Author's response to reviews

Title: Community health workers can improve child growth of antenatally-depressed, South African mothers: A cluster randomized controlled trial

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Author's response to reviews:

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Dear Editor

Thank you very much for the reviewer comments on the manuscript. Please find out point by point response below.

Yours

Prof Mark Tomlinson on behalf of all the authors

Reviewer: Robert Stewart

Reviewer's report:

This is a useful contribution to the field of maternal depression in LMIC and, in particular, to the untangling of the relationships between maternal depression, child outcomes and interventions aimed improving these. However, there are some issues in how the analysis is presented that the authors might wish to justify/modify:

Thank you for this comment

Major Compulsory Revisions

I wonder whether the authors have approached the analysis optimally. They have presented the results as the intervention moderating "depression's effect on infant nutrition". It would be helpful for the authors to adjust for confounders e.g. poverty, maternal nutritional status, as even if these are similar between intervention and control groups, they are unlikely to be so between depressed and non-depressed. If depression remains a predictor of infant growth after adjustment, it would be easier to justify using the term "depression’s effect on
infant nutrition”. It might be even more appropriate to present the results as showing whether depression (which was measured before the intervention) moderates the effect of the intervention on infant growth, as we know that the intervention and control arms are similar at baseline so confounding would not be an issue in this analysis.

We misstated in several places about the relationship between “depression and nutrition” and should have stated in each place “depression and growth.” Our revised article has corrected this mistake in each place.

We analyzed the relationship between depression and growth both within group and based on the intervention condition by antenatal level of depression reporting the interaction effects. We have presented the interaction effects, but not the within condition analyses by depression. The results were the same. There was a very narrow range of poverty in this study. All mothers were poor and when controlling for income the results are not different.

Minor Essential Revisions

Measures, maternal mood: Please state whether the EPDS was validated in this or similar population and whether different languages were used. If possible state the test characteristics at the EPDS#18 cut-off. What was the mean gestational age when EPDS was administered?

The mean gestational age at recruitment was 26.0 weeks (SD=8.2 weeks), similar across the intervention and the control condition.

The authors might add a paragraph in the discussion highlighting the strengths and weaknesses of the design.

We have added the requested paragraph in the discussion section.

The authors may also consider reading through the manuscript again to pick up any stray grammatical errors. E.g. Abstract – results line 1: delete both

We have done a careful editing of the article and found and corrected all the mistakes that were outlined by the reviewers.

Level of interest: An article of importance in its field

Thank you

Reviewer:Sari Ahlqvist-Björkroth

Reviewer's report:
Thank you for your manuscript. The topic of the article is highly important, as it focuses on the effects of maternal depression on infants’ growth in low- and middle-income countries. Your finding is also important and promising. An advance of this study is that the intervention and the data analyses were carried out by independent teams. However, there are some points that can improve the manuscript further.
Thank you very much for these kind comments

Minor Essential Revisions

Abstract:
In the objectives section, please define the outcomes for children. In the results part, please check if something is missing from the first sentence. The sentence is somewhat unclear. “Depressed mood was similar across the PIP and SC conditions both antenatally (35% rate).”

There was a mistake in the first sentence in the abstract, which we have now corrected. We report the percentage of mothers with EPDS scores over 18 at both the recruitment interview during pregnancy and at 6 months post-birth, the time in which we have examined the outcomes. We have added the outcomes in the abstract under the Methods section.

The other main finding in the study was that the intervention did not significantly moderate depression’s effect on infant nutrition in any assessment period. Should that also be integrated in the abstract?

This was an example of our use using the word “nutrition” inappropriately, when it should have been the word should have been “growth.” We eliminated the word “nutrition” throughout the manuscript and have replaced it with the word “growth.”

In the conclusion and relevance areas, a different concept (home-visiting program) is used to refer to the intervention. The conclusion in the last sentence is misleading. The results do not prove what is stated, but the results can imply these facts.

We have rewritten the conclusion and the relevance conclusions in the abstract and in the discussion section.

Introduction:
The introduction is mostly relevant. As the introduction focuses mostly on maternal depression’s effects on child development, the used intervention (PIP), which does not focus on maternal depression, requires better rationalization. The intervention is rationalized well in the beginning of the discussion when the authors are referring to references nos. 32 and 41. I would encourage the authors similarly outline in the introduction why the PIP intervention was chosen. That could clarify the introduction. The study questions could also be more clearly stated at the end of introduction.

We have rewritten the introduction in order to address the reviewers’ recommendations about including a rationale for why we did not address depression in the intervention.

The last sentence in the introduction’s second paragraph can be elaborated regarding how the results vary.

We have rewritten the last sentence in the introduction’s second paragraph.
Data collection:

It remains unclear when exactly the depression measures were collected. Was depression measured antenatally, at birth, at one week after birth, and at the age of 6 months? A clear presentation of the exact measurement time points here in the data collection paragraph would help many readers to follow the presentation of the results.

The measure of antenatal depression was assessed at the baseline interview when the mothers were first recruited into the study at 26.0 weeks (SD=8.2 weeks), as noted above and as we have added to the manuscript. We also added the rate of EPDS scores > 18 at 6 months post-birth to the manuscript. We did not collect the EPDS at the two weeks post-birth assessment.

After the last sentence in the data collection section, there is a single word “all.” Is something missing, or is it there by mistake?

This sentence was a failure to edit well. We have corrected this mistake in the data collection section.

Methods:

A reference for the used cut-off in the EPDS is missing.

We have added this.

Results:

In Table 1, EPDS cut off is reported as >13, but elsewhere in the manuscript, >18 is used. When were these baseline data collected: anytime during pregnancy when mothers were identified? Did any differences exist between the mothers in the intervention and the SC groups related to the time of identification and recruitment to the study?

We added the percentage of women who had cut-off scores higher than 18 on Table 1 and the rate of EPDS > 18 at six months.

For me, it remains unclear why in Table 2 the results related to weight-for-age-z-score, height-for-age-z-score, and weight-for-height-z-score are labeled “infant nutrition.” Are the values different from the values used in Figure 2 that are labeled as “infant growth”? Based on the abbreviations, they seem to be same thing. This should be clarified.

We used the word “nutrition” inappropriately; we should have consistently used the word “growth” and we had made a mistake throughout the paper.

Is there a typo in the end of fourth paragraph in the discussion? along # alone

Name of intervention:

Yes, there was a typographical error in the fourth paragraph in the discussion, which we have fixed.
The PIP—The Philani Intervention Program—is not a very informative name. Somewhere in the manuscript, the name The Philani Nutrition Intervention is used. That name provides more information about the type of intervention. If PIP is used, it could be clarified in the abstract in order to avoid readers confusion. It is also important to be consistent with the use of the name throughout the manuscript.

We have shifted the title of the intervention program from PIP to the Philani Intervention Program. We have removed the reference to the name of the community-based agency which was our collaborative partner in the paper.

Major Compulsory Revisions

Discussion:
The discussion is not well-balanced. The main findings could be sated more clearly at the beginning of discussion. Both findings (ones related to Figure 2 and Table 2) should be discussed. The discussion could be combined more clearly with the findings; so that one finding is discussed at a time. I would be especially interested in the authors’ view and discussion related to the finding that the intervention did not significantly moderate depression’s effect on infant nutrition in any assessment period. What does that mean?

We have followed the recommendations of the reviewers in the discussion section. We have restated the findings in the first paragraph of the discussion.

The discussion is also missing a statement of limitations of the work.

We have added a section on the limitations of our work in the discussion section.

As the result showed that the intervention had substantial effect on the child growth in the randomized controlled study setting, the possibilities and barriers of wider implementation of the intervention should be also discussed.

We have added a paragraph about the potential diffusion of this model and the potential strategies for improving the implementation strategy.