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

Reviewer’s report:

Major compulsory revisions

1- The main assertion stated by the authors in the title, as well in main results and conclusion, is that inadequate BF practices are associated with poor growth outcomes among the infants of their sample. However, this assertion relies on (i) unadjusted comparisons between mean Z-scores of anthropometric indices across categories of BF practices; and (ii) adjusted ORs for “bad” practices (as well as for other factors) but when a “poor growth outcome” for a particular infant is defined as exhibiting anthropometric indices belonging to the 2 lower quintiles of the observed distribution of the indices and is compared to infants whose indices belong to the 2 upper quintiles. I strongly question these 2 points:

a. Unadjusted comparisons of mean indices do not warrant that the differences across BF practices categories that are highlighted by the authors are linked to BF practices by themselves. Many confounders can be responsible of the statistical associations. The authors themselves have established a conceptual model for potential confounding factors to be taken into account in their analysis. Therefore, one wonders why they didn’t use it in this part of their analysis.

b. The authors correctly defined stunting and wasting (indices < -2 z-scores) according to WHO recommendations. Therefore one wonders why they didn’t use these definitions to look for factors associated to “poor nutritional status”. Comparing infants whose indices belong to the 2 lower quintiles of the distribution and those from the 2 upper quintiles is not the proper way for analyzing such anthropometric data and results are therefore misleading. This is particularly true for WLZ since the distribution of this index among their sample seems quite comparable to the one of the WHO growth reference data. Moreover, when doing this strange analysis they omit the median quintile, then reducing the statistical power. Also, this implicitly define that the prevalence of “poor nutritional status” is of 50% among the infants of the sample used in the analysis, which is meaningless. In addition, it is well known that ORs largely overestimate the true risk ratio when the prevalence of the “illness” is too high. For all these reasons, the conclusions the authors reached through this analysis are not scientifically sound.

2- On the whole, there is a need to clarify the objectives of the study and to conduct the statistical analysis, as well as to present the results, accordingly. The objectives that are stated in the abstract (and at the end of the introduction) are
as follows: “to describe current infant growth patterns in terms of the new
guidelines and to determine the extent to which these patterns are associated
with infant feeding practices, equity dimensions, morbidity and the mother’s
health-seeking behavior”. However, the title, the discussion and the conclusion
suggest that the main interest is in the influence of BF practices on growth
patterns and/or outcomes. Finally, the way the analysis is conducted and the way
the results are presented currently do not follow any of these 2 types of
objectives; only part of the expected analyses is shown and results are not well
organized (this is exemplified by the fact that the results refer successively to
tables 1, then 4c, then 2, then 3a, then 4b, etc.). My feeling, and my advice to the
author, is to choose one of the 2 following options:

a. Objectives as they are stated: to describe growth patterns and look for
associated determinants (including BF practices); then expected analyses and
results would be:
   i. Description of infants’ nutritional status (which is partially done);
   ii. Univariate analyses looking for determinants associated to mean indices
      and/or to the prevalence of stunting and wasting;
   iii. Multivariate analyses to look for independent effects of determinants identified
      at the previous step.

b. Focus on the effect of BF practices; then expected analyses and results would
be:
   i. Description of infants’ nutritional status (same as above);
   ii. Raw effects of factors of interest on the mean indices and/or to the prevalence
      of stunting and wasting (then taking into account only the so-called ‘inherent
      factors’);
   iii. Identification of potential confounding factors (i.e. factors that are linked to
      BOTH the nutritional status AND the BF practices);
   iv. Multivariate analysis to look for independent effects of BF practices on
      nutritional status when confounders are accounted for (and possibly to look for
      mediating effects and also for interactions between BF practices and
      socioeconomic factors).

3- Table 2 gives detailed results about factors associated (but in a univariate
analysis) to the practice of prelacteal feeding. This seems to be out of the scope
of the article. Also, this somehow duplicates results already published (2 previous
articles using same data). Moreover, in this table anthropometric indicators are
presented on the same level than other variables, while they are of course not to
be considered as “factors” of prelacteal feeding practices.

4- Another point that deserves clarification is that sometimes the entire sample is
used, sometimes only the 0-6 mo old infants are analyzed. The justification given
by the authors is that exclusive breastfeeding (EBF) can be defined only among
the later. This is true, but this does not prevent to use the whole sample for
analyses not including EBF. It would then be very interesting to know if different
associations or different level of significance are shown when using either the
whole sample or only the younger infants.

Minor compulsory revisions

5- Please use the terms “length-for-age” z-score (LAZ) and “weight-for-length” z-score (WLZ) instead of HAZ and WHZ, respectively, throughout the paper.

6- In abstract and methods, please specify “0-11 Mo old infants” (even if the term “infants” specifically refers to this age range).

7- Please give at least basic information on the sampling methodology, even if this has been presented in other papers (and especially as there is no constraint on space in such on online publication). By the way, I have been unable to determine, when reading other articles, if the sample was stratified according to the district (urban/rural). Please specify.

8- WHO definition of “early initiation of breastfeeding” is “to be put to the breast within one hour of birth”; authors should justify why they chose another categorization.

9- It is also amazing, for a study looking in depth into breastfeeding early practices, that nothing is said about the colostrum. Please provide information about this (if available; otherwise, at least state somewhere that this has not been recorded).

10- Please specify which piece of software was used to calculate anthropometric indices as compared to the new WHO growth reference.

11- Factor analysis to construct SES index: please specify if this was a “correspondence analysis” (mode adapted to the type of the variables used) or a “principal component analysis” (less adapted). Also, it is not precise enough to state that the first axis explained “most of the variance”: how much is “most”?

Discretionary revisions

12- Results regarding WAZ and underweight status are of little interest since a low WAZ can result from a low LAZ ad/or a low WLZ. Therefore, please avoid presenting the corresponding results in the abstract.

13- The definition of “inconsistent anthropometric values” used by the authors is not the standard one. Any reason to have chosen another one?

14- There is no need to state that the questionnaire included both a 24-h dietary recall and a recall on dietary practices from birth, since apparently only the later was used in the present analysis (as far as I understand it).

15- The paragraph about birth weight is also of little interest since too many infants have no data.
16- Please consider to use age as a continuous variable in multivariate analyses (instead of categories), unless the relationships with dependent variables have been shown to be non-linear.

**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