Reviewer's report

Title: Standard and advanced CBT techniques in a randomized controlled psychotherapy trial of PTSD in motor vehicle accident survivors (ISRCTN66456536)

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Reviewer: Anke Ehlers

Reviewer's report:

General

The paper describes an interesting RCT comparing a CBT programme for PTSD and subsyndromal PTSD following motor vehicle accidents with a wait list condition. The study has several important strengths, for example assessments of major depressive episodes before and after treatment, treatment fidelity checks, and interrater reliability checks. It represents an interesting and worthwhile contribution to the literature, and the paper is generally well written.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. The labels “advanced” and “standard” CBT techniques are misleading. I would recommend that the authors change this terminology throughout the text and de-emphasize this contrast. This is especially important in the title, which implies contrasting procedures (which was not the design of the study). Both terms are misleading. First, there is not a single “standard CBT” for PTSD. Rather, there are several CBT programmes that have been shown to be effective. The authors use the term “standard CBT” to describe the Blanchard and Hickling programme. It would be more precise to state that they added cognitive procedures and imaginal reliving to the Blanchard and Hickling programme.

Second, the term “advanced” procedures is unclear. The authors state that they included new procedures such as cognitive restructuring of guilt and anger (abstract and page 10). This is misleading as these are standard procedures in other CBT programmes for PTSD (the Resick and Ehlers/Clark treatment programmes, see the Resick and Schnicke, 1992, manual and the German manual by Ehlers, 1999, cited by the authors). It appears that what the authors mean is that they added these procedures to the Blanchard and Hickling programme. On page 10, the authors contrast, probably unintentionally, cognitions attributed to the Ehlers and Clark model (second, “…” with those related to guilt and anger (third, “…”). This is misleading as restructuring cognitions leading to guilt and anger is a primary target in the Ehlers/Clark treatment programme (see the German manual by Ehlers, 1999, p. 57 “…” 65). I therefore disagree with the authors when they state that the inclusion of these cognitive procedures into their protocol distinguishes this study from previous RCTs. At least 4 RCTs, three of which focused either exclusively or mainly on MVA, included such techniques. They should be cited, and the differences in procedures should be clarified. In essence, what the authors probably mean is that they added procedures from these treatments to Blanchard and Hickling’s protocol. It appears that the posttraumatic growth interventions were the main difference to previously evaluated CBT programmes. It would thus be of interest to describe these new procedures in greater detail.


2. The authors claim that their effect sizes are larger than those achieved in previous studies (e.g., more substantial symptom reduction...). This is incorrect and the conclusion needs to be changed. The data definitely offer further support for the effectiveness of CBT interventions in PTSD, but any claims about increases in efficacy are unsubstantiated.

The most relevant comparison is the Blanchard and Hickling trial, which is most similar to the present study as it also included subthreshold PTSD (other trials excluded such mild cases). In that trial, CAPS scores for completers dropped from 68.2 to 23.7, pre-post effect size \( d = 1.81 \) (compared to 47.6 to 18.3 in the present study, \( d = 1.55 \)).

Thus, the present paper does not show better treatment effects than the Blanchard/Hickling trial, patients just started at much lower symptom levels and thus ended treatment with somewhat lower levels. Similarly, the PTSD free rate in patients who met full symptom criteria in the Blanchard et al trial was 71%, compared to 58% in the present study for subthreshold PTSD the numbers are 83% versus 78 % PTSD free.

The main difference appears to be that the present study included more subthreshold PTSD cases, which led to low CAPS scores at the end of treatment.

Similarly, the effects reported in this paper do not exceed those in other recent RCTs of CBT programmes for PTSD, including those that focused largely or exclusively on MVA. For example, the effect sizes in the Ehlers et al. RCTs for intent-to-treat analyses for patients who met full diagnostic criteria are larger (2 and above), and the Resick et al. and Bryant et al. RCTs reported larger completer-effect sizes (2 and above). This needs to be adequately acknowledged.

3. The treatment and wait list groups differed in sex, with more men in the wait group. This is reason for concern as there are some suggestions in the literature that women respond better to treatment. Thus, one would want to see some efforts to control for the possibly confounding sex differences in the data analysis, e.g., by using sex as a covariate or by reporting whether there was still a significant interaction between treatment condition and time when only women were considered.

4. Several limitations should be acknowledged in the discussion. The large proportion of patients with subthreshold PTSD is such a limitation. Furthermore, the random allocation procedure appears to have restricted random assignment somewhat (the second person of a pair will always receive the other condition).

5. The effect sizes reported in the paper are difficult to compare with other studies in the literature. It is unclear how exactly they were calculated - it appears that they were taken from the interaction effects in the ANOVAs. Controlled (wait vs. CBT at post treat) and pre-post treatment effect sizes would be easier to compare.

6. There is one further RCT that should be cited:


What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions
Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests