Reviewer’s report

Title: Correlates of Stunting Among Under-five Children in Bangladesh: A Multilevel Approach

Version: 1 Date: 12 May 2019

Reviewer: Moses Ngari

Reviewer’s report:


General comments:

a) the manuscript needs English grammar review. There are many grammar errors.

b) Reading the title, it seems the manuscript aim is to examine childhood malnutrition, but later it becomes clear the article is about child stunting. I would suggest changing the title to reflect this.


c) Report results to one decimal for values <10% and integers only for large values >10%.

Title:

The title is misleading. The multilevel modelling (using logistic regression) was not used to determine the prevalence of child malnutrition but rather to determine risk factors for child malnutrition.

Abstract:

Report the number of children with MAM and SAM in that order rather than SAM and MAM. Were there factors associated with wasting and underweight? The abstract reports only factors associated with stunting.

Methods:

a) How were the anthropometric z-scores computed? We are only told they were compared to WHO growth standards.
b) Was any data cleaning done on the anthropometric measurements? Were there any outliers or date entry errors? There are some standard criteria for cleaning community surveys. https://www.ncbi.nlm.nih.gov/pubmed/24883244

c) Did the authors explore any potential selection bias? They mention the N included was 6965, however, the regression analysis (Table 4) use only 6900 children.

d) Were the features assessed for association with stunting selected based on any criteria?

e) The authors have correctly used a multilevel modelling to identify risk factors for stunting. However, they have not explained the multilevel approach used. For example, mixed effect models give much flexibility in handling this kind of data, one can specify multiple levels of variation/clustering.

f) Logistic regression is not the best regression model to handle analysis with a prevalence >10% (prevalence stunting was >10%) It is not clear if the regression model used stunted children (Moderate plus severe) or severe only. But whichever was used, the prevalence was >10%. Consider using GLM models and report risk ratios rather than odds ratios. (https://www.ncbi.nlm.nih.gov/pmc/articles/PMC4640017/).

g) The authors have not explained how the categorization of continuous variables was done. For example, age. It would be simple to have the groups; <6, 6 to 11, 12 to 23, 24 to 35, 36 to 47 and ≥ 48 months or use age as a continuous variable.

h) Was this study approved by any ethical body or pressure granted by the DHS to download and use their data?

Results;

a) What was the mean/median age of the children?

b) The variables included in the tables (Table 1) need to be arranged in some order. For now, the variables including infant and caregiver characteristics are mixed with no clear order.

c) Table 3 adds no value to the manuscript. Distribution of nutritional levels across various variables could be presented in either table 1 or 2 without having to test significant differences. To test any differences across these groups, the authors could have presented an univariate regression model.

d) The authors could have included other household, child and carer level exposures. For example maternal health (domestic violence, size at birth, WASH variables, sickness episodes-fever, diarrhoea etc)

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.
No

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

Unable to assess

**Are the conclusions drawn adequately supported by the data shown?**
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No

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