

*From the Editor*

In the discussion of the DSM-V process initiated by Allen Frances and developed at great length (and, let us hope, with some depth) in this issue of the AAPP Bulletin, one point stands out: the authors of DSM-V must surely rue their invocation of the Kuhnian phrase, ‘paradigm shift’, to describe—and promise—what we might expect in the new DSM. It has become all too apparent that, whatever the merits of DSM-V, they will hardly warrant the designation of a paradigm shift. A distinctly less flattering metaphor for the DSM process is that it remains in its Ptolemaic phase. That is, DSM-V shows every sign of leaving the DSM stuck in its geocentric model, creating epicycles (read ‘dimensional scales’) to cover over discrepancies in the descriptive diagnoses and gussy up the end product with a scientific sheen. We all await the Copernican transformation to a heliocentric DSM. When that will occur, and how it will look, is not clear, but we will not have it in 2013 with DSM-V.

Dr. Frances’ response, along with the commentaries, demonstrate the range of opinion among thoughtful people regarding the status of DSM-V (and the DSM process in general). Frances, in addition to reviewing and elaborating on his conservative attitude toward changes in DSM-V, articulates a skeptical view of DSM categories as constructs that will not prove to be real-world entities. Another view is expressed by the writers of the recent piece in *Science* mentioned by John Sadler in his editorial. What is striking about those authors’ proposal for “unraveling the biological causes of psychiatric illnesses” in some kind of integration of genomics and circuit analysis is the staggering complexity of how these analyses will unfold - not to mention how the “psycho” and “social” dimensions of etiology will be inte-

**Editorial****Waiting for the Miracle**

John Z. Sadler, M.D.

At the behest of our residency training director, my historian-of-medicine colleague, Steve Inrig, and I have just finished our teaching of the newly-required “history of psychiatry” module to our fourth-year residents. We structured our four-hour module into four 1-hour segments, each with a perennially-relevant psychiatric theme: 1) concepts of mental disorders, 2) explanation and understanding of mental disorders, 3) confinement (of patients), and 4) stigma. I was surprised at the enthusiasm and vigor the residents approached our reading material (see references below). One cross-cutting motif powerfully emerged in our discussions. One crestfallen resident described it as “we’re always on the cusp of a breakthrough.” The history of psychiatry could be summarized as the waxing and waning success of convincing the public about the emerging breakthrough that will transform lives. Psychiatrists (or at least organized scientific psychiatry) are always “waiting for the miracle” as Leonard Cohen would have it.

The current issue of the Bulletin presents a wonderful set of responses to Allen Frances’ commentaries on the DSM-V process. What has struck me about the DSM-V leaders’ self-congratulatory rhetoric about paradigm shifts and the “most open” process “ever” is the umpteenth iteration of the motif of being “on the cusp of a breakthrough.” DSM-V psychiatrists are not the only ones on the cusp of a breakthrough. Thomas Insel and NIMH colleagues, after declaring failure of the Decade of the Brain in finding revolutionary treatments for mental disorders and even proclaiming as misguided early biological/reductionistic psychiatric research, now have turned the corner and offer a new set of approaches on the cusp of a breakthrough. See the recently released NIMH Strategic Plan (referenced below), which emphasizes basic science, developmental trajectories, diversity of intervention with people (i.e., personalized medicine), and increased public health applications. In today’s *Science* journal, Akil and colleagues, (including our own Ken Kendler), describe the future of psychiatric research as understanding genome-neural circuit relationships. They also have a grim appraisal of the past twenty years and hundreds of millions of dollars’ worth of research for magic bullets: “. . . there have been no major breakthroughs in the treatment of schizophrenia in the past 50 years and no major breakthroughs in the treatment of depression in the past 20 years” (p. 1580).

Excuse me, but I’d like to point to the elephant in the room here. Why the spending of untold millions on undelivered miracles when the lessons of history and philosophy, along with the wisdom of elder statespersons like Frances and

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grated into the end-product diagnoses. It is not even clear whether in the DSM-X of the future we will have anything like the categories of DSM-IV (and V), or whether there will even be categories, as opposed to some other diagnostic structure. Stay tuned.

James Phillips, M.D.

## Symposium

### Allen Frances' Critique of DSM-V

*In a series of articles published over the past several months in *Psychiatric Times*, Allen Frances, Emeritus Professor of Psychiatry at Duke University and architect of DSM-IV, has launched a major critique of the development of DSM-V. Given the importance of this topic for the field of psychiatry, we are devoting this issue of the *Bulletin* to the discussion of DSM-V initiated by Professor Frances, with commentaries on both Frances' critique and the DSM-V process. Professor Frances has graciously agreed to write a response to the commentaries, and for that we express our sincere appreciation.*

*(Target articles published in *Psychiatric Times* can be found at the following links:*

*[www.psychiatristimes.com/display/article/10168/1425378](http://www.psychiatristimes.com/display/article/10168/1425378);*

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### The End of the DSM?

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Allen Frances recently sounded several warnings about the upcoming DSM-V (Frances 2009a; Frances 2009b; Frances 2009c; Frances 2009d; Frances 2009f; Frances 2009g). Among his concerns is the lack of transparency of the process, the lack of requests for feedback from the profession, and the ambitious agenda of creating a "paradigm shift" with the DSM-V. In a

response letter, the leaders of the American Psychiatric Association (APA) and the DSM-V committee failed to address his major concerns and disappointingly responded with an ad hominem attack on Dr. Frances (Schatzberg et al. 2009). The APA leadership claims the DSM-V is the "most open and inclusive ever." (Schatzberg et al. 2009). Yet as of January 2010 there are only a few pages on the APA website (<http://www.psych.org/>) discussing possible changes to the DSM with no justification (the statement "the literature shows" means little without providing actual references and detailed interpretations).<sup>1</sup> Regarding feedback, the defenders of the DSM-V suggest that a few presentations at professional meetings and an email comment line on the APA website count as appropriate feedback from the profession (Schatzberg et al. 2009; Carpenter 2009).

Research in psychiatry has grown exponentially in the last two decades. There is a large and diverse literature within each of the major fields of psychiatry (e.g. schizophrenia, bipolar disorder, etc.). Any arbitrary changes in diagnostic criteria could hamper research by limiting the integration of newer research into the previous body of literature. Any significant changes diagnostic criteria need to come from experts in the clinical and research community. Major changes cannot be legislated from the top by a political organization such as the American Psychiatric Association that does not represent serious research in psychiatry. Science is inherently (and appropriately) a conservative endeavor and major changes take place only when the majority of experts are convinced because of undeniable empirical data. No other branch of basic science or medicine would attempt to develop a single document that attempts to make changes throughout their specialty. Instead, in other areas of medicine groups of specialists work together to set criteria and make changes as needed within their specialty (e.g. the International Classification of Seizures, the American College of Rheumatology classification of Rheumatoid Arthritis etc.). The ICD-10 is then de-

veloped by adapting these expert guidelines, not the other way around as proposed in the DSM-V. In psychiatry we need to follow the lead of our colleagues in the rest of medicine.

The most useful accomplishment of the DMS-V would be to merge with the ICD-11 (Frances 2009e). This would allow the APA to focus on coding guidelines and protecting patients from their potential misuse, an appropriate place for a political organization to use its influence to advance public health issues and patient advocacy. Any meaningful changes in diagnostic criteria or additions of biomarkers and other biological tests should be left to evolve naturally from the experts in the field. These changes should be adopted only when they are accepted by the majority of experts as a result of a peer reviewed debate in the literature. Only when they are so justified will changes be accepted by the research and clinical community.

Frances and others were also concerned about the ambition of the DSM-V committee to create a "paradigm change" in psychiatry. Psychiatry has essentially gone through two Kuhnian paradigm shifts since the middle ages (Shorter 1997). The first was during the enlightenment when mental illness was viewed as a medical disease rather than a supernatural phenomenon. The second was the more recent shift away from dualistic approaches towards a biological view of mental illness. Each fit Thomas Kuhn's definition of a paradigm shift as they changed what questions were legitimate and how these questions should be structured and interpreted (Kuhn 1962). The recent hope of using biomarkers and biological tests in the DSM-V does not actually represent a Kuhnian paradigm shift but instead a natural expansion of the prior biological revolution in psychiatry. Any suggestion of the DSM-V as a paradigm shift is a gross misunderstanding of the term. Even the DSM-III did not represent a paradigm shift. Rather it was a natural product of the final accession of biological psychiatry.

Dr. Frances was the first to seriously express the concerns many feel about the entire DSM-V process and he deserved a serious response. The DSM-IV should be the last DSM. Instead of a DSM-V the APA committee should

merge the DSM-IV with the ICD-11 and focus on the political dimensions of the document. Any serious change in diagnostic criteria needs to come from the experts in the appropriate field. As the changes occur the ICD is flexible enough to incorporate them and is not frozen in time like the DSM. The next great advance in psychiatry will not be a paradigm shift. Instead it will be slow unraveling of the etiology of mental illness which will only confirm our faith in biological psychiatry.

#### Footnotes

<sup>1</sup>Shortly after this paper was written the DSM-V committee posted the draft of the DSM-V for review. However, the committee is allowing only 2 months of "open discussion" which once again resolves around sending comments via email. Amazingly, in this "full draft" there essentially no references provided to support the proposed changes. Therefore the publication of this draft does not significantly alter Dr. Frances or this articles conclusion and in fact provides further justification for our conclusions.

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### Overcoming the Psychic Law of Inertia

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The recent debate about DSM-V, between some of the individuals involved with DSM-III and DSM-IV and those involved with the current revision, raises questions about how we should be revising our psychiatric nosology. Basically two camps appear to exist: one (ironically led by figures associated with the revolution of DSM-III, like Robert Spitzer and head of DSM-IV, Allen Frances) we might somewhat ironically label conservatives; the second (led by the current leaders of the DSM-V revision such as David Kupfer, Daryl Regier and Alan Schatzberg) we might call liberals. The conservative view, as Frances has well described in recent articles in the *Psychiatric Times* and the *British Journal of Psychiatry*, is that changes in DSM-V should not be made unless strong scientific evidence exists to do so. A conservative baseline mind-set appears to exist such that revisions should always err on the side of not making a change unless notably strong evidence exists for change. The rationale, as Frances describes it, is partly so that the psychiatric profession is protected from rapid and unnecessary changes in nosology. The liberal view, using perhaps the unfortunate coinage of a "paradigm shift," calls for larger changes in DSM-V; this has led to a fervent re-

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action by the conservative camp. The term “paradigm shift” is widely abused. In Thomas Kuhn’s philosophy of science, a paradigm shift involves a major new way of thinking that had not previously existed before, upending major assumptions and axioms of previous science as a result of significant inexplicable anomalies that cannot be explained in older paradigms. For instance, Galileo introduced a paradigm shift in astronomy, Darwin for biology, and Einstein for physics. That is what Kuhn meant by a paradigm shift, not whether or not we should add or subtract a DSM diagnosis. The real question is whether revisions in DSM-V should be made conservatively or liberally: Should we have a very high threshold for making changes? Should we be erring on the side of not making changes? Or should we have a lower threshold for making changes? Also, should we be making changes based on the scientific evidence primarily, not caring on whether or not clinicians or patients or the pharmaceutical industry or the Food and Drug Administration (FDA) or divinity likes the changes that we make?

The conservative view is ironic because it contradicts the revolutionary attitude of DSM-III. At that time, much more so than now, there was opposition to radical proposed changes. One of the major proponents of DSM-III, Gerald Klerman, repeatedly said that a key feature of the new approach in DSM-III was that change should be feasible, practicable, and encouraged. There was no sense at all of a baseline predisposition against change, or a high threshold for further changes. Said Klerman: “There is implicit in the creation of DSM-III the necessity for change. The push for DSM-IV is already apparent. The changes that appear in DSM-IV should be determined by the state of evidence rather than the assertions of competing ideological camps” (1). Also: “We seem to spend more time fighting ideological battles than generating data. This debate is already an anachronism. I invite our colleagues to acknowledge the achievements of DSM-III and to join with us in gathering data based on science to revise it. On to DSM-IV” (1).

Klerman very clearly was arguing

that DSM-III should be accepted because it was a major change based on acceptable science, and furthermore, that it was a scientific attitude to make changes based on research evidence, not to resist changes on other grounds. Spitzer was part of that group, and Frances was in charge of DSM-IV, but now they seem ideologically opposed to further change. Specifically, they fear too many false positives if premorbid diagnoses are added to DSM-V, leading to over-treatment; also, they oppose dimensional ratings as unrealistic because clinicians don’t have the time or training to use them (2, 3). Yet, the scientific evidence for dimensionality in mood disorders and personality is immense (4-6), much greater than the evidence that was used for the changes that were incorporated into the DSM-III. Why all this resistance to including sub-threshold disorders based on political, economic, and social concerns about how the profession and the public would react when the scientific evidence based on clinical research studies is rather strong and growing?

The claim by the conservative group that changes should only be made based on a high threshold of scientific evidence conflicts with the actual practice of how DSM-IV came about. For instance, in a rare frank statement about the internal political judgments in DSM-IV, one of the members of the DSM-IV mood disorders task force revealed that a major influence on diagnostic decisions was a fear of overdiagnosis of bipolar disorder: “There was a sense from the Task Force that bipolar conditions should not be overdiagnosed in the community; if they are, lithium might be too broadly applied to patients with mood disorders” (7). Since hypomania was being included as part of a broadened type II bipolar diagnosis, the Task Force decided, based on no convincing scientific evidence (the DSM-IV field trials did not provide such data nor did they take into account conflicting data), to disallow the diagnosis of bipolar disorder if mania or hypomania occurred with antidepressants. This change has produced a great deal of confusion in the last two decades, and has

no scientific basis; in fact, repeated studies show that manic or even hypomanic switch in patients with true unipolar major depressive disorder is rare (8). Further, the natural history of a depressive episode followed by hypomania is common (9). Thus, we have changed our nosology so as to fail to diagnose bipolar disorder in many of these cases, not based on science, but based on a politically driven wish to avoid overdiagnosis. This all happened with DSM-IV, and yet now its leaders want to argue that DSM-V should be different. We agree in principle: science should trump politics; but such rhetoric given by prior leaders of DSM revisions should perhaps be acknowledged by an admission that this has not always been the case in the past.

In contrast, there is increasing evidence that the very broad and heterogeneous Major Depressive Disorder (MDD) category, which has remained more or less unchanged since DSM-III, has not led to advances in biological or treatment research. The MDD concept as it currently stands was created in DSM-III based on primarily political considerations, as documented in a recent history based on a reading of the minutes of the meeting of the DSM-III task force (10). Thus MDD, which is based on little scientific research and much politics, should be narrowed and revised based on an accruing amount of research evidence. Yet DSM-V is unlikely to make major revisions in either MDD or mixed episodes. If we want to follow the scientific evidence, there should be more change in DSM-V than is being proposed, not less.

Frances dismisses descriptive psychiatry as having “done about as much as it can to further our field,” while suggesting that psychiatric diagnosis based on etiology, though years away, is the only way to achieve a “paradigm shift” (11). We believe that we need to keep at it, rather than resign ourselves to our failure in any further progress in the psychopathology of psychiatric illness.

Some of this debate has become unnecessarily personal. The response by the DSM-V committee to the commentary by Frances was unfortunately emotional in tone, and less definitive in

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## The Multi-axial, Multi-layered Reality that is Mental Disorder.

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Dr Frances critiques the deletion of the multi-axial system of the DSM IV and is surely right to do so for a reason that embraces the metaphysics of mental disorder. The multi-axial system is (in effect) as follows:

Axis 1 Clinical disorder—notice that this is a disorder identified in the clinic and is implicitly regarded as separate from factors identified under axes II, III, IV, and V.

Axis II Personality and constitutional disorders—the distinctness of this element of the system is often noted to be difficult to maintain in the face of a critique of the deficit or purely biological model of mental disorder (Zachar, 2000; Gillett, 2009).

Axis III General medical conditions—these seem notionally distinct and to some extent independently assessable from the mental disorders they complicate.

Axis IV Psychosocial and environmental problems—again we might wonder about such things as ASPD or adjustment disorder both here and in relation to Axis 5.

Axis 5 Global functioning—this axis brings us face to face with metaphysics and natural kinds.

Metaphysics, if one follows the tenets of critical scientific realism, in philosophy of science, is the best specification we can give of an entity such that we understand its workings in the natural world and the laws and regularities that govern it. The thought is that naturalistically real things are governed by natural laws and their contours explained by processes studied in natural sciences. A more radical view, such as the empiricist structuralism of Van Fraassen (2008), would take us into highly contested philosophy of science but the critical realist stance is sufficient to make some cogent points about psychiatric disorders (so that we do not need to buy more trouble than is unavoidable to do justice to the phenomenon under dispute).

Let us accept, for the minute, that the phenomena we call mental disorders are, in part at least, manifestations of some mode of activity that captures a quasi-stable configuration of the human neural network. This looks to be a position common to both biologically oriented psychiatrists and other theorists so it is not a bad place to start the analysis of the troubles that one meets in a psychiatric clinic. The problem is that the brain is an organ designed to inscribe in itself techniques of adaptation that fit a person to get by in the human life-world (a world shared with others).

The importance of the human and interpersonal context of brain activity is underscored by neuroscience and information processing theory that treats the brain as a complex, multilayered neural network dynamically interacting with what goes on around it (Clark, 2008). Information is transmitted through the neuronal networks of the brain in ways configured by an individual's learning history and designed to complete loops of action and reaction that bring about life-enhancing results. Thus the brain begins with a certain (probably hardwired) associational skeleton, and has a vast network of possible connections in which associations and patterns of transmission are developed on that primordial base. Certain synaptic connections are strengthened (by processes such as long term potentiation) and others weakened (perhaps even lost through dendritic pruning) so that the brain registers patterns indicating significant features (and events) in the subject's domain of activity. In that way genetically programmed brain pathways form their actual connections as a result of social experience (Eisenberg, 1995, p. 1563) mediated by speech and other culturally mediated techniques of adaptation (Luria, 1973; Dennett 1991; Ying Zhu et al, 2008).

We are therefore enlightened by a mental explanation but not because it names physical states whose state descriptions enter into principled (antecedent cause type) explanations of the type studied in purely natural sciences. For instance, <She unloaded

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her truck in the front garden because she wanted to repay the householder for his rudeness> explains the truck driver's actions in a way that <The brain state called X caused the bodily movements clustered together under the term Y> does not (even if they designate the same physical events, a problematic notion that depends on a contested view of meaning and reference). The first explanation encourages us to look at certain features of the human story connecting the truck driver, the householder, and the bricks but the second tells us very little about the wider human life-world related significance of the brick-dumping behaviour. Mental explanation lays bare certain connections because of their significance to the individuals concerned in a particular historico-cultural setting rather than merely identifying physical states which, if we only understood them, would be shown to be causally connected to each other in biologically regular (thus discernible) ways.

Explanations of the mental or intentional type concerned serve to distinguish among events, to differentiate the networks and levels to which they belong, and to reconstitute the lines along which they are connected and engender one another according to their meaning (Foucault, 1984, 56). Therefore an intentional explanation locates the agents doings in a nexus of human meaning and relationships rather than in a network of physical processes (including neural activity). We can choose which descriptions and connections are the most revealing and, by so doing, enter what Bolton and Hill call the domain of decision, (1996) where we give reasons for our choices and actions and take responsibility for the kind of knowledge we valorise. In psychiatry, as elsewhere, to make a certain kind of choice is to render the contours of our inquiries into disordered human subjectivity in a certain kind of way. The way we choose to paint the picture of the disorder will open up some understandings and close off others but none are more ultimate or realistic than any other. What is more some may be counter productive because no significant interventions may follow certain descriptions—e.g. in terms of neurophysiology whereas certain very impor-

tant interventions may flow from others e.g. in terms of addressing economic insecurity and inequity or factors like marginalisation and political resentment (here we need to “get real” in the sense of what are commonsense realistic responses and not just techno-scientific babble removed from clinical relevance or plausible efficacy).

For that reason, causal explanations in psychology (identifying underlying brain mechanisms and their possible chemical disruptions are often not really what we are after in psychiatry. They may be helpful for cognitive neuroscience but even there may blind us to actual patterns of adaptation that fit an organism into a particular (ecological, or social) setting but can become dysfunctional in a wider context. The human organism relies on its brain to develop routines that use patterns of language-related activity to organise itself in relation to the human life-world. If the brain is malfunctioning in certain ways, for instance it is disorganised by hyperactive associations (as in acute psychosis) or is overcome by impulsivity and resentment, the person concerned becomes a misfit in our life-world (and some of these dysfunctions may be amenable to biological modifications of the type appropriate to Axis 1). If the mismatch causes brain-world malfunction in other ways, for instance because it is out of synch with the rhythm of human activity and give and take in our life-world, then the human being will fail to fit as a being-in-the-world-with-others in a different way (perhaps that seen in Autistic spectrum disorder). If the brain does not make the connections with emotional and interpersonal responses that others tend to make, then the terms of engagement between that individual and fellow members of Kant's moral kingdom of ends are changed (in a callous, calculating, and self-serving direction). Each of the discontents of the mind has causal underpinnings, perhaps discoverable in neurophysiological (or cognitive neuroscientific) terms, but the significance of what is going on is registered and articulated with our life-world in terms of the behaviour, con-

versation, and relationships of the person concerned. How those aspects of being human become configured in the context of the individual's life history produces a singular outcome that is best understood through “thick” or narrative rather than colourlessly scientific descriptions (Geertz, 1973).

One might conclude that a malady of the soul is not a natural kind (in the terms used in natural science) with a causal/biological configuration that allows the person's life problems to be understood it but is unique or particular to the situated person such that the suffering patient is an enigma (Levinas, 1996) rather than a code to be cracked by the dispassionate analyst. Inter alia he or she is a soul who cries out to other souls for recognition, witness, and a hand of compassion (or even healing).

The confluence of discourse, history, and a (di)stressed human psyche creates a dis-ease or malady of the soul the contours of which reflect both the world of meaning and the position of the subject engaged with it. The soul bridges worlds and its maladies are therefore cross-categorical (Armstrong, 2004) inhabiting the actual world and the discourse and sets of values that order it. The messy realism resulting from discursive naturalism is accommodated if we see psychiatric illness or mental disorder as a metaphor (Pickering, 2006) that assimilates a complex breakdown in the relation between a human being and the human life-world to a disease process. The metaphor is helpful because it focuses us on the functions of the individual that adapt him or her to a socio-cultural milieu as a complex cultural artifact produced in an image (of the human and its variations) under the imperative of the word (Lacan, 1977, 106), a realisation that carries with it both emancipation and responsibilities.

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## DSM, Groups, and Phases: Beyond the Laundry List

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Do mental states possess "haecceity"? That is, are there "natural kinds" of mentation? Do these types include not only normal mind but also mental pathologies? No one today really knows whether the answers to these questions are affirmative or negative. Hence, efforts to formulate taxonomies of mental disorders, i. e. "carve nature at its joints" with regard to the taxonomic parsing of mental

disorders, remain at present without any coherent conceptual foundation.

This void may help to explain why to date the formulation and re-workings of DSM have continued to entail processes more akin to chaotic struggles among Kuhnian factions aiming at institutional domination than rigorously progressive Popperian science trying to penetrate natural phenomena. The outcome evident in the content of DSM's most recent versions resembles the elaborated pronouncements of ancient Greek substance classification and Linnean zoology more closely than the paradigmatically succinct solutions of modern mechanics or post-Darwinian biology. For psychiatry to become truly scientific in the twenty first century, normative psychology must transcend ad hoc consensual nosologic categorization as all mature modern sciences have done. On-the-fly, messy, theoretically uninformed grab-bags of empirical classification must give way to synoptically principled prediction.

Do we have the conceptual means to mobilize unified predictive principles in the service of a rigorous psychiatric nosology? Perhaps not quite yet, as Dr. Frances points out. However, two possible contenders for a scientifically mature classification scheme in psychiatry can be gleaned from methodologies employed by today's physicists both in theory and practice: the group algebra of law-like invariances and the phase discontinuities of emergent symmetry-breaking. Group-algebraic invariance has proven its theoretical utility in the classification of sub-atomic particles using sets of equivalently energetic numerical quantum state designators to establish a firm relational structure for the standard model of physics. Discontinuously emergent symmetry breaking in phase transitions has proven practically valuable in the rigorous engineering of critical phenomena transforming materials vital to the manufacture of semiconductors.

Defenders of ad hoc empirical psychiatric classification might argue against analogies between behavioral norms and mathematical principles

relevant to inanimate matter. Those defenders might maintain, in accordance with Engel's biopsychosocial model, that living things as physical systems demonstrate emergent properties beyond those of their particulate micro-constituents. It is true that psychological phenomena until now have been more productively characterized through biologically emergent properties than through elemental physical substrates. Yet biologists are starting to make taxonomies of life yield both to the law-like logic of algebraic invariance at the micro level and to the phase transitions inherent in non-linear systems analysis; the meta-implications of such advances are also beginning to map out a path linking the categories of molecular biology and the emergent properties of behaving, mentating organisms. Those links have the potential to fuel unprecedented future insights regarding psychopathogenesis.

It has recently been concretely shown that both formal symmetries and phase-transitional stabilities suffuse biomolecular ensembles in vivo. Reduplication of homeotic DNA segments within individual genomes form repetitive nucleotide sequences whose differentiation may lead to a phenotypical periodic table of sorts. Moreover, non-linear interaction between some homeotic gene segments and the regulatory meta-functions of "dark" DNA show promise as possible generators of molar phenomena involving whole organisms. Hence, biological emergence no longer presents a theoretical barrier between the mathematical tools of micro-physics and the nosology of human macro-behavior.

These are hopeful beginnings. However, their eventual implications cannot be productively leapfrogged by premature taxonomic efforts, which most likely will merely produce another round of increasingly arbitrary DSM laundry lists with the kinds of unintended negative consequences about which Dr. Frances has warned. Only a disciplined mathematical exploration of normal and pathological psychological phenomena, employing the rigorous algebra of symmetry groups and spontaneous symmetry breaking, will sort out the actual invariant laws and critical phase transitions, i. e. the

natural contours that psychiatry might legitimately discern, applicable to human mental life according to the standards of a mature science. Such an exploration should be the process by which the next version of DSM, when the time is right, will be born.

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## Phenomenologic Contribution to Debate about our Ability to Detect Prodromal Schizophrenia

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Dr. Frances, in your various communications expressing your concerns regarding an overly zealous and precipitous DSM-V, you (Frances, 2010) rightfully place the early detection diagnosis of prodromal schizophrenia at the center. “The Psychosis Risk Syndrome is certainly the most worrisome of all the suggestions made for DSM5. The false positive rate would be alarming 70% to 75%...” Indeed, this entity remains elusive. In addition, the untoward consequences of misuse or inaccuracy of such forcing “subthreshold psychopathology into a diagnostic class as risk syndromes” would be enormous. To take one example, we really do not know the full impact of antipsychotic or related medications on the adolescent brain during a period of growth and development of precisely those areas (e.g., prefrontal cortex) thought by many to be most compromised in schizophrenia, thus endangering the false-positives of non-schizophrenic youth with misuse of this diagnostic classification. Indeed, Carpenter’s (2009a) response to your criticism indicates that he takes the problem very seriously: “But we must determine if classification is reliable by non-experts and if stigma can be minimized and excessive medication can be avoided... a vexing problem.” He (2009b) echoes these concerns when he writes in his editorial: “Will placing individuals in this diagnostic category do more harm than good because of stigma or the unwarranted administration of treatments with a poor benefit/

risk ratio?” (p. 841). Nevertheless, he argues that “without the class, a framework for early detection and intervention is lost, and the opportunity to develop evidence-based treatment will be minimized” (p. 842).

In the 1994 book (edited by J. Sadler, M. Schwartz and O. Wiggins) for which you kindly wrote the preface, some of us (e.g., Mishara, 1994) made the plea for continuing the largely European (and often untranslated) effort of descriptive psychopathologists not only to describe the subjective experience of patients suffering from neuropsychiatric disorders but also to *formalize* these experiences according to phenomenologic dimensions in a manner which could be operationalized for further empirical study. More recently, I (2010) have published a brief summary of one of these psychiatrists, Klaus Conrad, who advanced the concept of “delusional mood.” As Berner (1991) writes, “All hypotheses about delusional atmosphere presently discussed in German psychiatry refer in one way or another to Conrad’s (1958) ‘gestalt-psychological’ analysis.”

In a period prior to the emergence of delusions in acute schizophrenic psychosis, Conrad argued for a prodromal *delusional mood* or *atmosphere*, which lasts for days, months, or even years. During this period, the patient experiences increasingly oppressive tension, a *feeling of non-finality* or expectation. The subject experiences a marked change of *emotional-motivational* state, which, although starting with heightened awareness of perceptual aspects eventually pervades the patient’s entire experiential field. The patient may feel excitement, “intoxicated” anticipation, but also, suspiciousness, fear, depressive inhibition, guilt, a feeling of separation from others, and often, a combination of these. During the delusional mood, *nothing in particular has changed in the experiential field and yet it has changed in its totality in a hitherto unexperienced manner*. Something is “in the air,” but one is unable to say what. Conrad calls this initial, expectational phase, *Trema* (stage fright) as the

patient has the feeling that something very important is about to happen. Although this internal state imbues the entire field of experience with a transformed “*physiognomic*” quality (reflecting the changed or *increased* basal affectivity due to putative underlying neurobiological changes), the subject does not attribute the changes to his/her own state but to some, yet to be understood process in the world. By being unable to shift “frame of reference,” the delusional patient exhibits a “failure to transcend” the current perspective. “We perform this exchange of perspectives *without the slightest effort* innumerable times each day” (Conrad, 1958, 49, my trans). *The transition from delusional mood to the Aha-Erlebnis of the delusional revelation occurs precisely at the moment of loss of the patient’s ability to transcend the experience.*

While at first, the delusional mood sounds like a vague state which may be hard to operationalize, there are several reasons why I feel that delusional mood may not only prove to be one of the most important predictors of conversion to schizophrenia but may also throw light on the neurobiology of the early stages of the disorder. That is, it may provide precisely the criterion which would hopefully safeguard our youth from the false positives of over-expansive diagnoses meant to capture “sub-threshold psychopathology”:

1) The delusional mood as predictive of conversion has been established empirically in a large (n=267), albeit retrospective study (Hambrecht et al., ).

2) Critically, the patient experiences this state as something absolutely new, i.e., *in a hitherto unexperienced manner*, suggesting very specific underlying neurobiologic changes: “In the slow, insidious and torturous basal affectivity [of the delusional mood], the surrounding perceptual field obtains an alienating but also new *physiognomic* quality, which it never had before” (Conrad, 1958, p. 46, my trans). Moreover this state is protracted although the length before conversion to beginning psychosis appears to vary. Therefore, the specificity of delusional mood to beginning schizophrenia runs contrary to Fischer and Carpenter’s (2009) view that “psychotic experience

is to the diagnosis of mental illness as fever is to infection – important, but non-decisive in differential diagnosis” (p. 1).

3) We (Mishara and Corlett, 2009) proposed a plausible and also testable neurobiological model (i.e., aberrant prediction error of the implicated motivational-reward circuitry) to underlie the initial prodromal delusional mood and the transition to subsequent stages of progressive psychosis, including the delusions themselves (not described here). In the delusional mood, attention is drawn toward irrelevant stimuli, thoughts, and associative connections which are distressing and unpredictable. This reflects an impairment in the brain’s predictive learning mechanisms, such that unexpected events, prediction errors, are registered inappropriately (Corlett et al. 2007).

4) Following his “teacher” and clinic chief, E. Kretschmer, Conrad (1959) distinguishes the psychogenic or reactive delusions of reference resulting from a “sensitive” temperament (so-called “sensitive delusions of reference”) from genuine delusions resulting from schizophrenic psychosis: “In the sensitive delusions of reference, it is always only certain notable aspects of the environment which achieve any significance, e.g., a certain look, someone’s remark... it is never the entire field with all its components...” That is, with the intense emotion of the reactive state, “the inability to transcend one’s own current viewpoint” is (unlike acute psychosis) “only temporarily and functionally impaired” (p. 307, my trans). Clearly, we all have the tendency to focus more on ourselves and relate things in our environment to ourselves the more intensely we experience emotions, particularly negative emotions as in “road rage,” but this is not what occurs in the schizophrenic psychosis where schizophrenia patients are unable to shift the frame of reference that “everything appears to revolve about me,” and feels themselves in a delusionally fixed manner to be center of the universe (p. 55).

5) Contrary to his “teacher,” Kretschmer, and some recent interpreters, who call themselves “phenomenological,” however, Conrad

(1963) sees a complete disjunction between schizoid personality disorder and schizophrenia: “I am personally of the view that the subjective experience of schizophrenia (when considered from the inside or phenomenologically) has absolutely nothing to do with the continuation of the schizoid or schizothymic experience as if it were simply its increase to a highest level. Rather, schizophrenia moves in an entirely opposed direction. For schizothymia indicates on the subjective level an experience of exaggeration of the I or self from outside, that is, the fortification of I-boundaries or walls, or individuation; but schizophrenia is just the opposite, it is the experience of loss of self or the I, the breaking down of barriers between self and outer world...” (p. 99, my translation). Once again, this suggests the specificity of the phenomenology of the subjective experience in the differential diagnosis.

Although admittedly not always as specific as we would like, such phenomenologic distinctions base themselves on the formalization of the subjective experience of the patient. They can be operationalized and lend themselves for further neurobiologic study. I am aware that I make these observations in a Zeitgeist of precipitous optimism with regard to finding biologic markers for disorders which would pre-empt or at least guide our descriptive classifications and thus replace the hard work of the fine-grained phenomenology of the patient’s subjective experience (which I propose here). While our various neuroimaging technologies have allowed us to map mental function onto its neurobiological substrates, Martha Farah (2005) pointed to the danger that the public may over-value these findings. However, I would add that it is not merely the public but the researchers themselves who may over-value the significance of their own findings. For example, 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 subjective state-dependent prodromal delusional mood which I propose here as a possible differential diagnostic criterion to safeguard our youth from over-extended diagnostic classification. That is, “any talk of a “paradigm shift” (see Frances’ critique in his “A warning sign,” *Psychiatric Times*, June 26, 2009) should take into account the philosophical phenomenology of early schizophrenia rather than trying to leap over it for more “objective” measures, which may still be many years away.

Hilde Bruch had observed that the once rare and exotic anorexia nervosa (for her, a misnomer as there is no loss of appetite) had become an epidemic of “me too anorexics.” It is hoped that by restricting the diagnosis of prodromal schizophrenia to the subjective experience of delusional mood as defined above will prohibit a similar development of “me too schizophrenia.”

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## The Muddle that is DSM-V

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In reading Dr. Frances' ongoing critique of the DSM-V process, along with the few responses by DSM-V participants, the just-released "Draft Revisions," and the earlier documents such as *A Research Agenda for the DSM-V* published in 2002, it is easy to draw the impression that DSM-V is something of a muddle. It also seems apparent that the issues at stake might readily

profit from conceptual analysis. In this commentary I will begin with a review of the agreed upon problems of DSM-IV, the efforts of the DSM-V leadership and working task forces to address these problems, and finally my views on how this has progressed. In reviewing Dr. Frances' critique I will not discuss the issue of transparency, as I don't feel qualified to sort out that issue.

The list of problems with the DSM-IV is well known. They include the significant degree of comorbidity among patients, the related problem of poor separation among DSM-IV disorders (the problem of fuzzy boundaries or, in the language of Kendell and Jablensky, the lack of "zones of rarity"), as well as the poor separation of disorder from normality, the dramatic non-specificity of pharmacologic agents in treating the various disorders, the failure to identify clear physiologic bases for the disorders, including the failure to find even one biological marker for a psychiatric disorder since the publication of DSM-V, and the ever-more-clear lack of genetic specificity for the disorders.

This congeries of problems affects the integrity of the current diagnostic categories, and for that reason the shibboleth of DSM-V is: improve validity. We are all aware that the goal of DSM-III was to improve reliability, and it generally agreed that that goal was in large measure accomplished. At the same time, we also recognize that the usefulness of reliability depends on validity. It beggars the imagination to envision how many thousands upon thousands of dollars have been spent researching reliably constructed psychiatric disorders that in the end will prove to be useless constructs. Everyone agrees that the reasonable, common-sense list of validators outlined by Robins and Guze 40 years ago (1970) are not still achieved by the DSM-IV diagnoses. And Kendler's completely reasonable addition of differential response to treatment as an additional validator (1990) has become a shambles in the face of our current practice of using just about every class of psychotropic to treat just about every

class of disorder.

In the face of these dilemmas, the question becomes: what to do. And here, of course, is where Frances and the DSM-V group collide. In the Introduction to *A Research Agenda for DSM-V*, Kupfer, First, and Regier set the bar rather staggering high. The describe a goal of the "white papers" as "...to devise a research and analytic agenda that would facilitate the integration of findings from research and experience in animal studies, genetics, neuroscience, epidemiology, clinical research, and cross-cultural clinical services—all of which would lead to the eventual development of an etiologically based, scientifically sound classification system" (2002, xv). In face of the obstacles mentioned above, it is hard not to react: etiologically based, scientifically sound - are you kidding? The authors then go on to explain that the DSM-III diagnostic system (and by implication DSM-IV) adopted a "neo-Kraepelinian diagnostic paradigm" that served well for the reliability accomplishments of DSM-III but also left us with the validity problems of DSM-IV, and that "...an as yet unknown paradigm shift may need to occur. Therefore, another important goal of this volume is to transcend the limitations of the current DSM and to encourage a research agenda that goes beyond our current ways of thinking to attempt to integrate information from a wide variety of sources and technologies" (xix). Does this mean that the authors are indeed promising a paradigm shift for DSM-V, or only that they are encouraging work in that direction. They remain vague on that point, but they do seem intent on fixing the problems of DSM-IV.

On the question of what the fix amounts to, I find it hard to disagree with Frances that most of the changes, for a variety of reasons, make little sense. First there are the trouble makers, most notably the much discussed Psychosis Risk Syndrome. One can argue over the risk/benefit status of this diagnosis, but I find the argument compelling that the inevitable incidence of false positives and over-labeling outweighs the putative benefits of adding the diagnosis. Then there are the weirdos. Good examples are Sexual Inter-

est/Arousal Disorder in Women and Sexual Interest/Arousal Disorder in Men. Inasmuch as these peculiar disorders had a sort of place in DSM-IV, they are also emblematic of another tendency of the new manual: juggle the categories a bit, spiff up the descriptions and give them new names, and declare it all more scientific. Then there are the real shuffle-the-deck proposals, like those in the personality disorder section. It's hard to be for or against such proposals; they don't seem to be particularly better or worse, but what they do seem to be is arbitrary and not obviously scientific.

The rather arbitrary quality of the changes highlighted above doesn't leave one with the impression that we are creeping closer to real scientific validity - that we are getting closer to those infamous joints in nature where we are supposed to be doing our carving. Or that we are achieving the paradigm shift that will rectify the problems of the superannuated DSM-IV. For that shift we must move to the grand innovation of the new DSM, the near universal use of dimensional scales. They are indeed everywhere. To begin with there is the "Cross Cutting Dimensional Assessment" that precedes any particular diagnostic process. And then there are the scales of severity that accompany the individual diagnosis - in the case of some diagnoses like schizophrenia accompanying every symptom. These cumbersome dimensional scales seem like the product of a desperate, obsessive mindset convinced that enough dimensional detail will rectify the notorious problems of the previous manual. Will they make the manual more "scientific"? I suppose so, in a small way. Will they solve the problems of DSM-IV (comorbidity, fluid boundaries, etc.) and represent the desired paradigm shift? Hardly. Perhaps more relevant - will they be used? Speaking as one who spends time with patients, it seems certain that the answer is no. Who would have the time - or would want to take 20 minutes to do what an experienced clinician does instinctively in a flash? When the authors write that the scales can be administered quickly and will help the clinician follow the progress of the patient, one can only wonder, when is

the last time you have dealt with living patients?

On balance, I have to agree with Frances that the proposed changes in DSM-V do little to correct the problems of the previous manuals and at worst create unnecessary problems with their questionable innovations.

In view of the insurmountable problems achieving validity in psychiatric diagnoses, I want to invoke the distinction made by Kendell and Jablensky in their much-cited 2003 article - the distinction between validity and utility. They reason that, if real validity, as judged by the standard of, say, Huntington's disease, is not remotely possible for standard psychiatric disorders, we should abandon that impossible goal for the more modest goal of utility. The notion of utility addresses the descriptive, syndromal categories that we rely on in our clinical work. They add: "We propose that a diagnostic rubric may be said to possess utility if it provides nontrivial information about prognosis and likely treatment outcomes, and/or testable propositions about biological and social correlates" (2003, 9).

Kendell and Jablensky address the research implications of the utility criterion, such as for instance, that research should not be bound by strict adherence to the syndromal definition of a disorder. They do not, however, discuss sufficiently the distinction between utility in practice and utility in research. It is a paradox of the DSMs since DSM-III that while the respective authors insisted on the utility of the manuals for practitioners, what they produced in fact is were manuals that guarantee reliability (and thus utility) for researchers. Take, for instance, even the great innovation of DSM-III, the highly (and justifiably) touted diagnostic criteria. Who uses them? Neophyte clinicians and researchers. Experienced clinicians, in contrast, use prototypal, syndromal diagnoses, often a pragmatic mix of the currently ascendant biomedical categories with a varying admixture of psychodynamic considerations. Is there any hard evidence for this anecdotal (and personal) impression? I think not. If you

google the topic of 'use of DSM-IV diagnoses', you will find endless articles and volumes on *how to use* the diagnostic criteria and the manual, but nothing on how in fact experienced clinicians do in fact use it. The latter don't do criteria checks, they use syndromal prototypes. And with regard to the single dimensional scale in the DSM-IV, the GAF, they apply the "rule of tens" - 40, 50, 60, 70 - or if more careful, the "rule of fives." The only clinicians I am aware of who more finely tune their GAFs are those unfortunate individuals required to play the mindless game with insurance companies of demonstrating that the patient has progressed by one or two GAF points, thus demonstrating both progress and the need for further treatment. In the area of actual practice, an unacknowledged irony of the DSM-II to DSM-III revolution is that the great innovation was not the diagnostic criteria so much as the shift from psychoanalytically laden syndromes to descriptive (and biologically oriented) syndromes. For practitioners the diagnostic criteria simply spelled out that change.

The serious problem for DSM-V is that not only don't we have a so-called paradigm shift to make the nosology more valid, we don't even know how that will occur, or even if it will in fact ever occur. Genetics is not looking promising for sorting things out. Will neuroscience do the job? It will certainly tell us profoundly more than we know now about mental disorders, but will it provide us with a new and valid nosology. If we try to imagine ourselves into the future, we can do so at the level of the symptom or the syndrome. For a symptom such as depression, we can readily imagine multiple phenotypes, multiple genotypes, more neuroscientific clarity than we have now, possibility more targeted pharmacology, some kind of syndromal coherence, with persistent fuzzy boundaries among the depressive types as well with other symptoms like anxiety, and of course a net of connections among depressive types and the various genetic and neuroscientific factors. What I presume we will not have is depression as a symptoms like hypertension, with a simple phenomenological description

and a clear biological measurement, along with a clear explanation at a biological level and a host of clearly distinct etiologies. Heaven only knows how we will sort this out in our future nosology.

Much the same can be said at the syndromal level. After 100 years we are still trying to pin down the etiology of schizophrenia. We are now aware of the polygenetic, as well as overlapping, genotyping of this disorder. We are also aware of the variant phenotyping, as well as of overlapping phenotyping, for which we have the category of schizophrenic disorder. Finally, we now have extensive pharmacologic overlapping, with atypical neuroleptics being used as monotherapy for bipolar conditions. How will all this fall out nosologically, and what kind of help will we get from neuroscience. One can imagine a single mega-disorder, with a multitude of subdivisions, or a multitude of more or less coherent, more or less distinct disorders. What seems clear is that the decisions and divisions will not be made on a simple carve-nature-at-the-joints principle. We are dealing with a nature of countless joints, and where we do our carving will be decided by practical, inevitably arbitrary reasons.

In view of these facts - if I may permit them that status - how can we not follow Dr. Frances' advice and act conservatively, do less and not more, avoid changes when there is no real science to back them, avoid yet more unfounded diagnoses - all until we know a lot more than we know about the nature of mental disorders.

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## Paradigms, Consequences and the Normative Dimension

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Allen Frances has carefully criticized the path of development of the DSM-V. In the course of his critique, Frances notes that the only nosological change worthy of being dubbed a "real paradigm shift" would consist of a change from the current descriptive categories to a nosology based upon biological findings. It is important to understand that, at least in terms of philosophical assumptions about the "real" nature of mental disorders, an emphasis upon the biological would not represent a substantial shift. I think a guiding principle in the development of recent DSMs has been the assumption that mental disorders are best conceptualized as discrete biological abnormalities. The use of descriptive categories has been provisional pending the discovery of the root biological causes of disorder that will, at last, put our nosology on a firm objective footing. What I find more intriguing, from the standpoint of a shift in philosophical assumptions, is the emphasis that Allen Frances places on the practical consequences that will result from any change in our nosology. While the nosology of the current DSM has been regarded as strictly descriptive in nature, it has always been more. By demarcating certain clusters of signs and symptoms as disordered and in need of remedy, the DSM becomes at once descriptive and prescriptive: a sort of "call to action." As Allen Frances points out, under certain circumstances the subtlest change in how a disorder is described will result in a radical change in how the disorder is treated. Changing to a nosology based strictly upon biological findings would, of course, have its own consequences. I think there is

reason to believe that the consequences of such a change would be detrimental. Assessing the merit of a nosology, not only in terms of objective accuracy, but also in terms of how it functions, brings normative questions of how a nosology should function more clearly into focus. It seems to me that the explicit recognition of this normative dimension of nosology would represent a more significant paradigm shift for the DSM. Normative issues of moral and political legitimacy become intermingled with issues of empirical validity in the development of a nosology. It is interesting to note that there is much in Allen Frances's work that enriches nosology from the standpoint of grappling responsibly with this normative dimension. In particular his emphasis upon the importance of an "open process" in the development of the DSM warrants further analysis.

The DSM is famously neutral with regard to the etiology of mental disorders. Nonetheless, aspects of the current DSM make it highly compatible with the assumption that mental disorders are best understood in terms of biological abnormality. The fact that DSM mental disorders are distinct entities, felt to reside within an individual, having a threshold, such that they are either present or absent, makes the current categories "ontologically" suitable for underlying biological explanation (Sadler, 2005, pp. 194-196). I think there was initial hope that the signs and symptoms of the specific DSM disorders cluster together as they do because they share a specific biological etiology. Of course, this hope has been dashed by mounting evidence to the contrary. Patients will frequently display overlapping symptoms of multiple disorders that shift over time, such that a person may have a different diagnosis at different points in time or multiple diagnoses at the same point in time. Biological treatments have failed to show specificity such that psychopharmacological agents with vastly different biochemical qualities will show equal efficacy in the same disorder, the same pharmacological agent will have efficacy across different disorders, and people with the same disorder will respond differently to treatment with the same psychopharmacological agent. If

we maintain the assumption that mental disorders should be distinct entities with a specific biological etiology, disenchantment with the DSM naturally results (Fink and Levine, 2006). Of course, the true nature of mental disorder remains subject to debate. Mental disorders may be so complex and multi-factorial in origin that they will never rightly be understood in terms of a simple and specific biological etiology (Zachar and Kendler, 2007). In view of developing concepts of neuroplasticity it becomes difficult to understand neurobiology without implicating the social environment and the significance of the social environment for the particular person involved (Schore, 2003). One has to question whether understanding mental disorders strictly in terms of specific biological etiologies does more to satisfy the needs of scientists for certainty and parsimony than the needs of patients for relief from their illnesses. When we assess a nosology in terms of how it functions in the world there may be serious drawbacks to a nosology of mental disorders based solely upon biological markers.

A nosology of mental disorders based strictly on biological findings would be harmful in so far as it excluded or appeared to supersede the need for debate regarding pathological mentation. The belief that biological findings could stand on their own in a nosology of mental disorders may be based in the belief that nosology is strictly or primarily a matter of objective accuracy. Nosology must maintain a concern for objective accuracy but such a concern has never been sufficient. A collection of objectively true but arbitrary facts would hardly meet the needs of an adequate nosology. What is the significance of this biological difference? Why shouldn't it be treated as a normal biological variant? A nosology of mental disorders based upon biological findings would necessarily involve correlating biological differences to differences in mentation. Pathologizing biological difference would entail judgment regarding proper mentation. Biological findings or not, a nosology of mental disorders must engage in judgments regarding pathological mentation with concomitant judgments regarding the nature of a life

well lived. As Fulford has pointed out, the most objective biological finding in any branch of medical science entails a commensurate (negative) value judgment if that biological finding is to be considered pathological. He notes that what allows us to ignore or forget the necessarily implied value judgments in mental and physical disorders and talk about disease as strictly a factual state of affairs is, "... the extent to which the criteria for the value judgment expressed by it are settled or agreed – the more settled the criteria, the less marked (by and large) are its evaluative connotations" (Fulford, 1989, p.61). What allows us to treat a pathological finding in strictly objective terms is the fact that the value judgments involved are uncontroversial and generally shared. A broken bone can be treated as a fact rather than a value judgment because there will not be much debate about the negative value of a broken bone. Judging the proper function of mentation may be more controversial at times. Is the difference in mentation pathological or a matter of justifiable plurality? If the difference in mentation is unjustifiable, is it best conceived as a medical issue? For ethical reasons it is important that a nosology should never disguise normative judgments regarding proper mentation as matters of biological fact. When normative judgments remain strictly implicit, there is greater chance that these judgments may be unfairly prejudiced and harmful. The public would be better served by a nosology that recognizes the necessity of value judgments and is explicit about the process used to justify these judgments.

In addition to marginalizing normative discourse, a nosology that describes mental disorders strictly in terms of biological findings and explanations would obviously marginalize social and psychological levels of explanation. The corresponding prescription for action would accordingly emphasize the biological while deemphasizing the relevance of social and psychological interventions. An important achievement of the operationalized DSM was facilitating re-

search that justified the use of biological treatments for mental disorders. This expanded the horizon for people with mental illness such that it included the potential benefit of biological intervention. Certainly we should be leery of changing our nosology in a way that would foreclose the horizon to one of biological intervention and deprive patients of potentially beneficial social and psychological interventions. Rather than a reduction to the biological, decontextualized from its social and psychological milieu, it seems to me that the DSM-V would be improved if, as Allen Frances suggests, it also adequately addresses "the developmental, cultural and gender contributions to diagnosis" (Frances, 2009). The appeal to empirical evidence must clearly play an important part in any classification of mental illness. But, what counts as empirical evidence, much less persuasive empirical evidence, will involve evaluative theoretical commitments. If we start from the assumption that mental disorder is essentially abnormal biology, evidence derived from a methodology that differs from the biological sciences may be deemphasized, if not derided, as something less than scientific. On the other hand, if we recognize the value of a social category like gender in the development of a nosology, we will value evidence derived from methodologies that differ greatly from those of the biological sciences. Rather than being able to rely upon empirical evidence to settle any argument, it appears we have to rely upon arguments about practical relevance to determine what constitutes valid empirical evidence.

Another form of evidence that will be marginalized by an exclusive emphasis on biological findings is evidence derived from the first person experience of mental illness. Obviously, there is a certain primacy to this experience. This is the experience that we are trying to explain, intervene upon or correlate with biological findings. The development of the objective biological science of mental disorder relies upon a pre-understanding of the experience of mental illness for its relevance. A weak understanding of the experience of mental illness will inevitably lead to a weak understanding of the

biology of mental illness. In terms of argument about the relative value of evidence for our understanding of mental disorders, the biological scientist cannot argue against the value of evidence specific to mentation without shooting herself in the proverbial foot. But, all too often, this is exactly what occurs. Evidence derived from the first person experience of mental illness will in no way conform to the methodology of the biological sciences. Most likely such evidence would be derided as subjective and unscientific. Yet, as ironic as it may sound, empirical evidence from the first person experience of mental illness is essential to our objective understanding of mental disorders. As essential as the first person experience of illness may be for our objective understanding of mental disorder, it may be even more essential in the normative assessment of the consequences of our nosology.

It would be impossible to foresee all the consequences of a change to our nosology. It should be obvious by now that, from the standpoint of patient care, I think there could be multiple negative consequences from a change to a nosology based strictly on biological findings. Frances has pointed out that the DSM has practical consequences in multiple contexts. The needs placed on the DSM may shift from determining eligibility for reimbursement in an insurance context to determining responsibility for one's actions in a forensic context. It is far from clear to me that we can expect any one particular formulation of nosology to address the multiple needs to which the DSM has been subjected. Even under the relatively unproblematic rubric of "clinical utility" there is room for conflicting interests. Frances argues vehemently against a proposed change in the DSM-V to include categories for subthreshold or premorbid conditions. He fears that the introduction of these categories would be a boon for the pharmaceutical industry, legitimizing a more widespread use of psychopharmacology that would ultimately prove to be more harmful than beneficial to the general public. The guild interest of psychiatry is inextricably tied to the pharmaceutical industry. There is a warranted fear that the lines

of "clinical utility" could become blurred in a manner that unduly emphasizes the pharmaceutical. There are many stakeholders in the development of the DSM. Certainly, there are none quite so intimately affected by changes to the DSM as people with mental illness. The vulnerability of people with mental illness demands a professional ethic of psychiatry such that the interests of patients must always be prioritized. A nosology of mental disorders must embody this professional ethic. Attending carefully to the process of developing the DSM is essential in insuring that the needs and interests of patients will take precedence over the interests of the pharmaceutical industry and other potentially conflicting interests in the design of the DSM.

Allen Frances expresses concern over elements of secrecy in the development of the DSM-V and emphasizes the importance of an open process. In her work on the philosophy of science Helen Longino has emphasized how important an open process is for the objectivity of the sciences. As she puts it there should be access and appropriate response to "...recognized avenues for the criticism of evidence, of methods, and of assumptions and reasoning;" (Longino, 1990, p. 76). But the importance of an open process does not end at issues of objectivity. The work of the philosopher Jurgen Habermas has emphasized the importance of an open process for the moral legitimacy of our institutional norms (Habermas, 1990). The best way to know if our institutional norms are attending to the needs and interests of those affected is to include those people affected in un-coerced dialogue about those norms. The growing recognition that scientific norms have contingent consequences for the public at large has led to acknowledgment of the importance of democratizing science (Hogg, 1999; Kitcher, 2001; Kleinman, 2000; Sadler, 2005). Public participation is essential if scientific norms are going to reflect the needs and interests of the public. An open process for the DSM would entail public participation. I have

already noted how such public participation can enhance our understanding of mental illness, but it is also essential in insuring that our conceptions of mental disorder reflect the needs and interests of those suffering from mental illness. Of course all the open venues for criticism in the world are meaningless if those in authority are not sufficiently responsive to criticism. When there is a conflict in interests, whose interests will take precedence? An element of due process or fair process must also be invoked.

Allen Frances emphasizes the relevance of a nosological shift to biological markers. Barring such a shift he extols the importance of minimizing any change to the DSM in its current form. I am not sure that this conservative approach will entirely conform to Frances's intent to "first do no harm." An unfortunate consequence of the DSM in its current form is that all too often disorders are treated as if they are concrete and specific things analogous to a pathophysiological process. Frances notes that the DSM cuts these disorders at "awkward joints." Yet, the structure of the doctor-patient narrative can stray from these awkward joints only at risk of being out of joint with institutional parameters, not only in the forensic and insurance context, but in virtually every clinical context. Surely, the impression that these disorders do not change through time will only reinforce the illusion that they are immutable natural categories. The fact that research to date has been based on these disorders hardly seems good reason to perpetuate them. Two people that meet criteria for the same disorder are very likely to be different in a biologically relevant sense. They are even likely to be different in a phenomenological sense, because different combinations of signs and symptoms will meet criteria for the same disorder. Yet, research treats these disorders as if they are homogenous entities, further reinforcing the illusion that these disorders are specific and discrete in some underlying natural sense.

In terms of my analysis, the sig-

nificance of a shift from a nosology based on categories to one based on dimensions is uncertain. If dimensions are treated as some sort of natural kind having a specific (yet to be discovered) biological etiology, this would leave us with many of the problems we currently have: a tendency to marginalize the significance of the first person experience of illness, a tendency to marginalize social and psychological levels of explanation, and a failure to address the normative dimensions of nosology. I think a more significant change would result from a shift to an emphasis on the practical ramifications of our nosology. Explicitly addressing the normative question of what a nosology should do gives us the opportunity to address the ethical salience of prioritizing the interests of patients when conflicts of interest arise. The moral legitimacy of our nosology hinges upon this prioritization. Of course the interests of patients include a careful reckoning with reality, and in this context the importance of objectivity remains clear. Allen Frances's emphasis on the importance of an open process is relevant to these empirical issues. But, the relevance of different forms of evidence and intervention remains subject to debate. An open process proves critical here, as well. A scientific process open to public participation is a means of clarifying the needs and interests of the public. Of course clarification is not enough. A political process that facilitates the development of nosology that will attend to the needs and interests of those with mental illness is essential. An element of political legitimacy is therefore tied to the moral legitimacy of our nosology.

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## The DSM-V Debate: A (Brief) Reader's Guide

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In the newspaper exchange between Allan Frances, general editor of DSM-IV, and various DSM-V revision participants, it is difficult to know exactly what is under debate. Frances argues against the goals, biases, methods, and predicted consequences of DSM-V as he interprets them. These include the alleged DSM-V goal of enacting a "paradigm shift" from descriptive diagnosis to a nosology based in causal processes, which Frances says is premature given our current knowledge. He cautions against the personal interests of DSM-V Work Group members, who are inclined to promulgate the disorders they research. He challenges the methodology (and the writing style) of the DSM-V Work Groups, although the data analyses and field trials as they are commonly described sound much those of DSM-IV. And Frances predicts dire outcomes from the methods and draft

materials circulated thus far in the revision process. These include increased stigma against persons with mental disorders, forensic misuses, industrial opportunism, treatment side effects, misdiagnoses, epidemiological fluctuations, and lost ease of use. Various respondents counter his charges, reassuring readers that no paradigm shift is imminent, but that DSM-V conceives many disorders as spectrums, that its diagnostic criteria should detect sub-syndromal and prodromal states using dimensional diagnosis, and that it should minimize comorbidity. If this series of articles were read as a transcript of a roundtable discussion, one might wonder exactly what is under debate, as the focus shifts from methodology, to product, to outcomes, to *ad hominem* questions of who stands to benefit financially from various changes to the nosology.

I want to suggest that the subtext to this discussion is our perpetual uncertainty about the concept of nosologic validity. As yet, there has been almost no explicit discussion of this concept in the psychiatric literature. Like medicine generally, psychiatry strives to consider quantitative measures over qualitative ones; to eliminate personal, professional, or social biases from scientific decision-making; and to exclude social or financial interests in outcomes. Charitably interpreted, our mission is to elucidate psychiatric ontology, develop robust theory, and clarify our reasoning processes. We want to get the science *right*, and we want it to have an empirical basis.

But truth is notoriously elusive for empiricists. To philosophers, it is a value that either applies to a proposition, or does not. Applied to sets of propositions, truth becomes even more complicated, which led philosophers to turn toward semantic theories of truth and away from the syntactic view that shaped DSM [1, 2]. Eluded by truth, empiricist philosophers have shifted toward more nuanced views of theoretical merit. Psychiatry, however, has not. We have conspicuously avoided tackling difficult metaphysical and epistemological questions, leading us to call the DSM (from the third edition on) "atheoretical", an inaccurate but

forgivable maneuver against premature ontological commitments.

Psychiatry is not so misguided to think that DSM is appropriately evaluated as ‘true’ or ‘false’. Instead, we talk about ‘validity’, which unfortunately is as much a misnomer as is ‘truth’. Traditionally, ‘validity’ refers to arguments or proofs, not to systems of classification. The concept of validity has been modified by psychology to describe certain aspects of diagnostic tests rather than classifications. With respect to psychological tests, we might ask “Does this test measure what it seems to?”, “Does this test cover all the subjects we want it to?”, “Does it yield results that are consistent with other aspects of psychological theory, or broader scientific theory?” If we think about the application of psychiatric nosology as a diagnostic test, it sometimes makes sense to refer to the kinds of validity psychology uses: face, content, internal, external, construct, etc. However, the questions we ask about tests usually differ from those we ask about nosology, especially in the early stages of planning our nosology’s next iteration.

Psychiatry talks about ‘validators’ rather than ‘validity’, without saying what the relationship is between the two concepts. Beginning with the 1972 Feighner criteria [3], validators constitute nonempirical – or even pre-empirical – reasons for belief or theory endorsement. Many of these validators are epistemic values. Psychiatry uses epistemic values to elucidate its goals, and to articulate what should count as the success of a classification effort. Each DSM edition, however, favors different validators. DSM-III, for example, favored empirical adequacy and internal consistency over other possible virtues, leading to its emphasis on its so-called “atheoretical” diagnostic criteria, and on diagnostic reliability as an indirect measure of nosological merit. Both of these strategies have been criticized widely, though not always explicitly in terms of the epistemic values they reflect [4]. Frances’s DSM-IV strived to improve empirical adequacy by clearly articulating the standards of its research methodology. By defining carefully the methodology for DSM-IV’s literature reviews, data re-analyses and field trials; making the revision

process transparent; and scrutinizing reasons for changing the content of the DSM, the DSM-IV Task Force emphasized the importance of procedure over the content of the DSM product itself.

Michael First, general editor of DSM-V-TR and co-editor of *A Research Agenda for DSM-V*, favors a different strategy. First has criticized the DSM-IV on the basis of its product rather than its process. He cites diagnostic complexity as its main epistemic vice, arguing that the current nosology creates too much artifactual comorbidity, which limits the DSM-IV’s clinical utility [5]. By favoring the virtues of simplicity and utility, First and the DSM-V Work Groups have shifted the focus away from process and toward the product. The assumption seems to be that if a nosology is not usable, it must not be ‘right’.

Unfortunately but not surprisingly, we have no final arbiter of what goals and values ought to be endorsed preferentially. The best we can say at this point is that nosologic validity is some sort of assessment of the merit of the nosology. Each of Frances’s objections to DSM-V can be understood as a philosophical challenge to some nonempirical basis for ascribing or denying such merit. Each of his arguments reflects nonempirical reasons for belief or disbelief in a certain theory, entity, or methodology. Although they are seldom acknowledged as such, these nonempirical reasons for scientific endorsement undergird many of the public debates about DSM over the years, including debates about its validity. In his series of critiques, Frances challenges the presuppositions of the DSM-IV Task Force. Frances and his interlocutors sound like they are having different conversations because they are: they favor different nonempirical considerations for evaluating nosologic merit.

Despite the saltative form of his argument, Frances provides a service in his insistence that these nonempirical factors be open for discussion. I want to take from his words of caution the message that the DSM-V Task Force has not explicitly considered which nonempirical considera-

tions they favor, or their reasons for so doing. This concern is well founded, as there are a number of pre-nosological commitments that DSM-V ought to address.

To take just one example, let’s look at the DSM-V emphasis on dimensionalism. The choice between a categorical or a dimensional approach to nosology, or a hybrid of the two, was named as a research priority in the *Research Agenda for DSM-V* [6], and as a “fundamental decision” by Jablensky [7]. However, when key participants in the DSM process argue that “Mental disorder categories will eventually be redefined to reflect more useful diagnostic categories (“to carve nature at its joints”) as well as dimensional discontinuities between disorders and clear thresholds between pathology and normality” [8], they equivocate about what dimensionalism is meant to do. Psychiatry, as does most of medicine, conceives mental disorders as distinct classes, with more-or-less clear boundaries among them, as defined by the diagnostic criteria for each disorder. In other words, psychiatry presupposes a qualitative difference among mental disorders, and between disordered and healthy states. Just by saying that it strives “to carve nature at its joints”, and to identify a bright line between pathology and nonpathology, it commits to nosologic classes rather than symptomatic continua.

To add that there are “dimensional discontinuities between disorders” is inaccurate: though we may be uncertain about our current categories, we believe that ultimately qualitative distinctions are possible. To consider dimensionality is to consider an incompatible alternative to nosology, since dimensional views of psychopathology presuppose that all persons demonstrate certain psychological traits to a greater or lesser degree, and deny that mental disorders are disease entities that can be demarcated either in theory or in practice. Clearly, the APA is not considering a dimensional alternative to the DSM-IV classification.

Psychiatry does, however, want to conceive *diagnosis* as a matter of degree rather than as an all-or-nothing dichotomy. Whereas a dimensional alternative to categorical classification would logically deny the existence of

mental disorders – a considerable change in psychiatry’s ontological commitments – dimensional *diagnosis* allows quantitative rather than qualitative differences among the pathological and nonpathological traits exhibited by all persons. Like the DSM-V Task Force, Frances is mistaken, then, that “adding dimensions [to the DSM-V] would help to solve the categorical system’s problem with fuzzy boundaries” [9], but it would permit more nuanced clinical diagnosis. What we glean from this part of the debate is that psychiatry is ontologically committed to the existence of mental disorders, and wants the benefits of a categorical nosology; but we also want a less ambiguous conceptual schema, and a mechanism for more subtle and sensitive diagnosis. These are the nonempirical considerations, the “fundamental questions.” Interestingly, Frances agrees with the APA position that dimensions could improve “the accuracy and precision of psychiatric diagnosis” [9], but doubts whether we can do that yet in a meaningful way. So in the end he shares the nonempirical assumption of merit, but challenges our readiness to implement it.

This is but one example of how our imprecision about the concept of nosologic validity, and the nonempirical commitments that shape it, hinder our resolution of central problems that we face in revising psychiatric nosology. Although this series of articles does not elucidate the concept, it does remind us that many of the considerations that influence the DSM product are not empirical, and that they should be open to discussion. Many thanks to Dr. Frances for reminding us that these bases for nosological merit should not remain tacit.

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### On Wishing for a Paradigm Shift

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One aspect of the recent discussion of the development of DSM V has been whether it should aim to express, or perhaps bring about, a paradigm shift. Allan Frances has described the initial optimism about the possibility of such a change as ‘absurdly premature’. He writes:

The *DSM-V* goal to effect a “paradigm shift” in psychiatric diagnosis is absurdly premature. Simply stated, descriptive psychiatric diagnosis does not now need and cannot support a paradigm shift. There can be no dramatic improvements in psychiatric diagnosis until we make a fundamental leap in our under-

standing of what causes mental disorders. The incredible recent advances in neuroscience, molecular biology, and brain imaging that have taught us so much about normal brain functioning are still not relevant to the clinical practicalities of everyday psychiatric diagnosis. The clearest evidence supporting this disappointing fact is that not even 1 biological test is ready for inclusion in the criteria sets for *DSM-V*. [Frances 2009: 2]

In the first half of this short note, I will draw out some of the consequences of this on a broadly Kuhnian picture of science. Central to this picture is the connection between paradigms and the meaning of theoretical terms and hence the connection between changing paradigms and the consequent incommensurability of the meanings of terms across time. It is this that helps to support Kuhn’s theoretical scepticism about whether sciences can be said to progress. And this in turn calls into question whether it can be rational to wish for a paradigm shift.

In the second half, I address a related but more substantial point. Whilst the wish for a paradigm shift typically reflects optimism about the developments of neuroscience (notwithstanding the connections just summarised), psychiatry aims to use its technical innovations to relieve human distress. An improved psychiatry should thus be better able to address issues of distress. But if so its understanding of distress - which guides diagnosis, treatment, management and shared plans for recovery - had better remain closely wedded to the self-understanding of those it is supposed to help. And if so, any plans for a paradigm shift threatens to disconnect technical psychiatry from the understanding of human agents which should guide it.

### Paradigms incommensurability and progress

The widespread use of the word ‘paradigm’ in the description of scientific change is the fault of Thomas Kuhn’s *Structure of Scientific Revolutions*. Although he is said to have used it in at least 21 different ways, one basic idea is central [Masterson 1970].

Kuhn argues that scientific activity

falls into two kinds. In the main, scientists are engaged in 'normal science'. This comprises the articulation and application of stable dominant theories and meta-theoretical assumptions to new areas. Kuhn calls this background the dominant *paradigm*. During such periods, no serious attempt is made to refute or even defend the theoretical background which is instead simply presupposed. But these stable periods of normal science are punctuated by brief periods of revolutionary theory change. Sparked both by the accumulation of anomalous results and by the development of rival theories or even rival meta-theoretical assumptions, the dominant orthodoxy is cast aside and a new theory or set of theories put in its place. Only during these revolutionary periods is the truth of what will become the new scientific background called into question.

Thus whilst during periods of normal science, some measure of progress can be based on an increasing ability to solve recognised puzzles against the background of a stable paradigm, that measure does not apply over periods of revolutionary change since a change of paradigm changes what is regarded as a potentially soluble puzzle.

In fact, however, a broadly Kuhnian view makes the idea of progress across a paradigm shift even more difficult. This follows from his, at the time, influential view of the meaning of theoretical terms. Like other philosophers and historians of science, Kuhn reacted against an influential view of the meaning of theoretical terms taken from the Logical Empiricists of the 1930s [Feigl 1970]. On that older view, theories could be judged against the standard of theoretically neutral observations and that separation was supposedly maintained by the independence of observation from theoretical language. Although theoretical terms were grounded in the observational predictions they collectively inferentially warranted, observational terms were thought to be definable antecedently.

A group of arguments towards the end of the twentieth century undermined that distinction between theory and observation (establishing instead the 'theory dependence of observation'). Kuhn concluded that the holism

that had been thought to apply to theoretical terms - albeit a holism constrained from the outside by their implications for observations claims - must apply to theoretical and observational terms collectively. But without a stable set of neutral observation claims against which to judge them, the new holism seemed to imply that a change of overall theory would change the context and hence the meaning of all theory-observation terms. As a result this seemed to suggest that there was no standard by which to compare overall theories across a paradigm change since different paradigms defined different scientific languages leaving no resources for a translation manual. And thus paradigm change was *incommensurable* and there could, in principle, be no content to the idea that science progresses.

This is not the only way to view the meaning of theoretical terms and thus not the only view of possibility of comparing theories. But part of the force of the idea of a paradigm shift is that the change of world view is radical. Indeed Kuhn himself notoriously suggested that after such a shift, scientists inhabited a different world. Thus any less radical account of the consequences of theory change would undermine the point of the suggestive word 'paradigm'.

This, however, suggests that, at the very least, there is something strange about wishing to usher in a new paradigm. Without a standard by which to judge progress across such a change, what rational motive is there to wish for such a change? That, however, is not my purpose in recalling the close connection between paradigms, meaning and incommensurability. The real issue concerns the application of these ideas to psychiatry in particular

### **Neuroscience, human distress and the prospects of paradigm change**

In order to develop my real concern I will return to (and re-quote) the passage from Frances I quoted at the start. The most obvious reason for thinking that psychiatry is awaiting a paradigm shift are developments at the hard science end of psychiatry.

Even Frances mentions 'incredible recent advances in neuroscience, molecular biology, and brain imaging' when discussing others' confidence in the possibility of a new paradigm. Frances himself argues that 'descriptive psychiatric diagnosis does not now need and cannot support a paradigm shift' but he goes on to say that there 'can be no dramatic improvements in psychiatric diagnosis until we make a fundamental leap in our understanding of what causes mental disorders' and that the absence of biological tests in diagnostic criteria suggests that this has not been reached. But that comment does not distance himself from what might seem a plausible aspiration for a biomedical psychiatry. What is needed, on this assumption, is greater biological understanding of 'what causes mental disorders' and a sufficiently 'fundamental leap' that might give us the hoped for paradigm shift.

But I think that there are two fundamental complexities that this view - a view Frances does not sufficiently distance himself from - ignores. The first is that, within psychiatry, the focus of neuroscientific, biological and brain imaging technology is mental pathology. Progress has been recently made in these areas and more progress is needed but, additionally, progress is also needed in determining not just what *causes* mental disorders but what they *comprise*. What is it, in other words, for something to be a mental disorder? There is no reason to think that an answer to this question can be provided by neuroscience, molecular biology, and brain imaging since, insofar as these can help shed light on psychopathology, one needs first to have decided the extension of that concept then to study its neurological and biological underpinnings. Given the conceptual complexity of the very idea of mental disorder, and that what is so classed is so contested, any leap forward in knowledge of brain mechanisms needs to go hand in hand with answers to that question.

The second complexity stems from the first. Suppose that innovations in neuroscience, molecular biology, and brain imaging were used to articulate a form of psychopathology on the basis of its neurological similarity to currently identified forms but which had

no connection to any mental distress or suffering. That would not, I suggest, mark a triumph of neuroscientific psychiatry. Rather, it would amount to psychiatry losing its way by losing its connection to its particular subject matter.

This point suggests, however, a Winchian point [Winch 1958]. Psychiatry, unlike a more disinterested study of the brain, has an essential connection to human distress and suffering. But if so, it has an essential connection to the concepts with which we, as agents, make sense of ourselves. Whilst there seems no constraint imposed by the subject matter of much of natural science on the limits of conceptual innovation (as long as the concepts arrived at can still be understood by at least some scientists), the concepts of psychiatry need to retain some connection to those concepts in terms of which we ordinarily make sense of ourselves. Only so, can human experiences play at least some guiding role for psychiatric diagnosis, theorising and care.

If this is so, then the wish for a paradigm shift in psychiatry seems doubly misplaced. First, the connection to incommensurability undermines the rationality of the wish. Second, a change which did not sever the connection to the concepts we use to make sense of ourselves would not be a paradigm shift since the innovation would be merely partial leaving in place standards for rational assessment of the technical innovations. But a properly radical paradigm shift which rendered the pre- and the post- shift worldviews incommensurable would have to sever the connection to those grounding concepts and that could only be because psychiatry had lost its way.

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### Doing No Harm: The Case Against Conservatism

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We agree fully with the proposition that the diagnostic system used in psychiatry – and proposed changes to it – must “first do no harm.” And although we believe that the publication in 1980 of the DSM-III marked a monumental advance for our discipline, we are convinced that continued progress in psychiatry necessitates that our system of diagnostic classification undergoes radical revision. Allen Francis is entirely correct that the current state of sciences relevant to psychiatry makes formulation of a nosology based on etiopathogenesis grossly premature. He is also on target with his admonition regarding the potential of change to carry with it unintended negative consequences. What he does not address, however, are the negative consequences of leaving largely unchanged a taxonomy we know to be inadequate at best and simply wrong at worst. The progress-retarding effects of the current system on all areas of psychiatric endeavor – clinical, educational, and scientific – must be recognized and remedied.

Our appraisal of the DSM system begins with its overall structure – namely, the “axes.” Despite its authors’ explicit wishes to the contrary, the spurious separation of “mental disorders” from “general medical conditions” is reified by the multiaxial system, contributing to (among many other important fallacies) the

false conviction that there is actual substantive content to disputes over whether illnesses such as fibromyalgia or irritable bowel syndrome are “really” mental disorders or “physical” disorders. Similarly, the distinction between “clinical disorders” (Axis I) and “personality disorders” (Axis II), which masquerades as one of the deep and fundamental branch points of psychiatric diagnosis, serves as a nidus for some of the least productive and least meaningful exchanges in all of clinical psychiatry and psychiatric education.

Moving from the multiaxial structure of the taxonomy to the contents of its diagnostic categories, the first assumption we must challenge is that DSM diagnoses are “descriptive.” Even if it were true that all of its categories were defined in such terms, it is questionable whether or not the current system has identified what we need it to describe. The conviction of most clinicians, which seems borne out by recent data (see below), appears to be that it has not. Moreover, the assertion that DSM diagnostic categories are *in principle* defined descriptively is belied by the definitions of somatoform disorders, adjustment disorders, and disorders “due to a general medical condition.” Somatoform disorders are said not to be “fully explained by a general medical condition.” Since few (if any) entities in medicine are “fully explained” in any sense, that construct is problematic to say the least. In addition, the definitions of some of the somatoform disorders demand that they be associated with “psychological factors,” a diagnostic requirement that is as flexible as it is vacuous. The etiological assumption implicit in the category of adjustment disorders is obvious, but unfortunately leads many consumers of the diagnostic system to the spurious conclusion that, in contrast to adjustment disorders, more significant psychopathology must come “out of the blue,” a common misconception that flies in the face of what we know about the etiological contributions to many forms of psychopathology of adversity in the social environment. And the disorders “due to a general medical condition” – in addition to being another example of an etiological as opposed to descriptive organizing principle in the DSM – simply add an unne-

essary layer of psychiatric/medical dualism onto the already-familiar primary versus secondary distinction used in all other medical contexts. Related to this reification of dualism is the effect that this diagnostic category has in fostering the fallacious view that the etiopathogenetic mechanisms by which psychiatric syndromes occur come in two distinct flavors: “medical” (e.g., thyroid dysfunction, stroke) and “psychiatric” (e.g., inherited vulnerability, social environmental adversity).

While the above-described problems with the multiaxial framework of, and the etiologically defined categories in, the DSM are considerable, our most fundamental objection – and main justification for advocating a radical overhaul – relates to what is turning out to be the error of the neo-Kraepelinian assumption of the DSM-III/IV/IV-TR enterprise: that psychiatric diseases are discrete entities, as defined in the DSM, with which humans either are or are not afflicted. Accumulating evidence renders such a conceptualization antiquated, and the necessity of freeing psychiatric research, education, and practice from its adverse effects urgent. The powerful statistical tools of latent variable modeling have suggested that the phenotypic structure of clinical psychopathology as it exists in actual people differs considerably from the definitions established by DSM committees. Such findings call into question the validity of DSM subtypes of attention-deficit/hyperactivity disorder (ADHD) (1,2), depression (3), and anorexia nervosa (4); distinctions among ADHD, oppositional defiant disorder, and conduct disorder (5); the distinction between juvenile bipolar disorder and post-traumatic stress disorder (6); the distinction between depressive and anxiety disorders (7); and distinctions among psychotic disorders (8). Further evidence that the initial stab of DSM committees at formulating valid diagnostic boundaries missed the mark is provided by the near ubiquity of “comorbidity” among psychiatric patients. Thus, it appears that in many important instances DSM diagnostic categories neither encompass within them nor distinguish between them those clusters of cognitive, emotional, and behavioral abnormalities that com-

prise the phenotypes of actual patients. That recognition doubtless explains why attempts to link specific genes, environments, and their interactions to the etiopathogeneses of particular psychiatric disorders have largely been disappointing, and why psychiatric geneticists must frequently rely on other taxonomies to identify phenotypes to study. It likely also underlies the fact that effective treatments for psychopathology have very little diagnostic specificity, nor do the criteria for most DSM diagnostic categories demarcate syndromes that even approach homogeneity with respect to treatment responses.

Dr. Frances has again provided psychiatry a great service, this time by stimulating widespread thought and debate on the question of whether in the next edition of the DSM the diagnostic system of the past three decades should remain largely intact. Our answer is no. The validity problems associated with both the structure of the taxonomy and many of its specific elements are burdensome to the discipline as it is currently practiced and taught, and impede its ability to make needed progress. Criteria to be used in determining its replacement should emphasize consistency with currently available evidence regarding clinical presentations of psychopathology. Also of tremendous importance are usefulness in the clinical arenas of prognosis and treatment response, in the investigation of the genetic and environmental contributors to etiopathogenesis, and in the education and training of students and residents, where cessation of teaching and learning DSM diagnostic rules could make room in curricula for actual science. Determining the form of our next nosology is a formidable but exciting project that we must undertake for the wellbeing of our discipline and of our patients.

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## Allen Frances Responds

### *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 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 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 percent 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 nonspecific 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-V 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 in-

terests.

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 lumper 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-V?

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 medi-

cines 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 suggested DSM-V 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 potentially 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 de-

scription (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.

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(Continued from page 1, Sadler)

Spitzer, and the practical empiricism of the social sciences, are marginalized and unfunded?

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(continued from page 4, Ghaemi)

content than it should have been. No doubt there are plenty of aspects of DSM-V that are worthy of criticism. Our view would be that those aspects that are worthy of criticism are exactly those things that Frances defends: there is too little change in DSM-V, not too much.

Over time, revolutionaries tend to become conservatives, and reaction engenders counter-reaction. There is a psychic law of inertia, as the writer Henry Adams observed: What exists is valued simply because it exists, and much more effort is needed to push the boulder of dogma into motion than to leave it alone (12). Perhaps the physicist Max Planck is sadly all too right that new scientific truths are routinely resisted by prior generations, who are rarely convinced, and rather are only accepted by a changing of generations (13). Previous generations changed our nosologies, generally for the better, and we do them justice in continuing to push the boulder of science along, rather than allowing it to rest in some fallow place.

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