Author's response to reviews

Title: Trajectories of maternal symptoms of anxiety and depression. A 13-year longitudinal study of a population-based sample.

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Author's response to reviews: see over
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Dear Ms. Pafitis:

**Manuscript reference number: MS: 1730072456411678**

Please find enclosed the revised manuscript:

Trajectories of maternal symptoms of anxiety and depression. A 13-year longitudinal study of a population-based sample.
Anni Skipstein, Harald Janson, Mike Stoolmiller and Kristin S. Mathiesen

Thank you for the reviews and your comments on our manuscript. We are pleased that you and the reviewers found the paper valuable, and that you are willing to consider a revised draft.

We hope this revised manuscript addresses your concerns. We found the comments very constructive and useful and believe that their impact have improved the paper substantially. A detailed description of how we have addressed the issues raised by you and the reviewers follows below.

We will document firstly our responses to your editorial comments followed by those for reviewers 1 and 2.

On behalf of the authors, sincerely yours

Anni Skipstein
Editorial comments:

1. The name of the ethics committee which approved our study has been added within the Methods section of our manuscript (p. 7). The study is approved by The National Committee for Medical and Health Research Ethics (NEM) in Norway with the reference number S-04167.

2. All changes made when revising the manuscript have been done with 'tracked changes'.

Reviewer 1 (Christopher Dowrick):

1. In the description of socio-demographic variables it is not clear whether cohabitation status was measured at each time point. Needs clarifying.

   Our response: Cohabitation status was measured at each time point and this has been clarified in the Methods section under the subheading Measures (p. 10).

2. In discussion/conclusion it would be interesting to know whether the authors think that the high risk group can be reliably identified at t1, if so, that might have substantial implications for early interventions.

   Our response: This is an interesting point, and we have tried to say a little bit about this in the Discussion (p. 22 and 23). Our plan is to continue this research with studies of predictors from t1 in order to gain more knowledge for improving early interventions.

Reviewer 2 (Albrecht Jahn):

1. The paper addresses an important issue but has insufficient focus and coherence. This problem is exemplified in the abstract’s conclusion which states that there are socio-demographic differences without mentioning what the differences were.

   Our response: We have elaborated this point in the Abstract. After revising the manuscript we hope that the focus and coherence of the manuscript has improved.

2. The rational for the study should be better developed. Why should there be more studies?

   Our response: The rationale for the study has been clarified in the Introduction (p. 6).

3. In the method section the original objective for the TOPP study should be outlined. This is important because the current study uses data that were collected for a different objective. Otherwise the study would have started post-partum rather than at month 15. It would be useful to add a paragraph on limitations, summarizing issues around implications of the study design and other issues such as the effect of attrition.
Our response: The original objective for the TOPP study has been explained under Methods: Sample (p. 7). The study was designed to investigate the influences of environmental risk and protective factors on symptoms of mental health problems and competence among children and their parents. Most studies have been on the post-partum phase; hence the objective of this study was the childrearing phase after the post partum period and not another objective as the reviewer suggests. We thank the reviewer for making us aware of this unclear part.

We have also added more about attrition in the limitations paragraph at the end of the Discussion (p. 23 and 24).

4. Mismatch between sophisticated statistical analysis and generation of new knowledge and insights. Thus, significance testing should be limited to issues of relevance (e.g. table 4). This would also reduce the problem of multiple testing (see last paragraph of results on single cell tests at t3).

Our response: We are grateful for this comment and have chosen to remove the paragraphs about older/younger siblings from the Results and Methods sections. As detailed in the manuscript, we took measures to reduce the problem of multiple testing. We used a post hoc method for ANOVA and a more stringent p level for other tests. More about new knowledge and implication has been added in the Discussion (p. 22 and 23).

5. Fig 1 is not a model but a graph.

Our response: We acknowledge that using the term model in the sentence: “The model with six trajectories (see figure 1) showed one trajectory without any symptoms..” might create misunderstandings. The figure does show a model in the sense that it represents the six-trajectory model, but this might be unclear. To avoid the misunderstanding we have rephrased it to: Figure 1 shows fitted mean trajectories from the 6 class model (p. 14).

6. Circular conclusion regarding the sign change in the low-rising group.

Our response: We agree and think this is a good point; the argumentation has been removed from the manuscript.

7. Technical problems with the graph? No proper description of the x and y axis. The pseudo models are not well labelled and do not add to the meaning of the graph.

Our response: The figure has been improved. X and Y axis labels have been provided. The pseudo-class group values are explained in the manuscript (p.11) and kept in the figure to show that the pseudo model and latent model is very similar, hence the statistical model fits the data very well.
8. The results are difficult to read because of lack of focus. Some parts belong to the methodology rather than to the results. E.g. the second paragraph. Secondly insignificant and issues with little relevance should be removed, e.g. paragraph one. There is no substance to make an issue about t5.

Our response: We agree that the structure was unclear, hence we made some changes to the Results section (p. 13) and hope this has improved the coherence and structure.

We feel that the paragraph about t5 is important to show that the increase in symptoms at this time point is real and not a methodological artifact due to a different amount of items in the scale. But we agree that the paragraph was misplaced and we have moved it to the Discussion section (p. 17).

9. Data overload in table 4. Present key findings in a more concise format.

Our response: We agree that this is a large table, but after thorough consideration, we have decided that we would prefer to keep the table as it is because it provides both descriptive data and results of analysis that may be valuable for readers in order to evaluate our findings.

10. The limitations should refer to the observation that the 4 relevant socio-demographic variables presented in the results included 2 with a significant chance over time due to drop out (maternal education and workforce participation) as mentioned in the methodology section.

Our response: Limitations has been elaborated on. As mentioned in the manuscript (p. 12), our methods were MAR based which allows the inclusion of subjects with partial data and helps to reduce attrition bias. The MAR assumption is compatible with observed variables being significantly related to drop out. We did not include the predictors of drop out when we were extracting latent classes so there is some risk that the class extraction process was biased by attrition and we now mention this in the discussion section (p. 24).

11. In the discussion the authors should highlight their message more clearly. What new insights have been gained? Do these insights have implications for the management of depression and/or for future research?

Our response: This has been elaborated in the Discussion part of the manuscript (p. 22 and 23).

Minor
12. The Introduction is too long and should be shortened.

Our response: After having added minor parts mentioned above, we have reviewed the Introduction for possibilities to shorten. We have shortened it somewhat (p. 7), but
were not successful in achieving a major reduction without losing information that we considered important in the context of the whole text.

13. The information from table 2 could be summarized in the text.

Our response: This is done at page 13.