Reviewer’s report

Title: Poor growth outcomes are associated with inadequate early breastfeeding practices in Eastern Uganda: a community-based cross-sectional study

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Reviewer: Yves Martin-Prevel

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Review of the re-submitted BMC manuscript entitled:
“Poor growth outcomes are associated with inadequate early breastfeeding practices in Eastern Uganda: a community-based cross-sectional study”

Major compulsory revisions

While I acknowledge that the authors have done huge efforts to take into account previous comments made on the first version of the manuscript and to re-do some analyses, I’m afraid that I still question the results and the analytical strategy used. Indeed, the 2 main concerns expressed in the first review were about: (i) the unadjusted comparisons of means of anthropometric indices across breastfeeding (BF) practices categories; (ii) the comparison of infants belonging to the 2 lowest quintiles of the distribution of indices to infants belonging to the 2 highest quintiles, instead of the use of the standard cut-off value of -2 z-scores to define “poor anthropometric status”.

Point (i) has not been addressed by the authors. Even if they claim that these comparisons are only used for descriptive purposes, there is no scientific meaning to keep the comparisons unadjusted. It’s true that, strictly speaking, conclusions are not based upon these comparisons, but in fact this is very misleading. The other explanation given by the authors is that regression analysis is elaborated on with respect to cut-off values, which is not an acceptable reason in my opinion. There are several means to shorten the results section (see below) and adjusted comparison of means is much more meaningful than logistic regression to calculate ORs for a cut-off value of -1 z-score.

Point (ii) has been only partially addressed by the authors. As they now correctly used the -2 z-scores cut-off value, they also have added an analysis with the -1 z-score cut-off value that I strongly question. I’m not convinced at all by the arguments that the authors gave to perform such an analysis. Indeed, if they want to analyze the fact that the distribution of indices is only slightly shifted to the left (as they argue), then leading to low prevalence (mainly for WLZ), the correct analysis is to compare the means in an adjusted regression model, as said above. Again, even if the authors claim that they presented these results only for descriptive purposes, it adds a lot to the confusion (too many results of too many regressions are presented).
Moreover, the authors presented results from 2 types of adjusted regressions: the first one by using an “automatic” selection of variables, the second one by following a strategy in accordance to the conceptual model they presented. My opinion is that this is, again, misleading. The authors should present only the results derived from the strategy that they deem it is the best one. Otherwise, it’s very confusing and this even led to discrepancies between the results, as they are currently presented. For example, one wonders why table 4a shows that the use of prelacteal feeding is an independent factor of wasting for children <6m, while appendix 1 says that there were no independent factors (same sub-sample). As far as I understand, this might be due to either (a) the fact that so-called ‘inherent factors’ are probably (but not sure) not systematically taken into account in the ‘automatic’ regression (and one can therefore ask: why not?); or (b) some differences in the calculations between the two procedures (and then on can ask which of these two analyses is the most reliable).

In any case, in my opinion the objective of the paper is not to compare 2 types of regression methods. Finally, too numerous results are given, of which some seem scientifically sound while others are not. In the end, it’s very difficult for the reader to disentangle between them and therefore to trust the credibility of the assertions that are made.

We strongly encourage the authors to present the results as it was recommended in the previous review: for a particular factor, please present crude and adjusted effects on wasting (alternatively stunting) and on mean WLZ (alternatively LAZ); if too long, the crude effects could be omitted, but not the adjusted ones. In both cases, adjusted effects should be presented using only one type of multivariate analysis (please justify the choice); and possibly add the adjusted effects for the sub sample of infants <6 m (however, only if this adds to the results). Then, the interpretation of the result should be far easier.

In addition, as the authors claim clearly and as it’s now stated in all sections - abstract, introduction, discussion and conclusion - that the objective of the study was to identify factors associated to poor growth outcomes (“including feeding practices”, but not focusing on them), it’s not logical that the title highlights only the breastfeeding practices amongst these factors (especially since there is no clear indication about their effect on growth). Indeed, it’s not only a matter of the title. In the introduction as well as in the discussion section, emphasis is mainly put on BF practices. Once again, a choice has to be made. My feeling is that results are not convincing enough to put emphasis on the relationships between BF practices and growth, but the authors and/or the editor may disagree.

Minor compulsory revisions

1- In the abstract, there are sentences that are not clear enough, such as “Pre-lacteal feeding was the only factor (…) associated with WLZ”. One wonders what the exact meaning of “associated with WLZ” is. Having read the manuscript, it seems that this should read “associated with wasting” but the reader should be able to judge this association (give ORs and CI). This is particularly confusing because the next sentence is about WLZ < -1 z-score cut-off (which should
disappear in my opinion). Another sentence in the abstract is even less clear: “In addition, early initiation of BF, mother’s age (…) were positively associated with linear growth”. Here one wonders how “linear growth” is judged (mean LAZ? Stunting?) and if the analyses are adjusted or not.

2- In the method section, please provide a reference (statistical paper or book) where the “principal axis factoring” method is explained. Currently, references are given in which the same method is used, but I would like to understand the principles of this technique, which is very uncommon. Also, I don’t understand the sentence “…by allowing the factors to be correlated” (which seems to me in contradiction to the basic principles of all factorial analyses). And it seems to me that the method the authors employed looks more like a “correspondence analysis”.

3- Please re-write the manuscript (mainly the interpretation / conclusion section, but somehow the introduction too) in accordance to a sound interpretation of the results, especially as far as BF practices are concerned. And perhaps give less importance to BF practices and give a more balanced interpretation of all the factors of poor growth outcomes (as stated in the objectives).

Discretionary revisions

4- Table 5c: “mode of feeding” (and details of the responses) is used instead of “exclusively breastfed yes/no” throughout the rest of the manuscript. Please harmonize.

5- Size of the design effect: even if I agree with the author that the number of clusters probably prevent to observe a large design effect, it’s only from a theoretical point of view. It would be interesting to check if this assumption is verified or not by given an estimate of the design effect (at least for the main outcomes such as wasting and stunting). This can be important in the interpretation of the results since some associations that are highlighted are not highly significant from a statistical point of view.

Level of interest: An article whose findings are important to those with closely related research interests

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