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Letter to the Editor

We read with interest the article by Gerger and colleagues describing the results of a meta-analysis entitled ‘Integrating fragmented evidence by network meta-analysis: relative effectiveness of psychological interventions for adults with post-traumatic stress disorder’ (Gerger et al. 2014). This article attempted to summarize the available evidence on the effectiveness of psychological interventions for patients with post-traumatic stress disorder (PTSD). The authors included randomized trials in adults with full or subclinical PTSD that compared specific treatments head-to-head to wait-list or other control interventions.

The value of a meta-analysis depends heavily on the scope and quality of the included studies and the methodical, systematic and consistent way the search has been conducted, the studies selected, the analyses carried out and the way the results are interpreted. To this end, the study of Gerger et al. suffers from some limitations that hamper a reliable interpretation of their findings. For example, because of the considerable between-trial heterogeneity, the authors could not identify any intervention superior to other specific psychological interventions. This is not surprising given that the authors used broad eligibility criteria and included experimental studies ranging from non-clinical student samples lacking a formal PTSD diagnosis (but reporting a ‘past stressful experience’; Lytle et al. 2002) to studies with refugees suffering from complex conditions due to exposure to multiple and severe traumatic events (Paunovic & Öst, 2001).

This, and the inclusion of older studies that applied preliminary versions of the therapeutic procedures, such as eye movement desensitization (EMD) rather than eye movement desensitization and reprocessing (EMDR) therapy (Lytle et al. 2002), made it likely that patients’ response to the therapies varied significantly, and therefore obfuscated the interpretation of the analytic results.

It is unclear why the authors made a division into ‘small’ and ‘large-sized trials’ and why they used 60 or more patients per trial arm as a criterion for ‘large-sized trials’. Accordingly, five cognitive behavioral therapy (CBT) trials, three exposure therapy trials and one traumatic incident reduction therapy trial were considered ‘large-sized trials’ and subsequently included in the analyses, but for instance the Power et al. (2002) study (comparing prolonged exposure and EMDR therapy with wait-list) with 105 patients was not. It is unclear how this arbitrary criterion for trial size and the selection of studies influenced the results, showing that none of the three specific psychological interventions were superior to supportive therapies. However, the statement that there is ‘most robust evidence for cognitive behavioral and exposure therapies’ (p. 1) and labeling EMDR therapy ‘promising’ (p. 11), although the authors found a consistent (non-significant) trend of higher effect sizes for EMDR therapy after evaluating more than 20 trials (Table 1 and Fig. 2), is difficult to understand and lacks scientific merit. This is underscored by the fact that prior meta-analyses cited by the authors (e.g. Bisson & Andrew, 2007; Powers et al. 2010) have reported that CBT and EMDR therapy are equally effective and empirically supported. Nothing in these data seems to support the current authors’ puzzling interpretation.

Another important reason limiting the informative value of Gerger et al.’s meta-analysis is that, although just recently published, the literature search was carried out in 2010. Accordingly, the authors missed a number of studies that were conducted since then, such as the ‘large-sized’ comparison study of brief eclectic psychotherapy with EMDR therapy (n = 140; Nijdam et al. 2012). Therefore, the contribution of Gerger et al.’s meta-analysis to decision making in clinical practice about what intervention to use to date for patients with PTSD is marginal at best.

Declaration of Interest
None.

References


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A rejoinder from Gerger and colleagues

Systematic syntheses of individual trials have been described as the ‘gold standard’ for the evaluation of interventions (Sackett et al. 1996, p. 72). As pointed out by de Jonghe et al., systematic reviews and meta-analyses play an increasing role in the decision making of clinicians, researchers and policy makers. However, meta-analyses are, of course, not immune from bias. In their letter, de Jonghe et al. criticize our recent network meta-analysis of psychological interventions for post-traumatic stress disorder (PTSD) (Gerger et al. 2014b) for severe methodological shortcomings and question the relevance of our study. Many of the issues raised by de Jonghe et al. have already been considered in our paper. However, we would like to use the opportunity of this rejoinder to further clarify some issues.

de Jonghe et al. seem to be unsatisfied with our finding of equivalent effectiveness of specific psychological interventions. They argue that we did not identify superiority of any intervention ‘due to the considerable between-trial heterogeneity’, which they attribute to our inclusion of heterogeneous samples. The first part of this statement lacks scientific evidence, however. As described in the Introduction of our paper, the large majority of meta-analyses in the field of PTSD interventions conclude equivalent effectiveness of specific interventions (e.g. Bisson & Andrew, 2007; Watts et al. 2013) and none of the interventions has consistently been shown to outperform the others; not even in meta-analyses with less between-trial heterogeneity (e.g. Benish et al. 2008). However, as stated in our Limitations we admit that we did not control for possibly moderating effects of clinical patient characteristics, which have previously been shown to affect relative effect size estimates (Gerger et al. 2014a). We have, however, conducted a moderator analysis including the status of a full PTSD diagnosis (versus subclinical PTSD symptoms) to explain heterogeneity, which de Jonghe and colleagues may have overseen in our paper.

A further point of critique is that we distinguished trials with small to moderate samples from trials with larger samples. We elaborated extensively on the rationale for the cut-offs chosen in our analyses in the Method. We are therefore not clear about the actual critique here. Our cut-offs conform to those proposed by Schnurr (2007), which also rely on power considerations. Given the vast literature on the risk of bias that is typically associated with small samples (Egger et al. 1997; Sterne et al. 2000; Cuijpers et al. 2010a, b; Nüesch et al. 2010; Barth et al. 2013; Watts et al. 2013), we do not believe that the authors aimed at fundamentally questioning the relevance of sample size as a moderator variable.

Furthermore, our conclusion of eye movement desensitization and reprocessing (EMDR) as ‘promising’, which is not negative in principle, seems to contradict de Jonghe et al.’s expectations. The authors argue that, in the presence of more than 20 trials on EMDR, our conclusion ‘lacks scientific merit’. However, from our point of view and based on the extensive empirical literature on small sample bias, we feel very confident in repeating the conclusion regarding the lack of robust evidence for EMDR. We were unable to identify a single trial on the efficacy of EMDR that was adequately sized to detect relative intervention effects of moderate to small size. We therefore strongly argue for the need for collaborative research projects (such as the Social Phobia Psychotherapy Network by Leichsenring et al. 2009) that aim at maximizing the number of patients included in a comparative trial and at minimizing the potential for bias from researchers’ preferences (the so-called allegiance bias; see Munder et al. 2011, 2012) at the same time. Our evaluation of the evidence for EMDR also mirrors the