Author’s response to reviews

Title: Parental divorce in childhood does not independently predict maternal depressive symptoms during pregnancy

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REVISION LETTER

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Original title "Parental separation in childhood does not independently predict maternal depressive symptoms during pregnancy"

Dear Editor, Dear reviewers,
We thank you for the insightful and careful comment on our manuscript. We have carefully considered them and revised our manuscript accordingly. The revision led to many changes in the manuscript, and we have highlighted the consequential ones with yellow color. We feel that after the changes made the manuscript is now significantly improved.

Below, you can see our responses to the specific comments made. We have made all suggested changes.
Thank you again for the effort you put into commenting our manuscript. We hope that you find it satisfactory. Needless to say, we are willing to continue discussion and conduct further revisions if considered useful.

RESPONSES TO THE COMMENTS FROM THE EDITOR AND THE REVIEWERS
Editor's comments to the Author:

1. Especially the comment made by Reviewer 1 about the lack of information about women already suffering from depression before pregnancy is an important one you should address - if not with data than as a limitation in your discussion section. Moreover, discussion of the possible influence of other variables that were not measured - socio-economic conditions, social support etc. - should be included in the manuscript as well.

A: We acknowledge that this is an important issue but unfortunately, we do not have reliable information on the prevalence of depression before pregnancy. However, as discussed in our answer to reviewer 1, our aim was to focus on the presentation of depressive symptoms during pregnancy as such, even if the depressive symptoms had begun before the pregnancy. We have now discussion on this issue on the limitations section (lines 212-214).

As the socioeconomic condition of the pregnant women (educational level) is a mediating factor and not confounder, we chose not to include it in the analyses as this could have biased the results by diminishing a true effect. Discussion on this is also added to the manuscript (lines 204-208).

A: Unfortunately, we do not have reliable information on the prevalence of depression before pregnancy. However, as discussed in our answer to reviewer 1, our aim was to focus on the presentation of depressive symptoms during pregnancy as such, even if the depressive symptoms had begun before the pregnancy.

We have now added this issue on the limitations section (lines 205-206).

Reviewers' Comments to Author:

Reviewer: 1

1. The meaning of 'parental separation in childhood' was not immediately clear. This statement could be interpreted as a child being separated from parents rather than the separation of parent during a woman's childhood. I recommend the authors consider rewording.

A: We thank you for raising this point out. We have now replaced the term parental separation with parental divorce. The definition of parental divorce in our manuscript has been explained in the abstract as well as methods section (lines 11-12 and 76).

2. The authors state that prenatal depression affects 18% of women citing a 15 year old systematic review. A more up-to-date systematic would be beneficial. Several references cited in the introduction of the paper are more than ten years old (refs 1,2,3,6,10) when there is more current research available. I recommend more current references be used.

A: The references used are now updated to more current ones.
3. The hypothesis presented by the authors on Line 40 to be tested in the study, is weakly supported by the literature presented. More robust literature to support the hypothesis is needed.

A: This section is now rewritten and we have added more literature on the subject. As there are some differences in pregnant and general populations, we ask, if the association seen in the general population is also present in the pregnant population (lines 36-43).

4. On line 42 there is an error. The authors state 'separation predicts prenatal separation'.

A: Thank you for pointing this out. The sentence is now corrected (line 43).

5. One usually does not use the term 'Material' in the subtitle for methods section. This is likely a translation issue.

A: We have now changed the subtitle to "Methods".

6. The methods section is difficult to follow.

A: The methods section is now edited and, hopefully, can now be better followed.

7. The authors used data from the FinnBrain Birth Study. More information about the FinnBrain Birth Study would be helpful for the reader to understand the data being used for this study, including the focus of the study, how the sample was recruited, the sampling frame, etc. Was the current study a secondary data analysis of the FinnBrain Birth Study?

A: We have now provided more information on the FinnBrain Birth Cohort Study and the recruitment (lines 47-54). FinnBrain includes several substudies, and our analyses were a primary data analysis of one substudy.

8. The authors state that the FinnBrain questionnaire data was supplemented with data from statistics Finland and Finish Medical Birth Register — not clear what data was used from these other data sources. Were the data sets linked to participants in the FinnBrain Study???

A: The origins of each variable is now clarified (lines 73-74, 81-82 and 84-85). We also added information on the linking of the data from different sources (lines 54-55 and 85).

9. Good sample size 2,889 - however not clear how sample was recruited. Initial inclusion/exclusion criteria.

A: We have now provided information on the sample recruitment and initial inclusion criteria (lines 49-54).

10. With the exception of the EPDS, the authors provide limited information about the questionnaires used to collect data, for example about separation of parents during childhood, family violence, etc. What is stated is 'parent separation and negative childhood were assess in 3
categories 0-6, 7-12 and 13-18 at 14gweeks' - How? 'Health 2000 questionnaire' - need more information about the questionnaire. Not clear how this data was collected…

A: We appreciate the insightful remark on this matter. We have now included more detailed description on the questionnaires used (lines 75-80).

11. There is no information provided regarding the validity or reliability of the data collection instruments used (including the EPDS).

A: The information on the reliability of EPDS has now been added. The information on reliability and the validation of EPDS are provided on line 66. Unfortunately, there is no existing data on the validity or reliability of the other instruments used in the data collection.

12. Childhood family socioeconomic status obtained from statistics Finland registers….does this mean they linked data sets?

A: We clarified the linkage of the data from different sources in the Methods section (lines 54-55 and 85).

13. No research ethics review/approval identified?

A: We provide information on ethics approval on lines 230-232.

14. Why calculate difference in EPDS score using t-tests and then with Chi square between women who had parent who separated and women whose parents did not separate?

A: Thank you for pointing this out. We have now removed the Chi square analyses altogether.

15. Line 82 - All mothers with a valid EPDS score from any measurement point were included in the multilevel logistic regression - unclear what is meant by 'valid EPDS" score? Do the authors mean scores >13?

A: Thank you for raising this issue. This sentence is now clarified (line 92). Women who had at least one EPDS score during pregnancy were included.

16. Line 89-90 "parental separation of the 90 pregnant woman in her childhood" - unclear what this means…suspect error in translation.

A: This sentence is now rewritten (line 99).

17. Authors refer to figure 2 (line 122) before Figure 1 (line 123)

A: Thank you for pointing this out. The Figure-references have now been updated.

18. Some major weaknesses with the study include no measurement of the number of participants who experienced depression before they became pregnant. The authors do not define prenatal
depression - for example compared to depression before pregnancy or whether a distinction should be made?

A: We define prenatal depressive symptoms as depressive symptoms during pregnancy (EPDS score ≥ 13), even if the symptoms started before the pregnancy. This information is now presented in the Methods section (lines 67-69).

However, in this study, we wanted to focus on the presentation of depressive symptoms during pregnancy as such, and the distinction between symptoms that had been ongoing prior to pregnancy vs. symptoms emerging only during pregnancy was not crucial from the viewpoint of our research question. Having said this we acknowledge this limitation and agree with the reviewer on the general importance of being accurate in defining these concepts. This issue is now discussed in the limitations section (lines 212-214).

19. Measurement of other important variables identified in the prenatal/postpartum depression literature were not included in the study, for example social support.

A: We have added discussion on this issue in the limitations section (lines 204-208).

20. There are several grammatical errors and word usage issues (e.g. Materials) in the manuscript likely related to translation from Finnish to English.

A: The manuscript has now been examined throughout in case of grammatical errors or word usage issues.

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Reviewer 2:

1. They have also followed sound methodological procedures except strong limitations that may bias readers. The authors explained that depressed women may not get pregnant and pregnancy as a one-time event in life. So, they hypothesized that parental separation during childhood will not affect antenatal depression. But, what about the mediating role of socio-economic factors? The justification for the hypothesis does not look sound and adequate.

A: As mentioned earlier in the comments of reviewer 1, this section is now rewritten and we provide more literature on the subject (lines 36-43).

We also clarified the reasons for leaving some of possible covariates out of the analyses. As the maternal educational level is a mediating factor and not a confounder, it was not included in the analyses (to avoid bias by possibly diminishing a true effect). This is now discussed (lines 204-208).

2. What do the authors mean by "Yet, if parental 42 separation predicts prenatal separation, this association may reflect long-term consequences of 43 childhood conditions extending to the next generation."? Page 2 line 42.
A: Thank you for pointing this out. This sentence previously included an error, and is now corrected (line 43).