Reviewer’s report

Title: The effect of CBT and its modifications for relapse prevention in major depressive disorder: A systematic review and meta-analysis

Version: 1 Date: 27 Sep 2017

Reviewer: Lena Jelinek

Reviewer's report:

The authors present an interesting meta-analysis on relapse prevention in MDD. They have generally been responsive to the previous feedback and the quality of the paper has certainly improved.

However, some issues remain:

* Introduction: When describing CBT (p.3, l. 42-49) the information is redundant and may be condensed (statements on "problems in cognitions" and "automatic thought"

* Introduction: I am still unsure for what reasons < versus >= 3 previous MD episodes was chosen as criterion for number of previous episodes.

* Introduction: "review" vs. "meta-analysis" (previous round of review): on page 4 in the last paragraph of the introduction, it is only referred to "review", not "meta-analysis".

* In methods, the authors refer to a "detailed systematic search strategy", that was added to the appendix. Nevertheless, I would love to read the key words used for research in the method section.

* In methods, the authors point out "full or partial remission" as inclusion criteria. They refer to "strict diagnostic definition" as used in DSM and ICD. However, a short explanation in manuscript should be given. This might be more comprehensive for the reader to understand how partial and full remission was defined.

* Control condition: the handling of control condition is unclear: In the methods it is stated that "any comparator intervention" is included, but later the study by Stangier et al. is excluded (p.6) and "control arm" is defined as "treatment as usual" (p.6). I do not understand what kind of treatment this is referring to and this seems to be in contrast to the introduction. Furthermore, description of control conditions, presented in table 1, is inconsistent/missing. Control interventions are only explanations for a few studies (e.g. Kuyken, 2008 (m-ADM); Stangier, 2013 (psychoeducation)).
* In the discussion, it is stated that MBCT is "less helpful" in patients with 2 or less episodes. However, as far as I understand, analyses were insignificant.

* Methods: In their response to review the authors state that inter-rater reliability was not calculated. However, this would be possible as two independent rated assessed the same material and would be important in order to evaluate the results.

* The authors mentioned that "discontinuation researches" were excluded. Please add how many studies this were affected and also please add specific reasons for exclusion, or refer to figure 1.

* In part of Data Extraction and Quality Assessment the authors point out "proportion of ADM". This abbreviation needs to be explained in the text or added to the list of abbreviations.

* The structure of the discussion could be improved.

* General: "modifications of CBT": I think the term is misleading, as actually MBCT is the only CBT modification investigated.

* The forest plots are still hard to follow and need to be improved (see previous review), e.g. clear titles, improve graphic solution.

* Comprehension of the manuscript has certainly improved, but I would still recommend using a proof reading service as it is still hard to follow in some places (e.g., sentence on page 4: "In addition, we evaluated the influence of number of previous episodes by separating trials only included patients with 3 or more than 3 previous episodes from trials included other kinds of patients" or "CBT is one of the most frequently used psychosocial treatment for mental disorders, which targets at changing patients on the development of tactic to cope problems in cognitions (such as belief and thought) and behaviours"). Please also revised the ms for the use of the English language in general, e.g. "might failed" (p.4, l.1), "had paid" (p. 4, l. 25), "reported their study" (p.4, l. 26), "contract" (p. 7).

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

No

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes
Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I recommend additional statistical review

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