Reviewer’s report

Title: Prevalence and Determinants of Stunting in a Conflict-Ridden Border Region in Armenia - A Cross-Sectional Study

Version: 0 Date: 11 Apr 2017

Reviewer: Rachel Krause

Reviewer’s report:

This manuscript describes stunting of preschool-age children in an on-going conflict situation, and for that reason recommends itself for publication. The relatively large sample size and clear statistical methodology add additional value to this study.

In order to take full advantage of the interesting context of the study, the direct situation should be better described. For readers unfamiliar with the on-going conflict on the Armenian border, the history of the conflict in these particular communities should be better described. In that vein, it would have been interesting to include a question in the survey to measure the effect of the conflict on family units, as it has been demonstrated in the literature that children of single-parent families tend to have poorer nutritional status than their peers with both parents. It is recognized that this isn't possible at this stage in the research, however a note about this would help to clarify the study context.

STH infections were measured, based on fecal samples analyzed using Kato-Katz technique. This was appropriate, given local constraints. However, the data from this part of the study appears to be underutilized. There is no indication in the manuscript of which STHs were encountered. This is significant, because different STHs have different effects; for example, Ascaris is not a blood feeder, however the hookworms and Trichuris are, and therefore may be expected to have a larger effect on the nutritional status of children. It was also surprising that this was not considered, as anemia was another factor measured in the study. The non-significance of STHs in explaining stunting prevalence may be due to pooling of infection data; I suggest the authors retest effects of species of STHs separately. In future studies, I recommend that all children from 6 months and older be tested for STHs, as it is now recognized that children may acquire infections in their infancy, and their small size at the time of infection can lead to infections having a greater relative impact on their growth status.

The stool sample collection should be better described. Were the samples refrigerated after collection, and what was the time period between collection and analysis? This has implications for the reliability of the sample counts, as hookworm eggs may begin hatching under the right conditions, making it very difficult to accurately identify hookworm infections.

Regarding the statistical analysis: the analysis chosen was appropriate, and the rational for the variables chosen for the multivariate model was reasonable and well articulated. However, different cut-off p-values were employed for different studies. The authors should choose a p-value and use it consistently. For example, p < 0.05 was indicated in the logistic regression
models (e.g. Table 4), however variables with p-values above 0.05 are also described in the text as statistically significant (e.g. effect of prolonged diarrhea on odds of being stunted, page 8 line 24). According to convention, p-values above 0.05 are not considered statistically significant; it would be better for the authors to describe these variables as approaching statistical significant, or suggestive of a significant relationship.

Table 3 contains only odds ratios and their confidence intervals. Although this is important information, this table would be more useful if it also included mean values of the variables themselves. For the most part, the actual values of data are not mentioned in the manuscript (with the exception of Table 2), which is an oversight.

The authors have taken the time to compare urban and rural children. There are very good reasons for doing so, and their study design is appropriately set up to make this comparison, however this data appears to be under utilized. The opening description of rural versus urban children, page 8 lines 11-14, should contain a summary of the data, rather than simply a description of the comparisons made. Further, these differences should be explicitly addressed in the discussion.

The measurement of diarrhea is problematic. Although it is not possible to change the questionnaire at this point in the study, the question used, and more importantly, the interpretation of the question by the local population, needs to be better addressed. Recall of episodes of diarrhea is known to be problematic, and the authors need to justify why this particular, highly subjective wording was chosen, and why they believe it to be an acceptable representation of true rates of diarrhea in this population of children.

The proxy measures used for socioeconomic status are not ideal. There are very good, relatively simple methods for estimating SES from durable household goods that are well described in the literature that should have been used instead. In addition, maternal education is recognized in the literature as an important predictor of young child health outcomes in its own right, and perhaps should be listed separately.

On page 10, lines 17-19 of the discussion, the authors make the comment that higher stunting amongst boys may be due to "higher vulnerability of male children towards early life adversities in comparison with females." This is unclear but an interesting suggestion; please expand on this.

On page 10, lines 11-14 of the discussion, the authors discuss the relationship between mother's height and odds of stunting of their children. However, nowhere is the data on mothers' height given. It would be helpful to have the percentage of mothers who are stunted provided in the data somewhere - it isn't clear from the manuscript whether growth stunting is a relatively new phenomenon that has come about with renewed conflict in the area, or whether this is a multigenerational issue. Inclusion of maternal stunting values would help to illustrate this to the reader.

On page 10, lines 20-21 breastfeeding of 25-72 month-old children is discussed. Does this refer to current breastfeeding, or breastfeeding up to the age of 2 years?
There are a number of passages that require additional editing for clarity. For example:

- Page 4, lines 6-9
- Page 6 line 24
- Page 9 lines 17-18
- Page 11 lines 18-19

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

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