Comments to the authors

Summary: The paper investigates association between childhood stunting and socio-demographic and geographical factors in under-5 children of Bangladesh. Although this is an important public health issue for Bangladesh, it has been investigated extensively in many previous and recently published studies. The proposed multi-level model has also been previously used to address similar problems but using older data on Bangladesh. Therefore, the only novel contribution of the paper seems to be applying the multi-level model on the most recently published data, and the findings of the paper largely, and quite unsurprisingly, match already established findings.
The paper requires a thorough professional/expert proof reading. It is not written by following good standard of academic writing and there are many grammatical mistakes which often reduced the clarity of statements/arguments. There are also blatant mistakes in mathematical expression of widely used statistical models. In several instances, interpretations of statistical results are also incorrect. The paper also failed to acknowledge/cite similar, yet important, published studies on childhood stunting in Bangladesh. The motivation for investigating the problem is not adequately justified in the introduction and the conclusions cannot be justified by results of the paper alone.

More detailed comments are provided below:

Title

I believe the title wrongly suggests that the aim of the paper is to merely compute prevalence of childhood stunting and its associated factors in Bangladesh through the proposed multi-level model. The actual purpose of such a model is to 'identify factors associated with childhood stunting' which should be reflected in the title.

Abstract

Page 2, lines 5-7: I believe the stated objective/research question does not clearly reflect the main aim of the paper. To me, the objective should be 'identifying factors associated with under-5 stunting in Bangladesh using a multi-level mixed-effects model' or to 'evaluate association between under-5 stunting and its determinants in Bangladesh using a multi-level mixed-effects model'.

Page 2, line 10: For the design of the study, the authors should explicitly mention that BDHS data are essentially cross-section data.

Page 2, lines 17-18: The reported percentage of 38.54% (a row percentage from Table 1) possibly means that children from poor family are stunted in higher percentages when compared to those from higher wealth class. But this is wrongly interpreted as a column percentage, that is 'most of the stunted children are from poor families'.

Page 2, line 22-27: "The study also revealed that cesarean delivery causes….greater risk to give birth to stunted children." - any such claims of causal connection between outcome and covariates must be avoided as this is only a study of association.

Page 2, line 22-27: “The study also revealed that cesarean delivery causes….greater risk to give birth to stunted children.” - this sounds like these children will be stunted from birth which may not always the case as a child be born normal but become stunted at a later age. Also, since this a study of the children, the interpretation should be on children, not the mothers. 'Children whose mothers did not take iron pills during pregnancy are more likely to experience stunting than children whose mothers took such pills' could be a more reasonable interpretation.

Page 2, lines 31-32: The policy advice sounds like we should only care for children of specific age-group or certain districts and not worry about other children - which is definitely not morally acceptable and therefore, avoided. Also, targeting this specific age-group for policy intervention will be difficult.

Strengths and limitations of this study

Page 3: This section needs further clarification. For example, failure to incorporate certain possible risk factors in the study has been cited as a limitation, but why these factors could not be included (e.g., unavailability of data, missing information etc.) has not been reported. Similarly, for advantages, the method used in the study has been claimed to be strong without explicitly stating the merits of such methods over commonly used ones.

Introduction

Page 4 line 6: Since 'stunting' is a technical term not readily understandable to all readers, it may be useful to relate it to 'low height-for-age' when first introduced.

Page 4 line 13: “In Bangladesh, nearly 41% of children under five years old were stunted in 2017,…” - the reported statistics is not correct. In Bangladesh, nearly 41% of children under five years old were stunted in 2011, not in 2017. The cited paper for reference has been published in 2017 but used data from 2011.

Page 4 lines 23-24: “Bangladesh has also set an example of reducing child stunting in the last decade though it is minimal”. The authors must have presented published statistics on the rate of reduction of childhood stunting over the last decade to support this statement.

Page 4 lines 25-26: “The prevalence is highly concentrated on…” – this statement needs reviewing as prevalence of stunting can be high/concentrated on certain levels of some variables, for example at low wealth or low education but cannot be concentrated on variables themselves. A variable can be, however, associated with stunting.

Page 4 lines 29-30: “Other studies besides those…. without education” – the reference in support for this only show that childhood malnutrition is negatively associated with high parental education. It does not indicate that educated parents are better in raising their children, as claimed by the authors.

Page 4 lines 30-31: “In that case, child stunting is an outcome…. environmental problems” – it seems that authors tried to link parent’s education to mentioned determinants of stunting such as low-quality diet, contagious diseases etc. which is not supported by references.

Page 4 lines 48-54: “Moreover, the fundamental causes of stunting…..ethnic minority” – again all the references cited in support for this statement only established association between stunting and the reported factors. Therefore, the causal interpretation must be avoided. Also, there are numerous studies on Bangladesh which found such association between childhood stunting and reported socio-demographic variables. But none of these studies have been cited to support this statement and the authors only chose to refer to studies on other countries.

Page 5 lines 20-25: “As far as …..risk factors” – here the multilevel logistic regression model is used to identify risk factors associated with childhood stunting, rather than identifying prevalence of stunting and its associated factors which has already been published.

Page 5 lines 29-30: Correlation among observations introduced from clustered samples should impact the parameter estimates of ‘multi-level models’ or ‘single-level models?

Page 5 lines 39-41: The authors identified several studies using BDHS data which failed to apply multilevel models for addressing possible clustering affects. However, there are several studies which used such modeling approaches but were not cited by the authors. For example:

Sultana, P., Rahman, M. and Akter, J., 2019. Correlates of stunting among under-five children in Bangladesh: a multilevel approach. *BMC nutrition*, *5*(1), pp.1-12.

Das, S., Baffour, B. and Richardson, A., 2022. Prevalence of child undernutrition measures and their spatio-demographic inequalities in Bangladesh: an application of multilevel Bayesian modelling. *BMC public health*, *22*(1), pp.1-21.

Khan, J.R., Hossain, M.B. and Awan, N., 2022. Community-level environmental characteristics predictive of childhood stunting in Bangladesh-a study based on the repeated cross-sectional surveys. *International Journal of Environmental Health Research*, *32*(3), pp.473-486.

Methods

Study variables and measurement

Page 7 lines 12-13: The WHO reference manual used to define stunting must be cited. Also, it should be indicated that SD stands for standard deviation.

Page 7 lines 35-36: Some of the variables introduced are not properly defined. For example, 'antenatal care' and 'prenatal care' are likely be 'number of antenatal care visits made by the respondent' and 'whether taken any prenatal care service', respectively.

Page 7 lines 37, 48: Whether the child was born via c-section is introduced in two different forms: method of delivery and c-section, which is confusing.

The variable 'Partner's school type' that appeared in Table 1 (Page 23 line 41) was not introduced in this section.

Page 7 line 50: 'Size of baby at birth', an important predictor of childhood stunting, is mentioned but never used in the analysis. Authors must include this in their analysis or explain the reason behind ignoring it.

The authors should have stated how they have decided to choose this particular set of potential explanatory variables (e.g., by picking significant predictors of childhood stunting from recent literature or using own judgement etc.). They should have also explained why they have excluded variables like mother's BMI or height, birth weight or size of baby at birth which has been consistently found to be associated with childhood stunting in Bangladesh.

Data management and analysis

Page 8 lines 29-30: The authors mentioned that univariate logistic regressions were performed but did not provide their results anywhere in the paper.

Page 8 lines 39-42: The mathematical formula provided for the univariate logistic regression model is wrong. Here, the odds of stunting has been expressed as a linear function of *X.* But it is actually the log odds (logit) which must be expressed as the linear function of *X.* A similar mistake has been made when introducing the multivariable logistic regression in page 9 line 4.

Page 9 lines 12 and 27: The notations for the random intercept used in ICC formula (U_{0j}) and in the equation for multilevel model (u_j) do not match and therefore, are confusing.

Other notable issues involve failure to use italic fonts and improper use of super and subscripts when introducing math symbols or notations in many instances. The equations should have been numbered for easy tracking.

While the commonly used and widely known logistic regression model is describe in details, relatively less known model evaluation criteria such as sensitivity, specificity and area under the receiver operating characteristic (AUROC) have not been defined at all. Also, the multi-level mixed effects should have been contrasted with fixed effects and random effects models for justifying its appropriateness for this study.

Results

There are some series issues about interpretation of results.

Page 9 lines 50-54: The authors did not refer to the relevant tables when interpreting results. Some percentages are wrongly interpreted as column percentages instead of row percentages. For example, 38.54% is the percentage of poor children who are stunted, not the percentage of stunted children who are poor. Similarly, 30% is the percentage of children with more than three family members who are stunted, not the percentage of stunted children who have more than three family members.

Page 10 lines 14-15: Not the greatest number of children were found in agriculture-based families, but the highest percentage of children are stunted in families where the household head's profession is agriculture.

Page 10 lines 19-23: Causal interpretations like "mothers with higher education greatly contributed to reducing the prevalence" should be avoided in study of association like this. It should be interpreted as high maternal education is associated with low prevalence of stunting in children. Also, "with no education, 42% of mothers increase the possibility of stunting" is a wrong interpretation. The correct interpretation should be that 42% of the children with uneducated mothers are stunted.

Page 10 lines 29-47: Again 'prevalence of stunting is increasing/decreasing with the levels of a certain variable' are causal interpretations and therefore, should be avoided and interpreted in terms of association. The statement 'In Bangladesh, most of the households don't have improved toilet facilities (32%)" seems a sporadic sentence with no connection to stunting results. For cases of diarrheal episodes, fever, and cough, row percentages are again interpreted as column percentages.
Pages 10 lines 51-55 and page 11 lines 3-19: First, naming the two distinct models as 'multivariable' and 'multilevel' is incorrect. To my understanding, both models are multivariate models considering multiple explanatory variables but one is single-level while the other is multi-level. Also, most of the interpretations are either incorrect, vague or incomplete. For example, "Sylhet had 2 times (OR: 2.2, 95% CI:1.43-3.38) greater chance of being stunted than Dhaka" should be stated as 'children from Sylhet are 2 times as likely to be stunted as children from Dhaka' or as 'odds of being stunted for children from Sylhet are two times the odds of children from Dhaka'. " According to the mother's education level, categories such as primary and secondary were significantly correlated with children stunting" should be correctly interpreted as 'odds/chance of stunting is significantly higher among children of mothers with primary/secondary education in comparison to children of mothers with higher level of education' or simply 'low education of mothers is significantly positively associated with higher prevalence of childhood stunting'.

Page 11 lines 23-39: Similar interpretations issues raised above should be corrected.

Page 11 lines 41-50: It is not the lower log-likelihood but the higher log-likelihood which indicates better fit. The multilevel model is wrongly associated with lower log-likelihood value while it has a higher value than the single-level model. The ROC and relevant AUC should be defined in terms of sensitivity and specificity for intuitive understanding for the readers.

Discussion:

Page 12 lines18-20: The stated prevalence of stunting of 30.48% is slightly lower than the published prevalence of 31% in official report of BDHS 2017-18 which use the exact same data. The authors may want to explain this deviation.

The references used by authors for identifying similar studies seem sporadic and based mainly on African countries and India. There are numerous studies on childhood stunting in Bangladesh which established several results similar to this paper, but these papers were hardly cited. For example, when discussing association of stunting with parental education or wealth following papers could have been cited:

Hossain, M.B. and Khan, M.H.R., 2018. Role of parental education in reduction of prevalence of childhood undernutrition in Bangladesh. *Public health nutrition*, *21*(10), pp.1845-1854.

Mansur, M., Afiaz, A. and Hossain, M.S., 2021. Sociodemographic risk factors of under-five stunting in Bangladesh: Assessing the role of interactions using a machine learning method. *PloS one*, *16*(8), p.e0256729.

Sultana, P., Rahman, M. and Akter, J., 2019. Correlates of stunting among under-five children in Bangladesh: a multilevel approach. *BMC nutrition*, *5*(1), pp.1-12.

Page 12 lines 51-54: "Nowadays hospitals and health facilities centres are forced to perform caesarean delivery to earn some extra money" – this is a serious accusation and must be supported with proper evidence/reference.
Page 13 lines 5-7: "So, the iron tablet works as a protective factor against child stunting" – such a conclusion cannot be justified based on a study of association like this.

Page 13 lines 26-53: While discussing differential prevalence of childhood stunting across children's age-groups and regional divisions, speculations have been made in explaining the results. For example, high prevalence of stunting in the division of Chattogram has been associated with waterlogging which is a wild speculation. Such speculations should be avoided unless can be supported by data/evidence.

Conclusion

Causal claims like "we demonstrated Iron tablet consumption during pregnancy and the vaginal delivery method of childbirth reduces child stunting drastically" cannot be supported by the associational results of this paper and therefore, should be avoided. The policy suggestion to specifically target 24-35 month aged children and providing them nutritious food seem unrealistic. Educating mothers about important and sources of nutritious food seem a more reasonable general policy advice.