Reviewer's report

Title: Postnatal depressive symptoms in Vietnam: Investigating the influence of son preference, traditional confinement practices and other socio-cultural factors

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Reviewer: Benedict Weobong

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Postnatal depressive symptoms in Vietnam: Investigating the influence of son preference, traditional confinement practices and other socio-cultural factors.

General comments: 1
The paper may be considered for publication as it:
• Addresses and contributes to a mental health condition of public health importance that requires more research evidence to unpack its aetiology.
• Contributes to narrowing the mental health research-specific publication deficit in under-resourced regions of the world.

General comments: 2: Minor Essential Revisions
• The authors are encouraged to follow the STROBE-Cross-sectional studies guidelines in their report as this will extremely improve the structure/content of the paper and help with future systematic reviews/meta-analysis in this area of perinatal mental health.
• More seriously, the authors should stay focused on what they set out to do in this analysis as indicated in the title of this report. There’s some semblance of data dredging as most of the discussion was focused on findings that did not form part of the priori hypotheses e.g poverty, food security etc. This is largely due to the lack of structure with clearly defined essential sub-sections. E.g. what were the exposures of interest in this work, what were the potential confounders for this analysis etc….Whilst it is good practice to clearly define what is of interest in this report as in the title, the authors should know that the cross-sectional design permits the exploration of multiple potential exposures and so wonder why they chose to restrict their report to only socio-cultural.. etc factors, especially that it appears some extensive amount of data was collected.

Specific comments:
This paper could be improved considerably if the following are addressed:

Title: Minor Essential Revisions
The title should make it obvious to the reader that this is a cross-sectional study. This then means the right terminology is required as you couldn’t be assessing ‘influencing’ factors with this design. Suggest to use the term ‘correlates’- as used in the abstract. Also the title should clearly state what the normative sample is as we know postnatal depressive symptoms are also found in men as well!
Abstract: Minor Essential Revisions
• Background: state the analysis group
• Methods: last sentence on EPDS… should come before the analysis approach
• Methods: line 70; What does ‘hypothesis test’ mean? and how is this a statistical method of analysis? Sure GLM’s test hypotheses!
• Under results, please report prevalence with 95% CI. Also important to report findings at the core of this analysis first, with complete effect sizes and 95% CI. Based on what this paper sought to do, the results indicating whether son preference etc are correlates of PND symptoms should be reported first. Indeed what are the other findings for? And why report them here when you didn’t set out to investigate these?......goes back to my earlier argument about missing the beauty of the cross-sectional design!
• Under conclusion, in line 89; include ……’among mothers’ in ..... 
• Also suggest the key words to include words like ‘correlates’

Introduction: Minor Essential Revisions
• Spell out ‘PND’ in line 138

Methods:
General comments: Minor Essential Revisions
• Suggest this is extensively re-structured with clear headings; outcome measure (current section-depressive symptoms and wellbeing), exposures/correlates, potential confounders etc
• Have sub-sections for design and participants (describe who they are, including inclusion/exclusion criteria) etc. at the moment it’s all lumped up and makes reading difficult.
• Also it is not clear how participants were selected and if this was random. The authors appear to have randomly sampled the district areas (line 157-which is a good thing), then they go on to talk about stratified sampling of the communes. It is not clear whether the communes refer to the health centres, and it is also not clear what the stratification variable is- what did they stratify on? In any case, the most important issue with the description of the selection process is that we are not certain if selection of study participants was at all random. Could participants differ within clinic? And could this difference be systematic across clinics? This has the potential to introduce selection bias and thus a potential limitation of this work. Authors could empirically assess this by checking clinic level differences in distribution of depression prevalence, potential correlates. Analysis could also stratify by clinic.
• Sample size calculation: this is at all not clear. What’s the effect size assumed? And for which of your potential correlates? Authors appear to have arrived at the sample size based on what is required to estimate prevalence and I’m not quite sure this is right as the thrust of this work is to assess correlates.
• Line 164; a bit curious why the authors excluded women with babies with
congenital abnormalities. Surely, preference for a male child may be influenced by birth defects. Could the authors provide a rationale for applying the exclusion criteria specified?

- Line 179; inappropriate use of word ‘translations’ – consider revising
- Line 181; sentence starting with WHO-5 should be a paragraph

Data analysis:

General comment: Minor Essential Revisions
- This whole section needs to be presented more clearly as it’s crucial for the purposes of replication. The steps in conducting the analysis leading to building a parsimonious model should be clearly discussed. E.g. first, all variables were subjected to univariate analysis using xx; second, variables that showed significant associations with depression at the p<0.05 level were then…..merely making reference to Bursac’s approach is not sufficient in explaining how the analyses were conducted.
- Also not sure if potential effect modification was explored; could there be interaction effects? And were these explored? If not why?

Results:

General comments: Minor Essential Revisions
- Suggest to have a flow chart illustrating flow of participants in the study. E.g. number assessed for eligibility, number eligible, number consented, number assessed with EPDS, number with complete data on potential correlates, number in analysis. A bit curious that refusal rates are not mentioned-did all women approached agree?
- The presentation of the tables that over-run pages is challenging to navigate because of the missing header rows.
- Suggest to have a main table summarizing the results of the main objectives of this analysis. Also not clear how the gender expectations was analysed and reported as the categories listed are not immediately intuitive. E.g what does ‘expected’ mean? is this about the male child? Why don’t authors report these as gender preference as they appear to have set out to do?
- Also not really helpful to report only p-values and leave out the specific estimates of effect. What readers want to see immediately is the size of the effect and not whether it was significant.
- Line 263: it is not clear if any of these findings result from models adjusting for appropriate confounders. e.g. was the gender of the present infant taken into consideration when exploring the effect of gender of previous child?
- Line 268: sentence beginning ‘although’…..is not relevant in the results section-this is a discussion point.
- Line 282; please provide the t- statistic
- Line 310; don’t agree with the argument about detecting confounding as changes in coefficient of effect could be explained by effect modification as well!
These factors will then be effect modifiers and not confounders. As I indicated earlier, better to define apriori what the potential confounders are in this analysis. It gets even more confusing when the authors state in line 322 that some variables (don’t know if they are referring to carer attitudes?) met criteria for confounding! How is this a confounder in this hypothesis?

- Line 338. Sentence starting ‘in the process….’ Should be deleted as it’s repeated/or redundant.

- Lines 353-356: would be interesting to discuss this finding in detail in the discussion section as one would expect more affluence in urban than rural settings, so strange urban had a low wellbeing. Before then, do we know if these analysis were stratified, what was the distribution of affluence in rural/urban?

Discussion:

General comments: Minor Essential Revisions

- The authors have made quite strong arguments for the mental health needs of women to be given proper recognition, based on this piece of work. I am not certain that this report provides the appropriate platform to make such recommendations, and I see this to be quite inappropriate. A cross-sectional design provides exploratory, hypothesis generating findings necessary for a more analytical/definitive design to be commissioned. We do not know if any of the findings here are causal and therefore could not use this as a basis to make strong arguments for interventions as proffered in this report. Indeed, the correlations reported here are largely very weak and this calls for a cautious interpretation of the findings. The authors should be discussing their findings in the context of these important considerations, advancing potential strategies for collecting prospective data in order to appropriately answer the questions set out in this report.

- Linked to the point above, the authors should acknowledge this important limitation of the cross-sectional design; the direction of causality cannot be clarified because of the often chronic remitting and relapsing nature of depression—this is missing and indeed I don’t seem to locate a focused discussion on limitations in this report.

- Other limitations the authors ought to discuss are: inability to assess the effects of other potential correlates because they were not measured, effects of response/recall bias in interpreting the results; recall of such events may be affected by the mother’s current depression status, and could lead to bias.

- Null findings on son preference: This is one of the objectives of this study and coming up with a null finding that is inconsistent with findings in this particular region of the world deserves to be discussed thoroughly. What could possibly explain the finding in this study? Did the authors actually try to investigate this using the ‘gender-bind’ concept? How was the question posed in order to assess if preferences were met or not, and how this potentially affected their mental health?

- Lines 387; the use of the term ‘randomized’ is inappropriate and connotes an experimental design wherein patients were randomly assigned to groups. I am
not sure this is what this study did! Perhaps the authors want to say a randomly selected sample.....

• Lines 389; what do the authors mean by the claim that ‘social desirability’ was ensured? What of social desirability? Is it the effect of social desirability bias? And which of the questionnaires are they referring to?

• Line 392 to 394: This is the first time the issue of refusal rates is discussed! The results section does not mention this, and it also appears in the abstract that all the participants were successfully approached! Authors should discuss the implications of this on the external validity of the findings.

• Normally, the first paragraph in the discussion states clearly what the study sought to do and what the key findings are- this is not the case in this report and there’s an usual focus on discussing the prevalence and this makes me wonder if this is the main focus of this report.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests