Author’s response to reviews

Title: Determinants of antenatal depression and postnatal depression in Australia

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Author’s response to reviews:

November 24, 2017
Dr. Elizabeth Camacho
The Associate Editor
BMC Psychiatry

Dear Dr Camacho,

RE: Re-submission of Manuscript, BPSY-D-17-00596R1: Determinants of antenatal depression and postnatal depression in Australia (Ogbo, Eastwood, Hendry, Jalaludin, Agho, Barnette and Page)

Please find enclosed a manuscript entitled “Determinants of antenatal depression and postnatal depression in Australia”, revised according to reviewer comments for your consideration.

Reviewer reports:

Elizabeth Camacho, PhD (Reviewer 1): BPSY-D-17-00596 - review

This is an interesting paper which is generally clearly written. However there are a number of methodological areas which may benefit from some revision.

COMMENTS
1. What is the justification of the age groups selected? From a clinical perspective, 'older' mothers are generally considered to be 35+ years rather than 40. Furthermore because of the small size of the 40+ group in your sample, it could be argued that there are too few cases of depression in that group to draw meaningful conclusions. The analysis could be re-done with this alternative age grouping, or with age as a continuous variable, looking at how a 10-year increase in age is associated with risk of depression.

Response

Maternal age has been reclassified and reported in the entire revised manuscript, as requested by the Editor. For example, Page 6, Paragraph 1 and Table 1 & 2.

2. The selection of confounders is potentially problematic. The research aim is to identify factors associated with perinatal depression presumably so that 'at risk' individuals can be better identified and so that targeted prevention/treatment strategies can be developed. The confounders used in this analysis may themselves be risk factors which in the present analysis would not identify this. An alternative approach may be to conduct univariate analyses on all the potential risk factors and confounders and then include only those with a p-value <0.1 in a single multivariate model.

Response

We agree with the Editor that a number of the confounders included could also be considered study factors. We have performed additional analyses, and the results incorporated into the entire revised manuscript (e.g., Results section and Table 1 & 2) as requested by the Editor. Accordingly, we have also revised the statistical analysis section of the revised manuscript to reflect the new analyses (Page 7, Paragraph 3).

3. It is unclear why a three-level (none, distress, depression) depression variable was used for postnatal depression and only a two-level (none, depression) variable for antenatal. Unless there is an important clinical reason why this was the case, it would be better to have either both using the two-level variable or both using the three-level variable. It is also unclear, but appears that the results presented in Table 3 compare women with distress to the rest of the sample i.e. those with no depression AND those with depression. A better way of exploring the three-level outcome would be to have a single categorical outcome variable and conduct an ordered logistic regression or multinomial logistic regression.

Response

We have presented two-level variable for the outcomes (i.e., antenatal and postnatal depressive symptoms) in the revised manuscript for consistency as requested by the Editor.

4. The use of multiple imputation to explore the impact of missing data appears robust as there were only a few small differences between the results using complete case and
imputed data. However it is unclear whether both outcome and predictor variables were imputed or whether it was just missing EPDS scores - it may be sufficient just to impute the outcome variable and only include those with complete data for the predictors in the analysis but this would need to be discussed. It would be informative to present some analysis of differences in demographic characteristics of those with and without complete outcome data as there may be important differences, for example it may be that CALD participants are more likely to have missing outcome data.

Response

We note that the multiple imputation was conducted for a dataset based on complete outcomes (Spratt, et al., 2010 and Sterne et al., 2009). That is, missing values on study factors were imputed. This was described incorrectly in the Methods section of original manuscript, but noted correctly in Figure 1 and Figure 2. This has been clarified in the revised manuscript (Page 8, Paragraph 2):

The study investigated the potential effect of missing data on observed odds ratios in sensitivity analyses, which were conducted on an imputed data set based on the original cohort comprising complete outcome data (Figure 1 and 2). Multivariate imputation by chained equation was used to impute missing information on study factors included in the analysis.

We have also provided estimates of the extent of missingness in each of the outcome variables, prior to multiple imputation on complete outcomes. This has been noted in the Methods section in the revised text as noted above.

a. In the methods it says that the EPDS was completed via an interpreter for those who did not speak sufficient English. It also reports that the EPDS is available in a number of different languages. This makes it unclear whether the EPDS was administered in the woman's native language or the English version was administered through an interpreter.

Response

Midwives collected information on sociodemographic and psychosocial characteristics, and maternal depressive symptoms during pregnancy at the first antenatal care visit.

5. The predictor regarding supportiveness of partner could be collapsed so that it compares those who responded 'yes' with those who responded 'no/unsure' as the numbers in these groups are small.

Response

This variable has been collapsed into ‘yes’ and ‘no/unsure’ in the revised analyses as requested by the Editor (Table 1 & 2).

6. Throughout the paper it is important to be very clear on the temporal nature of the 'predictors' and outcomes. Cross-sectional data can only tell us about associations
between two factors. In this paper it is difficult to distinguish at what point data were collected, for example were mothers asked about supportive partners at both time points or just one? A partner may be more or less supportive in the postnatal period compared to the antenatal period. A table or diagram showing the timeline of when different variables were collected would help readers and the authors to better understand the temporality of the results. At present it appears that in Table 1 where risk factors for antenatal depression are reported that the eventual type of delivery for that pregnancy is included which would not be correct.

Response

We note that this analysis not was based on cross-sectional collection of data, and that there are clear temporal relationships between information collected in the antenatal and postnatal periods. The text has been clarified in the revised manuscript, and additional information has been incorporated into the entire revised manuscript to clearly articulate these temporal relationships as requested by the Editor. For example, Page 6, Paragraph 1 and 2, or Table 1 & 2.

7. Although possibly not powered to do so, some sub-group or stratified analyses looking at the CALD group specifically would help the paper to address some of the points raised in the introduction about the particular needs of this group. For example, is IPV or assisted delivery more frequent in this group?

Response

Additional analyses were performed by CALD group as requested by the Editor, and the results have been incorporated into the revised manuscript (Page 10, Paragraph 2):

Stratified analyses showed no substantial, significant differences between CALD and non-CALD groups. For example, mothers who experienced physical and psychological IPV were almost equally distributed in both groups (50% in both groups for physical IPV, and 49.0% and 51.0% among non-CALD and CALD population for psychosocial IPV, respectively).

8. In both the abstract and main text, be careful with the conclusions being drawn that they reflect what is being reported in the paper - the paper does not find that “screening will improve maternal and child health”.

Response

The text has been edited in the entire revised manuscript as requested (Page 2, Paragraph 4). The text now reads:

Our study suggests that screening for probable depression and timely referral for expert Sassessment of at-risk mothers may be an effective strategy to improve maternal mental health outcomes.
9. In the discussion, it may be less repetitive and contradictory to have a single paragraph discussing the pros and cons of the EPDS.

Response

The text has been edited in the revised manuscript as requested by the Editor; Page 14, Paragraph 1.

MINOR COMMENTS

10. In the abstract, include that depression was identified using the EPDS.

Response

We note that the information relating to how depression was measured, was incorporated into the original manuscript (Page 2, Paragraph 2):

…..were conducted to investigate the sociodemographic, psychological and health service determinants of antenatal and postnatal depressive symptoms, measured using the Edinburgh Postnatal Depression Scale (EPDS).

11. In the results section and abstract is states that 7.0% had depression during pregnancy but in Table 1 it is reported as 6.2%.

Response

This typographical error has been corrected in the revised manuscript.

12. On page 8 in the middle paragraph there is a typo in the sentence "Predictors of antenatal and postnatal depressive…", assume this should read "depression".

Response

This typographical error has been corrected in the revised manuscript (Page 8, Paragraph 1): …role in predicting maternal depressive symptoms during pregnancy in the current study.

Gracia Fellmeth (Reviewer 2): This is a well-researched article on an important topic. I have only minor comments to add.

You describe the population under study as one of Australia's most CALD populations, but it seems it would be more accurate to say it INCLUDES a high proportion of CALD women. It must include some non-CALD women for you to have assessed the effect of being from a CALD group. I would re-word or make this clearer - especially in the abstract where it sounds like the focus of the paper is solely on CALD women.

Response
We thank the Reviewer for the comments. The text has been revised to reflect that the study population relates to both CALD and non-CALD populations in the entire revised manuscript. For example, Page 2, Paragraph 4:

This study investigates the determinants of antenatal depressive symptoms and postnatal depressive symptoms in an Australian population, including people from culturally and linguistically diverse (CALD) background.

In addition to the overall prevalence, it would have been interesting to include the prevalence of across different groups - mainly among CALD vs. non-CALD (rather than just providing OR of being from CALD background).

Response

We note that estimates of the prevalence of CALD and non-CALD populations in the context of the outcome measures were provided in the original manuscript (Table 1 & 2). We have also incorporated additional text relating to new analyses on CALD and non-CALD populations in the Results section of the revised manuscript.

Some minor grammatical errors need addressing.

Response

We have reviewed the revised manuscript as requested by the Reviewer to identify minor grammatical errors.

Other than this I have no further suggestions.

Response

We thank the Editor and the Reviewer for their comments.

Please contact me should you require any further information.

Sincerely,

Dr. Felix Ogbo
(Corresponding Author)

References