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REPLY

Advocacy Through Science: Reply to Comments on Resick et al. (2012)

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The 4 comments on the review by Resick et al. (2012) of the complex posttraumatic stress disorder (CPTSD) literature highlight important theoretical and conceptual questions about the nature and utility of CPTSD and echo the very questions that motivated the review. We discuss the points raised in the comments, particularly with respect to the definition of CPTSD, its relationship to PTSD, and treatment implications. We suggest that setting high scientific standards for CPTSD research is an optimal way to advance the conceptualization of the construct and the treatment of this population.

We wish to thank the four authors who contributed comments in response to our evaluative review of complex posttraumatic stress disorder (CPTSD). The comments highlight important theoretical and conceptual questions about the nature and utility of CPTSD, and echo the very questions that initially motivated our review. In our article, we concluded that the addition of CPTSD as a diagnosis to the *DSM-5* is untenable at this time, given that the field is still in its early stages of developing the fundamental science (such as an operational definition and a reliable assessment instrument) necessary for advancement of the diagnosis. Perhaps nowhere is absence of consensus better illustrated than in the diversity of viewpoints offered by the authors of each comment, each an esteemed expert in the trauma field. We believe that the areas in which these comments disagree—particularly with respect to the definition of CPTSD, its relationship to PTSD, and treatment implications—exemplify the concerns about construct validity we originally described. We expand on some of these conflicting viewpoints below.

Conflicting Definitions

A cornerstone rule of scientific measurement (and of scientific progress in general) is that reliability must precede validity; that is, we must be able to agree that some stable, replicable construct exists before we can show that the construct is what we believe it to be. When we speak of CPTSD, it is clear that its definition remains an open question and this precludes progress in delineating a core construct to assess. For example, Bryant (2012) suggested that it was problematic for us to include CPTSD, disorders of extreme stress not otherwise specified (DESNOS), complex trauma, and other related terms in our search of the literature, arguing that these are separate, discriminable constructs (i.e., that these diagnoses have discriminant validity with respect to CPTSD). In contrast, Lindauer (2012) argues that the definition of CPTSD is more expansive and includes concepts of developmental trauma disorder, and Herman (2012) draws on an exemplar from the DESNOS literature (i.e., Zlotnick et al., 1996) to support an assertion regarding the prevalence of CPTSD symptoms. We argue that conducting an evaluation of the CPTSD literature without including these variants would be a nearly impossible task, given that the only psychometric measure in use expressly for the construct (i.e., the Structured Interview for Disorders of Extreme Stress [SIDES]) was developed as a measure of DESNOS. If we as a field do not agree even on the boundaries of the relevant

After Patricia Resick, all authors are listed in reverse alphabetical order (the opposite order of Resick et al., 2012).

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literature, it is hard to understand how reliable diagnostic criteria for CPTSD could be developed at this time.

In the same vein, the authors of the comments also disagreed on the boundaries, and even the relevance, of the types of traumas that can be linked to CPTSD. Goodman (2012) and Lindauer (2012) both state that prolonged, interpersonal traumas during childhood constitute the presumed basis for the diagnosis, whereas Herman (2012) and Bryant (2012) maintain that adult experiences of surviving war and mass violence may also elicit the disorder. Although the authors may not have intended their respective lists to be exhaustive, the contrast across them does exemplify the lack of a shared conceptual understanding for the etiology of CPTSD.

Moreover, Bryant (2012) contends that trauma history should play no role in defining CPTSD. We would note that at present the putative causal link to trauma is the only thing uniting several clusters of disparate symptoms and the conceptual basis for Herman's (2012) assertion that a CPTSD diagnosis is more parsimonious than multiple diagnoses based on symptom profiles alone. To remove etiology from consideration would seem counter to any effort to establish CPTSD as distinct from its phenomenological relatives. At the very least, this debate must be resolved for substantive discussions of the clinical utility of CPTSD to proceed.

The Structure of CPTSD and its Relationship to PTSD

Another topic on which the authors of the comments disagreed was the likeliest structure for CPTSD, with respect to it being a category or a dimension and in the ways it relates to PTSD. Herman maintained that CPTSD is ontologically distinct from PTSD, meriting independent recognition. Bryant's comments, in contrast, posit that CPTSD is a complex variant of PTSD, such that the CPTSD diagnosis requires the presence of PTSD. Bryant's approach is therefore consistent with the notion that CPTSD has a subordinate relationship to PTSD. Specifically, this viewpoint suggests that CPTSD might be viewed either as a discrete subgroup completely within a larger PTSD category, or as aligning on the more severe end of a PTSD dimension, but that in either case CPTSD does not have an independent existence from PTSD. Goodman (2012) described the possibility that both PTSD and CPTSD fall on a higher-order trauma spectrum, suggesting shared dimensions that vary in severity but not type. That stance appears to be in conflict, however, with Goodman's comparison of the relationship between PTSD and CPTSD to one of seizures and bone fractures. That analogy implies that there exist operational criteria that can reliably and validly split the universe of simple and complex cases of posttraumatic maladjustment.

We believe this is an empirical issue to be resolved through research, but note that the uncertainty seems another reason to forbear the creation of a new *DSM* diagnosis at this time. Since *DSM-III*, this manual has historically assumed an almost exclusively categorical model for mental illnesses, and we agree with Goodman's comments that these diagnostic categories do not necessarily convey valid information about etiology or prognosis. In fact, history suggests that the *DSM*'s categorical system was born from an early need to establish interrater reliability, not from valid scientific knowledge, and that modern advances have disproven many of the presumptions that discrete entities underlie extant diagnoses (Hyman, 2010). Further, adherence to institutionalized categories that are not reflected in nature has in some cases impeded, not facilitated, scientific progress. For example, behavioral genetics and imaging studies obliged to group subjects by rigid, largely arbitrary operational criteria have frequently produced negative results because the grouping obscured natural heterogeneity in the underlying level of analysis (Beauchaine & Marsh, 2006; Hyman, 2010).

Fortunately, times (and institutions) are changing, and there is no longer the same need to assert the existence of a category for pathology to be recognized. Hypertension is an example from the medical field where use of a dimensional classification system has become well accepted (i.e., according to systolic and diastolic blood pressure). In the realm of mental health, the National Institute of Mental Health (NIMH; 2011) has called for the development of new approaches to classifying psychopathology based on basic underlying dimensions of behavior and functioning that "[cut] across disorders as traditionally defined." Furthermore, prolonged or complex grief is a construct that continues to be developed and refined through rigorous research though it has yet to be included in the *DSM*. Thus, Herman's (2012) assertion that a diagnosis must appear in the *DSM* before research will be funded is no longer as accurate as it may once have been. Given this cultural and institutional shift, the trauma field will ultimately derive more benefit by taking the time necessary to establish the structure of CPTSD empirically. Codification of any diagnosis in the *DSM* is not sufficient to attract research funding where the science is not yet sufficiently developed.

Future research notwithstanding, there are practical reasons why a CPTSD diagnosis would be clinically unwieldy. The features proposed for CPTSD are not like bone fractures (where one either has a simple or complex break) because one can have interpersonal dysfunction and emotion regulation difficulties (for example) that range from mild to severe and these features can exhibit within-subject variation over time. Research has yet to tell us where to draw the line between a little bit of CPTSD symptoms that do not warrant a separate diagnosis and enough of such symptoms that do. Further, there are other symptoms that are frequently comorbid with PTSD (i.e., substance abuse) that are not conceptualized as part of CPTSD. Thus, it is unclear how capturing some, but not all, PTSD comorbidity under the CPTSD term would aid in parsimony. Carving up posttraumatic reactions into two different diagnoses would not be a parsimonious or efficient approach to capturing the heterogeneity in psychopathological responses to traumatic life events; rather, it

would create confusion and redundancy and complicate assessment and treatment.

Treatment Implications

Herman (2012) suggests that empirically supported treatments for PTSD may be "inadequate, or possibly even harmful, for [the treatment of] CPTSD" and that phase-based therapies are necessary for the treatment of CPTSD. Our review of the literature suggests that there is not sufficient evidence at this time to support these conclusions. First, the question of whether exposure-based treatment is harmful has been well studied because this concern was raised in the PTSD literature as well (e.g., Pitman et al., 1991). In their review of this issue with respect to PTSD, Feeny, Hembree, and Zoellner (2003) concluded that exposure therapy is tolerable and useful for even severe PTSD and that although obstacles exist for exposure therapy, like any other treatment, "[s]uch obstacles may not necessitate abandoning the use of exposure in difficult cases, nor introducing new, nonvalidated treatment components" (p. 89). Exposure-based treatments have since been used successfully with diagnostically complicated cases in the community (Foa et al., 2005). Because this issue has not been examined specifically with CPTSD, however, we fully agree with Herman that more research is necessary using CPTSD rather than PTSD as an outcome measure (if this construct can be measured reliably and validly).

Second, to our knowledge, no study to date has evaluated patients with threshold-levels of CPTSD and compared the efficacy of exposure alone to phased-based treatments. Cloitre et al. (2010) treated individuals who had experienced childhood abuse and met criteria for (simple) PTSD. Complex PTSD was not part of the eligibility criteria for that study. We agree with Herman (2012) and Bryant (2012) that the Cloitre et al. article is an important study and we value its contribution to the literature. The study did not, however, directly compare skills training in affect and interpersonal regulation-first versus exposure-first treatment, and although the authors argue that more participants dropped out of the support/exposure condition than either of the other two, a careful examination of the data suggests that the majority of these participants dropped out during the support phase (i.e., nine participants versus four during the exposure phase). This is potentially because these participants were not receiving an active treatment; had they been, the dropout rates might look more similar. Further, the participants in this study did not receive the full package of empirically supported exposure therapy (they did not participate in in vivo exposures). Moreover, other studies (e.g., Chard, 2005) have evaluated a sample from the same population as did Cloitre et al. (childhood abuse survivors with PTSD) and demonstrated clinically significant decreases in symptoms after a nonphase-based treatment (Cognitive Processing Therapy). Thus, we believe that additional research is needed to investigate the hypotheses that exposure treatment is harmful for CPTSD and that additional or newly developed treatments are necessary.

Advocacy Through Science

Herman's (2012) and Lindauer's (2012) comments raise the question of whether a high standard of research development for inclusion in the *DSM* is incompatible with the important mission of advocacy for this population. We do not believe that advocacy for survivors of trauma is at odds with scientific evaluation of the best approach to conceptualize and treat this population. Science is a rigorous enterprise. Progress is made when hypotheses and conceptual models are critiqued, tested from multiple angles, and improved through the scientific method. Advocacy for this population through public policy, new treatment development, or even new diagnoses is considerably strengthened by the setting of high scientific standards. Without such standards, we risk harming or invalidating the very people we aim to serve. We can all agree that clinical care for trauma survivors should be informed by the best possible research, and it is our hope that this dialogue galvanizes further high quality work on this important topic.

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