Journal of Traumatic Stress
October 2013, *26,* 567–569

ISTSS International Society for Traumatic Stress Studies

COMMENTARY

PTSD in the *DSM-5*: Reply to Brewin (2013), Kilpatrick (2013), and Maercker and Perkonigg (2013)

Matthew J. Friedman[1,2]
[1]National Center for PTSD, White River Junction, Vermont, USA
[2]Geisel School of Medicine at Dartmouth, Hanover, New Hampshire, USA

The greater emphasis on scientific evidence and the high threshold for changing any criterion in the *Diagnostic and Statistical Manual for Mental Disorders* (4th ed., *DSM-IV*) probably account for many key differences between the *DSM-5* and the *International Classification of Diseases and Related Health Problems* (11th ver.; *ICD-11*) with regard to diagnostic criteria for posttraumatic stress disorder (PTSD). Important questions about PTSD remain that can only be settled by future research. Additional research is also needed on subthreshold PTSD, a dissociative subtype described in the *DSM-5*; complex PTSD, included in the *ICD-11*; bereavement-related disorders; and adjustment disorders. We can all look forward to such scientific advances to inform our ongoing efforts to develop the best diagnostic criteria for trauma- and stressor-related disorders.

I thank Drs. Brewin (2013), Kilpatrick (2013), and Maerker and Perkonigg (2013) for their thoughtful commentaries on my review of the process (Friedman, 2013) of developing a section of the *Diagnostic and Statistical Manual for Mental Disorders* (5th ed., *DSM-5*; American Psychiatric Association [APA], 2013). They have focused mostly on differences between the *DSM-5* and the *International Classification of Diseases and Related Health Problems* (11th ver.; *ICD-11*) diagnostic criteria for posttraumatic stress disorder (PTSD). This includes the different directions regarding the dissociative subtype described in the *DSM-5* and complex PTSD included in *ICD-11*. I will discuss these as well as two other diagnoses, adjustment disorder and prolonged grief disorder.

Before addressing differences, it is important to recognize that both classification systems have moved PTSD out of the anxiety disorders category and into a separate chapter on stress-related disorders. Both schemes have maintained the intrusion, avoidance, and arousal symptom clusters included in the *DSM-IV* (APA, 1994). Both the *DSM-5* and the *ICD-11* Work Groups attempted to develop diagnostic criteria that were evidence-based and optimized clinical utility. And yet, the work groups have each come up with very different proposals for diagnosing PTSD. How could this happen? I believe there are two major reasons. First, the *DSM-5*-based and *ICD-11*-based procedures themselves inevitably led to different outcomes. Second, the two work groups evaluated the scientific evidence and appraised clinical utility from different perspectives and with different priorities.

Brewin's (2013) commentary is a good place to begin. The *DSM-5* was essentially a very conservative process in which very high levels of evidence were required to add, delete, or revise any *DSM-IV* diagnostic criterion. I agree with this approach. With more than 30 years' worth of clinical, epidemiological, psychobiological, and other data on PTSD, there should be a very high threshold for change in any diagnostic criterion. Even with the relatively modest empirically based revisions adopted by the *DSM-5*, there are important questions for research to sort out regarding reinterpretation of the *DSM-IV*-based PTSD data through the new lens of the *DSM-5*. The work group was very respectful of this approach and only proposed revisions that were strongly supported empirically.

In contrast, the *ICD-11* Work Group "was under no obligation to use the *DSM-IV-TR* or even the *ICD-10* as a starting point" (Brewin, 2013, p. 557). Therefore, they had license to radically alter PTSD criteria, and they did. As Brewin acknowledges, given important gaps in the literature, as well as a paucity of studies comparing related diagnostic categories, development of the *ICD-11* involved "a substantial element of guesswork" (p. 558). Although I am a strong proponent of the inductive process in science, hypotheses must be tested and validated before they are considered evidence. In the *DSM-5*, our proposals were based entirely on evidence rather than conjecture. That is not

Correspondence concerning this article should be addressed to Matthew J. Friedman, National Center for PTSD, VA Medical Center, 215 North Main Street, White River Junction, VT 05009. E-mail: Matthew.Friedman@Dartmouth.edu

DOI: 10.1002/jts.21847

to say that the PTSD criteria adopted by the *ICD-11* may not prove better in the long run. Before we can make that judgment, however, I agree with Kilpatrick (2013) that the *ICD-11*-based "PTSD proposals should be subjected to rigorous scrutiny and should require substantial empirical justification prior to their approval" (p. 565).

Here are some specific concerns about the *ICD-11*. As Kilpatrick (2013, p. 564) points out, rather than sharpen Criterion A, the *ICD-11* features a very general definition of a traumatic stressor as "an extremely threatening or horrific event." This definition conflates exposure to the event per se with the emotional reaction to that event. Given how much criticism has been leveled at PTSD because of Criterion A, I do not believe that the approach given in the *ICD-11* is either evidence-based or has clinical (or forensic) utility. In the *DSM-5,* we have sharpened the *DSM-IV*-based exposure (A_1) criterion, but discarded the acute emotional (A_2) criterion (Friedman, Resick, Bryant, & Brewin, 2011). Furthermore, focusing on symptom presentation rather than on Criterion A itself does not solve the problem; the intrusion and avoidance symptoms can only be understood within the context of the traumatic event itself (Friedman et al., 2011; Kilpatrick, 2013).

Brewin (2013) argues that the smaller, "more focused symptoms set" of the *ICD-11* will reduce comorbidity. (p. 557) Frankly, I do not understand the logic of that assertion because individuals must exhibit intrusion, avoidance, and arousal symptoms in both classification schemes. The *DSM-IV*-based numbing symptoms associated with PTSD are likely to be present either as associated symptoms according to the *ICD-11,* or as negative cognitions and mood symptoms according to the *DSM-5*. However, it is really unknown how many people will meet the *ICD-11*-based PTSD criteria without the presence of these associated symptoms and how many of those people will or will not also meet the *DSM-5*-based criteria.

I agree with Brewin that people with subsyndromal/partial PTSD tend to exhibit greater symptom severity, comorbidity, and functional impairment than nonaffected individuals (Friedman et al., 2011). However, such individuals are often less symptomatic than those with full PTSD based on the *DSM-IV*. Brewin (2013, p. 557) seems to be suggesting that they should be diagnosed with full PTSD because "diagnostic thresholds have been set too high" based on the *DSM-IV* and *DSM-5.* Again, it is an empirical question, but the current evidence in support of such a proposal is spotty and inconsistent, especially because different investigators have used different case definitions (with different symptom thresholds) in research on subsyndromal/partial PTSD. Until we know the consequences of such an action—and we have a standard case definition of subsyndromal/partial PTSD— I believe that we should continue to distinguish it from full PTSD.

Finally, Brewin argues that the good performance of brief screening instruments in predicting PTSD supports the narrow approach of the *ICD-11*. It is important to recognize, however, that screening instruments are designed to identify people at risk for a certain problem so that more extensive evaluations can be carried out. Whether it is a chest x-ray, a blood pressure test, or a brief PTSD-screening questionnaire, a positive result is only the first step in a clinical workup, not a conclusive diagnostic assessment in itself.

The World Health Organization's public health mission appears to have influenced the endorsement of a narrow PTSD construct with many fewer symptoms to assess in the *ICD-11*. Maercker and Perkonigg (2013) suggest that this simpler diagnostic algorithm will have greater clinical utility for "a wide range of health professionals across different specialty and primary care settings worldwide." (p. 561). This is certainly consistent with a belief that the *DSM-IV*-based and the *DSM-5*-based PTSD diagnosis has too many symptoms and is too complicated for nonspecialists to assess. It is an important question. But it is also an empirical question. We need investigations of the relative clinical utility of the different *ICD-11*-based and *DSM-5*-based diagnostic criteria in a wide variety of practice settings. As noted (Friedman, 2013), PTSD was one of very few diagnoses for which high interrater reliability was noted in the *DSM-5* field trials. So nonspecialists may do much better with the *DSM-5* criteria than the *ICD-11* Work Group expects.

Maercker and Perkonigg (2013) are very dismissive of the dissociative subtype as described in the *DSM-5* because there was "no research until last year" (p. 560). Actually, research on dissociation-related differences among individuals with PTSD has been going on for more than 10 years (Lanius, Brand, Vermetten, Frewen, & Spiegel, 2012). More important, the scientific evidence regarding clinical phenomenology, neurocircuitry, latent structure, and treatment outcomes (Lanius et al., 2012) was compelling to the rigorous *DSM-5* Scientific Review Committee (Friedman, 2013). In contrast, and much to my disappointment, a comparable body of solid scientific support for complex PTSD has not been published. So why was complex PTSD included in the *ICD-11*? Two reasons might account for this. First, because the development of the *ICD-11's* criteria was not bound by the same requirements for rigorous empirical validators as was the *DSM-5's*, it was possible to adopt a popular diagnosis that is seen to have great clinical utility (especially in the assessment of refugees and other patients) despite limited supporting evidence. Second, Maercker and Perkonigg (2013) indicate concern by the *ICD-11* Work Group that its narrow PTSD construct would exclude "the full range of clinical presentations and needs of a number of traumatized patients" (p. 561). Having exiled the *DSM-5*'s Criterion D symptoms (e.g., negative mood and cognitions) to associated symptoms' status, the addition of complex PTSD was a good way to include such patients within the greater PTSD construct and still retain a narrow definition of PTSD.

Many features of Herman's (1992) complex PTSD construct can be found in the *DSM-5* within Criterion D, E, and G (e.g., functional impairment, which includes relationship difficulties; Friedman, 2013; Kilpatrick, 2013). So, the scientific findings or clinical utility of many symptoms that were first proposed within the complex PTSD construct have hardly been ignored

in the *DSM-5*. The question is which diagnostic formulation is the best exemplar of such clinical presentations, the *DSM-5*-based PTSD with its dissociative subtype or the *ICD-11*-based complex PTSD? It is another important empirical question. We can all look forward to the future research on this.

Regarding inclusion of prolonged grief disorder in the *ICD-11* (Maercker & Perkonigg, this issue), research findings have not been consistent. Proponents of the prolonged grief construct have emphasized the duration of grief (Prigerson et al., 2009), whereas proponents of the complicated grief construct (Shear et al., 2011) have emphasized qualitative differences between this syndrome and normal grief. Although the *DSM-5* Work Group was convinced that a bereavement-related diagnosis would have great clinical utility, they could not find an empirical basis for choosing between the prolonged and complicated grief constructs. Therefore, they combined the two into the new diagnosis, persistent complex bereavement disorder, and placed it in the *DSM-5*'s Appendix (e.g., Section 3) to promote further research. Hopefully, a bereavement-related diagnosis will be ready for an updated *DSM-5* within a few years. It is important to remind readers that adjustment disorder remains available as a diagnosis for abnormal bereavement.

Adjustment disorders have received very little attention by investigators (Strain & Friedman, 2011). They have been redefined in the *DSM-5* as stress-response syndromes and research on adjustment disorders per se, and on their various subtypes has been strongly encouraged. If each subtype is a subthreshold (mood, anxiety, or conduct) disorder, research might uncover important differences with regard to phenomenology, longitudinal course, psychobiology, treatment response, etc. In that regard, I consider *ICD-11* Work Group's decision to eliminate all adjustment disorders subtypes premature and not evidence based. Once again, we will need to look forward to future research to address this important issue.

In closing, we will all benefit from thoughtful rigorous comparisons of the different *DSM-5*-based and *ICD-11*-based diagnostic criteria for PTSD. As suggested by Kilpatrick (2013), the web-based survey methodology adopted by the *DSM-5* Work Group offers an opportunity to complete such research efficiently with reasonably representative cohorts. Such online self-assessments were a valuable source of information for the *DSM-5* Work Group. I hope that my colleagues at work on the *ICD-11* will consider a similar approach.

References

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

American Psychiatric Association. (2013). *Diagnostic and statistical manual of mental disorders* (5th ed.). Arlington, VA: Author.

Brewin, C. R. (2013) "I wouldn't start from here"—An alternative perspective on PTSD from *ICD-11:* Comment on Friedman (2013). *Journal of Traumatic Stress*, *26*, 557–559. doi:10.1002/jts.21843

Friedman, M. J. (2013). Finalizing PTSD in *DSM-5*: Getting here from there and where to go next. *Journal of Traumatic Stress*, *26*, 548–556. doi:10.1002/jts.21840

Friedman, M. J., Resick, P. A., Bryant, R. A., & Brewin, C. R. (2011). Considering PTSD for DSM-5. *Depression and Anxiety*, *28*, 750–769. doi:10.1002/da.20767

Herman, J. L. (1992). Complex PTSD: A syndrome in survivors of prolonged and repeated trauma. *Journal of Traumatic Stress*, *5*, 377–391. doi:10.1007/BF00977235

Kilpatrick, D. (2013). PTSD: The *DSM-5* got it right: Comment on Friedman (2013). *Journal of Traumatic Stress*, *26*, 563–566. doi:10.1002/jts.21844

Lanius, R. A., Brand, B. L., Vermetten, E., Frewen, P. A., & Spiegel, D. (2012). The dissociative subtype of posttraumatic stress disorder: Rationale, clinical and neurobiological evidence, and implications. *Depression and Anxiety*, *29*, 701–708. doi:10.1002/da.21889

Maercker, A., & Perkonigg, A. (2013). Applying an international perspective in defining PTSD and related disorders: Comment on Friedman (2013). *Journal of Traumatic Stress*, *26*, 560–562. doi:10.1002/jts.21852

Prigerson, H. G., Horowitz, M. J., Jacobs, S. C., Parkes, C. M., Aslan, M., Goodkin, K., ... Maciejewski, P. K. (2009). Prolonged grief disorder: Psychometric validation of criteria proposed for *DSM-V* and *ICD-11*. *PLoS Medicine*, *6*(8), e1000121. doi:10.1371/journal.pmed.1000121

Shear, M. K., Simon, N. M., Wall, M. M., Zisook, S., Neimeyer, R. A., Duan, N., ... Keshaviah, A. (2011). Complicated grief and related bereavement issues for *DSM-5*. *Depression and Anxiety*, *28*, 103–117. doi:10.1002/da.20780

Strain, J. J., & Friedman, M. J. (2011). Considering adjustment disorders as stress response syndromes for *DSM-5*. *Depression and Anxiety*, *28*, 818–823. doi:10.1002/da.20782