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Process and Content of DSM-5

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Abstract

The third edition of the American Psychiatric Association's (APA) Diagnostic and Statistical Manual of Mental Disorders was a highly successful revision in large part because it was tremendously innovative. The authors of the fifth edition intended their version to provide another paradigm shift. However, the process of its construction was riddled with problems and controversies. Discussed herein is the presence of the internet, confidentiality contracts, no gold standard, and the inadequate documentation of empirical support. Also discussed was the failed attempt to provide a paradigm shift. Recommendations for the construction of future editions of the diagnostic manual are provided.

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Introduction

The third edition of the American Psychiatric Association's (APA) Diagnostic and Statistical Manual of Mental Disorders (DSM-III; APA, 1980) was a highly successful revision to the APA nomenclature in large part because it was tremendously innovative, described by some as a paradigm shift in the diagnosis and classification of psychopathology (Klerman, 1986). Some of its primary innovations, such as the deletion of theoretically-specific terms and the inclusion of specific, explicit criterion sets, met with considerable objection and opposition, but in the end they were nevertheless approved (Decker, 2013; Spitzer, Williams, & Skodol, 1980). However, for DSM-III-R (APA, 1987), the generation of new diagnoses in the absence of sufficient empirical support met with stronger opposition, culminating in the rejection by the APA Board of Trustees of the proposals for paraphilic rapism,

sadistic personality disorder, self-defeating personality disorder, and late lutual phase dysphoric disorder (Widiger, 2012). The authors of DSM-IV (APA, 1994) took a more conservative approach (Fischer, 2012). "The major innovation of DSM-IV will not be in its having surprising new content but rather will reside in the systematic and explicit method by which DSM-IV will be constructed and documented" (Frances, Widiger, & Pincus, 1989, p. 375). The Chair and Vice Chair of DSM-5 though intended their revision to provide another "paradigm shift" (Kupfer, First, & Regier, 2002, p. xix).

Problems for DSM-5

The construction of DSM-5 was probably the most problematic and controversial, relative to the prior editions of the APA diagnostic manuals (Decker, 2013; Frances, 2013; Greenburg, 2013). Some of the problems facing the authors of DSM-5 were largely unique to them, others were more fundamental to the process for any diagnostic revision. Discussed herein is the presence of the internet, confidentiality contracts, no gold standard, and the inadequate documentation of empirical support.

Influence of Social Media

A contributing factor to the magnitude of the controversies swirling around DSM-5 was the existence of the internet; the means whereby hostile critics could quickly generate a substantial body of vocal opposition and negative publicity. There was never a “Twitter revolution” but the web was very active in the creation of agitating blogs. One of the more prominent was provided by Dr. Frances, Chair of DSM-IV, for both *Psychology Today* and *The Huffington Post*, hounding DSM-5 proposals and announcements at most every turn. The authors of DSM-III, DSM-III-R, and DSM-IV were perhaps fortunate that the internet was not yet available for such immediate, vocal protest and opposition.

Confidentiality Contracts

The development of DSM-5 may have gotten off on the wrong foot with the requirement that work group and task force members sign confidentiality agreements. No such requirement had previously been imposed on work group members, nor is it really clear why there should be any constraint on work group members sharing their experiences, views, observations, and opinions with persons outside of the official process. The stated purpose of the confidentiality contracts was in part “to prevent the premature dissemination of internal deliberations about final decisions;” however, they were “not intended to prohibit timely discussion or public dissemination” (APA, 2012a). How one distinguishes between a “premature” dissemination from a “timely” dissemination is not at all clear.

It is understandable for a work group not to want to have preliminary ideas disseminated prematurely. Work group members will float ideas that might be outrageously flawed and are likely to be rejected fairly quickly. Their dissemination in the early stages of a work group effort might be inaccurately interpreted as serious proposals that have a strong possibility of receiving final approval, thereby generating unnecessary drama and controversy. On the other hand, any effort to suppress the dissemination of work group deliberations is at the cost of conveying an impression of secrecy, concealment, and censorship. It certainly does not convey a spirit of free and open expression of ideas and opinions. The authors of DSM-5 stated that “the process for developing *DSM-V* has been the most open and inclusive ever” (Schatzberg, Scully, Kupfer, & Regier, 2009), but never in the development of any prior edition of the diagnostic manual were work group and task force members required to sign confidentiality contracts to prevent the dissemination of work group deliberations.

Significant concerns with respect to the process with which DSM-5 was being constructed were perhaps first raised by Dr. Robert Spitzer, Chair of DSM-III and DSM-III-R, after having difficulty accessing minutes of DSM-5 Work Group meetings (Greenburg, 2013). Drs. Frances and Spitzer (Chairs of DSM-IV and DSM-III, respectively) eventually submitted a joint letter to the APA Board of Trustees on July 7th, 2009, expressing a variety of significant concerns with respect to the process with which DSM-5 was being constructed, including the confidentiality agreements (Greenburg, 2013). Whether or not Dr. Spitzer was being denied access to work group deliberations on the basis of the confidentiality contracts is not clear (Schatzberg et al., 2009), but their presence did not convey an impression of free and open disclosure (Frances & Widiger, 2012; Greenburg, 2013) .

No Gold Standard

There is no laboratory measure to document objectively the existence of a mental disorder (Kapur, Phillips, & Insel, 2012). The decision to consider a condition or behavior pattern a mental disorder is a matter of opinion (Frances & Widiger, 2012), hopefully supported by compelling research (Kendler, 1990; Meehl, 1986). One infers the presence of a mental disorder on the basis of documented impairment, dysfunction, distress, pathology, and/or dyscontrol (Spitzer & Williams, 1982; Widiger & Sankis, 2000), but there is a wide array of behavior patterns that are associated with these indicators.

There have been published and influential guidelines for what would be necessary for the validation of a mental disorder diagnosis, such as discriminant validity (delimitation from other disorders), family history (heritability), antecedents, risk factors, course, biological markers, follow-up (outcome), and treatment implications (e.g., Kendler, 1990; Robins & Guze, 1970; Smith, McCarthy, & Zapolski, 2009). However, there is no objective or consensus algorithm for how to interpret these findings or for concluding when there is sufficient empirical support. As in the case of any construct validation (Strauss & Smith, 2009), the findings must be interpreted and there will be differences of opinion as to the correct interpretation.

For example, there is research to support the validity of a hypersexual disorder (Kafka, 2010), a depressive personality disorder (Bagby, Watson, & Ryder, 2012), and to consider bereavement to be a mental disorder (Kendler, 2010; Zisook, Shear, & Kendler, 2007). At what point though this empirical support is sufficiently compelling is never certain and may always be debatable (Widiger & Mullins-Sweatt, in press). Most of the disorders within the diagnostic manual would appear to have adequate empirical support for their inclusion, but this is, again, a matter of opinion. Reasonable persons will disagree, and unreasonable persons have considerable wiggle room to disagree vehemently. It is in this context that proposals for a new diagnosis are generated, and it is then not terribly surprising that there will be disagreement, at times contentious, because what constitutes a mental disorder is not a trivial decision (e.g., see Bayer & Spitzer, 1982, for a discussion of the controversy surrounding the inclusion of homosexuality within prior editions of the diagnostic manual).

Proposed for DSM-5 was a revision to the criterion set for autism disorder that would appear to increase the threshold for diagnosis, leaving many persons diagnosed with DSM-IV Asperger's disorder to no longer qualify for special benefits, services, and support (Volkmar & McPartland, 2014). The authors of DSM-5 therefore made an essentially social-political decision to allow persons who had been diagnosed with autism using DSM-IV to continue to receive the diagnosis (APA, 2013, p. 51), even though they had concluded that the DSM-IV threshold was wrong. Similarly, the authors of DSM-5 proposed to eliminate the bereavement exclusion criterion for major depressive disorder because they did not consider it to be a valid exclusion (Kendler, 2010); however, in the face of substantial public opposition due to the implication that grieving would now be considered a mental illness (Wakefield, 2013), they decided to leave the decision largely up to the clinician, who can choose to consider it to be a mental disorder or choose not to (APA, 2013, pp. 125-126). In sum, in the absence of a gold standard, a lack of clear certainty or even confidence regarding the decision, the authors of DSM-5 have simply allowed clinicians to decide for themselves for two proposals that were particularly controversial and/or social-politically costly.

Inadequate Documentation of Empirical Support

In the absence of an irrefutable gold standard for the presence of a mental disorder, it becomes exceedingly important to provide documentation of the empirical support that does exist for a proposal. Work group members should not be left alone to simply create new disorders with minimal empirical support, particularly when many of the proposals can be accompanied by considerable social risks and costs (Frances & Widiger, 2012). Mental disorders are constructed by the authors of the diagnostic manual, and their opinions will not necessarily be valid, well reasoned, or inconsequential. This is not to say that their opinions will be necessarily wrong, but they do need to be subjected to critical review and empirical scrutiny.

The Chair and Vice-Chair of DSM-5 stated that the development of DSM-5 followed the procedure used for DSM-IV, including literature reviews, data reanalyses, and field trials (Regier, Narrow, Kuhl, & Kupfer, 2010). "The *DSM-V* development process is scientific, and it contains the same elements as the *DSM-IV* process described by Dr. Frances, including literature reviews, secondary data analyses, and field trials" (Schatzberg et al., 2009). Kendler,

Kupfer, Narrow, Phillips, and Fawcett (2009) developed guidelines for DSM-5 work group members that were indeed quite stringent. These guidelines indicated that any change to the diagnostic manual should be accompanied by “a discussion of possible unintended negative effects of this proposed change, if it is made, and a consideration of arguments against making this change should also be included” (p. 2). Kendler et al. further stated that “the larger and more significant the change, the stronger should be the required level of support” (p. 2). In sum, Kendler et al. provided a demanding and very commendable set of guidelines, but it doesn’t appear that work group members were actually required to adhere to them.

Adherence to the guidelines would seem to be required by the presence of a scientific oversight committee, whose purpose was to review the adequacy of empirical support for every proposal (Kendler, 2013). However, potentially complicating the objectivity of this committee is that one of its eight members was also a member of the DSM-5 Task Force, another was a member of the DSM-5 Child and Adolescent Disorders Work Group, and the Chair of this Scientific Review Committee (SRC) had been a member of the Mood Disorders Work Group as well as a central advocate for one of the more controversial proposals: the removal of the bereavement exclusion criterion from the diagnosis of a mood disorder (Kendler, 2010). The committee attempted to address these obvious conflicts of interest by having SRC members remove themselves from deliberations and votes concerning proposals from their own work group (Kendler, 2013). This is not, though, the most effective way of addressing potential biases, and it can even create its own significant problems.

First, it can be quite difficult for committee members to express opposition to proposals of colleagues on the same committee, particularly proposals by the Chair of the committee. Many journal editors attempt to address this problem by having outside persons serve as special action editors for manuscripts submitted by the editor or by an associate editor. Associate editors who review each other’s manuscripts can be drawn into an implicit self-serving collusion of mutual support. In addition, a recusal process can remove from the discussion the persons who have the knowledge and expertise that will be needed to evaluate a particular proposal. Persons with an expertise in schizophrenia (for instance) are unlikely to have the necessary expertise to review manuscripts concerning (for instance) personality disorders. Very few editors will assign manuscripts to associate editors who are unfamiliar with the existing literature. Such persons will not be able to identify the gaps and inadequacy in coverage; they will not appreciate the issues and concerns within the respective field of study; and they will be unable to identify the presence of a biased representation of the literature.

Persons external to the process of DSM-5 can judge for themselves, or at least for a period of time could have judged, the rationale and empirical support for all of the proposals, as these were posted on the DSM-5 website. The initial literature reviews that provided the empirical support for DSM-5 proposals were first posted in February, 2010. Revisions to these literature reviews continued to be posted through 2012. Many of the DSM-5 literature reviews did appear to meet the spirit of the Kendler et al. (2009) guidelines, such as the reviews for hypersexual disorder (Kafka, 2010), hoarding (Frost, Steketee, & Tolin, 2012), dissociative disorders (Spiegel et al., 2013), and the proposal to subtype conduct disorder for callous-unemotional traits (Frick, Ray, Thornton, & Than, 2014). However, others did not. For example, one of the changes to the diagnostic manual was the creation of a new class of behavioral addiction disorders that would subsume substance use disorders and pathological gambling, and allow for, if not encourage, additional diagnoses, such as internet and shopping addiction. The posted literature review that provided the rationale and empirical support for this major revision consisted of just two sentences: “Pathological (disordered) gambling has commonalities in clinical expression, etiology, comorbidity, physiology and treatment with Substance Use Disorders. These commonalities are addressed in the following selected papers from a relatively large literature” (APA, 2010c). There was no discussion of the potential costs or risks of this proposal. It is certainly conceivable that a thorough and compelling literature review could be generated that would support this proposal, but none was ever posted on the DSM-5 website. It is difficult to imagine a two-sentence review paper being published in a credible scientific journal, but apparently that was all that was needed for this revision to the diagnostic manual to be approved.

The DSM-5 Personality Disorders Work Group proposed to delete half of the 10 diagnoses, including the narcissistic and dependent (Skodol, 2010). Kendler et al. (2009) indicated that any such decision should be supported by a literature review addressing the construct validity and clinical utility of the disorders to be deleted. However, no such review for these disorders was ever posted (Mullins-Sweatt, Bernstein, & Widiger, 2012). The

Personality Disorders Work Group also proposed to replace the specific and explicit criterion sets with paragraph descriptions consisting of 10-17 often complex sentences with no rules or guidance as to how many of the features need to be present, nor even a requirement that each sentence be systematically considered (Skodol, 2010). This would have essentially returned personality disorder diagnosis back to the diagnostic methods of DSM-II (APA, 1968), despite the well established improvements obtained with the specific and explicit criterion sets (Kendler, Munoz, & Murphy, 2010), and would have even done so with substantially more complex and vague narrative descriptions than had been present in DSM-II (Widiger, 2011). Critical reviews of this proposal external to the DSM-5 process indicated that its empirical support was sorely limited and fundamentally flawed (Widiger, 2011; Zimmerman, 2011). Fortunately, the proposal was eventually withdrawn, due in large part perhaps to the external critical review.

The impetus for the inclusion of mood dysregulation disorder (initially titled temper dysregulation disorder of childhood), was the excessive number of children being diagnosed with bipolar mood disorder in the absence of adequate scientific support for the validity of this diagnosis in children (Fawcett, 2010). The DSM-5 Child and Adolescent Disorders Work Group (2010) felt that this over-diagnosis might be addressed through the development of restrictive diagnostic criteria. This proposal, of course, presumed that clinicians would actually adhere to the restrictive diagnostic criteria despite the research that indicates clinicians are unlikely to do so (Garb, 2005). In fact, the existence of the diagnosis within the APA manual could have the effect of increasing the inappropriate medication of children with temper tantrums by providing the diagnosis a considerable degree of credibility and perceived validity that it doesn't really deserve (Frances & Widiger, 2012; Hyman, 2010).

In addition, as acknowledged by the DSM-5 Child and Adolescent Disorders Work Group (2010), empirical support for the proposal was weak: "the work has been done predominately by one research group in a select research setting, and many questions remain unanswered" (p. 4). The work group though felt that its inclusion in DSM-5 would stimulate the research needed for its eventual validation. In other words, not only was there an acknowledgment of inadequate empirical support, the proposal was approved in order to generate the research that would (hopefully) support its validity. This would appear to be quite inconsistent with the guidelines of Kendler et al. (2009), yet the proposal was approved.

One of the significant changes made for DSM-5 was to collapse the diagnoses of hypochondriasis, pain disorder, and somatization disorder into one common disorder, titled complex somatic symptom disorder (APA, 2013), despite quite a bit of prior research documenting important differences between them (Bouman & Eifert, 2009). The literature review in support of this proposal indicated that "the proposal is to group together these heretofore separately recognized disorders because in fact, there are 3 diverse sources suggesting considerable overlap among them" (APA, 2010b, p. 1). One of the three sources was an unpublished survey of physicians, 52% of whom felt there was "a lot of overlap." This is barely a majority, nor is it at all clear what a physician's subjective impression of a lot of overlap really means. Does the impression of a lot of overlap mean considerable overlap, or enough overlap to indicate that the distinctions between them should be removed? The second source was observed diagnostic co-occurrence in clinical settings, but it was acknowledged here that there was in fact only "limited data regarding overlap." Three studies were cited. One study concerned the DSM-III-R (APA, 1987) criteria for hypochondriases (i.e., Barsky, Wyshak & Klerman, 1992). A second study addressed the Barsky et al. findings using a more distinctive criterion set, the authors concluding that they had in fact "identified a distinct hypochondriasis symptom cluster" (Fink et al., 2004, p. 161). The third study reported that 20% of persons with somatization disorder also had hypochondriasis (Escobar et al., 1998), which is hardly a problematic overlap. Finally, the third source for the "considerable overlap" was similarities in the treatment for hypochondriasis, pain disorder, and somatization disorder, which simply meant they all involved the use of cognitive-behavioral therapy and/or antidepressants, a commonality that is evident across many other mental disorders. In sum, the authors of this proposal acknowledged that it was a "major change" (APA, 2010b) to collapse the three diagnoses into one condition, but the research they cited in support of this major change was at best limited in actually demonstrating the existence of a considerable or even a problematic overlap of hypochondriasis, pain disorder, and somatization disorder; yet, this proposal was still approved.

One of the more unusual revisions for DSM-5 concerns the diagnosis of cyclothymia. The diagnosis of cyclothymia in DSM-III (APA, 1980) and DSM-IIII-R (APA, 1987) required that the symptoms of hypomania meet the diagnostic

threshold for a hypomanic episode. This was loosened somewhat in DSM-IV (1984) wherein it was only required that there be “numerous periods with hypomanic symptoms” (APA, 1994, p. 365), albeit many to most cases would likely be above threshold for a hypomanic episode. However, in DSM-5 (APA, 2013) it is required that the “hypomanic symptoms ... do *not* meet criteria for a hypomanic episode” (p. 139, our emphasis). This is a very clear and significant change, and it is one that is difficult to understand. What would be the rationale for requiring that the hypomanic symptoms be below threshold for a hypomanic episode? In DSM-5 persons who have hypomanic episodes (along with dysthymic) can no longer be given the diagnosis of cyclothymia; the diagnosis is reserved for persons who fail to have hypomanic episodes. It is difficult to understand the rationale for this revision, but it is even more difficult to find the rationale.

It was initially stated on the DSM-5 website that there were no proposed revisions for the diagnosis of cyclothymia (APA, 2010a). In the final posting on the website, the rationale for the revision simply stated, “the DSM-5 diagnostic criteria for this disorder are similar to those in DSM-IV, with very minor wording changes being proposed” (APA, 2012b). Requiring that the symptoms of hypomania be below threshold for an actual episode of hypomania does not appear to be simply a minor change in wording. It is a very substantial change to the diagnosis, comparable to changing the criteria for bipolar mood disorder to require that the manic symptoms be below rather than above the threshold for a manic episode. That would certainly not be a minor change in wording. In the section of DSM-5 for “highlights of changes from DSM-IV to DSM-5” (APA, 2013, pp. 809-816), there is again no mention of any change for cyclothymia. In an extensive summary of the rationale for changes being made to the bipolar mood disorders (including cyclothymia) co-authored by the Chair of DSM-5 as well as work group members in charge of cyclothymia, there is again no reference to the proposal to shift the cyclothymic diagnosis to below threshold for a hypomanic episode (Kupfer et al., 2011). In sum, there is certainly no literature review provided to justify the significant change to the criterion set for cyclothymia, nor even an explanation for its rationale, yet this proposal was approved. There are three possible explanations: the authors of the revision to cyclothymia were not aware of the DSM-IV criteria for cyclothymia; they didn’t think that requiring the hypomanic symptoms below threshold for a hypomanic episode was a meaningful change; or they knew it was a significant change but didn’t feel compelled to provide a supportive literature review, or even acknowledge its occurrence. None of these explanations seems particularly satisfying.

The field trials for DSM-5 were also quite different than was the case for DSM-IV. The purpose of the field trials for DSM-IV was to test empirically the validity of proposed revisions, compare alternative proposals, and indicate the potential impact of the proposed revisions (Widiger et al., 1991). This was not the intention of the DSM-5 field trials. The DSM-5 field trials were confined largely to questions of feasibility, reliability, and face validity (Clarke et al., 2013). “The main interest was to determine the degree to which two clinicians would agree on the same diagnosis” (Clarke et al., 2013, p. 44). The questions addressed by the DSM-5 field trial can be said to be interesting; more specifically, whether the proposed diagnoses would be diagnosed reliably by practitioners in the absence of the structured interviews used in research settings (Kraemer, 2014). However, existing research has already well established that psychiatric diagnosis tends to be unreliable in the absence of systematic clinical assessments of diagnostic criterion sets (Garb, 2005 Spitzer, Endicott, & Robins, 1975; Spitzer & Fleiss, 1974; Zimmerman, 2003). It is not really clear why the authors of DSM-5 believed that they would obtain new information with respect to this question or how that information would be relevant to a decision whether or not to implement a particular proposal. Of more importance for a field trial concerned with new diagnostic proposals is to address questions of validity (Kendler, 1990); compare alternative proposals to one another, including a comparison to the existing nomenclature (Widiger et al., 1991); and address the potential costs and concerns specific to each proposal (Frances & Widiger, 2012; Kendler et al., 2009). The DSM-5 field trials largely ignored what probably should have been their primary concerns.

The DSM-5 field trial reported that some of the proposed diagnoses did not obtain acceptable levels of reliability (e.g., mixed anxiety depressive disorder), but this was not a particularly meaningful basis for their exclusion from DSM-5. If it was, then the well established diagnoses of major depressive disorder, antisocial personality disorder, and generalized anxiety disorder should probably also have been removed from the diagnostic manual as they also failed to obtain adequate levels of reliability in the DSM-5 field trial (Regier et al., 2013). What was learned from the field trial was a replication of the well-established finding that it is difficult to obtain reliable diagnoses when the

latter are not obtained through systematic assessments of diagnostic criteria (Garb, 2005 Spitzer et al., 1975; Spitzer & Fleiss, 1974; Zimmerman, 2003). Not much useful information was actually obtained about any one of the specific proposals. In fact, the field trial was unable to obtain any data for some of the proposals (e.g., attenuated psychotic symptoms, bipolar II, hoarding, mild neurocognitive disorder, narcissistic personality disorder, and schizotypal personality disorder) due to the sites not having available a sufficient number of cases for data analysis (Clarke et al., 2013; Regier et al., 2013). It is unclear why sites were chosen if it was apparent that they would not have enough cases over the course of the DSM-5 field trial data collection (Jones, 2012).

Once the final decisions for DSM-5 were made, all of the literature reviews were removed from the DSM-5 website. Modified versions of many of the reviews have been published in scientific journals (e.g., Fawcett, 2010; Phillips et al., 2010; Skodol, 2012; Spiegel et al., 2013). However, it would appear that many of them are no longer readily available. It is unclear if the DSM-5 literature reviews that have not been submitted for publication will be made available. Persons still interested in retrieving them, though, can obtain them through websites that provide a means for retrieving historical website material that is no longer being posted (e.g., search “Wayback Machine” on the Internet).

Paradigm Shift?

The paradigm shift intended by the authors of DSM-5 was to convert the manual toward a dimensional model of classification (Kupfer et al., 2002). “We have decided that one, if not the major difference, between DSM-IV and DSM-V will be the more prominent use of dimensional measures” (Regier et al., 2010, p. 649). The authors of DSM-5 were able to implement a number of revisions that did shift the diagnostic manual closer to a dimensional model of classification. Autism and schizophrenia are now conceptualized in DSM-5 as spectrum disorders, with different variants of the disorder existing along a common dimension of underlying pathology. The problematic distinction between substance abuse and dependence was abandoned in favor of a level of severity. Reference is made within the introduction of the manual to the broad dimensions of internalizing and externalizing dysfunction that cut across existing categories. Included in Section 3 of DSM-5 for emerging models and measures is a 5-domain 25-trait dimensional model of maladaptive personality functioning that is aligned conceptually and empirically with the dimensional five-factor model of general personality structure. The introduction to DSM-5 explicitly acknowledges the failure of the categorical model: “the once plausible goal of identifying homogeneous populations for treatment and research resulted in narrow diagnostic categories that did not capture clinical reality, symptom heterogeneity within disorders, and significant sharing of symptoms across multiple disorders” (APA, 2013, p. 12). It is further asserted that dimensional approaches will “supersede current categorical approaches in coming years” (p. 13).

Nevertheless, the shift was probably considerably less than originally intended. For example, the primary shift was most likely to occur for the personality disorders. At the first DSM-5 preparatory conference, the Nomenclature Work Group, charged with addressing fundamental assumptions of the diagnostic system, concluded that it will be "important that consideration be given to advantages and disadvantages of basing part or all of DSM-V on dimensions rather than categories" (Rounsaville et al., 2002, p. 12). None of the members of this work group were personality disorder researchers. Nevertheless, they recommended that initial efforts toward a dimensional model of classification be conducted in particular with the personality disorders. “If a dimensional system of personality performs well and is acceptable to clinicians, it might then be appropriate to explore dimensional approaches in other domains” (Rounsaville et al., 2002, p. 13).

This initial conference was followed by a series of international conferences, the first of which, “Dimensional Models of Personality Disorder: Etiology, Pathology, Phenomenology, & Treatment,” was devoted to shifting the personality disorders section to a dimensional trait model (Widiger, Simonson, Krueger, Livesley, & Verheul, 2005). Amongst the final conferences of this series was “Dimensional Approaches to Diagnostic Classification,” that was devoted to shifting the anxiety, mood, substance use, and other disorders (including personality) to a dimensional model (Helzer et al., 2008). However, in the end, none of the proposed revisions to the personality disorders section was approved, including the proposal to shift to a dimensional trait model of classification.

It is unclear why the dimensional trait model was not approved, given the initial endorsement (Regier, 2008), the preparatory meetings (i.e., Krueger et al., 2008; Rounsaville et al., 2002; Widiger et al., 2005), and the considerable body of empirical support (Clark, 2007; Widiger et al., 2012). The failure to obtain formal recognition within the diagnostic manual was probably due to a confluence of factors (Gunderson, 2013; Krueger, 2013; Skodol, Morey, Bender, & Oldham, 2013; Widiger, 2013).

First, shifting to a dimensional trait model did not have uniform support among the DSM-5 Personality Disorder Work Group members. In fact, the work group initially proposed two competing, alternative methods for personality disorder diagnosis: a dimensional trait model and a narrative psychodynamic model. These two proposals were constructed independently of one another, and represented opposing perspectives on personality disorder conceptualization (Shedler & Westen, 2004). It was initially proposed that clinicians would conduct both assessments, with priority given to the narrative model when they were in conflict. With respect to the narrative model, Skodol (2010), Chair of the Personality Disorders Work Group, proposed diagnosing DSM-5 diagnostic categories on the basis of paragraph narratives that had a strong psychodynamic representation and perspective. This proposal was based on the research with the prototype narratives developed by Westen, Shedler, and Bradley (2006), that were considered by them to be in stark contrast to the dimensional trait perspective (e.g., Shedler & Westen, 2004). Advocates of the prototype matching proposal were staunch opponents of the dimensional trait proposal (e.g., Shedler et al., 2010). For example, in the initial post on the DSM-5 website, Skodol (2010) cited studies that questioned the clinical utility of a dimensional trait model (i.e., Rottman, Ahn, Sanislow, & Kim, 2009; Spitzer, First, Shedler, Westen, & Skodol, 2008), and failed to acknowledge the existence of clinical utility studies that were supportive of the dimensional trait model and that addressed limitations of the studies that were cited (e.g., Lowe & Widiger, 2009; Samuel & Widiger, 2006; see also Glover, Crego, & widiger, 2012, and Mullins-Sweatt & Lengel, 2012).

There was considerable disagreement and strife among the personality disorder work group members (Silk, 2013). Two members of the work group eventually resigned in frustration over the work group proposals, process, and deliberations (i.e., Livesley, 2012; Verheul, 2012). After the paragraph narrative proposal was withdrawn, the work group members cobbled together a compromise proposal that combined the psychodynamic and trait models. This proposal was rejected by the scientific oversight committee, perhaps because there was very little research on the validity of this new hybrid combination of psychodynamic and trait models (Skodol et al., 2013).

It also did not help that the authors of the dimensional trait model decided not to derive their proposal on the basis of existing research. They decided instead to develop their own, unique dimensional trait model (Miller & Lynam, 2013; Paris, 2013) by having work group members nominate a novel list of 37 traits, which Clark and Krueger (2010) then factor analyzed to yield six factors, constituting a brand new dimensional trait model. The predominant dimensional model of personality is the five-factor model (FFM; John, Naumann, & Soto, 2008). As expressed by Clark (2007), "the five-factor model of personality is widely accepted as representing the higher-order structure of both normal and abnormal personality traits" (p. 246). However, rather than coordinate their proposal with this model, the authors of the initial dimensional trait proposal for DSM-5 distanced their six-factor model from the FFM, stating that two of its six dimensions (i.e., compulsivity and schizotypy) did not align with the conscientiousness and openness dimensions of the FFM, respectively (Clark & Krueger, 2010). The proposal was then understandably criticized for lacking sufficient empirical support. As expressed by Shedler et al. (2010), "the resulting model no longer rests on decades of research, which had been the chief rationale for including it" (p. 1027).

By the time work was completed on DSM-5 the position of the Personality Disorders Work Group was much more favorable toward the FFM, perhaps recognizing that it would be important, perhaps even necessary, for the proposal to be aligned with a model that has substantial empirical support. On the basis of an additional factor analysis the 6-domain model was revised to a 5-domain model that the authors stated “represents an extension of the Five Factor Model" (APA, 2012c, p. 7). There was no longer an effort to distinguish the proposal from the FFM. In addition, publications by work group members and/or consultants were supporting empirically the alignment of the DSM-5 dimensional trait proposal with the FFM (e.g., De Fruyt et al., 2013; Thomas et al., 2013; Wright et al., 2012).

However, the authors of the proposal did not acknowledge the FFM personality disorder research that would have provided considerable empirical support for the proposal. As indicated by Blashfield and Reynolds (2012), there was very limited reference to FFM research even within the final review posted on the website (i.e., APA, 2012c).The vast body of research concerning the FFM of personality disorder that preceded the development of the dimensional trait proposal (Widiger et al., 2012) was still not being acknowledged. The literature review was confined largely to recent studies by work group members. In sum, there was little to counter the impression that the proposal had limited empirical support and was largely just a creation of the work group members.

Nevertheless, the dimensional trait proposal was at least placed within a section for emerging measures and models (APA, 2013, p. 729). As stated in *DSM-5*, “these five broad domains are maladaptive variants of the five domains of the extensively validated and replicated personality model known as the ‘Big Five,’ or the Five Factor Model of personality” (APA, 2013, p. 773). This dimensional trait model is now receiving a considerable body of research attention (Krueger & Markon, 2014). In addition, the proposals of the ICD-11 personality disorders work group are still on the table, at least for the moment, which include the deletion of all of the diagnostic categories and replacing them with five broad trait domains: emotionally unstable, anxious/dependent, asocial/schizoid, dyssocial/antisocial, and obsessional/anankastic (Tyrer, Crawford, & Mulder, 2011).

Conclusions

Conservative proposals will naturally receive less opposition than major revisions, but it’s not really clear how conservative one should be. Innovative advances in the classification of psychopathology will not occur if the construction of the diagnostic manual is to progress in a gradual piecemeal fashion. DSM-III was highly successful in large part because it was a leap forward in the face of considerable opposition.

It is also inherently difficult though to construct an official nomenclature for the classification and diagnosis of mental disorders, as there is no gold standard, no objective means for determining conclusively the presence of a mental disorder. Their existence and presence are invariably a matter of opinion, hopefully supported by compelling empirical research documenting the presence of impairment, pathology, distress, and/or dyscontrol.

There is also no gold standard for what would constitute sufficiently compelling empirical support for a respective proposal. There is no universally accepted, unambiguous or conclusive rule to determine which position or viewpoint is most likely correct, other than simply appealing to an ill-defined degree of empirical support. Persons will inevitably disagree with respect to any particular proposal, and they will further disagree as to the adequacy and/or nature of the empirical support.

Explicit guidelines for proposed revisions to the APA diagnostic manual though have been developed (Kendler et al., 2009; Frances & Widiger, 2012; Widiger et al., 1991). For example, authors of proposals should provide a systematic and comprehensive review that acknowledges and addresses the primary arguments and findings that are inconsistent with a respective proposal, as well as the potential costs and risks of a proposed revision. In sum, it is reasonably clear what should be done when developing an official diagnostic nomenclature. What will be important for the next edition of the diagnostic manual is to ensure that work group members actually adhere to these guidelines in some consistent and credible manner.

Even if systematic, comprehensive, and objective scientific reviews are conducted, this will not ensure that the correct decision will be made, or at least that the decision will in fact be based on the extent of empirical support. There will be differences of opinion with respect to the adequacy of a review, the extent of the empirical support, and how compelling the rationale was for a respective proposal. DSM-5 did have a scientific oversight committee that periodically reviewed proposals, but it was staffed by current and prior members of DSM-5 work groups and at times may not have had the requisite expertise to review a respective proposal. Proposals with clearly weak empirical support were approved, and proposals with substantial empirical support were rejected. It would seem preferable to have a scientific review committee be staffed by persons independent of the work group and task force membership, and have the requisite expertise to judge the adequacy of the rational and empirical support for a respective proposal.

To ensure the occurrence of informative, forthright reviews for the next edition of the diagnostic manual, they should be submitted to persons who are likely to be critical of the proposal and/or the review. Submitting a manuscript to critical review is a fundamental stage of any credible scientific journal. The literature review process for DSM-5 was analogous to a journal that had no editorial board, allowing work group members to post papers without much oversight, or at least with a tremendously liberal and permissive supervision. Critical review can be extremely useful to identify gaps in coverage, as well as to anticipate, acknowledge and address likely objections, concerns, and flaws. These critical reviews should ultimately be published alongside the review that is in support of the proposal (Widiger & Clark, 2000). This will reward as well as facilitate the quality of the critical review, and further motivate the author(s) of the proposal to be truly fair, accurate, forthright, and responsive.

References

American Psychiatric Association. (1968). *Diagnostic and statistical manual of mental disorders* (2nd ed.). Washington, DC: Author.

American Psychiatric Association. (1980). *Diagnostic and statistical manual of mental disorders* (3rd ed.). Washington, DC: Author.

American Psychiatric Association. (1987*). Diagnostic and statistical manual of mental disorders* (3rd ed., rev. ed.). Washington, DC: Author.

American Psychiatric Association. (1994). *Diagnostic and statistical manual of mental disorders* (4th ed.). Washington, DC: Author.

American Psychiatric Association (2010a). *Classification issues under discussion*. Retrieved from http://www.dsm5.org/ProposedRevisions/Pages/ClassificationIssuesUnderDiscussion.aspx

American Psychiatric Association (February, 2010b). *Complex somatic symptom disorder*. Retrieved from http://www.dsm5.org/ProposedRevisions/Pages/proposedrevision.aspx?rid=368

American Psychiatric Association (February, 2010c). *Pathological gambling*. Retrieved from: http://www.dsm5.org/ProposedRevisions/Pages/proposedrevision.aspx?rid=210

American Psychiatric Association (2012a). *Board of trustees principles*. Retrieved from: http://www.dsm5.org/about/Pages/BoardofTrusteePrinciples.aspx

American Psychiatric Association (2012b). *Cyclothymic disorder*. Retrieved from: http://web.archive.org/web/20121031095932/http://www.dsm5.org/ProposedRevision/Pages/proposedrevision.aspx?rid=158#

American Psychiatric Association. (2012c). *Rationale for the proposed changes to the personality disorders classification in DSM-5*. Unpublished manuscript, Washington, DC: American Psychiatric Association.

American Psychiatric Association. (2013). *Diagnostic and statistical manual of mental disorders* (5th ed.). Washington, DC: Author. http://dx.doi.org/10.1176/appi.books.9780890425596

Bagby, R. M., Watson, C., & Ryder, A. G. (2012). *Depressive personality disorder.* In T. A. Widiger (Ed*.), Oxford handbook of personality disorders*. NY: Oxford University Press. Pp. 628-647.

Barsky, A. J., Wyshak, G., & Klerman, G. L. (1992). Psychiatric comorbidity in DSM-III-R hypochondriasis. *Archives of General Psychiatry, 43,* 101-108. http://dx.doi.org/10.1001/archpsyc.1992.01820020021003

Bayer, R., & Spitzer, R. L. (1982). Edited correspondence on the status of homosexuality in DSM-111. *Journal of the History of the Behavioral Sciences*, *18*, 32-52. http://dx.doi.org/10.1002/1520-6696(198201)18:1<32::AID-JHBS2300180105>3.0.CO;2-0

Blashfield, R. K., & Reynolds, S. M. (2012). An invisible college view of the DSM-5 personality disorder classification. *Journal of Personality Disorders, 26*, 821-829. http://dx.doi.org/10.1521/pedi.2012.26.6.821

Bouman, T. K., & Eifert, G. H. (2009). Somatoform disorders. In P. H. Blaney & T. Millon (Eds.), *Oxford textbook of psychopathology*. NY: Oxford University Press. Pp. 482-505.

Clark, L. A. (2007). Assessment and diagnosis of personality disorder: Perennial issues and an emerging reconceptualization*. Annual Review of Psychology, 57*, 277-257.

Clark, L.A., & Krueger, R.F. (2010, February 10). *Rationale for a six-domain trait dimensional diagnostic system for personality disorder*. Retrieved from http://www.dsm5.org/ProposedRevisions/Pages/RationaleforaSix-DomainTraitDimensionalDiagnosticSystemforPersonalityDisorder.aspx

Clarke, D. E., Narrow, W. E., Regier, D. A., Kuramoto, S. J., Kupfer, D. J., Kuhl, E. A., Greiner, L., & Kraemer, H. C. (2013). DSM-5 Field Trials in the United States and Canada, part I: study design, sampling strategy, implementation, and analytic approaches. *American Journal of Psychiatry*, *170*, 43-58. http://dx.doi.org/10.1176/appi.ajp.2012.12070998

De Fruyt, F., De Clercq, B., De Bolle, M., Wille, B., Markon, K., & Krueger, R. F. (2013). General and maladaptive traits in a five-factor framework for DSM-5 in a university student sample. *Assessment*, *20*, 295-307. http://dx.doi.org/10.1177/1073191113475808

Decker, H. S. (2013). *The making of DSM-III. A diagnostic manual's conquest of American psychiatry*. NY: Oxford University Press.

DSM-5 Child and Adolescent Disorders Work Group (2010). *Justification for temper dysregulation disorder with dysphoria*. Retrieved from http://www.dsm5.org/ProposedRevision/Pages/proposedrevision.aspx?rid=397#

Escobar, J. L., Gara, M., Cohen, S. R., Waitzkin, H., Holman, A., Comptom, W. (1998). Somatization disorder in primary care. *British Journal of Psychiatry, 173*, 262-266. http://dx.doi.org/10.1192/bjp.173.3.262

Fawcett, J. (2010). An overview of mood disorders in the DSM-5. *Current Psychiatry Reports, 12*, 531-538. http://dx.doi.org/10.1007/s11920-010-0154-2

Fink, P., Ornbel, E., Toft, T., Sparle, K. C., Frostholm, L., & Olesen, F. (2004). A new, empirically established hypochondriasis diagnosis. *American Journal of Psychiatry, 161*, 1680-1691. http://dx.doi.org/10.1176/appi.ajp.161.9.1680

Fischer, B. A. (2012). A review of American psychiatry through its diagnoses. The history and development of the Diagnostic and Statistical Manual of Mental Disorders. *Journal of Nervous and Mental Disease, 12*, 1022-1030. http://dx.doi.org/10.1097/NMD.0b013e318275cf19

Frances, A. J. (2013). *Saving normal: an insider's revolt against out-of-control psychiatric diagnosis, DSM-5, big pharma, and the medicalization of ordinary life.* NY: Harper Collins.

Frances, A. J., & Widiger, T. A. (2012). Psychiatric diagnosis: Lessons from the DSM-IV past and cautions for the DSM-5 future. *Annual Review of Clinical Psychology, 8,* 109-130. http://dx.doi.org/10.1146/annurev-clinpsy-032511-143102

Frances, A. J., Widiger, T. A., & Pincus, H. A. (1989). The development of DSM-IV. *Archives of General Psychiatry, 46*, 373-375. http://dx.doi.org/10.1001/archpsyc.1989.01810040079012

Frick, P. J., Ray, J. V., Thornton, L. C., & Kahn, R. E. (2014). Would a Specifier for Callous-Unemotional Traits Enhance the Diagnosis of Conduct Disorder? A Comprehensive Review. *Psychological Bulletin, 140*, 1-57. http://dx.doi.org/10.1037/a0033076

Frost, R. O., Steketee, G., Tolin, D. F. (2012). *Diagnosis and assessment of hoarding disorder. Annual Review of Clinical Psychology,* 8, 219-242. http://dx.doi.org/10.1146/annurev-clinpsy-032511-143116

Garb, H. (2005). Clinical judgment and decision making. *Annual Review of Clinical Psychology, 1,* 67-89. http://dx.doi.org/10.1146/annurev.clinpsy.1.102803.143810

Glover, N., Crego, C., & Widiger, T. A. (2012). The clinical utility of the five-factor model of personality disorder. *Personality Disorders: Theory, Research, and Treatment, 3*, 176-184. http://dx.doi.org/10.1037/a0024030

Greenberg, G. (2013). *The book of woe: The DSM and the unmaking of psychiatry.* New York: Blue Rider Press.

Gunderson, J. G. (2013). Seeking clarity for future revisions of the personality disorders in DSM-5. *Personality Disorders: Theory, Research, and Treatment*, *4,* 368-376. http://dx.doi.org/10.1037/per0000026

Helzer, J. E., Kraemer, H. C., Krueger, R. F., Wittchen, H-U., Sirovatka, P. J., & Regier, D. A. (Eds.). (2008). *Dimensional approaches to diagnostic classification. Refining the research agenda for DSM-V*. Washington, DC: American Psychiatric Association.

Hyman, S. E. (2010). The diagnosis of mental disorders: the problem of reification. *Annual Review of Clinical Psychology, 6*, 155-179. http://dx.doi.org/10.1146/annurev.clinpsy.3.022806.091532

John, O. P., Naumann, L. P., & Soto, C. J. (2008). Paradigm shift to the integrative Big Five trait taxonomy: History, measurement, and conceptual issues. In O. P. John, R. R. Robins, & L. A. Pervin (Eds.), *Handbook of personality. Theory and research* (3rd. ed.). NY: Guilford. Pp. 114-158.

Jones, K. D. (2012). A critique of the DSM-5 field trials. *The Journal of nervous and mental disease*, *200*, 517-519. http://dx.doi.org/10.1097/NMD.0b013e318257c699

Kafka, M. P. (2010). Hypersexual disorder: A proposed diagnosis for DSM-V. *Archives of sexual behavior*, *39*, 377-400. http://dx.doi.org/10.1007/s10508-009-9574-7

Kapur, S., Phillips, A. G., & Insel, T. R. (2012). Why has it taken so long for biological psychiatry to develop clinical tests and what to do about it? *Molecular Psychiatry, 17*, 1174-1179. http://dx.doi.org/10.1038/mp.2012.105

Kendler, K. S. (1990). Toward a scientific psychiatric nosology: strengths and limitations. *Archives of General Psychiatry*, *47*, 969-973. http://dx.doi.org/10.1001/archpsyc.1990.01810220085011

Kendler, K.S. (2010). *A statement from Kenneth S. Kendler, M.D., on the proposal to eliminate the grief exclusion criterion from major depression, by Kenneth S. Kendler, M.D., Member, DSM-5 Mood Disorder Work Group*. Retrieved from http://www.dsm5.org/Pages/Default.aspx.

Kendler, K. S. (2013). A history of the DSM-5 scientific review committee. Psychological Medicine, 43, 1793-1800. http://dx.doi.org/10.1017/S0033291713001578

Kendler, K. S., Kupfer, D., Narrow, W., Phillips, K., & Fawcett, J. (2009). *Guidelines for making changes to DSM-V*. Unpublished manuscript, Washington, DC: American Psychiatric Association.

Kendler, K., Munoz, R. A., & Murphy, G. (2010). The development of the Feighner criteria: a historical perspective. *American Journal of Psychiatry, 167*, 134-142. http://dx.doi.org/10.1176/appi.ajp.2009.09081155

Klerman, G. L. (1986). Historical perspectives on contemporary schools of psychopathology. In T. Millon & G. L. Klerman (Eds.), *Contemporary directions in psychopathology* (pp. 3-28). NY: Guilford.

Kraemer, H. C. (2014). The reliability of clinical Diagnoses: State of the art. *Annual Review of Clinical Psychology, 10*, 111-130. http://dx.doi.org/10.1146/annurev-clinpsy-032813-153739

Krueger, R. F. (2013). Personality disorders and the vanguard of the post DSM-5.0 era. *Personality Disorders: Theory, Research, and Treatment*, http://dx.doi.org/10.1146/annurev-clinpsy-032813-153732

Krueger, R. F., & Markon, K. E. (2014). The role of the DSM-5 personality trait model in moving toward a quantitative and empirically based approach to classifying personality and psychopathology. *Annual Review of Clinical Psychology, 10*, 477-501. http://dx.doi.org/10.1146/annurev-clinpsy-032813-153732

Krueger, R. F., Skodol, A. E., Livesley, W. J., Shrout, P. E., & Huang, Y. (2008). Synthesizing dimensional and categorical approaches to personality disorders: refining the research agenda for DSM-IV Axis II. In J. E. Helzer, H. C. Kraemer, R. F. Krueger, H-U. Wittchen, P. J. Sirovatka, & D. A. Regier (Eds.), *Dimensional approaches to diagnostic classification. Refining the research agenda for DSM-V*. Washington, DC: American Psychiatric Association. Pp. 85-100.

Kupfer, D. J., Angst, J., Berk, M., Dickerson, F., Frangou, S., Frank, E., Goldstein, B., Harvey, A., Laghrissi-Thode, F., Leboyer, M., Ostacher, M. J., Sibille, E., Strakowski, S. M., Suppes, T., Tohen, M., Yolken, R. H., Young, L. T., & Zarate, C. A. (2011). Advances in bipolar disorder: selection sessions from the 2011 international conference on bipolar disorder. *Annals of the New York Academy of Sciences, 1242,* 1-25. http://dx.doi.org/10.1111/j.1749-6632.2011.06336.x

Kupfer, D. J., First, M. B. & Regier, D. A. (Eds.). (2002). Introduction. In D. J. Kupfer, M. B. First, & D. A. Regier (Eds.), *A research agenda for DSM-V*. Washington, DC: American Psychiatric Association. Pp. xv-xxiii.

Livesley, W. J. (2012). Tradition versus empiricism in the current DSM-5 proposal for revising the classification of personality disorders. *Criminal Behavior and Mental Health, 22*, 81-90. http://dx.doi.org/10.1002/cbm.1826

Lowe, J. R., & Widiger, T. A. (2009). Clinicians' judgments of clinical utility: a comparison of the DSM-IV with dimensional models of personality disorder. *Journal of Personality Disorders, 23*, 211-229. http://dx.doi.org/10.1521/pedi.2009.23.3.211

Meehl, P. E. (1986). Diagnostic taxa as open concepts: metatheoretical and statistical questions about reliability and construct validity in the grand strategy of nosological revision. In T. Millon & G. L. Klerman (Eds.), *Contemporary directions in psychopathology: Toward the DSM-IV*. New York: Guilford Press. Pp. 215-231.

Miller, J. D., & Lynam, D. R. (2013). Missed opportunities in the DSM-5 Section III personality disorder model: Commentary on “Personality disorders are the vanguard of the post-DSM-5.0 era.” *Personality Disorders: Theory, Research, and Treatment, 4*, 365-366. http://dx.doi.org/10.1037/per0000043

Mullins-Sweatt, S. N., Bernstein, D., & Widiger, T. A. (2012). Retention or deletion of personality disorder diagnoses for DSM-5: An expert consensus approach. *Journal of Personality Disorders, 26*, 689-703. http://dx.doi.org/10.1521/pedi.2012.26.5.689

Mullins- Sweatt, S. N., & Lengel, G. S. (2012). The clinical utility of the five-factor model of personality disorder. *Journal of Personality, 80*, 1615-1639. http://dx.doi.org/10.1111/j.1467-6494.2012.00774.x

Paris, J. (2013). Anatomy of a debacle. Commentary on “Seeking clarity for future revisions of the personality disorders in DSM-5.” *Personality Disorders: Theory, Research, and Treatment, 4,* 377-378. http://dx.doi.org/10.1037/per0000046

Phillips, K. A., Wilhelm, S., Koran, L. M., Didie, E. R., Fallon, B. A,. Feusner, J., & Stein, D. J. (2010). Body dysmorphic disorder: some key issues for DSM-V. *Depression and Anxiety, 27*, 573-591. http://dx.doi.org/10.1002/da.20709

Regier, D. A. (2008). Forward: dimensional approaches to psychiatric classification. In J. E. Helzer, H. C. Kraemer, R. F. Krueger, H-U. Wittchen, P. J. Sirovatka, & D. A. Regier (Eds.), *Dimensional approaches to diagnostic classification. Refining the research agenda for DSM-V*. Washington, DC: American Psychiatric Association. Pp. xvii-xxiii)

Regier, D. A., Narrow, W. E., Clarke, D. E., Kraemer, H. C., Kuramoto, S. J., Kuhl, E. A., & Kupfer, D. J. (2013). DSM-5 Field Trials in the United States and Canada, part II: test-retest reliability of selected categorical diagnoses. *American Journal of Psychiatry*, *170*, 59-70. http://dx.doi.org/10.1176/appi.ajp.2012.12070999

Regier, D. A., Narrow, W. E., Kuhl, E. A., & Kupfer, D. J. (2010). The conceptual development of DSM-V. *American Journal of Psychiatry, 166*, 645-655. http://dx.doi.org/10.1176/appi.ajp.2009.09020279

Robins, E., & Guze, S. B. (1970). Establishment of diagnostic validity in psychiatric illness: its application to schizophrenia. *American Journal of Psychiatry*, *126*, 983-987. http://dx.doi.org/10.1176/ajp.126.7.983

Rottman, B. M., Ahn, W. K., Sanislow, C. A. & Kim, N. S. (2009). Can clinicians recognize DSM-IV personality disorders from five-factor model descriptions of patient cases? *American Journal of Psychiatry, 196*, 356-374.

Rounsaville, B. J., Alarcon, R. D., Andrews, G., Jackson, J. S., Kendell, R. E., Kendler, K. S., & Kirmayer, L. J. (2002). Toward DSM-V: Basic nomenclature issues. In D. J. Kupfer, M. B. First, & D. A. Regier (Eds.), *A research agenda for DSM-V*. Washington, DC: American Psychiatric Press. Pp. 1-30.

Samuel, D. B., & Widiger, T. A. (2006). Clinicians' judgments of clinical utility: A comparison of the DSM-IV and five factor models. *Journal of Abnormal Psychology, 115*, 298-308. http://dx.doi.org/10.1037/0021-843X.115.2.298

Schatzberg, A. F., Scully, J. H., Kupfer, D. J., & Regier, D. A. (2009). Setting the record straight: A response to Frances commentary on DSM-V. *Psychiatric Times*, *26*(8).

Shedler, J., Beck, A., Fonagy, P., Gabbard, G. O., Gunderson, J .G., Kernberg, O., Michels, R., & Westen, D. (2010). Personality disorders in DSM-5. *American Journal of Psychiatry, 167*, 1027-1028. http://dx.doi.org/10.1176/appi.ajp.2010.10050746

Shedler, J. & Westen, D. (2004). Dimensions of personality pathology: An alternative to the Five-Factor Model. *American Journal of Psychiatry, 161,* 1743-1754. http://dx.doi.org/10.1176/appi.ajp.161.10.1743

Silk, K. R. (2013). Caught in an unconscious split: Commentary on “The ironic fate of the personality disorders in DSM-5.” *Personality Disorders: Theory, Research, and Treatment, 4,* 350-351. http://dx.doi.org/10.1037/per0000047

Skodol, A. E. (2010, February 10). Rationale for proposing five specific personality types. Retrieved from http://www.dsm5.org/ProposedRevisions/Pages/RationaleforProposingFiveSpecificPersonalityDisorderTypes.aspx

Skodol, A. E. (2012). Personality disorders in DSM-5. *Annual Review of Clinical Psychology, 8*, 317-344. http://dx.doi.org/10.1146/annurev-clinpsy-032511-143131

Skodol, A. E., Morey, L. C., Bender, D. S., & Oldham, J. M. (2013). The ironic fate of the personality disorders in DSM-5. *Personality Disorders: Theory, Research, and Treatment, 4,* 342-349. http://dx.doi.org/10.1037/per0000029

Smith, G.T., McCarthy, D., & Zapolski, T. (2009). On the value of homogeneous constructs for construct validation, theory testing, and the description of psychopathology. *Psychological Assessment*, *21*(3), 272-284. http://dx.doi.org/10.1037/a0016699

Spiegel, D., Lewis-Fernandez, R., Lanius, R., Vermetten, E., Simeon, D., & Friedman, M. (2013). Dissociative disorders in DSM-5. *Annual Review of Clinical Psychology, 9*, 299-326. http://dx.doi.org/10.1146/annurev-clinpsy-050212-185531

Spitzer, R. L., Endicott, J., & Robins, E. (1975). Clinical criteria for psychiatric diagnosis and DSM-III. *American Journal of Psychiatry*, 132, 1187-1192. http://dx.doi.org/10.1176/ajp.132.11.1187

Spitzer, R. L., First, M. B., Shedler, J., Westen, D., & Skodol, A. (2008). Clinical utility of five dimensional systems for personality diagnosis. *Journal of Nervous and Mental Disease, 196*, 356-374. http://dx.doi.org/10.1097/NMD.0b013e3181710950

Spitzer, R. L., & Fleiss, J. L. (1974). A re-analysis of the reliability of psychiatric diagnosis. *The British Journal of Psychiatry*, *125,* 341-347. http://dx.doi.org/10.1192/bjp.125.4.341

Spitzer, R. L., & Williams, J. B. W. (1982). The definition and diagnosis of mental disorder. In W.R. Gove (Ed.), *Deviance and mental illness*. Beverly Hills, CA: Sage. Pp. 15-32.

Spitzer, R. L., Williams, J. B. W., & Skodol, A. E. (1980). DSM-III: The major achievements and an overview. *American Journal of Psychiatry, 137*, 151-164. http://dx.doi.org/10.1176/ajp.137.2.151

Strauss, M., & Smith, G. (2009). Construct validity: Advances in theory and methodology. *Annual Review of Clinical Psychology*, *5*1-25.

Thomas, K. M., Yalch, M. M., Krueger, R. F., Wright, A. G., Markon, K. E., & Hopwood, C. J. (2013). The convergent structure of DSM-5 personality trait facets and five-factor model trait domains. *Assessment*, *20*, 308-311. http://dx.doi.org/10.1177/1073191112457589

Tyrer, P., Crawford, M., & Mulder, R. (2011). Reclassifying personality disorders. *Lancet, 377*(9780), 1814-1815. http://dx.doi.org/10.1016/S0140-6736(10)61926-5

Verheul, R. (2012). Personality disorder proposal for DSM-5: A heroic and innovative but nevertheless fundamentally flawed attempt to improve DSM-IV. *Clinical Psychology and Psychotherapy*, *19*, 369-371. http://dx.doi.org/10.1002/cpp.1809

Volkmar, F. R., & McPartland, J. C. (2014). From Kanner to DSM-5: autism as an evolving diagnostic concept. *Annual Review of Clinical Psychology, 10*, 193-212. http://dx.doi.org/10.1146/annurev-clinpsy-032813-153710

Wakefield, J. C. (2013). The DSM-5 debate over the bereavement exclusion: psychiatric diagnosis and the future of empirically supported treatment. *Clinical Psychology Review, 33*, 825-845. http://dx.doi.org/10.1016/j.cpr.2013.03.007

Westen, D., Shedler, J., & Bradley, R. (2006). A prototype approach to personality disorder diagnosis. *American Journal of Psychiatry, 163*, 846-856. http://dx.doi.org/10.1176/ajp.2006.163.5.846

Widiger, T. A. (2011). A shaky future for personality disorders. *Personality Disorders: Theory, Research, and Treatment, 2,* 54-67. http://dx.doi.org/10.1037/a0021855

Widiger, T. A. (2012). Classification and diagnosis: Historical development and contemporary issues. In J. Maddux and B. Winstead (Eds.), *Psychopathology: Foundations for a contemporary understanding* (3rd edition). Mahwah, NJ: Lawrence Erlbaum Associates, Inc. Pp. 101-120.

Widiger, T. A. (2013). DSM-5 personality disorders: a postmortem and future look. *Personality Disorders: Theory, Research, and Treatment*, *4,* 382-387. http://dx.doi.org/10.1037/per0000030

Widiger, T. A., & Clark, L. A. (2000). Toward DSM-V and the classification of psychopathology. *Psychological Bulletin, 126*, 946-963. http://dx.doi.org/10.1037/0033-2909.126.6.946

Widiger, T. A., Frances, A. J., Pincus, H. A., Davis, W. W., & First, M. B. (1991). Toward an empirical classification for DSM-IV. *Journal of Abnormal Psychology, 100*, 280-288. http://dx.doi.org/10.1037/0021-843X.100.3.280

Widiger, T. A., & Mullins-Sweatt, S. N. (in press). Classification and diagnosis. In J. C. Norcross, G. R. VandenBos, D. K. Freedheim (Eds.), *APA handbook of clinical psychology*. Washington, DC: American Psychological Association.

Widiger, T. A., Samuel, D. B., Mullins-Sweat, S., Gore, W. L., & Crego, C. (2012). Integrating normal and abnormal personality structure: the five-factor model. In T. A. Widiger (Ed.), *Oxford handbook of personality disorders*. NY: Oxford University Press. Pp. 82-107. http://dx.doi.org/10.1093/oxfordhb/9780199735013.001.0001

Widiger, T. A., & Sankis, L. M. (2000). Adult psychopathology: Issues and controversies. *Annual Review of Psychology, 51*, 377-404. http://dx.doi.org/10.1146/annurev.psych.51.1.377

Widiger, T. A., Simonsen, E., Krueger, R., Livesley, J., & Verheul, R. (2005). Personality disorder research agenda for the DSM-V. *Journal of Personality Disorders, 19*, 317-340. http://dx.doi.org/10.1521/pedi.2005.19.3.315

Wright, A. G., Thomas, K. M., Hopwood, C. J., Markon, K. E., Pincus, A. L., & Krueger, R. F. (2012). The hierarchical structure of DSM-5 pathological personality traits. *Journal of abnormal psychology*, *121*, 951-957. http://dx.doi.org/10.1037/a0027669

Zimmerman, M. (2003). What should the standard of care for psychiatric diagnostic evaluations be? *Journal of Nervous and Mental Disease, 191*, 281-286. http://dx.doi.org/10.1097/01.NMD.0000066149.40946.FA

Zimmerman, M. (2011). A critique of the proposed prototype rating system for personality disorders in DSM-5. *Journal of Personality Disorders, 25,* 206-221. http://dx.doi.org/10.1521/pedi.2011.25.2.206

Zisook, S., Shear, K., & Kendler, K. S. (2007). Validity of the bereavement exclusion criterion for the diagnosis of major depressive episode. *World Psychiatry*, *6*, 102-107.