Author’s response to reviews

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

Authors:

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Author’s response to reviews:

Dear Editor,

We have revised the paper according to reviewer’s comment. Revised parts are highlighted. Regarding the English grammar review, I took help from Jenny Jung, Yale University. If still it needs more correction, please let us know. We shall take help from some professional that you provided the link.

With best regards,

Papia

Reply of reviewer reports:


General comments:

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.

Author: Done.

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

Author: Updated.

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.

Author: Reported according to MAM and SAM. In this paper, we conducted advanced analysis to stunting only as an indicator of chronic child malnutrition.

Methods:

a) How were the anthropometric z-scores computed? We are only told they were compared to WHO growth standards.

Author: Those has been done by the authority of the survey: National Institute for Population Research and Training (NIPORT) of the Ministry of Health and Family Welfare

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

Author: The survey authority has completed all kind of checking and cleaning before publicly open the data for researcher.

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.

Author: For regression analysis we avoided the missing information.

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

Author: the features were selected reviewing literature and based on the availability in the data set.
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.

and

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/).

Author: In the revised paper GLM model has been use using logit link and binomial family as the dependent variable is binary. However, still we found that relative risk is not possible to get using the model. The referred paper in above also states so ‘Multiple logistic regression, a frequently used multivariate technique, calculates adjusted ORs and not RR’s’.

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.

Author: updated.

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

Author: I had to log in the link of DHS and had to write the purpose to access the data. Then they allow me to access the data.

Results;

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

Author: added in Table 1.

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

Author: Updated.

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.
Author: We are keeping Table 3 according to reviewer 2. However p-values are reported using 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)

Author: Actually most of those variables are not available in the data; size at birth has been avoided due to missing information.

Amin Salehi-Abargouei (Reviewer 2): A manuscript trying to assess child malnutrition and its related factors in Bangladesh. There are a number of major and minor concerns that should be considered before its publication:

1- Methods section: It is mentioned that World Health Organization (WHO) growth standards were used to assess the prevalence of malnutrition. However, no reference is provided. Furthermore, it is better that authors explain if Z scores or percentiles were used to assess malnutrition.

Author: Updated.

2- It is stated that data on Socio-demographic and Economic status were collected. However, the methodology for collecting such data is not provided. For instance, we readers cannot understand which reference was used for categorizing children as large, very large and ….. at birth or how wealth status was measured and categorized. Please try to explain about all variables utilized in the manuscript in the methods section.

Author: Added in the method section.

3- Table 1- Please provide the participants' characteristics based on gender and also in total population.

Author: Updated.

4- Please merge tables 3 a to c to one table. You can include variables like sex, region and … in rows and different malnutrition status in columns.

Author: Updated.

6- The multilevel method was used to assess the risk factors. Although this might be one of the strengths of this work. However, it might not be the only strength of the study compare to previous studies. I think it is better to remove the part the authors explain the multilevel modeling as a strength of the current work from the conclusion part of the manuscript.
The recommendations should be made based on the current study results. Please revise the conclusion accordingly.

Author: Updated.