Author's response to reviews

Title: Comparing the accuracy of brief versus long depression screening instruments which have been validated in low and middle income countries; a systematic review.

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Author's response to reviews: see over
Dear Mr Editor, I hope this finds you well,

I thank you for allowing my manuscript to be reviewed by the very best in the field. Indeed, the comments were very insightful and will ultimately improve this paper. I have made the corrections as suggested.

**Response to Reviewer #1**

1. The first reviewer raises the issue of the lack of rationale for studying instruments that were validated in HIV settings. Two paragraphs have been dedicated to this aspect, delving into the details as to why it’s important to include the aspect of HIV. This rationale has been included in the introductory part towards the end (just before the aims section of the introduction and background).

2. The reviewer also raised the issue of prevalence according to screening instruments. In the results section (description of the studies), we state that there were wide variations between instruments used, countries, and continents. Probably that’s why it wasn’t possible to summarize the findings as a meta-analysis.

**Response to Reviewer #2**

The second reviewer equally raises a number of pertinent issues which we have addressed.

1. The reviewer brings to light the issue of including pre and postnatal mothers. We included these because we still think that some researchers with interest in women’s mental health may find such information useful. In the results section, we start with a pre-amble regarding the inclusion of such studies in our study. We also state the rationale for their inclusion and add some references to this effect.

2. Of particular concern was the issue of major depressive disorder (MDD) vs common mental disorders (CMD). We appreciate the fact that including the full array of CMD would have been quite useful and more informative. However, we also state the possible ‘cons’ that could have resulted from such a search. The volume of work would have been quite enormous and difficult to synthesize. We include these articulated points in the limitations of the study. We fully acknowledge that the full range of CMD is important, but for now we will limit ourselves to MDD, and plan for CMD’s in the next review.

3. All studies that were not validated in primary health care (PHC) setting, as well as those validated in Taiwan were excluded. Because of this, we were able to exclude a further 7 studies, remaining with 16 studies. The Baggeley study was nested in a cohort, we acknowledge this fact, and indeed we classified it as fair quality due to this fact as well as the fact that it was conducted in a highly selected population.
4. We also took time to look up more studies we could have missed, including those suggested by the reviewer. The Tesfaye study had all the qualities we needed, but assessed CMD’s instead of MDD. The Uwakwe study lacked ROC /AUC results. We were able to identify one extra study conducted in Brazil by Figueira and included it.

5. In the results section, we have added the different weights the studies were contributing to the analysis.

6. In the discussion section, again by limiting our studies to those only conducted in PHC, we hope the discussion can have a better flow. The prevalence referencing has been re-done. The EPDS is stated as having ‘acceptable’ accuracy.

7. The referencing has been re-done to tally with the journal style.