Reviewer’s report

Title: Short-term versus long-term psychotherapy for adult psychiatric disorders: a protocol for a systematic review with meta-analysis and Trial Sequential Analysis

Version: 0 Date: 23 May 2019

Reviewer: Florian Naudet

Reviewer's report:

Dear authors,

I read with interest this manuscript. I have no concern from the methodological point of view. Everything is pretty strong in terms of data collection and data analysis. However, I have more concerns from the conceptual point of view.

- This meta-analysis will include various mental conditions, and the primary efficacy outcome, "symptom severity" combines widely divergent measures of depression, social anxiety, post-traumatic stress disorder, (perhaps) suicidality, eating disorders... across very different conditions. This highly heterogeneous mix will confound the clinical relevance of the findings. In addition, one might be more interested about function as an outcome than solely using symptoms. As it appear in the current version, quality of life is listed as the 3rd outcome. It should be the first one in my opinion, especially of one is interested in long term outcomes. Of course you might argue that this outcome will be poorly studied/reported. However, if there is no evidence on this outcome in the literature, it is of great concern and it reduces the importance of the findings.

- I missed a clear definition of what are short term and long term therapies. A protocol is intended to make the research reproducible. I'm not sure that it will be the case with such fuzziness. You suggest that you will use the author definition but this might again confound the findings of the meta-analysis : i.e. for a 3 months VS 6 months, short term could be 6 months and for a 6 months vs 12 months, short term could be 6 months... Can you reconsider and/or explain this choice in details in the method section ? It makes strong assumptions on the dose-response relationship. In addition, it is not clear to understand how you will handle short term therapies and long term therapies with the same number of sessions (a difference in term of frequency).

I agree that you acknowledge these 2 (somewhat fatal) flaws in your discussion. However, it does not solve the problem. I would like to suggest that sometimes (and possibly in this case) it is better to perform a systematic review without a meta-analysis.
- In the last part of the introduction you refer to non inferiority. You must be clear a priori about this and explain what possible magnitude of differences you will consider as non inferior. This is important to pre-specify this.

Minor suggestions:

- Please also consider to have a look at Pim Cuijpers' database (http://www.evidencebasedpsychotherapies.org/index.php?id=25);

- Please give the search strings for all electronic searches.

- You are saying that you have performed preliminary searches and identified a few studies comparing long VS short term psychotherapy for one ore more specific disorder.

  Could you indicate what kind of searches you have already performed? Please add information in the web appendix.

  Could you present in a table the studies that you have identified at this point? Please add this information in the main paper.

  Can you confirm that you have done systematic searches of previous meta-analyses already addressing this question (overlapping meta-analyses on the same topic)? If yes, please indicate this.

  By the way, these preliminary searches are not reported in the PROSPERO database (status = not started). Please edit.

**Level of interest**

Please indicate how interesting you found the manuscript:

An article whose findings are important to those with closely related research interests

**Quality of written English**

Please indicate the quality of language in the manuscript:

Acceptable
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