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:** 28 Jun 2017

**Reviewer:** Shadi Beshai

**Reviewer's report:**

This is a review of the manuscript titled "The effects of CBT and its modifications for relapse prevention in major depressive disorder: A systematic review and meta-analysis". The review is timely, discusses an important issue, and certainly has several positive features; however, I found the paper in its present form to be somewhat confusing. Further, I found that interpretations of some of the reviewed trials to be inaccurate (or at least poorly framed). Below, I enumerate what I believe are major limitations in the present manuscript.

1. As is currently analyzed and presented, it is difficult to see what the current review offers beyond already extant reviews and meta analyses (e.g., Beshai et al., 2011) of relapse and recurrence prevention in depression. The conclusions drawn are relatively mundane (e.g., read Bockting et al., 2015 for a recent review of the status of the literature). Further, and although I agree with the authors that in many MBCT trials (e.g., Ma & Teasdale, 2004; Teasdale et al., 2000) there appears to be a moderating effect for number of prior episodes, these conclusions were reached even within the trials themselves. At the very least, I think the authors should dedicate some space to discuss rationale, or what is new about their meta-analysis that is not offered in previous reviews.

2. The way the review is currently organized is not easy to follow. For example, many of the trials discussed under the subheading "Number of Prior Episodes" have already been discussed under other subheadings. This is mostly because many of the trials under that subheading do separate analyses of participants with a more chronic history of recurrent depression (e.g., 3+ or 5+ episodes), with consensus in the field that the more severe the history, the greater the benefit from preventative CBT.

In addition, and given the organization, there is just a lot going on and it is very difficult to follow. Why distinguish "CBT" from "CT"? They are essentially the same therapy but with some minor differences (and technically, CT is one type of CBT therapy). Why separate TAU from PLA when we're discussing psychotherapy? What is placebo exactly in psychotherapy? Sometimes it also seemed that the authors were "double dipping" in reviewing what is essentially the same trial, but different features of that same trial under another heading. I think the organization of the results section, and corresponding HR plots in the figures, needs to be much more simplified for ease of understanding.
3. I am not sure about some of the conclusions reached in the present review, or at least how these conclusions are framed. For instance, on page 9, lines 15-18 (under the MBCT vs m-ADM), the authors say "unfortunately, compared to m-ADM, MBCT failed to show a significant relapse prevention effect". The trial review (Kuyken et al., 2015) was a non-inferiority trial that attempted to examine whether MBCT will be as effective as maintenance ADM in reducing relapse risk, which it is. This is not a failing of the treatment.

4. The writing throughout is not very clear and the manuscript in its present form has several grammatical errors and awkwardly worded sentences. I would highly recommend use of an English language editing service.

5. I think more space should be dedicated to forwarding the theory. As mentioned in points 1 and 2, many of the conclusions reached are not novel and has been reached in other reviews on the topic (Beshai et al., 2011; Bockting et al., 2015). Therefore, considerable time should be spent on not only bolstering the rationale, but to forward theory in the field of recurrent depression given the conclusions drawn.

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