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: 25 May 2019

Reviewer: Ioana Cristea

Reviewer's report:

This is a very thorough and well-designed protocol. I would be happy to be able to use it as a model for what a meta-analysis protocol should be. I have no substantial methodological comments. The authors have described at length all the methodological choices they plan to make. I am looking forward to the results. My only worry if I can call it that is that not enough trials will be retrieved to allow for a meaningful conclusion.

As minor observations:

- The introduction appears a bit verbose, I don't think using a paragraph for each disorder is necessary. Readers will be aware of what these disorders entail. The description of the psychotherapies might also be reduced. For instance, the historical information is not really necessary, again readers will be informed.

- Instead the introduction could benefit from a discussion of previous meta-analyses of psychodynamic/psychoanalytic therapy. These issues are very briefly touched, but would benefit from being expanded, particularly in lieu of the extensive part describing the disorders. For instance, I was surprised this landmark meta-analysis was not even mentioned: https://www.ncbi.nlm.nih.gov/pubmed/22227111

- Did I understand correctly that all therapies that define themselves as psychoanalytic or psychodynamic be included?

- Will interpersonal therapy be considered in any category?

- The paragraph about pharmacological treatment (p.7, l.12-16) is superfluous. Are the authors using pharmacological treatment in any way?

- Regarding treatment duration, the authors will just extract total duration, as well as number of sessions. Maybe a measure of treatment intensity might be useful (e.g., sessions per week, or time between sessions), some of the short-term therapies might be more intensive and this might be a confound
- Instead of using overall risk of bias for subgroup analysis, given that I doubt many studies (if any) will have low overall risk of bias, the authors might consider using just one RoB domain for this analysis, for instance allocation concealment, shown to be the most strongly related to effect sizes in meta-epidemiological analysis, or incomplete outcome data, where in general all studies can be rated.

- For blinding, many studies will use self-report measures. Following the RoB 2.0 tool that the authors are following, these should get high risk of bias. But again this might be the majority of studies and it might mean that few studies overall will have low RoB.

- For the actual meta-analysis, I would like the authors to consider including a data sharing statement and, if feasible, to share their data and code.

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