**Author’s response to reviews**

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

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**Author’s response to reviews:**

Dear Editors and Reviewers:

Thank you for your letter of November 28, 2017 and for suggestions concerning our manuscript. We are very pleased to learn that our manuscript is acceptable for publication in BMC Psychiatry with minor revision. We quite appreciate your favorite consideration and constructive comments. Those comments were all valuable and very helpful for revising and improving our paper. Now we have revised the paper point-by-point exactly according to those comments of reviewers.

1. **To Shadi Beshai (Reviewer 1):**
   1) **Comment 1:** I invite the authors to read the paper carefully to make sure grammar is fine, and that sentences are clear and statements presented throughout are accurate.

   **Response:** Thank you for pointing these errors out. Based on your constructive suggestions, we carefully revised the manuscript to minimize grammatical errors, make sure that the sentences and statements become more accurate. I hope it has reached your standard.

   2) **Comment 2:** Abstract: Under the results "MBCT was significantly more efficacious than control only in patients with 3 or more previous depressive episodes and maintenance antidepressant medication" The way the sentence is structured makes this sentence slightly difficult to understand. Over which condition exactly was MBCT more significantly more efficacious?
Response: Thank you for your advice. We are very sorry for our incorrect writing. This sentence has been revised in the Abstract section.

3) Comment 3: Conclusion "the effects of MBCT was under the influence of number of prior.." should be "was moderated by the number of prior"

Response: Thank you for your outstanding advice. We have revised this sentence based on your advice, which make it become clearer and easier to understand.

4) Comment 4: Introduction: - Page 3, please provide a reference for "MBCT" the very first time it is mentioned in lines 50-51. (e.g., Segal et al. 2002/2012).

- Page 4, line 2 "MBCT might failed to reduce" should be "might fail to reduce".
- Page 4, lines 22 "another category of depression might be associated with rumination, namely patients with higher number of previous episodes" did you mean "another category of depression might be associated with heightened rumination, namely among patients with a higher number of previous episodes"

Response: - We are very sorry for our negligence and the reference was provided in the revised manuscript.
- Thank you for your advice for this low-level error. This mistake and similar mistakes in the manuscript have been revised.
- Yes, your explanation is correct. We have modified this sentence based on your statement. Thank you so much for your advice!

5) Comment 5: Results: Page 6, line 45. Try not to start the sentence with a number written numerically (e.g., "180"). Instead try "Patients (n=180) with three or more previous…". Page 6, line 51-52 "because the control arm was manualized psychoeducation, was did not meet our" should be "did not meet our".

Response: Thank you for your advice for these two low-level errors. This mistake and similar mistakes in the manuscript have been revised.

6) Comment 6: Discussion: - Page 10, lines 39-40 "In other words, MBCT was considered to be less helpful than control for MDD patients with 2 or less previous episodes" Is this what was found? It sounds what you found was that MBCT was not significantly different than the control condition among people with 2 or less previous episodes (at least this is judging from your results section, as well as the sentence that just proceeds this one).

- Page 10 lines 45-47 "however, for MBCT, the only one comparison from one trial did not show a positive effect at two years follow-up". That was never the intention of the authors (Kuyken at al. 2015). This was a non-inferiority trial, meaning they were hoping to show that
MBCT and m-ADM were not significantly different from each other after 2 years in terms of relapse/recurrence. That is, MBCT can be thought of as an alternative to m-ADM.

Response: -Thank you for your carefulness and pointing this out. I found that this statement was inappropriate. Therefore, this sentence and corresponding statement in Result section have been deleted.

- Thank you for your suggestion and this sentence has been revised based on your advice.

2. To Lena Jelinek (Reviewer 3):

1) Comment 1: 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).- I am still unsure for what reasons < versus >= 3 previous MD episodes was chosen as criterion for number of previous episodes. - "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".

Response: Thank you for pointing these out. - According to your comment, the too-long description of CBT were significantly shortened. - That's a good point. Actually, there was no explicit conclusion or statement for whether the effects of CBT and its modifications was moderated by the number of previous episodes. There were only a few conclusions from some researches as stated in the manuscript (MBCT might not reduce the risk of developing a new relapse of depression in patients with 2 previous episodes). One purpose of our review was to evaluate whether these statements were appropriate. There was just a simple exploration. So we used the criterion of < versus >= 3 previous MD episodes. □ Thank you, that's a good point too. Based on your suggestion, I have revised it as “review and meta-analysis” on page 4, line36 in the last paragraph of the introduction.

2) Comment 2: 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. -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.

Response: - Thank you for this constructive and helpful comment. We actually neglected to add key words in the manuscript. Based on your suggestion, I have added the key words which were used for research in the method section, Page 5, Line 31. - Thank you, that's a good point. I have added a short explanation of the partial and full remission in manuscript on method section, Page 3-4.
3) Comment 3: 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)).

Response: Thank you for pointing this out. The description of control condition is unclear. I have added the explanation of control condition on method section, Page 5, Line 11 and revised corresponding details on table 1. I hope it would meet with your requirement.

4) Comment 4: 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.

Response: Thank you for your carefulness and pointing this out. I found that this statement was inappropriate. Therefore, this sentence and corresponding statement in Result section have been deleted.

5) Comment 5: 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.

Response: Thank you for your advice. In order to evaluate the results, I know that inter-rater reliability is very important. But because my results could not be calculated as numeric form and the score of inter-rater reliability for the procedure of our study could not be calculated. So I failed to calculate the inter-rater reliability. In order to reduce random error which might generate from literature selection, data extraction, or quality assessment, we added a third investigator to check and coordinate the disagreements of two investigators.

6) Comment 6: 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.

Response: Because results of discontinuation researches are incomplete, I thought that if we meet discontinuation researches, we should exclude them. But in our selection of eligible researches, we did not meet discontinuation researches. So we only listed this in exclusion criteria.

7) Comment 7: 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.

Response: Thank you for your advice. I have added the abbreviation of ADM on method section, Page 5, Line 14 and added it to the list of abbreviations in the last part of the text.
8) Comment 8: The structure of the discussion could be improved.
Response: Thank you for your comment. I have merged paragraph 3 and paragraph 4 of the discussion section in revised manuscript. I hope that it's appropriate.

9) Comment 9: General: "modifications of CBT": I think the term is misleading, as actually MBCT is the only CBT modification investigated
Response: Thank you for pointing this out. The aim of our study is to evaluate the comprehensive effectiveness of CBT and its modifications for reducing relapse rate. In our retrieval, we did not limited any kinds of modifications of CBT and searched all kinds of modifications of CBT. But the results of our retrieval showed that only MBCT have been investigated for reducing relapse rate.

In the text, I really did not clearly described it and have added this explanation on Result section, Page 7, Line 19.

10) Comment 10: The forest plots are still hard to follow and need to be improved (see previous review), e.g. clear titles, improve graphic solution.
Response: Actually, I have written titles and graphic solution in the manuscript on Page 14 and in the submission system based on previous review which published in this magazine. Your hard to follow my forest plots probably because constructing of the PDF versions was not appropriate. I have adjusted it. Sorry for my negligence and I hope this version was appropriate.

11) Comment 11: 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).
Response: Thank you for this constructive and helpful comment. Based on your advices, I have carefully revised these points and other parts of the manuscript. We are very sorry for our negligence of these errors. Thank you so much!

We tried our best to improve the manuscript. I hope this will make it more acceptable for publication. If you have any questions, please contact us without hesitate. Thanks very much for
your kind work and consideration on publication of our paper. On behalf of my co-authors, we would like to express our great appreciation to editors and reviewers.

Sincerely yours,

Zuojie Zhang on behalf of the authors.