Reviewer's report

**Title:** Maternal common mental disorders and infant development in Ethiopia: the P-MaMiE Birth Cohort

**Version:** 1  **Date:** 3 August 2010

**Reviewer:** Mary De Silva

**Reviewer's report:**

Major Compulsory Revisions
None.

Minor Essential Revisions
None.

Discretionary Revisions

1) More consideration of the causal pathways between maternal CMD and child development are needed.

   a) A diagram showing the hypothesised causal pathways would be very useful in determining which variables are confounders and which are on the causal pathway. For example, the authors include hunger due to lack of resources as a potential confounder, but this could be on the causal pathway whereby CMD causes economic strain resulting in child under nutrition which impacts on child development. Equally, social support could moderate the effect of maternal CMD on child outcomes. A more detailed discussion of this is needed in the methods/discussion, as potential over adjustment for confounding could help explain the lack of association found.

   b) There is little mention in the paper about the relatively well established association between maternal CMD and infant growth. This association seems to be critical in the interpretation of this study as it is likely that any effect on child development is at least partly caused by children of depressed mothers suffering a nutritional deficit leading to developmental problems. In fact, the analysis in Table 4 shows this, with higher infant weight for age associated with better motor performance. Including infant growth as a potential confounder when it is actually on the causal pathway may therefore partly explain the lack of association found. It would be helpful to know if maternal CMD is associated with infant growth in this cohort. It is possible that maternal CMD affects growth first and development second, and therefore that 12 months is too early to pick up an effect on development. This possibility should be more carefully explored in the paper.

2) More detail and explanation is needed on the limitations of the study as possible alternative explanations for the lack of association found by the study.

   a) The null findings from the paper are very likely to be due to the small sample
size (n=194) resulting in a lack of power. The authors do not report any sample size calculations despite citing this analysis as the main aim of the study. Such calculations would be very useful in assessing the extent to which this study lacks power. More emphasis should be given to lack of power as a likely reason for the lack of an association in the interpretation of the findings (for example in the abstract).

b) Selection bias is a possible explanation of the lack of an association found, as women with antenatal depression may be less likely to have had the birth weight of their baby measured within 48 hours of birth, and therefore not be included in the cohort. The reasons for losses to follow-up were not explained in the paper. It is unclear why of the 1065 women initially recruited, only 521 were entered in the cohort, and then of those what proportions were selected as exposed/not exposed with CMD, and why these proportions were chosen. Though Figure 1 contains much of this information, a few sentences on the study population in the methods and results to describe this figure would be useful.

3) Measurement of CMD

a) It is unclear whether the exposure CMD was entered into the model presented in Table 2 was a continuous SRQ score, or a dichotomous case/non-case. This needs to be made clear as it greatly affects interpretation of results. If it was a continuous score as I suspect, then justification for this needs to be made as it is likely to be highly skewed and no transformations appear to have been made. This also contrasts with the models presented in Table 2 where CMD is treated as a case/non case variable. As such, I would suggest redoing the analysis in Table 2 with CMD case/non-case.

b) It would be more meaningful if the description of the sample in Table 1 presented the proportion of cases/non-cases of CMD rather than the median SRQ score.

4) Measurement of child development

a) Some discussion of why the different subscales produced different results, and why a global score was not used as an outcome would be helpful. For example, was no association found with cognition and language because 12 months is too young to differentiate between children in these fields?

5) Include a specific reference which validates the cut off of 6 for CMD case/non case in Ethiopia.

6) Make it clear which version of Bayleys was used (paragraph: Methods, Measures, Infant development) – II or III?

7) Include the rationale for testing for effect modification by SES and infant gender in the methods section.

**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 know professionally and have collaborated with some of the authors on this paper (CH and MP).