Author's response to reviews

Title: Determinants of infant growth 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, October 13th 2008

Dear Editor,

**Re: Re-submission of manuscript: “Determinants of infant growth in Eastern Uganda: a community-based cross-sectional study”**

The title is changed from: “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 “Determinants of infant growth 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 This manuscript is not being and will not be published elsewhere while under your consideration.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 again very pleased with the well raised criticism and constructive feed-back from the reviewer, and we have done our best to meet the concerns as we fully agreed to the comments. In line with the reviewer’s concerns we did the following: 1) reduced the amount of anthropometric cut-off values from ‘<-2 and <-1’ to ‘<-2,’ only, for the binary variables; 2) did adjusted logistic regression analysis according to a refined hierarchical conceptual framework, only; 3) presented adjusted mean anthropometric indices for feeding practices and wealth categories; 4) incorporated the design effect; and 5) improved the wealth assessment. Detailed answers to the reviewer’s concerns are given below.

Based on the re-analysis we have rewritten substantial parts of the manuscript according to our new findings. We found that parts of the conclusion we reached previously remained, but that we should give stronger emphasis to other factors as well. Most importantly, we saw how household wealth was related to stunting. As expected, we still saw significant relationship between early feeding practices and growth, and we hope we have managed to set this relationship in a better perspective and discuss possible limitations. Changing the title to a more “open” statement was done in line with the revised conclusion we landed at. We discuss possible implications for public health strategies.

We are presenting 5 tables and 3 figures. Figure 2 is the only figure which has not undergone revision. To make it easier to see where the changes are in the revised document compared to the previous document we attach a file with track changes (marked with TC). As most tables and figures in principal were completely changed, track changes format was not made for them.

Yours sincerely,

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Comments in italic to the review version 2, July 16th 2008 by YMP:

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 that 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).

Response: We appreciate that the reviewer again is explaining these two important aspects from a methodological and analytical point of view, and we as co-authors think we understood the constructive criticism better this time. We hope in this version that these two major aspects from the reviewer are revised satisfactory.

To point (i) above: Adjusted means across feeding practices are now presented. The factors the anthropometric means were adjusted for were selected based on linear regression models. Household, maternal and infant factors were included for adjustment. This is explained in the ‘methods’. The reviewer mentioned under point 5 in discretionary revision that design effect should be taken into account. This was now done for all analysis including the linear regression using the ‘svy’ command in Stata.

To point (ii) above):
Now we are only using <-2 as cut-off values for WLZ and LAZ in the logistic regression, and as mentioned above, we are comparing means in adjusted regression models. We agree with the reviewer that the previous version could
increase confusion, and we hope to avoid confusion in this version by adapting the advice.

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.

Response: The authors fully agree to this important point. We have reflected about the best method to use for the data we are having and have landed on the conceptual hierarchical model in line with C. Victora et al’s article from 1997 “The Role of Conceptual Frameworks in Epidemiological Analysis: A Hierarchical Approach.” As mentioned in the method chapter other epidemiologists support this method for this purpose. We have added in ‘underlying factors’ in our model according to current research implying that “neighbourhood wealth might predict behaviour and thereby outcome.” This is described in “Maternal and child health and neighbourhood context: the selection and construction of area-level variables. Health Place 2006, 12(4):547-556.”

We see now retrospectively that we confused more than clarified in the previous version by allowing for two adjustment strategies and we think were too eager to “cover everything” on the expense of overview previously.

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.

Response: In table format we now present crude information for wasting and stunting according to logistic regression and an adjusted model for stunting only. The reason we do not present adjusted factors for wasting was that according to our analysis no significant factors remained associated with wasting after adjustment. This is
explained in the result section. We did sub-group analysis of those younger than six months and we described the value that sub-group analysis added.

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).

The title is now changed to:
Determinants of infant growth in Eastern Uganda: a community-based cross-sectional study

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.

Response: By redoing all analysis according to the comments, results changed slightly. This has affected our interpretation and explanation too, although important points still remain the same. There is one striking trend which is now highlighted much more in the text and that is the household wealth as a determinant for stunting. Intermediate and proximate factors did not disturb that tendency. We do report on adjusted mean indices with respect to different feeding practices, but add in the effect household wealth also has on the mean indices. In the discussion we reflect on the limitation concerning ‘feeding mode’ as we have presented it, and advocate for future improvements in questionnaire design according to recent literature on the “feeding index” and anthropometry from Africa (Burkina Faso and Madagascar) which the reviewer might recognise.

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).

Response: Completely rewritten

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.

Response: Linear growth is now replaced with length/height-for-age when appropriate. The word “linear” is now used in relation to “linear regression.”
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”.

Response: We did some additional reading and additional studies on wealth assessment and have landed on the “traditional” principal components analysis (PCA). References are now given to Filmer and Pritchett, DHS and the INDEPTH network. We have deliberately changed the term from “socio-economic-status” to “wealth” and differentiate between household wealth and mean sub-county wealth. The reason why we use “wealth” as a term instead of socio-economic status is because we anticipate that the 1st Principal Component expresses wealth in our model. Socio-economic-status should embrace social components e.g. like education which we think should be investigated separately. Just to inform; swopping back and forth between different data reduction techniques did not generate very different results, correlation coefficients were above 0.97 for the different techniques. Remodelling the wealth assessment made us consider the included variables more closely which explains the rise in “variance explained.” We categorised wealth according to Filmer and Pritchett’s examples.

The previous version did not use “correspondence analysis.” As we now are convinced to use PCA for the purpose of wealth assessment, and that is what is relevant for the current manuscript, we have not provided any documentation for ‘principal axis factoring’ which is an extraction technique for factor analysis. SPSS 15 provides it. In other words, we were convinced not to use it.

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).

Response: We have changed discussion/conclusion according to our new interpretation. We now think we have a balanced discussion summarising results, presenting design issues, presenting limitations and discussing our main findings. Less space is given to BF practices and more space to wealth and equity related issues.

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.

Response: Done
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.

Response: All results presented underwent analysis in Stata using the “svy” command. Descriptive GLM output from SPSS was compared to our calculations from our linear analysis in Stata.

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