Reviewer’s report

Title: Determinants of antenatal depression and postnatal depression in Australia

Version: 0 Date: 11 Sep 2017

Reviewer: Elizabeth Camacho

Reviewer’s report:

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.

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.

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.

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.

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.

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.

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.

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?

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".

9. In the discussion, it may be less repetitive and contradictory to have a single paragraph discussing the pros and cons of the EPDS.

MINOR COMMENTS

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

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

Quality of written English
Please indicate the quality of language in the manuscript:

Acceptable

Declaration of competing interests
Please complete a declaration of competing interests, considering the following questions:

1. Have you in the past five years received reimbursements, fees, funding, or salary from an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

2. Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this manuscript, either now or in the future?

3. Do you hold or are you currently applying for any patents relating to the content of the manuscript?

4. Have you received reimbursements, fees, funding, or salary from an organization that holds or has applied for patents relating to the content of the manuscript?
5. Do you have any other financial competing interests?

6. Do you have any non-financial competing interests in relation to this paper?

If you can answer no to all of the above, write 'I declare that I have no competing interests' below. If your reply is yes to any, please give details below.

I declare that I have no competing interests

I agree to the open peer review policy of the journal. I understand that my name will be included on my report to the authors and, if the manuscript is accepted for publication, my named report including any attachments I upload will be posted on the website along with the authors' responses. I agree for my report to be made available under an Open Access Creative Commons CC-BY license (http://creativecommons.org/licenses/by/4.0/). I understand that any comments which I do not wish to be included in my named report can be included as confidential comments to the editors, which will not be published.

I agree to the open peer review policy of the journal