Author's response to reviews

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

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**Author's response to reviews:** see over
To
The Editor
BMC Public Health
Bergen, Norway, February 23rd 2008

Dear Editor,

Re: Re-submission of manuscript: “Poor growth outcomes are associated with inadequate early breastfeeding practices in Eastern Uganda: a community-based cross-sectional study”

We are pleased to re-submit a revised version of our manuscript by Ingunn Marie S Engebretsen, Thorkild Tylleskär, Henry Wamani, Charles Karamagi and James Tumwine entitled “Poor growth outcomes are associated with inadequate early breastfeeding practices in Eastern Uganda: a community-based cross-sectional study” for possible publication in the BMC Public Health. This manuscript is not being and will not be published elsewhere while under your consideration.

We were very pleased with the rapid, concise and extensive review. We were pleased to read the criticism and constructive feedback and we have revised the manuscript accordingly. Detailed response follows below.

Having had the chance to re-do and double check all analyses we provide more information than previously according to the reviewer’s request. We are therefore submitting 5 tables and 3 figures in addition to the manuscript. We have also added an appendix that we think does not need to go into the article, but which could remain a web-appendix according to your discretion.

In re-doing the analysis we find that the conclusion we reached in the first round regarding the importance of infant feeding on growth remain. We therefore suggest keeping the title as it is. Further, the introduction, the discussion and the conclusion remain substantially the same.

To make it easy to see where the changes are in the revised document compared to the original revision we attach a file with track changes. The tables and figure 3 are in principal completely changed so track changes format was not made for them. Figure 1 and 2 are in principle the same.

Yours sincerely,

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Response regarding revision submitted 30th of May:

Our comments are applicable for the reviewer’s report stating compulsory revisions:

We thank this reviewer for insightful, clear and specific feedback. We have addressed all the 16 comments systematically and thereby changed our analysis accordingly. Our comments are written in italic, following immediately after each of the reviewer’s comments from 22nd of April 2008.

**Author’s response on review from 22nd April 2008**

**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.

*We agree that mean indices across BF practices which we highlighted in the first version could be confounded by other things. The mean indices are now mentioned in the end of the result for the purpose of description and we do not base our conclusions/statements on these findings primarily as we did in the first draft. In addition, effect size was calculated using eta squared from the two-way analysis of variance (sum of squares between-groups/total sum of squares) for the respective feeding practices, and effect size was found to be small to moderate. Effect sizes are presented in the last two sections under ‘results.’ Regression analysis is not done with respect to these mean indices by feeding practice as it extensively elaborated on with respect to set cut-off-values; see our answer to point b below.*

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.

The authors fully agree to all the reviewer’s concerns in point 1b and have redone all analysis based on these arguments. That implies that there are no comparisons of infants belonging to the two upper quintiles with the two bottom quintiles in the revised article. Our cut-off values for dichotomising the anthropometric values is now <-2 z-scores including all infants aged 0-11 months. This gives us information about risk factors for wasting, stunting and underweight, the latter only presented in the appendix. In addition: regression analysis with <-1 as a cut-off values for WLZ and LAZ was also included and it was emphasized that this was for descriptive and supplementary purposes only. This community recruited population presented with a wasting prevalence of around 4%. We were therefore interested in looking at characteristics also including those in the lower range of the normal distributon. Further, we did sub-group analysis of the infants aged 0-5 months as we expected breastfeeding patterns to be particular for this group.

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).

Having re-done all analysis based on other criteria (<-2 z-scores) the reviewer’s arguments in point 2 were taken into consideration. We landed on the suggested model 2a. We have: (i) described infants’ nutritional status more thoroughly, for instance by adding age specific information; (ii) done crude analysis for WLZ and LAZ, dichotomized on the cut-off values <-2 and <-1. All associated factors are addressed in the text; and, (3) we did
multivariate analysis to look for independent effects. The multivariate analysis was done according to two principles which are described in the method chapter and presented in table formats (table 3 and 4). First, we did an adjusted regression analysis where we excluding factors on the basis of significance level alone (table 3). Second, we did conceptual hierarchical analysis according to figure 1/described in the 4th last paragraph in the method section. The tables and figures should now be in order.

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.
The authors agree that this is a bit out of scope and have deleted the previous table2. The frequencies are now presented in table 3.

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

We have as described under pt 1b used all infants with z-scores <-2 for WLZ and LAZ as the main focus in the article. Analysis for those younger than 6 months are presented as sub-group analysis. We have been aware of that fact that switching denominator (0-11 months:723 infants and 0-5 months:412 infants) might confuse the readers and have tried to avoid that as much as possible.

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. 
Done throughout in text and tables

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

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. 
The sampling methodology is specified in the first paragraph of the methodology, and it is specified that the sample was not stratified on urban/rural status.

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.
We agree with the reviewer that it would have been clearer if we were collecting information about breastfeeding initiation within the first hour after birth, but we did not collect information about that. We were only asking for breastfeeding immediately and within the first two hours etc. We addressed this topic in the discussion in the paragraph covering limitations (2nd paragraph).

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).

We have provided available information about the women’s perceptions about colostrum, but unfortunately the data set did not cover practices regarding colostrum. We have specified that practices regarding colostrum was not covered both in the method section (paragraph on definitions and analysis) and in the discussion in the paragraph covering limitations (2nd paragraph). We have presented the perceptions about colostrum in the results in the end of the first paragraph.

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

Done. It is specified in the method chapter under Data collection, measurements and handling that we used WHO Anthro 2005 software (http://www.who.int/childgrowth/software/en/).

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”? Also, it is not usual to separate the sample in two parts according to such an index, but much more sound to split it into tertiles. Why not here?

It is now clarified in the methods under the paragraph ‘data collection, measurements and handling’ that the factor analysis we did involved principal axis factoring, what that involves and why we chose that method. The variance explained by the first axis is also given. Socio-economic status in now split into tertiles.

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.

We agree and the results regarding WAZ are omitted from the abstract and not in the focus of the main document.

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

Firstly, we have to clarify that the reviewer helped us with identifying an error: The criteria was not WLZ more than +3 and LAZ less than -2 as written in the first submission, but WLZ more than +2 and LAZ less than -3. We based our exclusion on previous EpiInfo manuals and from WHO’s fieldtesting of their new standards (link inserted in the reference list), and applied their criteria conservatively. The reason why we applied the strict rule for the WLZ was that we did not trust the high WLZ in combination with the low LAZ in our dataset to represent anything, but length measurement error.
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). 24-hour recall was used presenting mode of feeding (exclusive breastfeeding (EBF) versus mixed feeding (MF)/replacement feeding (RF) in the descriptive tables (table 5c) and in regression analysis (table 3 and 4). The since birth recall is used as the basis for calculating mean and duration.

15-The paragraph about birth weight is also of little interest since too many infants have no data. We agree. The statistical information and calculations done from the birth weight recordings are omitted, and the information about that we include birth weight as a health contact is described in the method section under the paragraph on definitions and analyses.

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. The relationship was linear and age was replaced as a continuous variable.