Reviewer's report

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

Version: 1 Date: 10 August 2010

Reviewer: Albrecht Jahn

Reviewer's report:

This paper addresses an important issue, but has insufficient focus and coherence.

This problem is exemplified in the abstract’s conclusions, which just states that there are shows socio-demographic differences, without even mentioning what the differences there are.

Major Revisions:

The rational for the study should be better developed, beyond the statement that "There is a lack of population-based studies of developmental trajectories". Is there a hypothesis? Why would more of these studies make a difference?

In the methods part, the original objectives of the TOPP study should be outlined. This is important, because the current study uses data that were originally 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.

By and large the methodology is solid. However, there is a mismatch between the sophistication of the 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).

Fig 1 is not a model but a graph, which includes a circular conclusion with respect to the low-rising model. This is the only group with significant change over time, but was defined by the very fact of increasing scores. There are also technical problems with the graph as there is no proper description of the x and y axis included. The pseudo models are not well labelled and do not add to the meaning of the graph.

The results are difficult read, because of lack of focus: Firstly, 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.
Table 4 may be useful for the authors as part of their analysis, but it is not very helpful for readers, because of its data overload. The authors could present the key findings in a more concise format. The data file could be kept as additional file.

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

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

Minor:

Introduction is too long and should be shortened

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

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

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests