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: 2 Date: 8 July 2013

Reviewer: Lieven Huybregts

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I wish to thank the authors for answering to the many questions and taking into account the many remarks. There are unfortunately still a number of essential items that need clarification in the text and tables. I notice that some of the modifications were not added to the text, although they were announced in the rebuttal letter (study limitations).

All of these remarks are compulsory

1. The authors have now clearly stated what their hypothesis was (ie effect modification by maternal education as proxy for equity, of intervention on adherence and on mortality). My previous remark suggested to take a closer look at the <6y and the >=6y groups. The authors rightfully identified that mothers with less education were significantly older, had a higher parity and lower BMI at 8 weeks of pregnancy. These observations suggest that the effect modification by maternal education could actually be an effect modification of higher parity (in case of mortality!), age (adherence!) and BMI at 8 weeks (mortality?!). Therefore, it is imperative that the authors present the results from the analysis adjusted for these confounders (table 2 and 3), or present the unadjusted and adjusted analysis. Basically we want to know if the education effect is still present adjusting for age, parity, BMI.

2. I regret to insist again on the accuracy of your trial data. I believe a paper cannot be published if the data is not consistent.

From the trial profile I deduce 3591 (602+604+591+598+608+588) deliveries available, which is tantamount to 3625 (608+610+595+605+612+595) total live births and 3560 (597+599+587+592+604+581) singleton live births. So I fail to understand from your methods how you were able to obtain 3659 live births (first line study site and participants) and the 3627 children that were included for analysis”, it doesn’t match up with your trial profile and the previous publication. In case further analysis demonstrated that your estimates are correct and the previous were not correct, I propose the reason why is added to the methods. The JAMA paper mentions in the first § of the results section 3625 live births, which corresponds to what you have shown in your trial profile (also in the tables) I would rewrite the first § under ‘study site and participants’:
“A total of 4436 pregnant women were randomized resulting in 3591 recorded deliveries. This number was associated with 3625 live births of which information on maternal school attendance was available for 3591.”

Also check the first line of your results, it seems incorrect if compared to figure 1. On the other hand the numbers are correct for your table 3.

3. Figure 1: I would replace ‘birthing women’ by ‘deliveries’. Also change ‘Live’ into ‘live’ (lower case) in the first box.

4. Table2, drop ‘examination’ (two times) in the text below the table. Also correct ‘siginificant’.

5. Table 2: I must add that the table is still not so clear. I would remove the results from the interactions and put these in the text (results section). It is written two times that UFe60 (<6 years) was different with EMMS (one time with P=0.0001, one time with P=0.98) I guess this is a typo.

6. Abstract: you mention that HR from the JAMA study under ‘results’, however, they belong to the background section, since they were already published. Under ‘results’ one can only enlist results from this particular study.

7. Study limitations: The strengths and limitations are those of the main intervention study. However, the present study entails a secondary analysis (subgroups). So it might be more useful to focus on the real limitations. I don’t believe that the addition of a real control group (with no intervention) would allow the authors to conduct an adherence analysis. I suggest to add following limitations: (1) that the adherence was not until child birth in the case of the food supplements which might hamper external validity. (2) In your rebuttal you wrote that you would add differential reporting by educational level as a limitation, however, I cannot retrieve this sentence. Please add the latter (you can indeed argue that at the start compliance was more easy to measure).

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:
I declare that I have no competing interests