham and Kraepelin model for nearly a century, starting with the 1917 system made by the APA. A 17th century leader, Sydenham's conception of disease and its classification included several basic points:

- Use of empirical observation
- Classification using the *more botanico* / categorical method
- That the categories were applicable across humans and across places

Sydenham's principles were integrated into the medical profession that coalesced in Enlightenment France. There, the "Anatomic-Pathologic" method, which combined descriptive observation of illness with pathologic findings, produced the most terrific breakthroughs, especially in the conditions due to infectious agents—gold standards for further characterization of syndromes.

Like Sydenham, Kraepelin viewed illnesses as valid or real entities, and his textbook for the most part included 17 main categories of illness. In addition his scientific outlook was that ultimately classificatory attempts based either on symptomatology, course, or etiology would converge around the same valid groups. Finally, Kraepelin viewed his work as heuristic models with practical benefits, particularly towards communication within the field. He

would write in 1920, “I want to emphasize that some of the clinical pictures outlined are no more than attempts at presenting part of the material in a communicable form.”

It is notable that at this time in chemistry, the Periodic Table was developed—it was an analogous scientific method: continued description of atoms and their behavior did lead to the production of a valid classification of atoms. This was a positivistic ideal. And between 1917 and World War II, the APA’s classification was updated 10 times to provide the section on mental disorders in 10 editions of the American Medical Association’s “Standard” classification system. American psychiatry’s nosology was psychologically oriented during the post-war period as psychodynamic principles informed the system until 1980, when the neo-Kraepelinian movement took hold in official APA classification, marked by its use of descriptive operational criteria sets, its heuristic nature, its atheoretical status, and the use of reliability as its measure. In their specific proposals DSM-5’s authors now propose some departures from these tenets. The next section reviews tensions and problems any classifier, such as those of DSM-5, faces.

Recurring Philosophic Issues in Nosology

Nominalism/Realism. One of the first questions that invariably arises is whether or not mental disorders exist at all. Szasz argued that mental illness was a myth, which gave support to the anti-psychiatry movement. On the other extreme are those who assert that mental disorders are entities that are tangible. Where one stands on this continuum informs one’s approach to classification. DSM-IV did not assert validity except to the extent that the symptoms in its criteria sets were associated with each other.

Sui Generis/More Botanico. *More Botanico* refers to Sydenham’s view that illnesses have typical features across humans and that they can, thus, be classified categorically as plants had been. On the other side, the *Sui Generis* concept refers to the concept that each

individual’s mental disorder is special or unique to himself. This latter perspective was germane to the psychobiology of Adolf Meyer as well as to psychoanalysis, and is antithetical to categorical descriptive psychiatry.

Local/Universal Illness. Along the same lines, many psychiatrists have seen disorders as local—occurring only for a specific location. For example, George Beard’s neurasthenia was defined by the American environment and Industry. He called it “modern and originally American.” But this is in opposition to Sydenham’s perspective. Charcot, too, saw hysteria as “valid for all countries, all time.” DSM-IV’s coordination with the WHO indicates its “cross cultural” intention—to be applicable across nations and cultures.

The Range of Psychopathology. All classifications have had to grapple with the range of the psychopathology defined by the system. Freud’s explanatory system could explain the psychopathology of everyday life, but this would be restricted by DSM-IV’s requirement of clinical significance or by restriction of clinical attention to the institutionalized patients. A system’s range of coverage—based much on how it is written or its criteria sets written (in an operational classification) has a great effect on public health, stigma, and treatment utilization.

Specific Technical Issues in Psychiatric Classification

Splitting/Lumping. This is a tension that arises in a categorical system. The ultimate perspective of the “lumper” is that there is only one kind of mental illness, and this cuts across theoretical backgrounds. Many leaders have asserted that there is solely one mental disorder—in the 19th century both the neuroscientist Greisinger (who proclaimed mental disorders are brain disorders) and the early psychologist Neumann both held this view. And in the 20th century Karl Menninger held to the “Unitary Concept.” At the other extreme, the 2400-item classification of Sauvages (who tried to rep-

licate the binomial method of Linnaeus) was rejected as impractical.

Categorical/Dimensional. Dimensional approaches are desirable for the description of some aspects of mental disorders—specifically for continuously-distributed phenomena, ideally to be converted into a numerical form. Except for the GAF scale, none exist in DSM-IV. They might be best suited to personality disorders.

Fad. Excess levels of diagnosis at a particular time may occur naturally, but in some cases disorder levels are artificially heightened either due to popularity or the definition of the illness, or a combination of both. DSM-IV’s alteration of the definition of Autism and the Pervasive Developmental Disorders as well as ADHD each allowed for sudden increases in the disorder.

Forensic Misuse. As far back as Esquirol’s Monomania, there have been disorders that were used inappropriately in forensic settings. The definition of monomania had been a celebrated achievement of Esquirol, Pinel’s protégée. However over the early 19th century it was used so frequently in courts in France that its meaning became diluted, ultimately leading to a backlash by the French judiciary, followed by a reduction in its forensic and clinical use. Today’s PTSD is one such disorder that may be misused—and misuse is possible in the whole range of forensic settings, including the administrative proceedings of Individualized Educational Plans for children and adolescents.

Empirical/Rational Classification. There have been no shortage of theories of psychopathology, and this has ranged from psychological to biological conceptions. In the 19th century, Broussais claimed that all mental disorders had an etiologic basis in the GI tract. But there have been many instances where others have called for a “fallback” on a heuristic symptomatology as the means to classification. Pliny Earle, one of the APA’s founders, said, “In our present state of knowledge, no classification of insanity can be erected upon a pathological basis...the pathology is unknown. We are forced to fall back upon the symp-

tomatology of the disease.” Or, Samuel Orton said in 1917, classification by empirically-observable phenomena was “a necessary result of our limited understanding of etiology.” And this empirical perspective resumed sway in DSM-III.

Description without Gold Standards. A gold standard for a diagnosis, whether an etiological agent or a distinctive pathological finding, can serve as a centerpiece while other information is discerned about a particular syndrome. To date there are no gold standard findings among psychiatric disorders. The hope of the neo-Kraepelinian model is that continued improvements in reliability will coalesce around valid disorders for which gold standard findings will be discovered. However, one must also recognize, as did Kraepelin, that reliability is important but it may not yet approximate validity, and that the heuristic process may, but may not, lead to success.

Historical Context's Application to DSM-5 Proposals

There are many considerations that have constantly arisen in the production of psychiatric diagnostic classifications, and they would apply in the formation of DSM-5. For example:

1. Splitting/ Lumping—In the lumping of the abuse and dependence diagnoses of substance use.

2. Empirical/rational Epistemology: The DSM-5 proposal for psychosis risk syndrome is wanting for a gold standard for psychosis—but is there one?

3. Dimensional/Categorical Diagnosis: the proposal to create a continuum for the pervasive developmental disorders in DSM-5 is welcome but possibly not ready.

4. The range of Psychopathology: the range would be expanded in the alterations proposed for grief, mild cognitive disorder, and ADHD.

Conclusion

The anatomic-pathologic descriptive method had led to some successes in psychiatry, but our frustration should not lead us to once again prematurely abandon it as we had done in the actual paradigm shift of DSM-I. Psychiatry

should proceed with caution and recall our predecessors, such as Charcot, or I.S. Wechsler who wrote in JAMA in 1930, “It is no discredit to psychiatry to acknowledge that it has barely emerged from the descriptive stage...”

Thesis/Antithesis/ Synthesis

Allen Frances, M.D.

Dr Mack has provided us with a brief, lucid recapitulation of the history of psychiatry and presents rich anecdotes illustrating the recurring, seemingly, insoluble debates in psychiatric classification. Whenever a debate manages to persist for decades or centuries, the assumption should be that there is considerable truth on both sides. Let's start with a brief note on the historical context and then point at some potential syntheses that could modulate the intensity of the debates.

I think the most important paradigm shift in the history of psychiatry was the development by classic Greek medicine of the four humors theory of personality and disease. The Greeks intuited that behavior and illness were the product of an imbalance in the biology of the body, discounting previous beliefs that they were caused by the gods or spirits or a curse or the placement of the stars. The next paradigm shift was delayed for two thousand years until the development of systematic descriptive classifications during the age of enlightenment. We are now (hopefully) close to the end of this tired era and are impatiently waiting for the next paradigm shift that will replace mere description with explanatory models. The understanding of brain psychopathology may not be reached for many decades and certainly won't be achieved at the same time for all disorders. It will doubtless be much more particular and less elegant than the theory of evolution or the periodic table. The brain is too

complicated for simple, sweeping, causal explanations.

Kraepelin's work was a solid job of careful observation and of thorough summation, but it did not represent a paradigm shift and he had the insight and modesty to know it. Kraepelin was building upon and assimilating the dozens of diagnostic systems that had been developed during the nineteenth century, starting with Pinel's. His method was based on the painstakingly systematic charting and collating of symptoms, course, family history, medical history, and findings on autopsy. (I have seen his patient charts in Munich—his notes were voluminous and remarkably neat—he used pens of different colors for each category of information). Kraepelin wrote the most popular textbook of psychiatry and its table of contents became his classification.

Kraepelin's goals were appropriately limited given the scientific information on causality available in his time (and still in ours); “I want to emphasize that some of the clinical pictures outlined are no more than attempts at presenting part of the material in a communicable form.”

This is really all that can ever be expected of a descriptive classification and Kraepelin knew it. There have been only two important advances in nosology since Kraepelin: 1) the extension of psychiatric diagnoses to less severe outpatient conditions, which was stimulated mostly by Freud; and, 2) the introduction of diagnostic criteria in DSM-III by Spitzer. The diagnostic system has not improved since DSM-III. It will not improve in DSM-5 and may get worse.

Now for the attempted syntheses of Dr Mack's pairs of recurring theses vs antitheses:

Realism vs Nominalism. For now, the nominalist, second umpire clearly rules. Our currently defined mental disorders don't stand up as unified diseases and will be picked apart as heterogeneous causal explanations for psychopathology are gradually discovered. In time and with accumulating scientific knowledge, we will gradually get to see things more as they “really” are and will gradually become more like first umpires.

More Botanico vs Sui Generis

(which includes Local vs Universal). To paraphrase Sullivan, everyone is more simply human than otherwise. Psychiatric disorders tend to be generic—to present in more or less the same way across times, places, and cultures. But the equivalence is only "more or less." There is lots of "sui generis" too. First off, each of our defined mental disorders is remarkably heterogeneous in its presentation. Then there are the definite cultural variations, the changes in presentation over time (whatever happened to catatonia and conversion disorder), and the fact that everyone is unique on many variables that may count importantly, especially in planning treatment. The best synthesis here is to be equally sensitive to the common patterns and to the individual variations on the theme.

Categories vs Dimensions. I prefer dimensional measures for IQ, height, weight, and the balance in my bank account. I prefer categorical description for colors, foods, and in fact for most things. We evolved to be much better at vivid naming than at more accurate numbering. Computers are better at numbers. Medicine has been almost exclusively categorical (exceptions are hypertension, cancer staging and in DSM-IV, severity ratings and the GAF). Categorical description loses information in describing continuous phenomena that lack clear boundaries—like mental disorders. Dimensional measurement can be cumbersome and bloodless. It makes sense to gradually combine both methods in clinical work and to move more toward dimensions in our research efforts. The obstacle to a synthesis is the tremendous resistance to dimensions among practitioners. DSM-5 will likely stiffen by offering ridiculously complex dimensional proposals that no one will ever use.

Lumping vs Splitting. There has to be a happy medium somewhere between the 1 mental disorder category of the extreme lumper and the 2400 of the extreme splitter. Eventually, many decades from now, 2400 may seem more right when we have deciphered many of the mysteries of psychopathology and discovered its myriad interacting causes. For now, with a classification based only on description, DSM-IV seems a bit cluttered with its almost 300 categories. It is a splitter's dream

conceived with the purpose of enhancing reliability. This is fine except for the artificial comorbidity caused by the splitting of more complex syndromes. The best approach is to be mindful that DSM disorders are no more than descriptive building blocks and much less than homogeneous diseases. .

Lean vs Full Coverage. There is no clear boundary between normal functioning and having a mental disorder and no operational program to decide which disorders are to be included in the diagnostic system, which to be left out. My intuition and reading of the literature tells me that DSM-IV is already far too inclusive and that DSM-5 is likely to make things very much worse.

Empirical vs Rational Systems. I love Dr Mack's Pliny Earle quote; "In our present state of knowledge, no classification of insanity can be erected upon a pathological basis... the pathology is unknown". We have learned lots of things, but the quote still stands 150 years later. No descriptive system makes much more sense than any other in the absence of deeper understanding. All current attempts at causal explanations are speculations.

Forensic Use vs Forensic Misuse. The cautions in the introductory sections of DSM-IV are meant to encourage the appropriate and necessary relationship of psychiatry and the law and to discourage misuse and misunderstanding in this relationship. The abuse of psychiatry in the involuntary commitment of sexually violent prisoners is a disturbing example of how delicate is the balance and how fragile are both constitutional rights and professional integrity in the face of a serious societal problem (see comments by Drs Szasz and Piaseki).

Loose vs Tight Standards For Change. Absent a gold standard for making decisions about change in the classification, what standard should apply. The purported "validators" of descriptive psychiatry have been disappointingly unconvincing, inconsistent, and a poor guide to change. The drug industry is ready to pounce on any DSM change to promote false, faddish, and risky epidemics. A high

threshold for change seems reasonable—one that places a heavy burden of proof, an exacting standard for evidence, and risk/benefit analysis before any change is accepted.

A Grandfather Reflects on the Younger Generation

Henry Pinsker M.D.
Teaneck, NJ

When I had the privilege of participating on the APA Task Force that produced DSM-III, I was characterized by a colleague as "a clinician-administrator." Writing about planning for DSM-V, I will maintain that posture rather than adhering to the discipline of scholarly writing which recognizes and credits other people's ideas.

The APA's website for DSM-5 proclaims "The Future of Psychiatric Diagnosis" "One of the most anticipated events in the mental health field." In the 1960s Community Psychiatry was described by leaders of our Association as "the third psychiatric revolution." (The first was Pinel's releasing patients from their chains, the second was the introduction of Freudian analysis.) Today we remember Community Psychiatry as an innovation in delivery of services that faded when Federal funding ended. Lasting benefits of Community Psychiatry were modest: outpatient treatment became more acceptable and available to a larger portion of the public, and the professional status of non-physician psychotherapists was enhanced. Two Nobel Prizes have been awarded for treatment of mental disorders. The most recent, in 1949 was for pre-frontal lobotomy, a procedure that helped many patients, but is cited today by our critics as evidence that psychiatrists are potentially evil. We should do our work without awarding ourselves medals or employing the superlatives of the entertainment industry. "Paradigm shift" is a designation to be made by historians looking back at the impact of a new program or idea, not by the program's enthusiastic proponents.

The popular press characterizes DSM as "the psychiatrist's bible."

Although “bible” is defined as “a book authoritative in its field,” the implication is that it is a book to be revered. We should do what we can to dispel this image. DSM-III occupies a special place in our history because it introduced important procedural changes and ratified conceptual changes that had become widespread in the American psychiatric community. With its successors, DSM has become an institution, an industry. Although the DSM process has served psychiatry well, it is now time to begin planning for psychiatry to join the medical community and use ICD as the official source of diagnostic terms. We would be better off if the public thought of us as being guided by technical manuals rather than by a “psychiatrists’ bible.”

With the creation of DSM-III, every step of which was shared with the public, American psychiatry demonstrated that the diagnostic system, which originated in observations of patients in 19th century German hospitals, was not engraved on stone tablets, but was a body of work that should be modified continuously in response to new information. The path to this event had been blazed a few years earlier when the entire membership was asked to vote on the elimination of homosexuality as a category of mental disorder. Awareness that the diagnostic scheme is transient is one of DSM-III’s major contributions to psychiatry. The use of criteria in DSM-III, a move intended to enhance reliability, was acceptable because there was a growing sense in our field that exactness was preferred over vagueness. While described by its authors as “atheoretical,” the repudiation of theory was focused on assumptions of causality based on inferences about unconscious process. For example, Depressive Neurosis was described in DSM-II as “an excessive reaction of depression due to an internal conflict or to an identifiable event such as the loss of a love object or cherished possession.” Physicians diagnosed Depressive Neurosis when the patient had symptoms of depression, without regard for the physician’s belief in the unconscious or evidence of internal conflict. The diagnostic process usually consists of selecting a term that seems to explain the patient’s problem or that seems appropriate for

the intended treatment.

DSM is a policy and procedure manual with an educational mission. Axis II was established to “ensure that consideration is given to the possible presence of disorders that are frequently overlooked.....” Physical disorders were declared an axis because the psychologically-minded psychiatrists of that era tended to ignore them. Evaluation of stressors, axis IV, was intended to provide information relevant to prognosis. The DSM-III Task Force understood that many patients with diagnosis of schizophrenia—the diagnosis on most inpatient charts—really had mood disorder and consequently, if properly treated, a better prognosis. Antidepressant medications had recently been introduced. Mood disorders were declared trump, so in a number of places, the criterion (i.e. instruction) is: “not due to a disturbance of mood.”

When I was in medical school, the classification of mental disorders was of little more import than classifying beetles or butterflies. American psychiatrists diagnosed schizophrenia whenever they found a little thought disorder. That some of the patients might have been manic-depressive didn’t really matter because treatment choices were essentially analytically-oriented psychotherapy or sedating medication. Fretting about formal diagnosis was thought to be a denial of the patient’s individuality, and the diagnosis would not affect the treatment. The introduction of lithium in the late 60s as a treatment specific for mania and the introduction of antidepressants in the 70s suddenly made diagnosis excitingly relevant. When DSM-III was written, it appeared that psychiatry now had two major disorders and for each there was a specific treatment. Correct diagnosis would lead to correct treatment. We did not anticipate that thirty years later, everyone would be diagnosed with everything and would be receiving every medication.

The first months of the Task Force’s work were devoted to an attempt to define the term “mental disorder.” I thought that this was an impossible task and proposed that the name of the book should be changed

from “Diagnostic and Statistical Manual of Mental Disorders” to “Psychiatrists’ Diagnostic Manual,” shifting the focus from subject matter - Mental Disorders - to the users - Psychiatrists. My idea was speedily and unanimously rejected. I was delighted to read Frances’s observation that “Mental disorder is what clinicians treat and researchers research and educators teach and insurance companies pay for.” His statement gives me courage to suggest again that the APA book be reconceptualized as a manual of clinical practice and that ICD become the official classification. When the first DSM was produced, in 1952, ICD did not provide adequate support for contemporary psychiatric practice. Since that time, ICD has evolved, influenced, of course, by DSM. ICD includes codes for “headache,” “fatigue,” “depression,” “anxiety,” and other symptoms for which patients seek care. Moving from DSM to ICD might lead to use of diagnoses that are at times more simplistic than what we are accustomed to, but more realistic. Psychiatrists seem obliged to report they are treating specific disorders, while the rest of the medical profession, realistically, is permitted to treat symptoms. The “Psychiatrists’ Diagnostic Manual” would be a companion publication, providing relevant instructions for diagnosis and treatment. The word “statistical” as a major element of the title reflects DSM’s mission in 1952—to stabilize nomenclature and facilitate statistical coding of case records. Those who use the book today are seldom concerned with “statistical” implications. We can find more meaningful ways to honor the past.

The problem of early diagnosis illustrates how procedural rules have become intertwined with the diagnostic classification. Because schizophrenia was often overdiagnosed, the creators of DSM-III elected to protect patients from the social stigma associated with this diagnosis and from the harmful effects of the [phenothiazine] medications likely to be prescribed. The diagnosis could not be made unless there had been “continuous signs of the illness for at least six months.....” Early diagnosis of schizophrenia was no longer permitted. Now, “Psychosis Risk Syndrome” has been proposed for

DSM-5. What has happened? The medications used to treat the condition are not as likely as the old ones to cause permanent neurologic damage, and new findings give hope that early treatment may reduce subsequent disability. To allow (or justify, or encourage) early treatment, the framers of DSM-5 have proposed a new disorder. It is well established in medicine that new findings may lead to drastic changes in treatment recommendations. It should not be necessary to change the classification of disorders in order to change treatment plans. The diagnostic system and the procedure manual should be separate books.

The purpose of criteria in DSM-III was to ensure that people who used the same term were talking about the same condition. The descriptions and definitions of mental disorders follow the medical model of assuming that the diseases are real, although we know that many are best understood as constructs scarcely more accurate than the old classification "fevers." The problem with a "risk syndrome" is that it appears to be a construct created from a set of criteria, not from an attempt to describe something that had been observed.

Bipolar II was added to DSM-IV because there appeared to be clinical evidence that it was as real as the other constructs. "Psychosis Risk Syndrome," it is feared, will be overdiagnosed and will be associated with excessive pharmacologic treatment, as has been the case with Bipolar II. Controlling physician behavior, however, is an educational or administrative matter, and should not be addressed in the classification of disorders. If a condition is real enough to merit a place in the classification, it should not be denied on the grounds that physicians are not skillful enough manage it. Physicians routinely decide when to initiate treatment for mild or subclinical conditions, weighing the risks and the benefits. Guidance about when to treat and how to treat comes from the textbooks, manuals, and current literature—it is not the function of the classification.

All areas of medical activity are shaped by administrative and legal concerns, which may be more involving than scientific and therapeutic goals. Students of disease were creating clas-

sifications as an intellectual and scientific challenge before administrative concerns required an interface between classification and medication. In all societies, people have ingested non-food substances with the goal of relieving distress, whether it be pain or infertility or unhappiness. Because society has placed control of certain agents in the hands of physicians, and because we have a system of payment that is oriented around treatment of diseases, it is necessary that patients have named disorders. Pharmaceutical companies are free to develop agents that may alleviate distress. When they succeed, the new agent must be approved as treatment for a specific condition, so if the condition does not have an accepted name, one must be created. If the patient has reimbursement-type insurance, a disorder must be diagnosed. Even if the patient is in treated by salaried physicians, quality review procedures monitor the appropriateness of prescribing. When, however, treatment does not involve a potent pharmaceutical product, or when insurance reimbursement is not requested, it is not necessary that the patient have a named disorder.

Criteria are instructions to ensure that diagnostic terms are applied correctly. Criteria belong in the proposed Psychiatrists' Manual, not in the diagnostic classification. Dimensional rating scales offer a brilliant approach to describing patients. Rating scales belong in the proposed Psychiatrist's Manual, not in the classification of disorders. It is clearly stated in the introductions to both DSM-III and DSM-IV, that the classification classifies disorders, not patients.

Changes in the classification of disorders should be limited to those made necessary by changing conditions or made possible by solid new evidence. The major changes introduced by DSM-III, even if not a paradigm shift, were appropriate because of the gulf at that time between clinical practice and the old diagnostic system. No such gulf exists today, so major change is not called for. Change is appropriate as a response to new information or to remedy deficiencies in the current manual.

Eliminating Axis II and changing personality disorders to personality types is such a remedy. Change is potentially disruptive, so it should not be done for the purpose of being "one of the most anticipated events in the mental health field."

The Wisdom of the Ages

Allen Frances, M.D.

Henry Pinsker was my first teacher of psychiatry and has remained a great influence. He always has a clear eye for the strike zone and calls them just as he sees them.

Henry begins by pointing out the tendency of psychiatry to have recurring "revolutions" that ultimately fail to live up to the enthusiasm they initially generated—often these are most complacently trumpeted just prior to being abandoned for the next "revolution." The proof in real paradigm shifts is in the historical pudding—their lasting value, not the drumbeating hype. Psychiatry, like medicine in general, seems tantalizingly close to the real paradigm shift of scientific understanding. But like the Tantalus myth, catching the fruit has so far been elusively impossible—the more powerful the scientific tools we develop, the more we learn just how complex are the problems they are seeking to solve. Psychiatry may be decades away from a paradigm shifting revolution.

Like Henry, I have been troubled and embarrassed by the description of DSM-IV as a "bible" and never felt any reverence whatever for it. DSM-IV seems to me to be no more and no less than a useful (but necessarily makeshift and temporary) compendium of current diagnostic assessment using limited descriptive methods. We tried to highlight the many fallibilities of the DSM approach in the Introduction and in presentations, but many people place more faith in the manual than we do.

Henry suggests that psychiatry "join the medical community and use ICD as the official source of diagnostic terms" and "that the APA book be reconceptualized as a manual of clinical practice and that ICD become the official classification." Actually, we

already use the codes of ICD, and pretty much the same terms. Regarding official status, DSM is really just an (admittedly mammoth) American Psychiatric Association sponsored gloss on ICD. The fact that the gloss is often given such independent authority places great responsibility to do it competently, consensually, and cautiously—standards I feel that the work on DSM-5 has so far failed to meet. The larger question is whether the APA (or any one professional association) should be permitted to retain such an important franchise, especially when it has not provided nearly enough quality control. APA has treated DSM-5 more as a publishing asset than a public trust.

Henry also nostalgically recalls the time not so long ago when it appeared psychiatry might achieve a rational and specific differential treatment selection based on a reliable system of descriptive diagnosis. "We did not anticipate that thirty years later, everyone would be diagnosed with everything and would be receiving every medication." Clearly, things have gotten out of hand.

I disagree with only one of Henry's points, but the disagreement is important. He is much less worried than I am about making "Psychosis Risk Syndrome" an official diagnosis. "Controlling physician behavior, however, is an educational or administrative matter, and should not be addressed in the classification of disorders. If a condition is real enough to merit a place in the classification, it should not be denied on the grounds that physicians are not skillful enough manage it." Henry temporarily becomes a first umpire who believes that conditions are demonstrably "real" so that untoward consequences should be dealt with external to the classification system. I think that Psychosis Risk Syndrome will become "real" in a real sense only when it has a reasonable low false positive rate and a safe and effective treatment. As it stands today, it may have a false positive rate of 75-90%, no effective treatment, and will promote the terrible side effects of antipsychotic medications. The makers of the classification cannot responsibly take a hands off attitude toward the

way their decisions are likely to be misused. Physician and patient education is no protection since so much of it is influenced by the drug industry.

Final Comment

Henry Pinsker, M.D.

My comment about the proposed Psychotic Risk Syndrome was limited to the somewhat libertarian position that education about good practice is not the function of the diagnostic classification. This does not mean that I endorse the inclusion of this diagnosis. Until we adopt a different system, several of which have been described in this series of comments, our diagnostic scheme continues to be based on the notion that the disorders we diagnose exist out there somewhere, even though their nature has eluded discovery and many agree that they are constructs more than they are phenomena. It is a convention that underlies much of medical practice.

From this perspective, a risk syndrome is a contrivance, not a disorder. Medical practice includes prescribing for conditions which, in the physician's judgment, are latent, early, or incipient. It is enough that we continue to split off new entities based upon patient behavior. We don't need to open the door for new entities that give names to treatment decisions.

The Paradigm Shift for Psychiatric Diagnosis is Already Here !

Avi Peled M.D.

Technion: Israel Institute of Technology

I have no training in philosophy and I will address the issues relevant to psychiatric diagnosis from a practical medical approach which assumes that curing patients is the bottom line in any medical discipline.

In this regard I am fond of some of the definitions of mental disorders

described by Allen Frances in the section of "The Elusive definition of mental disorders," for example that mental disorders are such because of "accretion and practical necessity," and that they are "what clinicians treat and insurance companies pay for." In sum, mental disorders relate to human suffering and disability, are dysfunctions that bring people to clinicians, and most seek medical insurance to guarantee treatment, and resolve the disorder.

The major issue concerning the debate about future psychiatric diagnoses relates to the idea of a "Paradigm shift." This is because intuitively most psychiatrists feel there is a need for a revolution so that we will really know what is wrong with our patients and will actually be able to cure them. For this to happen we need major new ways of thinking as defined by Thomas Kuhn.

In the section titled "Descriptive psychiatry gets long of tooth" Allen Frances places current psychiatry within its historical scientific perspective, the premise of the DSM as a descriptive method is that "any domain receiving systematic observation and classification would eventually display causal patterns. This approach was enormously successful in each of the major paradigm shifts in science."

Also According to Allen Frances descriptive psychiatry has done as much as it can to further our field, etiology based psychiatry is the paradigm shift needed. Descriptive psychiatry has offered reliability; we now need a diagnostic system with validity.

In this regard James Phillips (2010) indicates that "not only don't we have a so-called paradigm shift to make the nosology more valid, we don't even know how will that occur, or even if it will in fact ever occur."

Donald Mender (2010) asks, "Do we have the conceptual means to mobilize unified predictive principles in the service of a rigorous psychiatric nosology?" he goes on to state that "living things as physical systems demonstrate emergent properties beyond those of their particular micro-constituents." It is clear that single neurons and even entire neural circuits do not demonstrate characteristics such as consciousness, awareness, feelings and personality, such characteristics arise as

emergent properties from whole brain organizations.

Mender goes on to indicate that scientists are beginning to develop the taxonomy from non-linear systems analysis linking the levels of "molecular biology and behaving mentating organisms." He argues that "those links have the potential to fuel unprecedented future insights regarding psychopathogenesis."

I agree with Allen Frances (2010) in his rightful positioning of psychiatry in its historical and scientific coordinates. Our descriptive system precedes the next phase of discovering the causes of mental disorders and generating a valid brain-related taxonomy for psychiatry.

As for the arguments forwarded by Mender I would like to indicate that the new non-linear taxonomy is already here, insights from nonlinear systems, and mathematical neural network and neural-computation models provide for an initial conceptual framework to reformulate mental disorders as disturbances to optimal brain organization.

Mender eventually argues that these "hopeful beginnings cannot be productively leapfrogged by premature taxonomic efforts." I would argue that changing the taxonomy and conceptualization is productive. An old Chinese saying states that wisdom begins by calling things by their correct name; instead of "psychosis," "disconnection syndrome" (Friston & Frith 1995) has a brain-related orientation that will lead future psychiatrists to research it in patients and eventually validate it and develop "re-connecting" interventions to cure it. As Mender properly indicated, it has the potential to fuel unprecedented insights not thought of before.

The morphology of the neuron tells us that what the neuron does best, i.e. connect; each neuron is capable of connecting to hundreds of thousands of other neurons via its elaborate axonal-dendritic structures with their numerous synaptic spines. Thus the fundamental function of the brain made up of billions of neurons is **CONNECTIVITY**.

As early as the end of the 19th century Theodor Meynert (1885) addressed the relevance of connectivity to higher mental functions; he stated that

when we have a thought, an idea or an experience it is represented by activations of neuronal ensembles in our brain. Accordingly associations are interconnections forming among related ideations, i.e., neuronal groups. According to Meynert each individual has his personal experiences and thoughts, thus develops his personal brain connectivity configuration. Meynert called this individual brain connectivity organization "*Ego*." Later on Freud developed this concept unrelated to the brain and thus shifted our field away from neuroscience.

Today we know that physical systems like the brain can embed information within their connectivity organization. Hebbian dynamics (Hebb 1949; Rumelhart, & McClelland 1986) causes repeatedly activated neuronal ensembles to strengthen their interconnectivity and strongly interconnected neurons to become more active in comparison to weakly connected units. There are enough neurons and connections in the brain to form internal brain configurations of the human experience; and in effect, if we apply state-space description to such a system, we can conceptualize topological mapping that forms internal maps or representation of the outside world within the physical brain.

In a "state-space" formulation, the "state" of the system is its current instantaneous neural activation pattern, and the "space" comprises all the possible combinations of all patterns of activations. When patterns of activations are strengthened they form "attractors." Those are the patterns the system readily activates, i.e., those states that the system is readily "attracted" to, that is why they are called attractors.

"Matching Complexity" (Tononi 1996) and "Free Energy" (Friston 2007) are concepts relevant to the way the brain creates an internal model of the world, one that is concordant, flexible and adaptable with the ever-changing occurrences in the real world. "Matching Complexity" describes how the statistical configurations in patterns of inputs create statistical correlates of neural activations by forming input-related synap-

tic connectivity strengthening among neural ensembles in the brain. "Free Energy" describes Bayesian statistics in the brain responsible for ongoing reductions between internal activations of the brain and sets of input patterns. Based on these insights we can begin to understand how the brain creates and maintains a flexible updated adaptable model of reality.

Carl Rogers (1965) suggested that the best vantage point for understanding behaviour is from an "internal frame of reference" of the individual himself. He called this frame of reference the "experiential field" that encompasses the private world of the individual. According to Rogers, "organismic evaluation" is the mechanism by which a "map" (i.e., the internal configuration) of the experiential field assesses the psychological events of everyday life. Object relations psychologists talk about internal objects that create the internal reference according to which, we perceive ourselves and others. These descriptions ultimately explain how we react and behave in psychosocial contexts and thus explain our personality traits and development.

We can now begin to define personality as the result of evolving flexible ever-changing neural-networks constructs and organizations in the brain that provide for the adaptable interactions and behaviour with the environment and other behaving brains. With this definition we can now begin to approach personality disorders from a brain-related perspective.

If during development, for some reason, markedly unusual, erratic and unbalanced experiences occur, then Hebbian neural organization would be impaired and experience-dependent-plasticity processes would reflect biased experiences creating internal representations that can be markedly removed; i.e. mismatching of real-world occurrences. Such a mismatch would cause non-adaptive attitudes or responses and behaviour due to the disparity between what an individual perceives (according to his internal representations) and what is actually occurring.

At this point we see that by using the connectivity system approach, a complicated phenomenon such as per-

sonality disorders can begin to be reformulated as brain-related alterations.

Connectivity is also a dynamic phenomenon that can help explain other mental disorders such as those of mood and psychotic clinical pictures. The dynamics of connectivity can be explained with concepts such as neural-plasticity and neural-resilience, defining the changes in neural connectivity over time and the ability of each neuron to dynamically interact with other neighbouring neurons. We know that these properties are related to mood changes. Anti-depressive treatment has been found to correlate with synaptic genesis and dendritic-spin-genesis (Kapczinski et al 2008), while depression has been found to relate to neuronal death and atrophy (Yasuda et al 2009). This indicates that depression is associated with impaired neuronal resilience and reduced neural plasticity, while increase in neural plasticity and resilience is associated with anti-depressive effects (Pittenger, & Duman 2008).

The brain system as a whole is more flexible and adaptable with synaptic genesis and dendritic-spin-genesis. This flexibility enables better matching between internal and external representational constructs resulting in optimization dynamics that as an emergent property that improves mood and is antidepressant.

According to this theory any metabolic, hormonal or other factor that inhibits neuronal reliance results in reductions of flexible matching to external stimuli causing mismatch and de-optimization that emerges as depression. This is probably the mechanism of endogenous depression.

Environmental stressors typically involve some radical change of incoming external stimuli; for example the loss of a loved person (or a function) will result in the loss of external sets of stimuli that belonged to that person or function. The discrepancy is between the internal representations that still hold the missing person or function, and the actual incoming information in which the function no longer exists. This discrepancy and mismatch is expressed by de-optimization dynamics and depressed mood. With this formulation we can now explain how environment stressors trigger depression

(Peled 2008).

While plasticity and resilience dynamics occur during long periods of days and weeks, connectivity dynamics have very-fast millisecond range dynamics. Today, it is recognized that nervous systems facing complex environments have to balance two seemingly opposing requirements. They need to quickly and reliably extract important features from sensory inputs and the need to generate coherent perceptual and cognitive states allowing an organism to respond to objects and events, which present conjunctions of numerous individual features.

The need to quickly and reliably extract important sensory features is accomplished by functionally segregated (specialized) sets of neurons (e.g., those found in different cortical regions); the need to generate coherent perceptual and cognitive states is accomplished by functional integration of the activity of specialized neurons through their dynamic interactions (Tononi et al, 1994).

The mathematical concept of "neural complexity" (Tononi et al, 1994) captures the important interplay between integration (i.e., functional connectivity) and segregation (i.e., functional specialization of distinct neural subsystems). Neural complexity is low for systems whose components are characterized either by total independence or by total dependence. Neural complexity is high for systems whose components show simultaneous evidence of independence in small subsets, and increasing dependence in subsets of increasing size. Different neural groups are functionally segregated if their activities tend to be statistically independent. Conversely, groups are functionally integrated if they show a high degree of statistical dependence.

In order to adapt to the shifting paradigms required by high mental functions such as attention and working memory, it is likely that brain function requires integrative as well as segregative capabilities. The balance between integrative and segregative functions in the brain is achieved when neural complexity is optimal.

Small World Network organiza-

tion describes neighboring closely related networks that are linked by many densely connected pathways, far apart networks that have less connections and distant regions that are sparsely connected. This type of organization was termed "small world" as shown in internet web networks where this organization enables the transfer of information in relatively few steps and junctions around the globe. Small world network organization has been described for many biological systems that have multiple interacting units, including the brain with its interacting neuronal ensembles (Micheliyannis et al 2006; Liu, Y. 2008).

Perturbations and disturbances to the neural complexity and small-world network organizations result in specifically defined brain-dysfunctions and cognitive disturbances (Micheliyannis et al 2006). For example disconnection dynamics, where neuronal ensembles act in a statistically-independent manner result in fragmentation of experience and mental functions and lead to loose associations, illogical references i.e., delusions and hallucinations. Typically psychosis arises from fragmentations of neural network organizations (Yoon et al 2008; Volpe et al 2008; Williams 2008). Over-connectivity dynamics result in overly constrained information processing, reducing neural-computation, resulting in poverty of thought perseverations, and a clinical picture of deficiency, negative-symptoms as those described for residual schizophrenia (Peled 1999)

Perturbed connectivity balance may cause the system to oscillate between disconnectivity and over-connectivity dynamics as occurs in the course of schizophrenia between psychotic episodes and increased deficiency periods. These oscillations probably perturb the hierarchy by eliminating higher-levels of transmodal organizations and causing the elimination of higher-level functions such as motivation and volition (Mesulam 1998).

Using formulations of disturbed neural complexity and small worldness we can now begin and reformulate schizophrenia spectrum disorders

as disturbances in fast dynamic connectivity balances in the brain (Peled 2008).

Based on the formulations so far, 1) schizophrenia spectrum, 2) mood spectrum and 3) personality spectrum disorders can be reformulated as disturbances in 1) neural-complexity organization, 2) neural resilience optimization dynamics, and 3) connectivity constructs of internal representations, respectively.

Other clinical descriptions can result from combinations of these three disturbance patterns. For example in obsessive compulsive disorders (OCD), intrusive ideations can be described by repeated activations of neuronal ensembles representing the repeating ideations; this is a result of increased connectivity for those neuronal ensembles, but it is an increased connectivity that does not reach the extreme magnitude and extension of over-connectivity characteristic of the negative signs in schizophrenia. However the repeated activations of neuronal ensembles for OCD are enough to create a certain amount of de-optimization dynamics, to cause emergence of depression and anxiety.

Another example is Anorexia Nervosa, where delusional or perceptual ideation is limited to body representation and perception and is not extended to full blown clinical manifestation of psychosis.

In effect the combination of all of the above perturbations to brain organization participates to various extents in all forms of mental disorders. Clinical experience teaches us that it is rare to find a pure form of clinical manifestation; for example psychosis can frequently manifest with mood changes resulting in a schizoaffective clinical manifestation. It can be concluded that most mental disturbances can be defined in a 3 dimensional space of brain disturbances: that of disturbances in 1) neural-complexity organization, 2) in neural resilience optimization dynamics and 3) in connectivity constructs for context and internal representations. Such definition can be given the title of Clinical Brain Profiling (CBP) (Peled 2008).

CBP involves describing mental disorders as brain disorders and brings psychiatry back to the realm of neuro-

science where it belongs. Psychiatrists await neuroscientific discoveries before advancing and proposing a novel brain-based diagnosis for psychiatry, but by doing this they miss the opportunity to contribute to a much needed paradigm shift for psychiatric diagnosis (McHugh 2005).

Validating CBP has a groundbreaking relevance for psychiatry, not only by providing an etiologic diagnostic system, but by offering to develop effective curative interventions. Few examples involve medications that boost neuronal resilience and devices that can act as "brain pacemakers."

Experience-dependent-plasticity can be enhanced by medications that boost neuronal resilience to the extent of offering brain plasticity similar to childhood developmental plasticity. This provides unlimited possibilities to reorganize brain disturbances and effectively correct developmental disorders.

Technology of neuronal stimulation (e.g., DBS, TMS, Optogenesis) offers opportunities to intervene and control neuronal network activity in the brain; for example if hallucinations emerge from disconnectivity dynamics between the temporal cortex and the rest of the brain, then designing a reconnecting "brain pacemaker" can reconstitute the normal connectivity needed to cure the patient of hallucinations. Metaphorically, just as a cardiac pacemaker corrects cardiac arrhythmias curing heart failure, so will the brain pacemaker correct perturbations to brain organization, curing brain insufficiency, i.e., mental disorders.

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Only in the Eyes of the Beholder

Allen Frances, M.D.

Dr Peled is a clear thinker who has developed an elegant and inspiring model. In contrast, the DSM classification is messy, inconsistent, and uninspiring. Why don't we junk the DSM and get with the exciting findings from neuroscience by substituting for it a

new, improved, consistent, and rational model? The rub is that there are too many candidate models and none have proven themselves ready for prime time.

Every few months, someone (usually very smart and passionate like Dr Peled) sends me a proposal for a new diagnostic system offered as an alternative to the jumbled, pedestrian, atheoretical, and purely descriptive method used in DSM. The new system is invariably theory driven, clever, neat, and plausible. Surely, it is quite easy to be more coherent than a DSM that consists of a jumble of disorders gathered together largely through a historical accreting process based mostly on clinical observation and descriptive research—without a unifying theory or deep knowledge of causality.

The new systems come in 3 types: 1) Brain biology—these used to be based on correlates with neurotransmitters, but recently (as with Dr Peled) neural networks of various kinds are much more popular; 2) Psychological dimensions; and 3) Evolutionary psychology.

Unfortunately, none of these approaches, however interesting or promising, is remotely ready to replace or be included in the official system of psychiatric nomenclature. DSM must by its very nature be a conservative document that follows and never 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. Proponents of rival systems can make about equally valid claims for their respective pet methods. Moreover, most clinicians have absolutely no interest in any of them.

I feel sure that our clumsy descriptive classification is not the only, or optimal way, to sort things for future research. But I feel equally certain that DSM remains necessary to carry forth the current, everyday, practical clinical and administrative work that are its first priority. 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 now still useful system.

At this stage in this arena, the wisdom of the philosopher Vico trumps the much greater and better known Descartes. Descartes sought to use what we now call Cartesian rationality and mathematical order to sort what were previously seemingly disorderly phenomena. This turned out to be a screaming success in the mathematical, physical and chemical worlds, but has (as Vico predicted it would) much less purchase in understanding the sloppy complicatedness of human affairs—including psychiatric diagnosis.

Functional First: Creating a Pragmatic and Progressive Diagnostic System

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In Allen Frances's charming and telling analogy, there are three umpires playing a marathon epistemological game with the balls and strikes of diagnosis. Each sees the role differently: call them as they are, call them as I see them, and there are none until I call them. There is a fourth however. Call them in a way that advances the game.

These three umpires are not just arguing about epistemology. They are arguing about ontology: about what is real. Once umpires are drawn in a game of determining what is real, however, the game can go on virtually forever without thinking to ask or answer a central question: So what? Decades can go by, each happily named by NIH committees, and billions can be spent. In defense of this work nothing really need be said other than "we are making progress in our understanding (of what is real)." The part in parentheses is often not said aloud, but it is a powerful organizing assumption. Unfortunately "progress in understanding what is real" is far too flexible a criterion to determine whether the game is advancing or not. As a result, anything a powerful research group is studying can be put into the diagnostic system, whether or not it makes good sense and ad-

vances the discipline or its practices.

Most of science is built on elemental realist (i.e., mechanistic) assumptions: what are the preexisting parts, relations, and forces in the world and how do they go together to create complexity? That view of science gives a bear hug to ontology because finding these preexisting parts and organizing them into comprehensive models is the very definition of scientific truth. Likewise, scientific theories that are built on Platonic formism, a favorite of nosologists, are intensely focused on what is “real.” They may not have quite the expansive vision of the elemental realists since knowledge need not fit into a grand model, but they have constructed their theoretical and empirical buildings on the common sense idea contained within human language itself, that is, that we “understand” when we can consistently and unambiguously name preexisting events and their attributes. And there we are back to ontology.

What if we adopted a view of truth that was more humble, pragmatic, and local? Yes, we live in a real world (or at least the one world, whatever you may choose to call it). Fine. And in that world we have work to do. Let’s get about doing it.

This is what our patients want from us. This is what busy clinicians want from applied scientists. This is what the funders and payers want. What would happen if we as researchers and theoreticians started there as well?

If we want to empower umpires interested in advancing the game, we would have to start by specifying what the game is and what we mean by advancement. Allen Frances’s article contains a number of positive and pragmatic positions. In his view, mental disorders are social constructs that should help us treat patients successfully. They should not lend themselves to medicalizing everyday actions and issues. They should not run afoul of real world consequences in the areas of insurance, forensic issues, and medications.

This is a good list, and I agree with it. In the context of DSM-5 they make particular sense because so many pragmatic outcomes are threatened by the expansionism it contains. The DSM –5

work group [1] rightly pointed to the current situation: “All these limitations in the current diagnostic paradigm suggest that research exclusively focused on refining the DSM-defined syndromes may *never* be successful in uncovering their underlying etiologies. For that to happen, an as yet unknown paradigm shift may need to occur.” (p. xix; italics added). Unfortunately, paradigm shifts cannot be ordered up like a plate of spaghetti and the DSM-5 itself looks more like a mishmash than a course ahead.

While I like Allen Frances’s list, it does not go far enough to change directions. Pragmatism can do more than hold at bay the hubris of researchers wanting to name categories because they believe them to be real regardless of their functional impact. It can reorganize the research agenda in front of us.

Functionally speaking, what do we want? What is the game we are playing? I would argue that we need concepts and models that are empirically based, systematized, and as simple as the goals of diagnosis will allow. Concepts need to be precise, so that they can be applied with rigor, but they need to have broad scope so that a small number of key ideas apply to a range of phenomena. They need to cohere across levels of analysis, that is, they need to have depth. Nothing that is known in psychotherapy should contradict what is known in neuroscience; nothing that is known in the social basis of pathology should contract what is known in evolutionary biology. Indeed, we need concepts that actively support productive research across levels of analysis. They need to be demonstrably useful in organizing the complexity of human suffering into treatment and prevention responsive units. They need to facilitate discovery of the biopsychosocial processes of change that functionally explain the etiology and course of disorders and the processes responsible for treatment outcomes. They need to lead us toward new treatment methods that maximize benefits and minimize costs and side effects. In summary, the scientific game of diagnosis is to develop concepts with precision, scope, and

depth that help us over time in identifying etiology and course, and the moderators and mediators of treatment impact, and that help us innovate in treatment and prevention.

In the history of medicine, purely descriptive approaches have bogged down when core processes can give rise to an array of outcomes, when processes interact in a complex and systemic fashion, or when a single outcome can be produced by a wide variety of processes and their interactions. After decades of effort in the modern era, it appears more and more likely that this is the situation we face in the mental health disciplines. Pragmatism provides another way forward. If we are willing to let “truth” refer to how we well are playing the game *and nothing else* we can respectfully leave ontological claims behind, since they add nothing functional beyond what we already know, pragmatically speaking.

One of the best ways to play the game pragmatically is to cheat. Let me explain. My young son likes to play mazes. He’s quite good at it, but he can get bogged down. When he does he’s recently started cheating in a creative way: he puts his little finger at the end of the maze and works backward.

If we stay focused on the game we are playing we can cheat in the same way. There are a growing number of transdiagnostic processes that are *already* known to help explain the development and course of multiple problems currently viewed as distinct disorders [2]. Some have already been shown to function as moderators or even mediators of treatment outcome. Examples include memory processes such as selective memory or over general memory; attentional processes such as selective attention, or self-focused attention; emotional regulation processes such as thought suppression, behavioral escape, or experiential avoidance; and cognitive processes such as interpreting ambiguous stimuli, or rumination.

Especially as these become systematized, explained, and clustered, they can provide processes of some known importance—functional end points—that can then be traced back to help develop functional dimen-

sional diagnostic systems. Instead of starting with lists of signs and symptoms in hopes that functional processes will emerge, we can start closer to the end of the maze now, identifying functional processes we now know about, and then backing up into the biological and psychological understanding of these processes.

If I were to nominate a single example of what I mean it would be experiential avoidance. It is a concept from my own research program, which I apologize for, but the science is fairly well developed. Experiential avoidance refers to efforts to alter the frequency or form of unwanted private experiences, including thoughts, memories, emotions, and bodily sensations, even when doing so causes personal harm [3]. The attempt to suppress or avoid difficult emotions, bodily sensations, sensory experiences, memories, or thoughts, is an incredibly toxic process. There is a vast and growing literature that suggests the harmful impact of many if not most of the symptoms commonly focused on in our diagnostic system comes not from their presence but from the avoidant responses they evoke. For example, experiential avoidance does a better job predicting who will develop chronic pain and be disabled by it than does pain intensity or the degree of injury [4, 5, 6]. Experiential avoidance does more to determine whether traumatic experiences such as sexual abuse [7, 8], combat violence [9], or interpersonal violence [10] leads to PTSD than does the severity of the trauma. Over 25% of the variance in depression and quality of life for persons suffering from command hallucinations despite medication compliance is determined by avoidant coping applied to the voices [11]. Prospective studies [12] show that depressive symptoms are more likely to emerge from stressors in those who are experientially avoidant.

We could continue with a listing like this for quite a while because experiential avoidance is associated with an amazingly broad array of mental and even some physical health outcomes, accounting for 16 to 25% of the variance in most behavioral health areas [13]. We know that experiential avoidance is key to treatment and is modifiable because it moderates [14] and me-

diates [13] treatment outcome. Neurobiological studies shown that people who are highly experientially avoidant respond to aversive stimuli in a more lateralized way [15], suggesting that this is in part a verbally / cognitively mediated process.

Experiential avoidance is a good example, but I'm making a more general point. We can today reduce a wide variety of topographically defined problems into a much smaller set of known functional processes. Such processes right now can provide demonstrably more information about course and response to treatment than our current diagnostic categories. They seem far likely to be a useful focus of attention for studies of the etiology and underlying neurobiology of mental problems.

The point is not to create new categories such as "experiential avoidance disorders" and the like. Rather my point is that if diagnosis is going to be progressive we need to do more to lay the foundation for a functional dimensional diagnostic system. Dimensional systems built on issues of severity, chronicity, topography, broad personality styles, and the like will either quickly morph into incredible complexity or be yet another topographical cul de sac.

Diagnostic distinctions that focus on what is "really there" while forgetting functionality are like trying to determine the perfect ball without throwing it. Pragmatism can help correct that error because pragmatism contains within it a call for a different kind of science. We can build an alternative approach based on high quality concepts (i.e., those with precision, scope, and depth) that tell us some of what we want to know *now*. For example, while detailed knowledge of etiology appears to be in the distance, we can reasonably insist that researchers provide us with concepts and distinctions that link tightly to the course of mental health problems and to the moderators and mediators of treatment. By demanding functionality first, we can establish a pragmatic filter that will keep the umpire's eyes on what advances the game.

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Forced Choice: Be Pragmatic OR Progressive- Usually Hard To Be Both

Allen Frances, M.D.

Dr Hayes and I are both practical people and agree on everything conceptual. We both favor "a view of truth that is more humble, pragmatic, and local?". Where we disagree is on the practical question of what to do next with the diagnostic classification. Dr Hayes sees all the impracticalities of the current system and suggests a much more elegant alternative. "I would argue that we need concepts and models that are empirically based, systematized, and as simple as the goals of diagnosis will allow".

I agree in principle, but I also see all the impracticalities of changing systems and also the failure so far of alternative models (however attractive) to

gain wide acceptance. Dr Hayes and I part company immediately with his subtitle "Creating a Pragmatic and Progressive Diagnostic System." I regard this as a contradiction in terms. For me, an official diagnostic system that is pragmatic can never expect simultaneously to also be progressive. An official nomenclature must follow the field, it cannot possibly lead it. It must be the culmination of consensus. It cannot a new research agenda to "advance the game." The game has to be advanced on its own steam until it is gets included by general consensus.

This is the same issue discussed previously in my response to Dr Pelled who was suggesting his own elegant model of a very different sort. My remarks there apply here as well.

Weighing the Evidence and Rendering Judgment on the DSM: Do We Need a Supreme Court?

Douglas Porter, M.D.
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Reading through the variety of responses to proposed changes in the DSM contained in the recent bulletin of the AAPP, I was struck by the reality and depth of the discord on how we should proceed. Even if we all agreed on the scientific validity of pertinent empirical claims, and I don't think that we do, we would still disagree about what those scientific facts mean for a nosology of mental disorders. This is because of our differing theoretical assumptions about what a nosology should accomplish. I believe this is why some authors could confidently claim that "science" clearly dictates a conservative approach and other authors could just as confidently claim that "science" clearly dictates a liberal approach to change in the DSM. Perhaps it is the belief that the scientific facts should be clear-cut and overwhelmingly persuasive that leads to an expectation of consensus in scien-

tific matters. But, given the reasonable room for disagreement about the proper goals of nosology, it should come as no surprise that, when it comes to the DSM, instead of "science" and consensus, we have a plurality of "sciences" and dissensus. While consensus would have deemphasized the importance of the political process used to determine whether or not to make changes in the current DSM, the reality of discord highlights the importance of this process. We may all agree on the importance of an "open process" for the objectivity of our science. But, at some point, if we are to make a decision on whether to proceed with changes in the DSM, the process must become closed. Just how the process becomes closed is a matter of no small importance. I think that instead of lamenting the intrusion of politics into the science of nosology, it is more productive to carefully consider just what kind of politics will guide the development of nosology. Will the political process favor the arbitrary assertion of power or a fair hearing of conflicting arguments? Perhaps if, after a requisite period of due process, intractable and significant conflict of opinion persists: a "supreme court" of the DSM should render decisive judgment.

I am afraid that I speak of such a "supreme court" only half jokingly. While the donning of long robes may be considered optional, conceptualizing the criteria for such a court does provide further opportunity for reflection on criteria for fair judgment in these matters. Allen Frances notes that Work Group members are "too attached...to be objective" about the risks of their suggestions. It appears that a degree of impartiality is required to judge fairly about proposed changes. It may be impossible to make atheoretical observations, but it does seem only fair that judges should not be so prejudiced by theoretical commitment as to be unable to hear arguments that stem from contrasting theoretical assumptions. While fairness may require a sense of impartiality when it comes to theoretical assumptions, it requires a high degree of partiality in another important sense. Insofar as the supreme court of the DSM is sworn to uphold the constitution of the American Psy-

chiatric Association then it is sworn to "promote the best interests of patients and those actually or potentially making use of mental health services". *

There is an ever present danger that the practical significance of judgments regarding nosology for the concrete lived reality of vulnerable patient populations will be lost in the abstractions of scientific argument. That the vulnerability of patient populations demands the prioritization of patient interests in practice has long been recognized to form the cornerstone of medical ethics. The vulnerability and concomitant ethical demand spill over into the development of a nosology of mental disorders. Interpreting the best interests of patients points to the difficulties inherent in a practical scientific task such as developing a nosology. Certainly patients have an important and irreplaceable level of expertise in determining their own interests. But complications arise because we are determining those interests in light of scientific evidence. This requires a level of scientific expertise, but one that should never become detached from the first person experience of illness that grounds the meaning of the entire endeavor.

So, there you have it. The composition of the court requires, if not first-hand experience of mental illness, at the very least sensitivity to the distress caused by such illness and a commitment to prioritize a concern for this distress in any decisions rendered. The composition also requires a level of scientific expertise such that the merit of various technical arguments may be measured. The merits of scientific arguments should be judged without undue prejudices toward particular theoretical orientations but with all due prejudice toward any practical repercussions various theoretical assumptions may have upon patient welfare. One thing is for certain. Should the justices decide to don long robes, they will certainly have earned them.

* American Psychiatric Association 1998. *Constitution and Bylaws*. Washington DC: American Psychiatric Association as referenced in Sadler, J (2005). *Values and Psychiatric Diagnosis*. Oxford University Press, Oxford: p. 381.

Yes Surely, More Now Than Ever

Allen Frances, M.D.

Dr Porter raises three crucial points: 1) there is disagreement on the strength of the science supporting the suggested DSM-5 changes and also on how much weight to give to other factors and risks that haven't been (or can't be) scientifically measured; 2) there is no psychiatric Supreme Court for the final adjudication of differences of opinion; and, 3) the best interests of our patients should always come first.

At the risk of getting into the annoying "this is how we did it" routine, there are some useful lessons from past DSMs. The first has to do with monitoring and governance. Realizing that workgroup members always overvalue the science supporting their pet suggestions, we developed an obstacle course to curb their enthusiasms. First off, we conducted a series of methods conferences to train everyone in a "consensus scholar" method of doing literature reviews that was meant to reduce biases and result in as thorough an evaluation of risks as of benefits. Next, the DSM-IV Task Force was encouraged to shoot down all low flying work group suggestions. The DSM-IV leadership was ever alert to the risks in new diagnoses and eagerly disposed to find their fatal flaws. Then there was a deep and expert external reporting chain. The DSM-IV Task Force reported to the Committee on Diagnosis and Assessment, which reported to the Research Council, which reported to the APA Trustees. The process all along the way was completely transparent and open to wide interchange and correction from a large circle of advisors and from the field at large.

DSM-5 took the opposite (and much more dangerous) path in every regard. Work groups were encouraged to innovate, with no guidance on how to do literature reviews and risk/benefit analysts and little or no supervision from the Task Force or DSM-5 leadership. The Task Force until recently reported directly to the Trus-

tees, with no intermediary groups that are more expert in psychiatric classification. In effect the work groups posted raw, unedited, unmoved suggestions that often present a biased and overvalued interpretation of a very limited and equinox science base. And they have in many cases been inhibited by confidentiality agreements and by working in relative isolation from the field, with input only from a very small circle of like minded advisers. The products are predictably problematic.

We desperately need a Supreme Court now only because there have been no lower courts to provide review along the way. Dr Porter does not indicate how the "Supreme Court" should be constituted. There are no easy answers. I am close to losing faith in the American Psychiatric Association as the final arbiter of the DSM system. Their governance and quality control mechanisms have so far failed badly and show no signs of self correcting even now. It seems likely that future DSM's should be prepared under the auspices of the NIMH- but with extensive input from clinicians to balance the likely bias of a research institution.

As Dr Porter elegantly demonstrates from the words of the APA constitution, it is the patients who count most when making decisions about the diagnostic system. And to quote his own wise words, "There is an ever present danger that the practical significance of judgments regarding nosology for the concrete lived reality of vulnerable patient populations will be lost in the abstractions of scientific argument. That the vulnerability of patient populations demands the prioritization of patient interests in practice has long been recognized to form the cornerstone of medical ethics," I have heard the following disturbing statement repeated numerous times in one form or another by people working on DSM-5—"I am following the science, I can't predict or be responsible for possible misuse of my suggestions." This statement is epistemologically naïve and medically irresponsible. The "science" of diagnosis is far too weak and equivocal to lead anyone anywhere unless he starts with a preconceived notion of where he wants to go. The risks to patients, though impossible to

quantify, are often far more tangible and should settle all the many scientific tie scores in favor of the caution: Do No Harm.

Who's on First? Mental Disorders by Any Other Name?

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In response to Allen Frances' *DSM in Philosophyland: Curiouser and Curiouser*, we agree that diagnostic classification must steer between the Scylla of naïve biological realism and the Charybdis of social constructionism, or alternatively, logical empiricism and post-modernism (Frances' First and Third Umpires). But in what way does Frances' pragmatic compromise (his Second Umpire) provide a solution? Perhaps merely asserting that psychiatrists should be driven by consensus, reliability in diagnosis, or a common language ("calling them as I see them") is not enough. The pragmatic definition of mental disorders ("forging a common language rather than a common truth") as "what clinicians treat" invites circularity. After all, it still begs the essential question, what kind of entities are mental disorders? As Mishara (1994) argued, to the extent that DSM-III and the following DSMs base their putatively reliable descriptions of mental disorders on everyday language, then folk psychological and other kinds of assumptions, including *metaphysical* assumptions, creep into the classification system. In their neo-Kraepelinian zeal for reliable diagnosis, DSM-III advocates (Umpire I) had overlooked that the Hempelian approach they adopted was only one approach that neglected more phenomenologic approaches (Schwartz and Wiggins, 1987), e.g., Jaspers, Conrad and Ey (see below).

Even Gerald Klerman, "the highest-ranking psychiatrist in the federal government at the time," who had at

first appraised the movement from the DSM-I and II to the DSM-III as a "victory for science," later revised his view that DSM-III was largely "a political document" (cited by Mayes and Horwitz (2005)). That is, by adopting Hempel's logical empirical approach to science, the neo-Kraepelinians' presumable "revolution" in conceptualizing and classifying mental disorders actually pre-empted alternative approaches, which were philosophically informed, but in a manner different than Hempel, that is, the German tradition of philosophic phenomenology. In fact, the German phenomenologic psychiatrist, Jaspers (1963) had written that to the extent that psychiatry ignores philosophy, it is inevitable undone by it in one way or another.

For example, although DSM-III and the later DSMs ultimately rely on the patient's reports of their own subjective experience of symptoms and the clinician's observations of signs that the patient may not directly experience, there is little or no effort in DSM to formalize and/or operationalize subjective experience itself. Despite this lack of precise conceptual relationship to what it presumably and ultimately targets (the patient's subjective experience, i.e., suffering in self and/or others), DSM-III and its successors pose the dangers of a "hegemony" (Schwartz and Wiggins, 2002), a co-opting of clinical practice and clinical research such that research grants, publications, conference presentations, insurance reimbursement and the like are compelled to make use of reliable DSM diagnoses (despite DSM's own initial caveats that the categories are only provisional and therefore, still lack conceptual foundation). Nevertheless, for reasons that provoke speculation — loss of credibility in courtrooms, insurance companies and the Halls of Government? Fear of "anti-psychiatric" or like-minded "philosophic" critics? Credulity? — many of DSM-5's adherents currently posture that all questions about conceptual foundations have already been discussed, solved and put to rest. Their "naïve realism" permits their leapfrogging over major conceptual problems, including the problem

of the human person, the mind body problem, the so-called hard problem of how to map conscious experience (qualia) onto underlying putative neurobiologic mechanisms (see Mishara, 2009 for one approach to this problem), and finally the definition/reification of mental disorders (as entities, types, dimensions, etc.). These DSM-5 protagonists employ what is essentially a rhetorical strategy to obviate any conceptual criticism by posing (falsely) that this preliminary work is already done (as in the DSM-5 reluctance to develop a Conceptual Issues Work Group proposed by Kendler et al., 2008, and see below). In contrast, when an earlier work group put together a volume on the conceptual issues involved in diagnostic classification (Sadler et al., 1994), Allen Frances (Chair of the DSM-IV Task Force) graciously agreed to write the preface. He welcomed the volume by stating that it is to be "congratulated by having assembled an especially comprehensive probing analysis of the theoretical issues that inform psychiatric classification... DSM-IV is a manual of mental disorders but it is by no means clear just what is a mental disorder and whether one can develop a set of definitional criteria to guide inclusionary and exclusionary decisions for the manual...The failure of definition of a mental disorder or disease which reduces to what clinicians treat is tautological and potentially self-serving is probably the best of a bad lot of ways of defining these necessarily imprecise terms" (p. viii-ix). We are delighted by Allen Frances' conceptual openness to the issues but think there may be more productive ways of philosophically *grounding* how to proceed in the systematic phenomenology of the patient's experience. Even if we are wrong, others may suggest better ways and we would not want to close the door.

One problem is that rather than bridging clinical practice and clinical research, DSM-III's logical empiricist agenda inserted a wedge between clinician and clinical researcher which still has not been appropriately addressed. In their historical analysis, Mayes and Horwitz (2005) write: "Spitzer selected a group of psychia-

trists and consultant psychologists who were committed primarily to medically oriented, diagnostic research and not to clinical practice" That is, there appears to be a divide between DSM-III and later DSM's prescriptive diagnostic practices for the researchers and what the clinician actually does in practice. At the very least, the DSM's following DSM-III have been burdened by a dual role: to serve as both the standard for clinical researchers who attempt to explain the disorders (find and treat the underlying mechanisms) and for clinicians who attempt to understand and treat the persons suffering from the disorder.

Schwartz and Wiggins (1987) had argued that clinicians in their practice use a different approach than that outlined by the neo-Kraepelinian embrace of Hempelian nomological science: the clinician's experience is already pervaded by typifications which help to structure the clinician's diagnosis meaningfully. In fact, Husserl had indicated that perceptual meaning is itself based on such a typification process: That is, we never perceive the individual themselves but always in terms of the type that implicitly subsumes it. We perceive the not yet known in terms of the known, i.e., in terms of the general type that is activated in the particular perception. With each view, there is built a reference to the next anticipated view based on past experience of this and similar objects. The references between aspects are anticipatory constraints, which are nevertheless open to revision or cancellation in their structure so that each aspect prefigures its successor in seamless transition as belonging to the same perceptual object (for Husserl's concept of "type" as pervading the perception of both things and persons, see Uhlhaas and Mishara, 2007).

The two phenomenological psychiatrists, Klaus Conrad (1958) and Henri Ey (1978), employed the nineteenth century neurologist, Hughlings Jackson's approach to classification in terms of describing and formalizing the subjective experience of the patient as a field of consciousness which is disrupted in its organizing activity precisely in response to the degree of severity of the underlying neurobiologic disturbance. Therefore, Klaus Conrad

compares the disruption of psychosis to the organization of conscious experience to dreaming: "In sleep, there is radical dismantling (*Abbau*) of higher functional levels... this characterizes the negative side of the phenomenon. At the same time, we find in dreaming the expression of the release of deeper ...levels of functioning as positive symptoms of this occurrence." However, psychosis is not simply a form of dreaming or sleep. Rather, "the difference lies in how quickly the everyday meanings are broken down, i.e., the tempo of their dismantling. In psychosis, the dismantling is incomplete. The components or aspects of the field of consciousness are not impacted so uniformly and radically as in sleep... [Referring to his French contemporary and phenomenological psychiatrist friend, Henry Ey] Without previous knowledge of Henri Ey's efforts, I repeatedly referred to the dream as a typical state of protopathic transformation of Gestalt-meaning of the total mental field." (1953, our trans). Indeed, the similarities between Conrad's and Ey's work are noteworthy. Ey's classification of the different mental disorders according to the *depth* of "dissolution" (Jackson) of consciousness resembled Conrad's own efforts to describe psychosis as a progressive "deformation" of the field of conscious experience, which affects greater and greater portions of this field depending on the severity and/or course of the disturbance. Conrad (1958) finds that the Jacksonian "release" of pathological behaviors of the earlier stage of perceptual-meaning cannot be said to be merely a component of the later process but is its own productive or positive transformation. (Since the phenomenologic application of the Jacksonian concepts is relatively complicated, and not well known, we provide a more detailed description of this approach and its relevance for diagnostic classification in future publications). Following these efforts, Mishara (1994) proposed that psychiatric classification could be reconceptualized in terms of the patient's subjective experience of the disorder according to what extent the phenome-

nological categories of space, time, embodiment, intersubjectivity and self are disrupted (see the discussion of the current impact of social neuroscience in both clinical neuroscience and psychiatry, below).

However, we would like to ask whether current diagnostic classificational efforts may be too precipitous in still other directions than those outlined by Dr. Frances? To the extent that we allow current genetic results, results from neuroscience in so-called bio-markers to organize our descriptive classification, we have the danger of allowing this to replace the phenomenology of the patient's subjective experience.

Now we would like to raise the following questions: is it possible that you, Dr. Frances, have not gone far enough in your critique on two matters (ones that we suggested in our contributions to the "Philosophic Perspectives" book (Sadler et al., 1994)):

- 1) precipitous optimism with regard to finding biologic markers for disorders which would pre-empt or at least guide our descriptive classifications
- 2) the continued neglect of the systematic study of the patient's subjective experience to guide both our classificatory systems and our measurement of treatment outcome

For example, the clinical researcher, Mary Phillips (see First, 2006) proposes a "psychiatric toolbox" (i.e., neuropsychological tests, neuroimaging, genotyping) to develop disorder "biomarkers" that are persistent, rather than state-dependent. This would obviate the phenomenological research of the patient's subjective experience of the disorder. The danger will be, however, that we will define disorders in terms of what technologies we have available. A problem with clinical researchers defining the categories that they have themselves helped to validate has the inevitable by-product that even the best scientists are nevertheless still guided by their human bias to promote what they most believe (or the real need to provide positive findings, or at least positive pilot data, for future grant funding). As a result find-

ings are often interpreted over-optimistically (see Farah, 2005). Neuroimaging studies, which merely compare statistical differences of activation of certain regions of interest when samples from clinical populations and healthy individuals perform the same cognitive task, overlook a host of methodological problems (Mishara, 2007). These include the fact that the blood oxygenation level-dependent (BOLD) effect in fMRI does not, *without absolute quantification* of resting metabolic activity, reveal anything about resting perfusion or whole-brain activity which nevertheless may differ in clinical populations. Moreover, the two groups may be employing different “strategies” during task. That is, to claim that our “tool box” is so advanced that we can replace descriptive psychopathology with cognitive measures is precipitous.

On the other hand, recent findings in the rapidly developing field of social neuroscience support the view that the human brain evolved to be a “social brain.” This approach confirms the previously philosophic phenomenological view that the human brain or mind never works in isolation, i.e., as an isolated Cartesian subject, but in relation to others (in what some recent researchers call the “grounding problem,” i.e., how to ground cognition in terms of an embodied self that is already intersubjective, or a so-called “two-person” psychology). To the extent that neuroscience, descriptive phenomenologic psychopathology, and diagnostic classification work together in the future, it is possible that many of our current diagnostic categories will have to be reconceptualized as disorders of an embodied, intersubjective human self embedded in interactions as social agent.

Despite Allen Frances’ concern that DSM-5 may promote “a wholesale medicalization of everyday incapacity” that could have real-world consequences in how disorders are diagnosed, Kendell (2000) expresses a view that could lead to such proliferation: “If physical disorders are so common, there is no reason why psychiatric disorders should not be equally common and it would be a big mistake both scientifically and politically, to change

our definitions in order to reduce their apparent prevalence” (p. 6). Similarly, Kendell writes, “The only reason most mental disorders are still defined by their clinical syndromes is that the human brain is infinitely more complex machine with a much wider range of functions than the heart, kidney, or liver” (p. 13). But is this the only reason? Have we as neuropsychiatric researchers or neuroscientists overcome the explanatory gap proposed by philosophers? Is there not something of the explanatory gap in those mental disorders in which we have not as yet found unequivocal etiologies? Our current medical model views illness in terms of underlying physiological processes, not acknowledging that they are inseparable from the person’s life. *The symptom indicates both organic change but also what this means for the person.* These are mutually exclusive, but also inseparable processes (v. Weizsäcker, 1950). Insofar as the embodied human brain clearly mediates experience and functioning and remains the substrate of our interventions, our target remains the subjective experience of the embodied individual who experiences these symptoms, deficits, anosognosia (lack of insight), etc.

With regard to the problem of what we are classifying when we classify mental disorders, phenomenology “brackets” ontological claims about the reality of entities while examining the meaning that contributes to their construction. That is, the phenomenologic approach requires that we must be sure that our operational constructs actually capture what they claim to in the patient’s experience of symptoms or the clinician’s descriptions of the signs of a disorder before being operationalized/quantified for their further scientific study.

To return to Dr. Frances’ theme, how can we put all of this together in a format that is valid yet sufficiently “pragmatic” for myriad psychiatric purposes: for the interests of our patients, their families, society at large, clinicians, researchers, the courtrooms, and other third parties? To achieve this goal, we return to Hughlings Jackson, medical researcher and

clinician par excellence. Working with the categorical diagnoses of his day, Jackson was able to appreciate their utility in his daily clinical work, while at the same time he could see their limitations when it came to scientific investigations. His proposed solution was a two-tiered system for diagnosis – with one tier reserved for clinical practice and a second for research: “There are two kinds of classification of diseases: one scientific, generally called theoretical, for the advancement of knowledge; one empirical or clinical, for practice” (Hughlings Jackson 1879, p. 33). Interestingly, for much the same reason, DSM-III pioneer George Saslow (Kanfer and Saslow 1969) had unsuccessfully advocated for an analogous solution for psychiatric classification. In this regard, it is also helpful to remember that the original criteria used as the initial basis for the specified diagnostic criteria for the major diagnostic categories of DSM-III were regarded exclusively as “research diagnostic criteria” (RDCs; see, for example, Williams and Spitzer, 1982). Our inclination, given the current state of psychiatry, is to view such a dual tiered framework as an advance. Perhaps there are better ways, but surely the topic commands interest. Grist for the DSM-5 (or 6, or ICD-12 or 13) Conceptual Issues Work Group, already proposed by Ken Kendler et al. (2008) and others – a proposal that has so far fallen on deaf ears. This absence of response hardly exhibits the “openness” for discussion and debate supposedly tendered by DSM-5 enthusiasts. Alternatively, we propose that using the patient’s subjective experience of “symptoms” as standard, there should be ongoing studies of bidirectional feedback between clinical practice and the diagnostic classifications operationalized by researchers to further refine these classifications. We will demonstrate how this is done concretely in future publications.

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Phenomenology vs Operationalism

Allen Frances, M.D.

I am certainly no expert on Jaspers's, but my vague and uninformed memory is that his phenomenological approach doesn't lend itself very well to Hempelian operationalism. Human experience is so personal and ideographic that it is hard to reduce to nomothetic categories. Following Jaspers can help us be sensitive to fine and individualized distinctions, but it is not clear to me how these could become the basis of a diagnos-

tic system. Perhaps Drs Mishara and Schwarz could do a reply to this reply spelling out in more detail what they have in mind. I know that they plan to present their thoughts in future papers, but a bit of a teaser here would be nice.

The tautological circularity of my half joking definition of mental disorders as "what clinicians treat and researchers research" is indeed an absurdity and an embarrassment, but I have not yet seen or thought up a better definition.

I agree that the "hegemony" of DSM-IV has unduly co-opted psychiatric research. Granting agencies and journals should be open to other diagnostic approaches. But I think DSM-IV is an appropriate tool for clinical and administrative purposes. The Introduction to DSM-IV tries to undercut reification, but few people read it and follow this advice. In my reply to Dr Phillips, I take up the prototype/criteria set issue.

People probably expect too much from the DSM's. DSM-III was directed at what was then the major problem facing psychiatry—the inability to deliver reliable diagnoses. It helped to greatly improve reliability in research settings and probably improved reliability (although much less so) in clinical settings as well. We don't have a basis at this point to expect more from our diagnostic classification than that it be reasonably reliable, reasonably representative, and reasonably safe and useful. DSM-IV meets these minimal goals.

I was never a first umpire and instead always had a healthy skepticism that biological psychiatry was being oversold. But the last twenty years of psychiatric (and more generally of all medical) research has been disappointing beyond anyone's expectations. We are learning amazing things about the normal genome and the normal brain, but very little about the mechanisms that underlie psychopathology. Everything is much more complicated than anticipated and there are no clear breakthroughs in the pipeline. I take this as a reason to avoid dramatic changes in the descriptive classification until we know much about the basic pathways.

The lack of a reasoned conceptual

base is the original source of DSM-5 problems—then this bad start was exacerbated by excessive ambition, secrecy, and sloppy methods. Rather than the several-year futile exercise of attempting prematurely to establish biological markers, the DSM leadership should have sponsored a freewheeling discussion of the appropriate goals of the diagnostic system and the means available to meet them. Without the clarity that could have come from a conceptual work group, DSM 5 has often been flying pretty much blind and usually in the wrong direction.

Final Comment:

Phenomenology and Operationalism: Not Opposites but Mutually in Need of One Another

Aaron Mishara, Ph.D., Psy.D.
Michael A. Schwartz, M.D

Dr. Frances, thank you for your kind and thoughtful response, entitled, “Phenomenology vs. Operationalism.” In this reply, we take this opportunity to clarify some frequent misunderstandings about phenomenology’s contribution (at least, our view) to diagnostic classification. In fact, our argument will be that the phenomenology is not the antithesis to operationalism but precisely the step required to translate the patient’s subjective experience of symptoms, etc., into workable *operationalizable* hypotheses which can be quantifiably measured using the experimental methods of clinical neuroscience (see Mishara, 2007). That is, qualitative description and quantitative measurement are not opposites but are ultimately inseparable (von Weizsäcker, 1950a), working together in both clinical judgment (e.g., the patient’s depressed mood is *more* or *less* severe) and in its scientific study (e.g., *operationalizing* this “more or less” into a clinical rating scale).

By appealing to phenomenological psychiatrists such as Jaspers, Conrad or Ey, we are not referring to the idiographic-nomothetic opposition which comes precisely with the *realist* assumptions of the nomologic approach.

Rather, as in our previous contribution, we paraphrased Husserl (founder of the phenomenological approach), the perceived individual (whether object or person) is already perceived in terms of a type which subsumes the perception as a meaningful unit. More recent experimental findings have demonstrated the rapidity and automaticity of these processes in decision-making and everyday social cognition.

There is of course a certain way that phenomenologic psychiatric approaches have traditionally emphasized the importance of the patient’s “uniqueness” in the “encounter” between patient and doctor, i.e., the patient’s person as a “thou” *transcends* each of the clinician’s provisional diagnostic judgments. As we wrote in our previous contribution: “*The symptom indicates both organic change but also what this means for the person* [as] mutually exclusive, but also inseparable processes” (von Weizsäcker, 1950b). This call for the clinician’s empathy in taking into account in each case the patient’s narrative history, i.e., ways of making sense of, coping with the symptoms, may be the reason that Dr. Frances and certainly others misperceive phenomenology’s main contribution as accenting the individual, as concerned only with the idiographic, without seeing that phenomenology systematically provides a bridge between subjective experience and more general operationalizable hypotheses about mental disorder. In this brief reply, we indicate two ways that the nomothetic-idiographic opposition does not apply to phenomenology’s contribution to the current debate: the first concerns Jaspers; the second concerns Conrad, Ey and other like-minded phenomenologic psychiatrists who were more influenced by Husserlian phenomenology than Jaspers.

1) Jaspers is often cited for applying explanation-understanding opposition (having achieved its heyday during Jaspers’ time) to psychiatry. This does not mean that he remained restricted to Windelbrand’s well-known idiographic-nomothetic distinction, which he overcomes precisely in

his use of ideal types. Here, Jaspers describes three domains of mental disorder:

I. Somatic processes. These include mental disorders in which we know the etiology unequivocally. General paresis of the insane (GPI) is a good example. Once we learned the cause (i.e., syphilis infection), the clinical phenomenology reduces in importance.

II. Unfortunately, the GPI model (biologic reductionism) has not worked for most psychiatric disorders, where the etiology continues to remain unclear. Therefore, Kendell’s (2000) pronouncement remains wishful, “The only reason most mental disorders are still defined by their clinical syndromes is that the human brain is infinitely more complex machine with a much wider range of functions than the heart, kidney, or liver” (p. 13). But this view that all disorders, as we have defined them, will resolve as diseases with unequivocal biologic causes remains at best a guiding “idea,” or what Kant called a “regulative idea.” As Jaspers (1963) puts it: “We have classes of disease in mind although their definitive causes and nature are not known, but in fact one is always confined to types” (Max Weber’s types) (see Schwartz and Wiggins, 1987). Here, the notion of “ideal type” provides a certain flexibility in the interplay between clinician’s diagnosis and its operationalization in scientific study which we hinted at in our previous contribution, elaborate below, and discuss further in future publications. We will merely emphasize here that conceiving psychiatric diagnoses as “types” provides psychiatry with a flexible nomenclature that is descriptive and operationalizable yet at the same time far more open to broad-ranging inquiry – hence far more “atheoretical” – than are the descriptive diagnostic categories of DSM-III and IV. Such openness allows us to move much more adeptly between the two levels of clinical practice and clinical research (described by Hughlings Jackson and outlined in our previ-

ous contribution).

III. Variations of human life that are far from the average and qualify for treatment because they cause psychic distress.

2) There is a way that the more Husserlian minded phenomenologic psychiatrists, Conrad, Ey but certainly others go beyond Jaspers on this point. These phenomenological psychiatrists anticipated the kind of modeling of mental disorders later done by neural networks, animal models, and drug challenge studies with healthy individuals in the following sense. If we are able to describe and verbally capture everyday "healthy" consciousness in terms of its "field" organization, then we are able to model disorders by seeing how this "field of consciousness" is specifically disrupted in a particular mental disorder (as we currently classify it) by disabling this or that component of consciousness. To elaborate on this analogy. Neural network models, for example, simulate mental disorders by "damaging" this or that part of the network. Similarly, animal models lesion a crucial part of circuitry and drug models alter neurotransmitter signaling. In each case, the mental disorder is "modeled" by systematically removing or altering some aspect of healthy functioning thought to be implicated in the disorder. Similarly, these phenomenological psychiatrists begin with healthy waking consciousness and by "damaging" or "removing" healthy components of this consciousness (in as it were introspective, phenomenologic thought experiments, what Husserl called "imaginative variation"), attempt to produce the subjective experience of symptoms until they arrive at a plausible model. In this way, we suggested in our previous contribution, "Who's on First? Mental Disorders by Any Other Name,?" that both Conrad and Ey apply a Jacksonian hierarchical approach to nervous functioning in the organization of the patient's "field of consciousness." "Psychosis is not simply a form of dreaming or sleep. Rather, the difference lies in how quickly the everyday meanings are broken down, i.e., the tempo of their dismantling. In psychosis, the dismantling is incomplete. The components or aspects of the field of consciousness are not impacted so uniformly and radically as in

sleep..." (from Conrad, 1953, our trans). As we will demonstrate in a subsequent publication, it is possible, following Conrad and Ey, to phenomenologically model many aspects of the subjective experience of schizophrenia by "disabling" the subject's "intentional mental activity" in the same way that it becomes reduced while sleeping or dreaming, and also through providing hypotheses for their neuroscientific study. We emphasize again that such modeling (as any other modeling) does not replace the operationalization required for experimental research. Rather, it generates useful hypotheses for this research (Mishara, 2007). We hope in this and other contributions to indicate the power of Jaspers' vista on psychiatric nosology. Furthermore, we hope to indicate how an appreciation of the phenomenology of the patient's subjective experiences can contribute to diagnostic classification through proposing *provisional* classifications of this experience

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Miscellany Past and Present

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This second cycle of discussions of Allen Frances/DSM-5 developments is another good opportunity because of several coincident opportunities that were not available (or quite as visible) at the time of the AAPP Bulletin original solicitation: most pointedly the DSM-5 draft criteria, a series of literature reviews in various areas address DSM-5 proposed changes, and remarkably, an explosion of discussion in the *Psychiatric Times* online and print editions, as well as Medscape online and (as well as others I haven't yet found), about DSM-5. Thanks to Jim Phillips for the opportunity to discuss DSM-5 and Allen Frances' critiques again. My apologies for those AAPP members who do not find the DSM-5 as endlessly fascinating as I do (there are many of you) and are already fed up with DSM-5. You'll have to suffer along at least until 2013, and perhaps longer!

I would like to organize my points into separate topical sections. Some are new issues/questions and some revisit earlier topics.

1. Against politicization. I would like to speak out against the (unhelpful) politicization of the DSM-5 discussion. The politicization is symbolized neatly by some of my friends' characterization of "conservatism" and "liberalism" in characterizing DSM-5 critiques, but extends elsewhere in the emerging debate. For AAPP and philosophers of psychiatry I think this is a dire mistake. Early in the debate, spearheaded by Bob Spitzer (and including me and others) regarding the "openness" issue in DSM-5, Robert Freedman (editor of AJP) was pushing back on American Journal of Psychiatry "Issues for DSM-V" editorial submissions on the grounds that some were too "political." This is how conceptual work gets a bad name. Dr. Freedman's response reminded me how easily conceptual cri-

tiques are perceived as political jockeying, and that is not what AAPP and the philosophy of psychiatry are about. For me, the value in Bob's and Allen's critiques is not whether they are "conservative" or "progressive", but whether their insights and experience give us reasonable pause and valid cautionary historical experience. I think they do, though I don't agree with all they say. Secondly, as a person who applauded Frances' contributions in *Psychiatric Times*, I was starting to wonder whether I was a "conservative" or "liberal", then realizing that these encompassing labels simply distort viewpoints and lead people down premature-closure paths. My views are complex (as you will see a sample of below), as are others', and they don't belong in pigeonholes. Third, I devoted a whole chapter about politics in classification for *Values and Psychiatric Diagnosis* (VAPD), pointing out "good" and "bad" politics for a DSM, and indeed, as those who read it and remember, the labeling of positions as "conservative" or "liberal" best fits into a Politics4 move—rhetorical persuasion not philosophical argument. In my view this is not "good" politics for a DSM. Let's debate the issues not the labels.

2. Impoverished diagnostic criteria. One of my prior observations on DSM-IV/TR is the marked discrepancy in what might be termed descriptively rich vs. impoverished diagnostic criteria sets. The issue persists in the DSM-5 draft criteria, with some exceptions. Consider the diagnostic criteria for Schizophrenia, which are phenomenologically rich with multiple symptom sets, stipulations, and qualifiers. On the other hand, consider the DSM-IV/DSM-5 diagnostic criteria sets for disorders like the Paraphilias, Kleptomania, Pyromania, Pathological Gambling, and Intermittent Explosive Disorder. These criteria sets represent little more than single symptom clusters embedded in standard DSM language with stipulations like "not due to another disorder" or the clinical significance criterion. I'd like to hear Allen's response if "impoverished criteria sets" were identified as an issue in the DSM-IV era or in DSM-5 today. I think it is an issue in that monosymptomatic disorders have a rich history (the

"monomanias" of Esquirol), and promote, to repeat a DSM-IV era phrase, "artifactual comorbidity". It's not hard to figure out why. If I invent a descriptive category based on a single relevant descriptor (like "mammals are four-legged animals") then you will get a lot of false positives (alligators) as well as a lot of false negatives (humans). If this was identified as an issue, why did so many criteria sets end up as impoverished? I recognize the knowledge base may not be there to provide diverse kinds of validators or descriptors. But is that all there is to it? The DSM-5 draft criteria for Pedophilia have at least partially addressed the issue through adding, in the case of Pedophilia, a criterion item on viewing child pornography (as an example).

3. A related area is the issue of what counts as a primary disorder versus a primary disorder with "features of". Some of the monosymptomatic DSM-IV diagnoses had notorious comorbidity with other disorders (firesetting/Pyromania) being a prime example (Geller 1992). The problem with the criteria of tension-mounting and release in Pyromania is they are easily fulfilled, in that even angry criminal arsonists may get tension relief from their fire-setting, and rationales for fire-setting are easy to contrive or deny. Was fire-setting ever considered as a complicating feature of other disorders (psychotic, antisocial, conduct disorder, etc)? When a symptom complex repeatedly appears as comorbid with other diagnoses, did the DSM-IV leadership seriously consider making these symptom complexes complications of other Axis I disorders? For instance, "schizophrenia with fire-setting" or "personality change due to a general medical condition, with pedophilic features." Why/why not?

4. Some DSM-5 draft categories/criteria and implications.

A. Allen has railed against hypersexual disorder and "behavioral addictions" as false-positive playgrounds (forgive the pun), and on the basis that they represent, in my interpretation of Allen's comments, as invalid, poorly-chosen medicalizations of human foibles. The philanderer becomes a sex addict. The

video gamer who undergoes negative consequences, even extreme ones, for excessive play becomes a gaming addict. On the other hand, I can't speak for all clinicians, but these problems, disorders or not, are very common in (outpatient) clinical practice today. If not in the DSM, then where? V codes? If the criteria cannot be written well to address false positive/negative concerns, then that's an issue, but doesn't mean the condition is necessarily a non-starter.

B. I may have missed it, but I think Allen has not said much yet about the DSM-IV "appendix" for proposed disorders and criteria. Do you think this DSM-IV innovation a success, and if so, should DSM-5 consider placing these new proposals (like hypersexual disorder) in a similar portion of the DSM-5?

C. I'm curious to Dr. Frances' response to the new NIMH RDoC (Research Domain Categories) [<http://www.nimh.nih.gov/research-funding/nimh-research-domain-criteria-rdoc.shtml>] and their relationship to reaction to the DSMs? Any insights into the genesis of the Research Domain categories?

D. A and B above raise an encompassing question. Allen, do you think the DSMs could be (have been, will be) in any way effective in stemming the tide of medicalization and mental disorder expansionism? Why/why not, and how/how not?

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Politics; Lumping vs Splitting; What Place For Conceptualizing

Allen Frances, M.D.

It is always interesting to have a dialog with John Sadler.

1) Re labels: "conservative" is a currently much misused term and one that I would not normally adopt as a self characterization. In regard to DSM-5 I mean it in the simple Edmund Burke sense of conserving the past institutions, not necessarily because they are terrific or inherently deserving of particular loyalty or affection, but rather because of skepticism that proposed changes will be more beneficial than harmful. I've said more on this in my reply to Dr Waterman.

The introduction of the term "politics" into DSM debates is usually part of a purist's polemic and is almost always derogatory as in "I happen to know the real scientific truth and you are just talking politics here." Heated debates about diagnostic decisions arise precisely because there is little compelling science to decide them. As noted by many of the commentators in this issue, the casting vote must always be pragmatic common sense about what is likely to bring most good and least harm to patients. Aristotle would probably include this as "politics," but that was before politics got such a bad name.

2) Re descriptively rich vs. impoverished diagnostic criteria sets: the DSM is a historical hodgepodge. Some diagnoses are complex, heterogeneous, well established by long usage, and the subject of tens of thousands of papers. Others are simple and relatively unstudied. There is no overarching definition of mental disorder that governed their inclusion in the diagnostic manual and no complexity threshold that had to be passed.

DSM-III was created with a splitters mentality. The effort was to improve diagnostic reliability by dividing categories into convenient component parts that clinicians might more easily agree upon. More complex notions ("anxiety neurosis") have been divided into simpler modular units. This of course created "artificial comorbidity," particularly troubling among those who regarded a DSM diagnosis as a real entity rather than as descriptive building block.

I am personally a lumper, but (following the conservative discipline already discussed) was unwilling to impose this preference on DSM-IV. If we were starting from scratch, a num-

ber of the simpler categories might have been nested in larger ones, considered only as subtypes or cross cutting symptoms, or dropped altogether.

3) "What kind of efforts go into imposing uniform or coherent approaches to cross-cutting conceptual issues? How successful were they? How do cross-cutting conceptual issues 'rise to the top' of the list to be addressed?"

My experience with the last three DSMs was that the APA governance structure had no idea what it wanted or interest in discussing conceptual issues. The Chair of the Task Force is appointed with no conceptual restraints and has free rein to pursue his own path. Spitzer's main concern was to provide a needed legitimacy to psychiatric diagnosis by making it reliable. My main concern was to provide a needed stability to psychiatric diagnosis by making it evidence based. The DSM-5 main concern has been to be innovative. In each case, the direction was set by the DSM leadership unencumbered by external discussion or restraint. This is a serious mistake. Anything as important as the diagnostic system should be guided by a thoughtful, inclusive discussion of the underlying issues and the status of the field. Decisions that will so consequentially impact on the profession and our patients should not be left to the personal preference of one individual.

Once established, the DSM-IV goal of stability and evidence-based decision making was effected through a series of methods conferences. These had both an information gathering and an educational function. We wanted to create uniformity and quality control to guide and monitor the individual work groups. The problems with DSM-5 can be traced in part to a failure at the outset to discuss the implications of an innovative approach and how best to implement it in a way that would be safe for patients and convenient for clinicians. The work groups have pretty much fended for themselves without theoretical, practical, or editorial guidance—resulting in a remarkably inconsistent product.

How can this be done better in

the future? The Chairs of future revisions should not receive a blank check as in the past. Before her or his appointment, the mental health field as a whole should have a mechanism for thrashing through the conceptual issues, evaluating the science base, and considering the practical problems facing the diagnostic system. This would then lead to a specific and guiding charge to the task force chair and colleagues. The DSM should reflect the general will of the field, not the personal preferences of its leaders.

4) Why not have "diagnoses" like "hypersexuality" or "internet addiction" or "binge eating" since clinicians sometimes see such presentations and want to help the patients? The problem is that we know very little about the diagnosis and treatment of such problems and the consequences of medicalizing them by including them within the diagnostic system. The reach of psychiatry has already expanded dramatically in recent decades—perhaps beyond our grasp. NIMH estimates that twenty five percent of the population qualifies for a mental disorder in any given year. A prospective study found that by age thirty two, fifty percent of the general population had experienced an anxiety disorder, forty percent a depression. We are expanding the boundaries of mental disorder too far and too fast, without adequate research and clinical experience. The best solution for new and untried disorders is current diagnosis under the most appropriate NOS or V code rubric and inclusion in the Appendix to encourage future study.

5) The NIMH RDOC project is a novel attempt to integrate its enormous resources in the search for the neural network underpinnings of certain important dimensions of psychopathology. It is early days but this is a promising approach.

De-Centering the Subject of DSM

Donald Mender, M. D.
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Allen Frances, if I correctly understand the arguments of "DSM in Phil-

sophyland: Curiouser and Curiouser," invokes two premises in order to justify his conclusion opposing radical changes in DSM. His first premise is metaphysical, his second ethical. It seems to me odd that I find myself agreeing with his two premises yet in complete disagreement with his conclusion.

His metaphysical premise rests on three related empirical facts with which I certainly concur. The first fact is the genesis of all psychiatric taxonomies to date from a gradual historical accretion of practical, historically conditioned imperatives. The second fact is the demonstrable failure of current neuro- and molecular biology to short circuit, through an epistemically powerful breakthrough, this pattern of gradual pragmatically fueled nosological evolution. The third fact is the continued imprecision of psychiatric disease categories by way of both mutual overlap and internal incoherences. The metaphysical stance supported by these three facts is the idea that mental illnesses as we have conceived them so far are not definable in any clear one-to-one correspondence with natural objects existing in the "real" world but rather emerge as constructs reflecting the workaday utilitarian and culturally hegemonic agendas of its architects.

Dr. Frances rests his ethical premise on yet another empirical observation: small changes in DSM, in the direction of either greater sensitivity or greater selectivity, have too often had incommensurately massive, unanticipated, and often harmful impacts, i. e. excessive or inadequate intervention, on patients. This assertion is also certainly true.

Why, then, might one differ with Dr. Frances in his conclusion, which advocates cautious conservatism rather than massive overhauls of psychiatric nosology? The problem with this inference from the above premises is his additional but unwarranted assumption that scientific progress is inductive in nature.

As he portrays the process, since the beginning of the Enlightenment most successful branches of maturing science have advanced through two stages: first comes a cataloging of many observed facts, and then, from these data, clear causal patterns

emerge. Examples mentioned by Dr. Frances include the extrapolation of Newton's physical laws from Kepler's observations, Darwinian biology from Linnaean classification, and Bohr's atomic model from Mendeleev's periodic table of the elements. Mental illnesses, however, are said to resist transition from the first stage of the empirical catalog to the second stage of clearly patterned causal inference.

Dr. Frances seems to believe that the reason for this resistance lies within the metaphysically non-objective nature of psychodiagnostic concepts as outlined above. Yet another possibility exists: that "revolutions" advancing the sciences of celestial mechanics, evolutionary biology, and the physics of the microcosm have entailed something more than mere induction, and that this added component may yet bring clarity to future psychiatric thinking.

What might that magic ingredient be? Not mere political power shifts as Kuhn conjectured, or else the directionality of scientific "advance," including the demonstrated augmentation of the classical physicist's explanatory and predictive power by today's Standard Model, would have no meaning. Not mere formalistic substitution of inductive verification by experimental falsifiability, or else elegance and synoptic simplicity, i. e. "algorithmic depth," would have no intuitive scientific appeal. Not mere cataloging of objective data according to any system, insofar as Quine's "web of belief" will always search out alternative theoretical sophistries that minimize the need for revising theoretical orthodoxies in the face of anomalous experimental findings.

The Talmudic quotation, "We don't see things as they are.... We see things as we are," cited by Dr. Francis himself, provides a clue to the solution of this mystery. Great leaps forward in the coherence of science's encounters with natural objects have required revolutionary changes in scientific subjectivity's view of itself. These changes have been wrenching and painful in proportion to their profundity, because they have de-centered the subject of science by challenging particular historically

situated orthodoxies that had nourished institutionalized narcissistic illusions of the relevant epoch's scientists.

Hence, for instance, the path of advance that led celestial mechanics from unwieldy geocentric epicycles through the tractable heliocentricity of Copernicus and Kepler to the relativities of Galileo, of Newton's reactive law, and of Einstein's electrodynamics traced a progressive demotion of the scientific observer's locus from the navel of the cosmos to no universally absolute place, time, or velocity. Hence, the human biologist's illusory throne fixed atop the pre-Darwinian tree of life yielded to a fungible address on a twig configured by "fitness" contingent upon shifting selective pressures. Hence, the very presumed equivalence of causality, order, and rationality on which the originators of classical mechanics prided themselves crumbled in the face of quantum theory's irreducible probability amplitudes, non-distributive logic, and non-unitary opacity to the scientist's measurement of canonically conjugate observables.

Perhaps some similar humbling disruption, not yet apparent, awaits psychiatrists. One can only guess at what guise it might take, but we as agents of psychiatric praxis can expect that the revolutionary shift in perspective, which may finally unmask at least some clear "joints" in psychiatry's underlying intrinsic architecture, will be a traumatic blow to our sense of our own professional centrality in the universe of the mind.

This might seem at first glance a terrible vocational price to pay for progress, but it could also bestow many ethical blessings. "Sensitivity to initial conditions," as non-linear dynamics might describe the outsized and unintended consequences of even small changes in DSM's currently hazy formulations, would likely give way to much more linear proportionality relating informational input increments to operational output jitter. That kind of improvement in ballistics followed the Newtonian revolution; similar precision in population genetics issued from the theory of natural selection; technologies calibrated at the nanoscale were born from quantum insights. Why can we not expect similar benefits

in psychiatry, if we are willing and able to shoulder whatever epistemic self-sacrifices might be needed?

I Don't Believe in Magic

Allen Frances, M.D.

I thank Dr Mender for restating my views more precisely and poetically than I was able to do in the original. We agree completely on what he calls my "metaphysics" and "ethics" and also on the value of a decentering point of view in any endeavor - although he expresses much more hope for its potential than I do. We disagree on this one latter point because Dr Mender believes in the possibility of a "magic ingredient" that may lead to a great leap forward - while I am a skeptic who simply doesn't believe in magic. Dr Mender awaits the future psychiatrist/messiahs able somehow to climb out of our self centered observational cave in order to see things as they really are. If psychiatry has not yet enjoyed a Copernicus, Newton, Einstein, or Bohr, it is still early days. New and powerful observational tools used with genius (and a decentered perspective) may help find simplifying regularities that explain the seeming surface confusion.

This is where Dr Mender and I part ways. Although I hope he turns out to be right, my best guess is that the problem lies less in the observer and much more in the "chaotic" complexity of what is being observed. My pessimistic prediction is that psychopathology is not just heterogeneous at the surface level - it is also bewilderingly complex in the large variety of underlying etiologies that can cause the very same presentation. I expect there is no low hanging fruit, even for the best informed and most decentered observer wielding the most powerful tools.

What is the evidence for my pessimism? Thus far, the most interesting findings on the complex "diseases" in medicine and in psychiatry wind up explaining just a few percent of the variance. Most likely, there will not be one type of breast cancer or schizophrenia—more likely there will be hundreds of "causes" of each (and of most

"diseases" which unfortunately seem to have no simple causes). This suggests that scientific advances will be more trench warfare than blitzkrieg—steady, but agonizingly slow; retail, bit by bit, rather than wholesale great leaps forward as per Newton or Einstein.

If I turn out to be right, the next question is why is it that psychopathology is so hard to crack? A possible paradox might explain why (inherently?) there is no magic ingredient to make everything clear. The normally functioning brain is a triumph of non-chaotic development. Our DNA is a superb engineer, somehow orchestrating the most complex interactions with so few errors that we develop from a single cell into a run of the mill human—all achieved with remarkably little variance. Trillions of things that could go wrong, don't. When everything works out, we get the nonchaotic miracle of the more or less normally functioning person. But having a complex system, however fine tuned and self corrective, is always a gamble with the probability gods. If something can go wrong, sooner or later it occasionally will. When complex systems go wrong, they do so in complex ways that are impossible to predict and extremely difficult to track down. Figuring out how the normal brain works is turning out to be much easier than anyone could have imagined—once we got the magic ingredients of molecular biology, genetics, and imaging. By comparison, figuring out the crapshoot of the intricate and probabilistic causes of psychopathology will likely be the painstaking and pedestrian work of generations. No Newton. No Einstein.

I could be dead wrong. It may turn out that there are much simpler high order regulators that go wrong in much less complex and more easily understood ways. For fifty years the neurotransmitter models were the prime candidates to provide explanatory power, but they have failed to deliver on early hopes. The vogue now is neural networks (see Dr Peled) and certainly this is a fascinating window. But the incurable skeptic in me suspects that we are dealing with probabilistic, multimultivariate, com-

plexities that will always elude grand unifying schemes.

Skepticism breeds conservatism. Our current diagnostic classification is creaky at its non joints and certainly not a very clear mirror to nature. But it does its workaday job serving many useful practical functions. I don't see any magically better alternatives for the moment—and regrettably for quite a while.

Final Comment

Donald Mender, M.D.

Dr. Frances and I agree that the prospects ahead for a sweeping, massively simplifying, and paradigmatically clarifying revolution in psychiatric disease taxonomy presently remain uncertain. Hume's fork makes predictions about the future of science shaky on the basis of past historical patterns. However, to the extent that upcoming scientific revolutions may be able, as in the past, to "renormalize" into coherently finite convergence the infinite divergences of apparent chaos, a cautious optimism might be justified.

I do very much disagree with Dr. Frances regarding his apparent take on the term "magic," which in my view is a wholly relativistic concept. At least in the past, great leaps forward from less to more adequate explanatory paradigms have repeatedly transformed what had formerly seemed magical into pristinely rational science. An agent of such revolutionary change should not be understood a priori as some mysteriously genial and unique "messiah"; no single mastermind presided over the entire shift from Newtonian to quantum physics, which in fact successfully engineered the goals of alchemy from muddled incantation into the protocols of particle accelerators.

It may well be that, as Marxists have argued, the most definitive fuel for foundational scientific progress flows, albeit through channels carved by individual innovators, from the collective demands and historical dialectics of economic production. For example, though Copernicus and Kepler tried to revive an ancient heliocentric alternative to medieval geocentric epi-

cycles, only Galileo's improved telescope, created in response to the navigational needs of seafaring merchants from the city states of Renaissance Italy, was able to introduce the crucial concrete evidence, in the form of Jupiter's orbiting moons and the phases of Venus, that precipitated development of a new, widely accepted Newtonian cosmology.

What evolving forces of economic production relevant to possibly a future leap forward in psychiatric thinking are in play today? One such impetus, I suspect, may be the nascent emergence of quantum computers which, though they at present are still not practically exploitable, may eventually overwhelm digital technology through sheer computational power. Just as intellectual fashion during the 19th century era of steam engines conceptually framed mind/brain relations in neurally hydrodynamic and libidinally thermodynamic terms, and just as Boolean spins on information processing now occlude normative holes in our current neurocognitive models, so with the dawn of a future economy dominated by radically probabilistic qubits we may come to see consciousness as necessarily linked to quantum brain dynamics.

If so, whole paradigmatic frameworks, including mechanistic causality itself along with its problematic "variances," may reveal themselves as moot, having been grossly misapplied by our contemporaries to psychobiology, whether normatively simple or complexly pathological.

Another DSM on the Shelf?

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While we have the ear of the architect of DSM-IV, I want to question him further about a point I addressed in the previous issue of this Bulletin: the question of utility of the DSMs for practitioners. I noted in my previous commentary a disjunct between, on the one hand, a statement of purpose in the DSM-III and IVs that the manuals

were designed to be useful for clinical use, and on the other hand, the seeming reality that the manuals are designed primarily for reliability in the research community, not for clinical usefulness. The evidence for this argument is that experienced clinicians don't in fact use the DSMs in the manner directed by their authors: that is, clinicians don't check diagnostic criteria to make a diagnosis; rather, they rely on syndromal prototypes which they have learned and integrated into their diagnostic evaluation. My evidence is primarily anecdotal - my own experience and that of colleagues, who tell me they rarely take the DSM-IV off the shelf. It is hard to find empirical evidence for this opinion, since no one seems to study the actual use of DSM-IV. Searching for such studies will get you many manuals on how to use the DSM, but no studies on how it is actually used. The limited empirical evidence I could find (Cantor et al 1980; Jampala et al 1986, 1988, 1992) supports the impression that practitioners, if they use the manuals at all, use them in a loose, informal manner and are comfortable ignoring diagnostic criteria and making their diagnoses following an informal prototypal pattern. While researchers may use diagnostic criteria carefully to insure homogeneity across research subjects, clinicians are comfortable with prototypal, syndromal diagnoses, usually a mix of biomedical categories and, in some cases, psychodynamic factors.

My question then is the following: Allen, the first paragraph of the Introduction to DSM-IV contains the following statements: "The utility and credibility of DSM-IV require that it focus on its clinical, research, and educational purposes and be supported by an extensive empirical foundation. Our highest priority has been to provide a helpful guide to clinical practice. We hoped to make DSM-IV practical and useful for clinicians by striving for brevity of criteria sets, clarity of language, and explicit statements of the constructs embodied in the diagnostic criteria. An additional goal was to facilitate research and improve communication among clinicians and research-

ers" (2000, xv). What were you (as well as the DSM-III task force) thinking? Did you really think that busy clinicians would spend their time reviewing diagnostic criteria before making a diagnosis? And what do you think of the prospects of DSM-5 in this matter? The available hints are not promising. Regier and colleagues have written: "The single most important precondition for moving forward to improve the clinical and scientific utility of DSM-V will be the incorporation of simple dimensional measures for assessing syndromes within broad diagnostic categories and supraordinate dimensions that cross current diagnostic boundaries. Thus, we have decided that one, if not the major, difference between DSM-IV and DSM-V will be the more prominent use of dimensional measures in DSM-V" (Regier et al 2009, 649). Again, a promise to improve clinical utility linked to a program of dimensional measures that will surely be perceived as tedious and unnecessary for clinical work, and will be summarily ignored. Working clinicians already give scant attention to the GAF in DSM-IV. We can hardly expect that they will pay attention to still more dimensional scales in DSM-5. It's hard to disagree with Michael First, who had already argued for using clinical utility as a criterion for any change in the existing manual (2004), that the dimensional scales will corrode clinical utility in DSM-5 (2005). Will DSM-5 be one more manual gathering dust on the clinician's book shelf?

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Using Clinician Prototypes vs Criteria Sets In Making Diagnoses

Allen Frances, M.D.

James Phillips is probably right that most clinicians base their diagnoses of most patients on a mental syndromal prototype rather than doing all the extra work of performing a systematic checklist of the pertinent *DSM* defining items. But I disagree that this means the manual could usefully dispense altogether with criteria.

First off, I would argue that psychiatric diagnosis would be much more reliable and accurate if indeed it had to be justified and documented based on the systematic checklist approach. Much of the unreliability of diagnosis comes from clinicians reading off different scripts. This uniformity would be impossible to mandate in office practice, but would probably be a useful requirement in clinic, hospital, and training settings. And even in office practice, my guess is that *DSM-IV* does come down off the shelf to assist when-

ever there are more confusing and unfamiliar diagnostic decisions.

Second, even if clinicians are not faithfully using the *DSM* with every patient, the prototypes they form of each mental disorder are probably mostly derived from the *DSM* criteria sets. In bridging the clinical/research interface, it is desirable that clinicians and researchers all have the same starting point, even if their ultimate adherence to the criteria sets varies widely.

I do agree that, unless corrected, *DSM-5* will be most cumbersome and difficult to use. The writing is imprecise and inconsistent across sections. The dimensional approaches suggested are so complex and user unfriendly that they will bury dimensions in undeserved infamy. *DSMs* cannot ever be an easy or fun read, but they should strive for simplicity, clarity, consistency.

Final Comment Not so Fast

James Phillips, M.D.

First, inasmuch as I complained about lack of empirical evidence for actual use of the *DSM* by practitioners, let me thank my eagle-eyed colleague, Ron Pies, for pointing to a recent study by Zimmerman & Gallione (*J Clin Psychiatry*. 2010 Mar;71(3):235-8) on use of the *DSM-IV* criteria in diagnosing depression. As Pies summarizes: "As it stands, nearly one-quarter of psychiatrists indicate that they usually do not use the *DSM-IV* DMM criteria when diagnosing depression, and nearly half of nonpsychiatrist physicians indicate that they rarely use the *DSM-IV* MDD criteria to diagnose depression" (<http://www.psychiatrytimes.com/blog/couchincrisis/content/article/10168/1601688>).

More to the point, let me half agree and half disagree with Allen Frances on two of his points. First, when he responds that "even if clinicians are not faithfully using the *DSM* with every patient, the proto-

types they form of each mental disorder are probably mostly derived from the *DSM* criteria sets," I think he is largely right. At least that is true of me. What he leaves out, however, is that when working with patients clinically, our lack of rigid adherence to the criteria sets allows us to be flexible in our diagnostic assessment, to include, for instance, bits and pieces of psychodynamics, family process, and other factors in our assessments. I'm aware that one of the much-trumpeted triumphs of *DSM-III* was the banishment of psychoanalytic assessment from the *DSM*. But in the real world in which we live and work, people don't leave their psychodynamics at the door when they enter our offices.

Second, he responds that "psychiatric diagnosis would be much more reliable and accurate if indeed it had to be justified and documented based on the systematic checklist approach...And even in office practice, my guess is that *DSM IV* does come down off the shelf to assist whenever there are more confusing and unfamiliar diagnostic decisions." Yes to part two of this statement. Or at least, it comes down off my shelf in moments of diagnostic unfamiliarity. But regarding the first part of his statement, I'm tempted to say, are you kidding, you want me to pull down the manual every time I see a depressed or anxious patient (the bulk of my and most colleagues' practices)?

But there is a much larger issue lurking is this latter point that, I humbly submit, Dr. Frances misses. It again has to do with the difference between research and clinical work. Researchers need diagnostic criteria and reliability. They are interested in diseases and correct diagnoses. Clinicians operate with a quite different set of assumptions. They deal with living, breathing, individual patients. Their first order of business is, how do I approach this patient, not how do I get the most reliable diagnosis. To make this point in the most provocative manner, diagnostic criteria are clearly necessary and good for research, but often bad for clinical care. I hear my critics screaming, how can you claim good clinical care if you haven't secured the correct diagnosis? Yes, if I've diagnosed the bipolar patient as schizophrenic, my

critics has a point. But for every such lapse there will be a multitude of others in which this objection is bogus, in which the formal diagnosis will represent only a limited window on the rich complexity of the individual I am treating.

...Still, I Wonder

Claire Pouncey, M.D., Ph.D.
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How disappointing that we cannot move our discussion of nosologic merit past a basic framework of realism versus empiricism versus constructivism. And how disappointing that Dr. Frances doesn't see where metaphysical and epistemological commitments have already been made, and thus where they are no longer under debate. In psychiatry, we are already committed to empiricism. The more pertinent question is what sort of empiricism we can commit to, why we do so, and how we overcome its limitations. No one in this conversation about DSM-5 is a Platonic realist: we all agree that we know the world via our sensory abilities (both direct and technologically-enhanced), and we agree that although we strive for intersubjective agreement about those perceptions, the perceptions themselves do not necessarily reveal a truth that exists beyond us. That said, we need to elaborate what sort of agreement we seek. We all agree with Frances's second umpire that we "call them as we see them", but any empiricist worth her ivory tower knows that doesn't get us very far, since we have no direct insight into whether what we perceive represents anything apart from our own biases and expectations. We need to articulate reasons for endorsing our ontological, epistemological, and theoretical commitments in a way that freshman year philosophy classes do not teach us to do. We know that sociopolitical values and settings influence the outcomes of the most basic scientific investigations. This does not mean that our scientific ontologies are "mere constructions" that should be dismissed as works of fictions. Rather, this means that we must go beyond the observations them-

selves to justify our beliefs. This is especially important in medicine, where we intervene in potentially harmful ways based on our studies. It is more important in psychiatry, where the intersubjectivity of perception itself often is the object of study.

Frances does seem to appreciate the ubiquitous uncertainty that pervades all empiricist approaches to knowledge (though why he localizes this to quantum physics is unclear). Yet he does not follow this awareness to the logical conclusion that psychiatry has a tremendous responsibility (1) to articulate our ontological commitments (i.e., mental disorders and neuropsychiatric theory); (2) to give reasons for maintaining or altering them; (3) to defend our characterizations of those commitments with intersubjectively appreciable reasons; (4) to make explicit our methods and history of theory development; and (5) to justify our contemporary interventions based on that existing theory. His articles circumvent these fundamental questions; his response to the commentaries in the *Bulletin* ignore them entirely. Good philosophy requires criticizing one's own position and challenging oneself to reason. Frances describes an irrelevant historical debate and engages with his own empiricist commitments not at all.

An Apology For Dumb Utilitarianism

Allen Frances, M.D.

I am used to disappointing people (including myself) and I accept Dr Pouncey's criticism that I haven't even attempted to articulate a systematic, consistent way to decide which empirical position is most defensible on any given question in psychiatric diagnosis. I am not sure to what degree this represents my ignorance or my intellectual laziness (both lively contenders) or whether psychiatric classification is an inherently messy activity that would defy even clear and energetic thinkers like Dr Pouncey. I wouldn't know how to begin to develop an approach that

would "go beyond the observations themselves to justify our beliefs." The usual organizing principles—or validators—of descriptive diagnosis have certainly not proven to be very compelling guides. If there is anything better than ad hoc, "do least harm," case by case, rough and ready utilitarianism, I have not yet stumbled across it. So Dr Pouncey, the future work in this area is your wicket (at long last, we get to leave behind all the tired baseball metaphors).

Afterword

Allen Frances, M.D.

So what has DSM learned from its brief adventure in philosophyland. I can't be sure that I am a fair judge and that we have a fair sampling of opinion. It is in the nature of learning and of knowing that different people will perceive the dialogues differently and take home different lessons. These, then, are no more than my own impressions—doubtless biased by my own preconceived notions and by the fact that the commentators are a small and very select group. The field at large may continue to see things quite differently.

1) The second umpire rules. As psychiatric diagnosticians and classifiers, there seems to be almost uniform consensus that we call them as we see them, not as they are or as we make them up to be. There was a surprising unwillingness to defend a pure umpire one approach. Assuming that this is not just selection bias, the acceptance of a nominalist position may represent a departure from the majority epistemological opinion during the heady, early days of biological psychiatry when everything seemed so simple and real and a deep understanding of causality was only a matter of time.

2) The scientific enterprise in psychiatry has an extremely bright, but extremely difficult future. The powerful tools at our disposal guarantee a steady succession of remarkable finds in psychopathology. The complexity of the problem guarantees that each will explain a small percentage of the variance. The only possibility for a quick walk or grand slam home run would be

the finding of higher order nodal regulators that provide a final common pathway channeling the complexity of basic causes into more manageable pathogenetic units. Neurotransmitters theories were once promising, but now seem to be disappointing, candidates. Neural networks are next up to bat. But the nature of brain functioning may offer neuroscience no slow, easy, pitches right down the middle of the plate.

3) Descriptive psychiatry as embodied in the DSM is enormously useful (really indispensable) in everyday clinical practice, but it is fallible in many ways and perhaps not the best (and certainly not the only) guide to future research discovery.

4) Elegant theory driven (and partially empirically supported) substitutes for the DSM approach are an important vehicle for advancing the game of research, but are not yet major leaguers ready for inclusion in an official nomenclature.

5) It is a great misfortune that there is no operational definition for the concept—"mental disorder"—that is at the very core of the diagnostic manual. We have no bright line telling us which conditions should, or should not, be included; who needs a diagnosis and who should be spared one. These decisions—both on categories and on people—can be made only case by case on practical, utilitarian grounds uninformed by conceptual clarity and only very partially informed by scientific evidence.

6) The decisions made for DSM-5 must consider the potential practical consequences and cannot claim an immunity from responsibility because they are "following the science." The available "science" underlying DSM decisions is never very deep or generalizable and is always subject to widely varied interpretation. The most important guide to decision making must be the ancient, practical dictum—First, Do No Harm.

7) The huge advances in the neurosciences have thus far had no impact on psychiatric diagnosis. Until we know more, there is no justification for major changes in the diagnostic system. Changes, when they come, will likely be retail and piecemeal, each explaining only a small portion of the presen-

tations in any of the existing categories. The best hope for more wholesale understanding is the NIMH RDOC project.

8) The reach of psychiatry should not exceed its knowledge base. We must avoid the temptation to medicalize both normality and criminality. If we don't know how to treat something, it is probably not a great idea to make it an official diagnosis—particularly if it will result in the diagnosis and possible mistreatment of millions of people. We should not create categories that can be misused in the legal system to promote preventive detention via involuntary psychiatric commitment.

9) Our dilemmas are not ours alone. Most of the above applies almost equally well to all of medicine. There is no clear and universal definition of medical illness and no simple path of pathogenetic understanding. The body is not as complicated as the brain, but it is pretty complicated.

Finally, a huge thank you to James Phillips for conceiving, organizing, and editing this exercise. It has been an illuminating illustration of the central role that conceptual discussion should play (but hasn't) in the development of our psychiatric classification. It is never wise to build an edifice on shaky conceptual foundations. The DSM-5 process could have avoided many of its mistakes had it begun its efforts by appointing a "Philosophy Workgroup" under the leadership of Dr Phillips. Hopefully this issue will stimulate further discussion toward getting the diagnostic system back on track.

(Continued from page 1, President)

Patients are unique and idiosyncratic, of course. That is one thing that makes it so difficult to tailor science to patients. Developing a science of psychiatry that is responsive to the individual is a daunting task, and the epistemic problem of attending to difference while attempting also to formulate generalizations is a significant problem not only for the sciences but for all theorizing. But to frame epistemological and ethical responsibilities in terms of patient

values and voices is not to strip each person of his or her embeddedness in culture: we are individuals, yes, but always also living in the midst of social categories, organizing principles, and more or less oppressive societies. We are individuals, yes, but we are also members of humanity together—and whatever else that means, it is a call for us to work toward mutual flourishing in the eudaimonistic sense. And we are also gendered, racialized, raised in ethnic and religious and political communities; we form perspectives based, in part, on the good or bad luck of our lives and the cultural meanings we ascribe to that 'luck' (Was that merited? Was that deserved?) Combining EBM and patient values, not to mention the identification, critique, and necessary weeding out of assumptions and values that render the field of psychiatry inaccurate and sometimes damaging, is no small task. It is a project that we philosophers, psychiatrists, and others can and should continue to work collaboratively on.

(Continued from page 1, Editor)

vatism, alternative models, clinical utility vs research, etc.—I will choose the first, since I find that, while serving us richly and charmingly, the umpire metaphor has also led to some confusion—and I don't just mean for our non-North American readers, to whom baseball is probably as comprehensible as cricket is to me, i.e., not very. The point of possible confusion is umpire #2, with whom Dr Frances identifies himself, and whom Mishara and Schwartz describe as steering "between the Scylla of naïve biological realism and the Charybdis of social constructionism, or alternatively, logical empiricism and post-modernism." Calls of alarm regarding umpire #2 come from Kinghorn, Cerullo, and to a lesser extent, Ghaemi, Pouncey and others.

Frances describes the second umpire as: "There are balls and there are strikes and I call them as I see them" (p 3). He adds later: "[M]ental disorders don't really live 'out there' waiting to be explained. They are constructs we have made up—and often not very compelling ones at that" (p 4). So what is a second umpire? Kinghorn remarks

that Frances at first appears to be a realist of some kind, but then quotes the above citation and remarks: “But if this is true—if the standard for diagnostic classification is not what exists ‘out there’ but rather in ‘getting to what works best,’ if indeed ‘our mental disorders are no more than fallible social constructs (but nonetheless useful one...)’—then Frances fits the type of the third umpire perhaps even more cleanly than the early Szasz...” (ibid). Frances resists this charge, affirming that there *are* ball and strikes out there, a “knowable underlying reality to what we now call mental disorders, just...remarkably complicated and heterogeneous...” (p 27).

Michael Cerullo enters this discussion with a naturalist/normativist distinction, arguing that most illnesses involve a combination of both perspectives. Quoting Frances’ statement that “We must accept that our diagnostic classification is the result of historical accretion and accident without any real underlying system or scientific necessity ... Our mental disorders are not more than fallible social constructs (but nonetheless useful if understood and applied properly),” Cerullo concludes that “Together with the earlier baseball analogy this places Frances in the uncomfortable position of being an umpire who believes there really are balls and strikes but who feels his rulings have absolutely no relationship to them whatsoever, and who is OK with

this!” (p 43). Frances again asserts misunderstanding, insisting that he takes naturalistic understanding for granted, but adding about Cerullo, “We are very different second umpires and disagree on how easy it is to make the calls separating the balls from the strikes” (p 44).

It begins to look as if our problem is in clarifying what exactly are balls and strikes? To be quite concrete, I take Frances to mean the following. For the second umpire balls and strikes are symptoms and symptom clusters/syndromes: I can see them and you see them. ‘Diagnoses’ are concepts/constructs that formalize these syndromes. ‘Balls’ and ‘strikes’ may also refer to these diagnostic constructs (thus the confusion). There is a potentially knowable naturalistic, biologic underpinning to the symptoms and syndromes, but it may not match up clearly with the diagnostic constructs. In the absence of further knowledge, we remain uncertain as to how much, and in what way, the diagnostic construct tells us about the underlying reality of the respective symptoms or syndromes. For the first umpire, of course, the balls and strikes represent the pathophysiologico-psychosocial reality that the second umpire can see obscurely at best with his/her constructs.

I am left with one question for our second umpire, Dr. Frances. Taking a note from Cerullo regarding

naturalistic understanding, and Kendell and Jablensky (Am J Psychiatry 2003; 160: 4-12) in their discussion of conditions that meet their standard of validity (Down’s syndrome, Huntington disease, etc.) —a discussion that parallels the present one on the perspectives of the first and second umpires— would Frances agree that, say, in the case of Huntington disease, we have a sufficient understanding of the condition to grant it umpire #1 status? In point of fact I put this rhetorical question to our second umpire, and he agreed that in the case of Huntington disease he would assume a first umpire stance. This is not to say that we understand everything there is to know about Huntington, only that we know enough to feel confident that our diagnostic construct matches the underlying pathophysiologic reality of the disease. [Note from current *UpToDate* entry on Huntington Disease: “Huntington disease (HD) is an inherited progressive neurodegenerative disorder characterized by choreiform movements, psychiatric problems, and dementia. It is caused by a trinucleotide (CAG) expansion in the Huntington gene on chromosome 4p and inherited in an autosomal-dominant pattern. The pathophysiology of HD is not fully understood, although it is thought to be related to toxicity of the mutant Huntingtin protein.”]

JP

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