Reviewer's report

Title: Prevalence and determinants of mental distress among University students in Ethiopia: a systematic review protocol

Version: 0 Date: 28 Mar 2018

Reviewer: Ioana Cristea

Reviewer's report:

Please do not capitalize spelling of "university".

P.3, l 41: "mental" instead of "metal"

P 3. L 48-49: the authors should explain more in depth what is the problem with previous studies of prevalence, low statistical power can mean many things. Moreover, if all previous studies are affected by these problems, why do a meta-analysis, which would just combine many small studies? A new, larger survey might be more useful. These aspects need to be made clear.

P.3 l 55-60: I do not understand what the authors are trying to say here. The mix various concepts like pooled prevalence, which cannot be a goal in itself, and epidemiological data, which would suggest to the reader a new observational study is being proposed. Pooled prevalence is only relevant if the sources to pool are enough and have some reliability. It only makes sense as an assumingly more accurate estimation of the true prevalence, and not a goal in itself.

There is no mention of previous reviews on this topic. Are there none, not even non-systematic ones? Not even including more countries?

p.4. Data sources: I recommend the authors use the MeSH dictionaries (and its Emtree equivalent). Some of the terms they indicate are not enough because they could have many synonyms which are not covered (e.g., few papers will probably use "common mental disorders"). The authors need to familiarize themselves with MeSH and use it for their search strategy. Previous reviews need to be identified and their reference lists consulted.

Inclusion criteria:

Why are the authors restricting to the clearly methodologically weaker cross-sectional studies and excluding longitudinal studies?

More details need to be given, for instance what is a recognized instrument? What will happen with a study that includes a sample only partly composed of university students?
Insufficient effect size data cannot be an exclusion criterion: where data is insufficient, authors can be contacted. Also, what would represent sufficient data for the authors?

How will effect sizes be calculated? What will be effect size indicator?

The description of what exactly will be extracted from the eligible articles, particularly in reference to what information will be used for effect size calculation is superficially described.

The description of the study synthesis is completely insufficient and does not take into account the nature of the data, which are binomial (I am assuming the authors will combine proportions) that cannot simply be combined like continuous outcomes. The authors need to consult specific methodological sources for meta-analysis of proportions or prevalence, such as: https://www.ncbi.nlm.nih.gov/pubmed/23963506.

The specific method used to compute the standard error (and what method will be used to stabilize the variance, if necessary), and derive the confidence intervals needs to be stated. Metaan is not an adequate package for meta-analysis of proportions; package metaprop_one is dedicated to this and I strongly advice the authors look into it (https://www.ncbi.nlm.nih.gov/pubmed/25810908).

I would also advice them to consult recently published meta-analyses of prevalence. As it is, this section is lacking most of the necessary information. The Egger's test is not appropriate for this kind of data.

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