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: 0 Date: 12 Jul 2017

Reviewer: Lena Jelinek

Reviewer's report:

Thank you for inviting me review the present manuscript. In this paper, the authors aim to review the effect of CBT (as well as CT and MBCT) on relapse prevention in depression in comparison to control interventions. The topic of the paper (relapse prevention) is interesting and of high clinical relevance.

However, there were a number of limitations of the research and the manuscript that dampened my enthusiasm of publishing the paper in its current version. Especially the methodology and results section should be revised substantially. Generally, the paper also difficult to follow in many places.

My specific comments are as follows-- and I hope that the authors receive my critical comments in the constructive spirit with which they are intended:

* Content of the paper: the title of the paper states that the authors did a systematic review and a meta-analysis. However, this differentiation does not consistently reappear in the rest of the paper and it is unclear what is "review" and what "meta-analysis" (20 vs 18 studies as described in the beginning of the results section). As one example, there is only one subtitle in the results section on "meta-analytic results" and none for the systematic review.

* Introduction: While CBT is described in some detail in the introduction, MBCT is not introduced. I would recommend to also describe MBCT in the introduction (e.g., use the sentences now in the discussion section).

* Methods: The methodology is not clear. What were the "strict diagnostic criteria" mentioned in the selection criteria? What search term/keywords were used? Most importantly, what was the theoretical basis for comparing subjects with >= 3 previous episodes (could also be included in the introduction). Was the study preregistered? Was inter-rater reliability calculated?
* It is not clear which of the studies included had relapse as a primary and which as a secondary outcome measure. This information should be included and discussed, also in light of a publication bias.

* Results: It is not clear why 33 of the 53 initially retrieved articles were excluded. This information should be added to figure 1.

* The authors claim to have used funnel plots to test for publication bias. I recommend including them in the manuscript.

* The forest plots are hard to follow, e.g., legend/title is missing; Sample sizes of studies are missing; If CIs are reported, there is no need in additional z-statistics in my opinion; Subgroup analysis if there is only one study do not make sense to me; Not all headings are precise and do not match the subtitles of the manuscript (e.g. "all", "3 or 3 more previous episodes"); The figure is full of statistical values, which are repetitive to the manuscript. You may want to reduce the detail of statistical values either in the text or in the figure; Some studies are included twice (e.g. Bondolfi, 2010 and Godfrin, 2010); Subgroups should be formed in a way that the exact same study results are not included in more than one subgroup.

* The discussion includes detailed comparisons within different therapeutic approaches. Those detailed qualitatively analysis may be better integrated in the results section.

* Comprehension of the manuscript is not always easy and needs to be improved. I am not a native speaker myself and certainly know how difficult it is to write in another language. Still, I think the manuscript should be checked by a native speaker (e.g. for the correct use of tenses). Moreover, consistency could be improved (e.g., regarding statistical values, sometimes it is written "95% CI: 0.37-0.86" and at other points "95%CI, 0.87-1.39" to give just one example) and many of the mistakes do not need a native speaker (e.g., opening or closing parenthesis, spaces, a major typo right at the beginning of results sections - "16 trials were depicted" it should be 18).

**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.

No

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

Quality of written English
Please indicate the quality of language in the manuscript:

Not suitable for publication unless extensively edited

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