Reviewer's report

Title: Equity in adherence to and effect of prenatal food and micronutrient supplementation on child mortality. Results from the MINIMat randomized trial, Bangladesh

Version: 1 Date: 20 April 2013

Reviewer: Lieven Huybregts

Reviewer's report:

This paper investigated the role of social differentials on adherence and on the effect of prenatal food and micronutrient supplementation on child mortality. The research question is a relevant and interesting one. The paper could benefit from some important restructuring and providing more details on methodology and analysis. The robustness of the conclusions needs to be checked with other characteristics that might confound the reported associations. I’m looking forward to some additional data (analysis) and revised version of the manuscript.

Major Compulsory Revisions

1. Analysis strategy is a bit unclear. At the end of the introduction, it is announced that subgroup analyses will be conducted according to maternal education and socio-economic status (HH assets), however throughout the article the main focus is on maternal eductaion, while HH assets stratification is only used as a sort of secondary analysis. In the discussion some arguments are given for this choice. I recommend the rationale for this choice is already given and more thoroughly argumented in the introduction as why maternal education is more important than an asset based estimate of HH socio-economic status. As a consequence, the objective should also be slightly modified as maternal education is not the same as ‘equity’. I think caution should be exercised not to oversell the results based on maternal education as a straightforward equity analysis. Such analysis entails many more dimensions (decision power, ownership, rights,…), so a bit more careful wording in the discussion (last part) is recommended.

2. You have reported far more analyses than described in your methods section. I notice the testing of interactions below your tabels, but nothings was mentioned on testing interactions in models.

3. Why did you use the asset score as a binary variable and not in tertiles (or regrouped quintiles) as one might have expected? Moreover, in table 1 you report is in tertiles. I suggest you present the results for tertiles.

4. I recommend you report the association between your assest based score and the maternal education level. This is an important result to understand the data structure.

5. I noticed a very important detail below table 2. Adherence was only assessed until week 30’s examination. Either this is a mistake or if true: why is this the
case? I assume adherence could have further been monitored after 30 weeks when replacing the monthly doses of food and also capsules? This aspect should be added to the methods and discussion. It limits the external validity of the results somehow.

6. Table 2 is not easy to read. The results from the tests are not really shown in the table. Maybe adding the subtotals makes it easier to add the statistical test to the difference. I think it’s better than to write ‘14’ packages and ‘14’ capsules below the table. I would also put the anova results not related to schooling in the text of the results section and remove them from below table2. In the results/abstract section you write (83 vs 68) food packages, but here you mention a difference of 14 packages.

7. Discussion pg 13: you consider it unlikely that there would be differential reporting according to schooling, however you cannot really demonstrate this aspect. Therefore, I would add this to the limitations of the study.

8. It is suggested that most of the food was consumed in community centers. I find this difficult to believe since weekly portions were distributed (for 6 days a week). Does this mean women came to the community centers every day and group sessions were organised?

9. Discussion pg 14 last §: This is my main point of comment. I would really compare the two groups of women with different level of schooling more than it is now the case. The statement that materal education explains different effects on child mortality and adherence deserves a more solid analysis. Did the two groups for instance differ in BMI which might explain the higher compliance for food for lower schooled women? If the main objective is to assess the effect modification by schooling, I think a descriptive table with the characteristics of these women (age, parity, BMI at inclusion, father’s schooling, hemoglobin at inclusion) really necessary. I would replace table 1 by such a table (current table 1 is already published).

10. Finally, why wasn’t father’s schooling used for stratified analysis?

Minor Essential Revisions

1. Please check your data again. It seems there are 3625 liveborn and not 3659. This number refers to the number of women analysed. I base myself on the JAMA paper and also the data mentioned in this paper (later on on page 11 ‘mortality paragraph’). Also the JAMA paper mentions that usual supplementation started at 20 weeks. In this paper 17 weeks is mentioned (pg5, abstract)

2. Abstract: add the stratification variables in the methods section

3. Abstract: second line of results add “a little less to micronutrient”. If expressed in % the difference in food supplements is much larger compared to the difference in capsules.

4. ‘Statistical analyses’ section: you mention that you have tested the differences in baseline characteristics, however, this doesn’t make that much sense. Your power is too low to test such small difference and it is no a study hypothesis.
5. ‘Statistical analyses’ section. Not so much is mentioned on the stratified analysis by HH asset score. Also explain how interactions were tested.

6. ‘Statistical analyses’ section. You mention that you conducted analysis on the effect of food supplementation and MMN on mortality, replace this by that the analysis was previously done (+add reference to JAMA paper).

7. Results section. Last line first paragraph: you reports expenditure status and daily wager. However, nothing is mentioned in the methods. Please enlist all items you have been asking about (also the ones not statistically significant) in methods section.

8. “Adherence to food and MMN supplements’ section. The first 3 lines are a bit redundant. I think it’s logic that less supplements will have been taken by women starting at 20 weeks. I would drop these lines, results are in tabel anyhow.

Discretionary Revisions

1. pg 5 bottom. ‘if pregnancy was confirmed by ultrasound and gestational age was <14 weeks she was randomized” This seems not very logic if the early food supplementation starts at 9 weeks already. Does this mean that early food was before 14 weeks rather than at 9.5 weeks on average?

2. pg 6 Interventions –section: ‘ongoing’ instead of ‘on-going’

3. pg 7: is the company’s name Apex or Aprex? For the eDEM device.

4. post hoc is written in italics

5. The P for P-value, is always a capital letter, and in most journals it is italic.

6. student t-test (italic)

7. Results section (pg10)., line 4,5 and 6: replace ‘and’ by ‘vs.’ For the comparisons between brackets.

8. pg 11: 4th last line: “households” instead of “household”.

**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.