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Scientific Evidence versus Outdated Beliefs: A Response to Brewin (2016)

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Abstract

We find Brewin’s (2016) critiques of the narratives, power, and coherence measures in Rubin et al., (2015) without merit; his suggestions for a “revised formulation” of coherence are contradicted by data readily available in the target article, but ignored. We place Brewin’s commentary in a historical context and shows that it reiterates views of trauma memory fragmentation that are unsupported by data. We evaluate an earlier review of fragmentation of trauma memories (Brewin, 2014), which Brewin uses to support his position in the commentary. We show that it is contradicted by more comprehensive reviews and fails to include several studies that met Brewin’s inclusion criteria but provided no support for his position, including three studies by the present authors. In short, the commentary’s position does not stand against scientific evidence; attempts to rescue it through arguments unsupported by data advance neither science nor clinical practice.
General Scientific Summary

Rubin et al. (2015) showed no evidence for trauma versus control memories being less coherent in PTSD versus non-PTSD participants. Brewin (2016) faults Rubin et al. for using “global coherence concerned with an entire text” and ignoring “local coherence, concerned with neighboring clauses in a text” and thus capturing ‘hot spots’. However, 21 of Rubin et al.’s 28 measures assessed local coherence by the Brewin’s definition. None showed Brewin’s proposed interaction. Moreover, previous work does not support the claim of ‘hot spots’ being more disorganized. Brewin’s revised formulations lack empirical support.
The commentary (Brewin, 2016) does not focus on the data presented in the target article by Rubin, Deffler, Ogle, Dowell, Graesser, and Beckham (Rubin et al. 2016) or even work by the particular authors involved in it. It is also used as a platform for a general critique on work by “Rubin, Berntsen, and their colleagues” (p. 00), only rarely specifying which work is disagreed with, how it is seen as being flawed, and which authors are involved. Because of this, we begin by describing the methods and results of the target article and show how they should have informed the commentary. We then offer an alternative data-based history in response to the commentary’s critique of ‘Rubin, Berntsen, and their colleagues’. To do this we argue with the commentary’s (p. 00) claim that according to “clinical theories,” trauma memories tend to be “disorganized and fragmented” in posttraumatic stress disorder (PTSD). We disagree with this claim and that the commentary speaks for most ‘clinical theories’ and thus most clinicians.

The target article and how it should have informed the commentary

The commentary critiques the study in the target article for not being adequate. The target article’s participants were 60 clinician-tested, trauma-exposed adults, half with and half without a PTSD diagnosis, equated for many factors other than PTSD. Each participant orally provided to a staff member memories of their traumas and most positive and important events. These were analyzed using 28 different measures of memory coherence from different fields, including clinical psychology, linguistics, education, and narrative theory. The comprehensiveness of the design is unprecedented in PTSD research on memory coherence.

To demonstrate incoherence in trauma memories in PTSD, it is not enough to show all memories are less coherent in PTSD; such a general memory deficit could be a risk factor or sequelae of PTSD. It is also not enough to show trauma memories are less coherent in all people; that would not be a property of PTSD at all. Rather, there needs to be an interaction between the coherence of trauma versus non-trauma memories and PTSD versus control participants with trauma memories being especially incoherent in people with PTSD. The target article tested this hypothesis. Across 28 different coherence measures, there was no evidence for this interaction. In response to these findings, the commentary critiques the target’s narratives, statistical power, and measures of coherence. We address these critiques individually.

Narratives. The commentary considers the target’s narratives inadequate, implying that they were not collected in a clinically sensitive way through careful interviewing and thus are likely to underestimate ‘hot spots’ and fragmentation (p. 00). However, as noted in the target article, the
narratives were provided orally in a clinical setting in a Veterans Administration Medical Center. Before each participant was cued for narratives, s/he and a trained clinical staff member had time to learn about and get comfortable with each other in sessions in which the participant was consented, completed medication and treatment history, standard trauma inventory, depression, dissociation and other instruments, and structured clinical interviews for PTSD and other disorders. The narratives produced were long and elaborate. When transcribed the trauma, positive, and important oral narratives averaged 348, 436, and 331 words long, respectively, and contained intimate details and apparent ‘hot spots.’ The following categories accounted for 80% of the traumas: combat, adult violence, adult physical or sexual abuse, and adult physical or sexual assault. It is hard for us to imagine a better sample for the target article’s purposes.

**Power Analyses.** The commentary states that Rubin (2011) did not have enough power to detect the interaction between the coherence of trauma versus non-trauma memories and PTSD versus control participants. We do not see this as a power problem, but as a symptom of there being an extremely small effect size, one indistinguishable from no effect. In the target article, the last column of Table 3 shows the number of participants needed to obtain a $p < .05$ interaction effect 80% of the time for each of the 28 measures of coherence. Nine of the measures would need 1,000 or more participants; the median number needed is 642. The measure with the largest effect size would need 132 participants, and the trend of that interaction is opposite to what the commentary predicts. Thus, the most parsimonious conclusion is that there is no measureable effect.

Moreover, similar results occur for other studies in the literature, with most showing no interaction. Taken as a whole, the literature calls into question whether the few studies that do show interactions are Type I errors. Such errors are most likely to occur where there is a desired effect that multiple studies try to obtain, the effect size is small, and the studies obtaining positive results are underpowered for the expected effect size.

**Measures of coherence.** The commentary proposes that fragmentation occurs more in ‘hot spots’ and therefore needs studies of local coherence. This is a surprising claim, given that the only study directly examining coherence in ‘hot spots’ (Jelinek et al. 2010, Table 2) found that the greatest difference was in more organized thoughts in hot spots than the surrounding trauma narratives. Nonetheless, the commentary faults the target article for focusing on measures that involve “global coherence concerned with an entire text” and ignoring “local coherence, concerned with neighboring clauses in a text”. Twenty-one of the target’s 28 measures were assessed at the local level of pairs of “neighboring clauses” or even smaller units and thus measured local
coherence by the commentary’s definition. Only 7 measured global coherence: the 3 participant ratings, 2 of the ratings of global coherence (narrator, emotion), and 2 of the Coh-Metrix measures (deep cohesion, latent semantic analysis).

As is evident in Tables 2 and 3 in the target article, the commentary’s proposal that local measures are associated with more fragmentation is also unsupported. Six measures of the trauma memories were less coherent and 6 were more coherent than control memories when averaged across both PTSD and control participants. In both cases, 2 of the measures were global and 4 were local. Thus, the results needed to test the local versus global hypothesis are already in the target article, they provide no evidence that local measures will evince more incoherence, and most seriously they were ignored by the commentary.

In summary, we find the commentary’s critiques of the target article’s narratives, power, and coherence measure without merit; its suggestion for a “revised formulation” is contradicted by data readily available in the target article but ignored. The reader may be left wondering what is at stake and why the issue of fragmented memories can spur such reactions. In the following we first place the commentary in a historical context and second evaluate Brewin’s (2014) review of fragmentation of trauma memories reiterated in the commentary, a review that is counter to both the target article and comprehensive reviews of the same issue.

The larger picture

A history of claims of fragmentation in PTSD Memories. The original conception of the PTSD diagnosis in the DSM III in 1980 as well as subsequent revisions were strongly influenced by Horowitz’s (1976) model of Stress Response Syndromes (see, Berntsen & Rubin, 2014; Berntsen, Rubin, & Bohni, 2008). This model has two main tenets concerning the role of memory. One is that voluntary (strategic and controlled) remembering of the event is impaired. The other is that the memory of the stressful event repeats itself in an involuntary and uncontrollable fashion. Both characteristics are claimed to be caused by faulty encoding of the traumatic event and defense mechanisms. Impaired voluntary recall reflects deficient encoding due to defense mechanisms and a poor cognitive match between the traumatic event and preexisting schemata. Enhanced involuntary remembering of trauma reflects the memory of the trauma being in a special memory system from which it is repeatedly activated instead of having a normal encoding and integration into long-term memory.
Although these two tenets were formulated more than 40 years ago, and have little empirical support (e.g., Berntsen & Rubin, 2014), some contemporary models of trauma memory in PTSD still share them. Notably, in Brewin’s dual representation theory a shallow encoding of the trauma, focusing on sensory, perceptual, and emotional aspects of the event at the cost of deeper conceptual processing and contextual integration causes poor intentional recall, on the one hand, and vivid involuntary recollection, on the other (Brewin, Dalgleish, & Joseph, 1996; Brewin, Gregory, Lipton, & Burgess, 2010).

In a series of studies ‘Rubin, Berntsen and colleagues’ examined the thesis that the accessibility of voluntary trauma memory is reduced while the accessibility of involuntary memory is enhanced in PTSD. First, these studies have shown that counter to the commentary’s claims, involuntary and voluntary recall follow the same pattern in response to stressful and traumatic events, in that both types of memories are enhanced in response to trauma and emotional stress (e.g., Berntsen & Rubin, 2014). Second, Berntsen and Rubin (2006) introduced the Centrality of Event Scale (CES), which measures the extent to which a traumatic event is perceived as central to the person’s life story and identity and as a reference point for thinking about the past and future. Higher scores on the CES therefore indicate high voluntary memory accessibility and integration. In contrast to the view that trauma memories are poorly integrated and hard to access intentionally, the CES score for trauma memory correlates positively (not negatively) with the level of PTSD symptoms, in college students, combat veterans, women with histories of childhood sexual abuse, community-dwelling adults with PTSD, and other populations (see Berntsen & Rubin, 2014, for a review).

The commentary (pp. 00) dismisses the significance of these findings by stating that “in general trauma theorists have always emphasized the great significance traumatic events have for identity” and that the rationale for the centrality measure is “simply a rewording of what the majority of clinical theorists have always suggested”. We disagree. First, the CES measures the extent to which the traumatic memory is integrated into the life story and identity, and the extent to which it serves as a reference point for inferences about other autobiographical events (Berntsen & Rubin, 2006). Second, the CES is robustly and positively correlated with other measures of memory accessibility, such as vividness and rehearsal frequency. The positive correlation between PTSD symptoms and the CES therefore is opposite to Brewin’s claim that trauma memories are hard to access intentionally.
During the repressed memory controversy, many clinical psychologists claimed that traumatic events were poorly encoded, maybe repressed, and thus hard or even impossible to remember intentionally, and that the impaired intentional access is reflected in poor integration of the traumatic memory into the life story of the person as well as internally incoherent and fragmented memories. The commentary (pp. 00) invigorates some of the old repressed memory disputes by claiming that “recovered memories are possible” and that “memory impairment is most strongly associated with PTSD”.

**Reviews of memory fragmentation.** The commentary claims that “impairment to voluntary trauma memory in PTSD . . . was confirmed in a series of reviews.” (p.00). In order to evaluate this claim, we examined the existing two reviews of the literature on memory fragmentation. O’Kearney and Perrott (2006) reviewed nineteen studies addressing fragmentation and related characteristics of trauma narratives and concluded that “the data addressing the prediction that PTSD trauma narratives are fragmented are inconclusive” (p. 90). Crespo and Fernández-Lansac (2016) examined 22 studies published since the 2006 review and arrived at the same conclusion (p. 151).

In contrast, the commentary introduces Brewin’s (2014, p. 88) review of nine studies on memory fragmentation of which seven were claimed to show that “fragmentation or disorganization” is supported by “a considerable amount of evidence”. We found five additional studies that met the inclusion criteria, predate the Brewin (2014) review, and were not included. None supported the claim. This leaves half of the studies offering no support in line with the ‘inconclusive’ results of the two general reviews. Even though Brewin (2014) argues with ‘Rubin, Berntsen, and colleagues’, he did not include three papers from that group: Rubin (2011), Rubin, Boals, and Berntsen (2008), and Rubin, Dennis, and Beckham (2011). A fourth omitted paper, Hagenaars, van Minnen, and Hoogduin (2009) found no difference between a PTSD and a panic disorder control group (a disorder not known for incoherence) on disorganization. A fifth, Jelinek et al. (2010) found no difference between PTSD and control groups on four measures of disorganization and their composite. Furthermore, as noted in considerably more detail in the target article, the nine studies included in Brewin (2014) provide little support for incoherence, even though the commentary claims they do, so we would give the fourteen total studies much less than an even split on supporting the commentary’s claim. Readers are directed to the target article to make their own evaluation. Thus, the two general reviews, the review offered in the target article,
and our more complete review of our literature all contradict Brewin’s (2014) review and his interpretations in the commentary.

Everyone occasionally misses citations and theoretical implications. But here it seems to fit a pattern of selective omissions. First, the commentary does not acknowledge information in the target article that challenges its critiques and proposed solutions concerning its narratives, power analyses, and measures of coherence. Second, the Brewin (2014) review that was used prominently in the commentary omits articles that meet its inclusion criteria, including three by the authors the commentary critiques. Third, and in many ways the most troubling, this pattern continues in other attempts to save problematic theoretical views (see, Brewin & Andrews, 2016, and the replies by Lindsay & Hyman, 2016, and Nash, Wade, Garry, Loftus, & Ost, in press). Theoretical views should be revised in the face of scientific evidence, not saved through arguments unsupported by data.
References


Brewin (2016, p. 00), hereafter the commentary


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