Reviewer’s report

Title: Associations of sociodemographic and clinical factors with perinatal depression among Israeli women: a cross-sectional study

Version: 0 Date: 19 Jun 2019

Reviewer: Elizabeth Camacho

Reviewer's report:

BPSY-D-19-00434

The authors have made a good start to exploring the associations between EPDS score and other factors. However there is still some work to be done to strengthen the quality of the paper. I have made some suggestions for the authors to consider.

1. Background - a few points could be made clearer in the background

   a. you state that "Identifying risk factors of perinatal depression is important for encouraging women with depressive symptoms to seek help". I would suggest rather that it may potentially help healthcare professionals to better identify those with depression if they are extra-vigilant with mothers with many risk factors; encouraging those who develop depression (which will not only be those with the risk factors) to seek help is a separate issue.

   b. in the second paragraph you state that "the EPDS showed considerable heterogeneity" and go on to describe a figure with a very tight 95% confidence interval, so should this perhaps be homogeneity instead? If not then perhaps describe in more detail what the heterogeneity relates to. In the same paragraph, it would be useful for readers to know what the prevalence was in the Hahn-Holbrook study.

   c. it would be useful to have a sentence or two in the background about the Israeli health system and what healthcare funds are (e.g. is this a private insurance plan or more like medicaid?) and so whether the sample from this healthcare fund are likely to be representative of the general population or biased in relation to SES.

2. Methods - quite a bit more detail is needed to describe the methods.

   a. What statistical tests were used to derive the p-values in Table 1? What p-value was used as the level of significance? It does not appear that any p-value correction was used to account for testing multiple variables individually (e.g. Bonferroni), have the authors considered this?
b. For each characteristic explored, describe it in terms of what type of variable it is (categorical, continuous, binary) and for non-continuous variables say what all the categories are. For example it is unclear whether the 'periphery' variable is binary (i.e. yes/no people live in the periphery) or otherwise specified. Also the religion-based variables are they individual (Arab: yes/no; Orthodox Jew: yes/no) or is there a single variable in which everyone belongs to one category (Arab, Orthodox Jew, Other)? A single variable with all the religious categories would be preferable because as individual variables you would be comparing Arabs to everyone else (which includes Orthodox Jews) and Orthodox Jews to everyone else (which includes Arabs) so there is the potential for some confusion here.

c. I have understood the methods section to be saying that the "periphery" measure is your measure of SES but it is not fully clear how SES is measured and how robust a measure this is likely to be.

d. It may be more informative to convert blood/urine measures to binary variables for "normal versus abnormal", for example in the discussion the results are compared with a study which looked at anemia as a binary variable rather than absolute levels of hemoglobin.

e. In the methods is states that data are available for fasting glucose and glucose tolerance tests (which are not reported in the results anywhere) but in the discussion it says that only chronic diabetes was measured - would it not be possible to use these test results to identify gestational diabetes?

f. Define all abbreviations on first use including blood tests e.g. TSH is not defined.

g. It is unclear whether the results being reported in Table2 are the results of a series of univariable logistic regression models or a single multivariable model. Also if it is a series of univariable models, then it appears that no adjustment for expected confounders has been done. Logically a number of the factors are related, for example socioeconomic status and smoking, therefore a single multivariable model would take some of this into account. As univariable models it is not possible to say whether the relationship between depression and hypertension was related to the relationship between depression and smoking (which is a risk factor for hypertension). Including all the variables in a single model would allow you to explore this.

3. Results - Table 1

a. In the row for mean age the SD is reported differently in the two columns (i.e. as ± in one column but not the other).

b. Only Arabs and Orthodox Jews are represented in the table but these are the minority group, report data for the other group as well.
c. Report the units for all blood/urine measurements

d. Indicate for each characteristic how many people provided data in each group (as has been done for the blood tests), for example rather than reporting a single number it could be reported as n/N (i.e. number with the characteristics/number providing data for that characteristic).

4. Results - it is unclear how the last paragraph of the results section should be interpreted with the current level of detail about the methods, perhaps revisit this paragraph once the methods have been reviewed.

5. Discussion - some of the variables which appear to be statistically significantly different by depression status are very small differences in real-world or clinical terms. For example when age is a categorical variable there is only a 1.5% difference between the proportion of people in the <25 group (10.3% versus 8.8%) and the difference in the proportion of people with cardiovascular disease is 0.6%. It is worth noting this so that readers can interpret your findings in context.

6. Discussion - the authors cite the accepted cut-off score for depression in Israel, but it may be worth noting the literature (from other countries) which recommends different cut-offs for pre- and post-natal depression and what implications this may have for your analysis. It is a potentially important limitation that you cannot identify for all participants when they completed the EPDS, however it may be an interesting addition to the paper to do some exploratory analysis on each of the two sub-groups who completed the measure pre- or post-natally.

7. Discussion - in the methods section it states that the health ministry protocol suggests that all women complete the EPDS twice however it appears that you only have one data point for each of your participants and you also say that only around half of the women who visited a health centre completed it at all. It would be interesting to discuss this mismatch between the health policy and what is happening in reality, there may be biases in who is completing the EPDS which could have implications for these findings (some of which have been discussed already).

8. Discussion - you suggest that Orthodox Jewish mothers may be less likely to report symptoms of depression due to social stigma, would this not also be the case for Arabic mothers?

9. Discussion - this paper would be strengthened by adding something in the discussion about what this paper contributes to current knowledge, whether it is the first paper to address a particular research question. Also some discussion of what the clinical/policy implications (e.g. better identification of women with depression) etc are likely to be.

10. Discussion - you have not explored the relationship between parity and depression or described how many previous children the participants in your sample have - this has
been shown to be associated with perinatal depression so if it is not possible to access these data then this is also a limitation of your study to note in the discussion.

11. Conclusion - this section should recap the key findings from this study and the sentence about effects of depression on families is not appropriate here, and would be better placed in the discussion (see comment 9).

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