Reviewer’s report

**Title:** A systematic review for the antidepressant effects of sleep deprivation with repetitive transcranial magnetic stimulation

**Version:** 0  **Date:** 13 Aug 2015

**Reviewer:** Joanne Ryan

**Reviewer's report:****

Although this paper is generally well presented, there are a few important concerns which must be addressed, and I really question the utility of undertaking a meta-analysis on so few studies, which come from only 2 research teams.

The Illic 2010 reference, one of the 4 studies included in the meta-analysis, is only an abstract which was presented at a conference. This explains why important information is missing about this study, as shown in Tables 1, 2 and Figure 2. I also don't see how this study fit the inclusion criteria as listed on page 8 and 9, as they don't have most of the details from the published abstract. For example there was no mention in the abstract that DSM criteria or MINI was used. I am thus assuming the authors could not get further information about the study from Illic et al when they tried to contact them? It is very surprising if this work was presented at a conference meeting in 2010 and has not yet been published. Could it be that there were problems with the work, such that it could not be published?

Actually, what I strongly suspect is that that Illic 2010 study is actually exactly the same as the Kristic et al., 2014 study. The author lists overlap, the study design presented in Table 1 and 2 is identical, with the inclusion of 2 more participants only. Thus I do not believe these constitute independent studies and both cannot be included. If I am incorrect, and there were undertaken at different times with different participants, I still don't believe that Illic et al 2010 can be included if the results of this study have not be published (in more than a conference abstract presentation with no figures/tables and insufficient study details).

The issue with the Illic 2010 study, described above, combined with the skewed data from Eichhammer et al., 2002, mean only 2 studies can be included in the meta-analysis. Is this worthwhile? Would it just be better to describe the results from the 3 independent studies (Eichhammer et al., 2002; Kreuzer et al., 2012; Kristic et al., 2014) and discuss how they differ and what the overall findings might be?

The authors can no longer conclude anything about PSD vs TSD, nor the rTMS parameters which were most effective (1 vs 10 Hz as they have said) because the Illic 2010 results should not be included.
The data shown in Figure 4, 5 and 7 is no longer valid and needs to be removed.

Other more minor points are:

- Table 1 should detail the actually diagnostic criteria and tools used to assess depression

- Figure 3: the authors state that the weight is a reflection of sample size, but this doesn't match with the numbers shown in Table 1. From Table 1 the larger sample is Kreuzer study, so shouldn't this have the greatest weight?

- There are a number of references which appear to be "Letters to Editor" or "Editorials". The authors should be quoting the original research articles, not commentaries or personal opinions. For example Epperson et al., 2014 - but I also wonder about reference 3, 16, 19, 54 and maybe others (I didn't check them all). There also appears to be an issue with the page numbers for ref 30.

- The introduction is good but a bit long. Paragraph 2 could be removed for example, as not directly relevant here.

- There are numerous limitations which need to be discussed at the end of the manuscript, including the fact that there are so few studies and appear to be from only 2 research groups, as well as positive publication bias which doesn't not seem to be mentioned.

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