Psychological Medicine (2014), **44**, 3357–3360.

Correspondence

Psychological Medicine, **44** (2014).
doi:10.1017/S0033291714002025
First published online 29 August 2014

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

Bisson JI, Andrew M (2007). Psychological treatment of post-traumatic stress disorder (PTSD). *Cochrane Database of Systematic Reviews*. Issue 3, Art. No.: CD003388.

Gerger H, Munder T, Gemperli A, Nüesch E, Trelle S, Jüni P, Barth J (2014). Integrating fragmented evidence by network meta-analysis: relative effectiveness of psychological interventions for adults with post-traumatic stress disorder. *Psychological Medicine*. Published online: 16 April 2014. doi:10.1017/S0033291714000853.

Lytle RA, Hazlett-Stevens H, Borkovec TD (2002). Efficacy of eye movement desensitization in the treatment of cognitive intrusions related to a past stressful event. *Journal of Anxiety Disorders* **16**, 273–288.

Nijdam MJ, Gersons BPR, Reitsma JB, de Jongh A, Olff M (2012). Brief eclectic psychotherapy v. eye movement desensitisation and reprocessing therapy in the treatment of post-traumatic stress disorder: randomised controlled trial. *British Journal of Psychiatry* **200**, 224–231.

Paunovic N, Öst LG (2001). Cognitive-behavior therapy vs exposure therapy in the treatment of PTSD in refugees. *Behaviour Research and Therapy* **39**, 1183–1197.

Power KG, McGoldrick T, Brown K, Buchanan R, Sharp D, Swanson V, Karatzias T (2002). A controlled comparison of eye movement desensitization and reprocessing versus exposure plus cognitive restructuring, versus waiting list in the treatment of post-traumatic stress disorder. *Clinical Psychology and Psychotherapy* **9**, 299–318.

Powers MB, Halpern JM, Ferenschak MP, Gillihan SJ, Foa EB (2010). A meta-analytic review of prolonged exposure for posttraumatic stress disorder. *Clinical Psychology Review* **30**, 635–641.

A. DE JONGH[1,2], C. DE ROOS[3] AND I. A. E. BICANIC[4]
[1] *Department of Behavioral Sciences, ACTA, University of Amsterdam and VU University, The Netherlands*
[2] *School of Health Sciences, Salford University, Manchester, UK*
[3] *Psychotrauma Center for Children and Youth, MHO Rivierduinen, Leiden, The Netherlands*
[4] *National Psychotrauma Center for Children and Youth, University Medical Center Utrecht, The Netherlands*

(Email: a.de.jongh@acta.nl)

Psychological Medicine, **44** (2014).
doi:10.1017/S0033291714002037
First published online 5 September 2014

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.* 2014*b*) 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.* 2014*a*). 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.* 2010*a*, *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