Author's response to reviews

Title: Household and community socioeconomic and environmental determinants of child nutritional status in Cameroon

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Author's response to reviews: see over
Response to Reviews (see also highlighted sections in the text)

Reviewer 1

1- Major Compulsory Revisions

1.1 Change in the nutritional status of children and explanatory factors of change

Although the reviewer raises an important issue, the concern is somewhat outside the scope of our analysis and primary message.

The reviewer is correct that the distribution of DHS sample changed between the two surveys. Part of this may reflect a real socio-demographic shift (e.g. more rural populations due to massive urban-to-rural migrations during the crisis, and higher fertility declines in urban compared to rural areas during this period (see page 12 for references on this)). A deterioration in average national nutritional status, even if it is a result of population shift, is still of interest. At the same time, in reflection to the reviewer’s comment, we have added to table 3 (previously table 2b) (page 29) the comparisons of nutritional indicators across years adjusted for place of residence, age and sex of children. The evidence for deterioration remains.

Given that the focus of this paper is on determinants of nutrition, and not on the determinants of change, we have removed any statements about the reasons for this deterioration.

Based on our revisions, we welcome specific suggestions from the editor in order to resolve any minor remaining wording issue.

About the weighting of multilevel modeling, statistical packages commonly used for this purpose such as MLWIN, STATA, SPSS and SAS do not allow for the sample weight provided by the DHS data. Also, because the analysis is conducted simultaneously at multiple levels (individual and cluster levels), and because within the same cluster, individuals have the same weight in our data, the inclusion of the sample weight may not be justified.

1.2 Interaction terms (pages 9, 10 and 11)

This issue is resolved on pages 9, 10 and 11. The reviewer is correct that in a normal regression, a single dummy variable (for the year 1998 for instance) also interacted with other independent variables would be used. However, if we want to estimate the effects of independent variables in both years simultaneously, it is much simpler to create interaction terms between each independent variable with a dummy indicator for each year. We have illustrated this issue using both equations and additional text to explain the model. Note that both equations are algebraically equivalent. We welcome editorial input into further clarification of this model.
1.3 Interpretation of table 6 (previously table 3d) (pages 11, 15 and 35)

Since we pooled the 1991 and 1998 DHS data and estimated equation 2 described in the statistical analysis section of the current version (page 11), we were able to show changes in the coefficients of independent variables.

1.4 Collinearities (pages 14, 31, 32, 33 and 34)

Correlation coefficients between maternal education/place of residence on one hand and economic status/MHSB on the other hand were in the range of 0.34-0.64, substantially smaller than 0.90 (or 0.70 in the strict case), where multi-collinearity is expected to be a problem. However taking into account the reviewer’s suggestions, we estimated in separate regressions the effects of economic status and MHSB, and the effects of maternal education and urban/rural place of residence. We also addressed the issue of high correlation between variables in general (see tables 5a-b (previously tables 3b-c), models 4-6) (pages 14, 31, 32, 33 and 34). Given that collinearities did not constitute a huge issue in our study, we also estimated a full model including all variables (model 7), and the results did not appreciably differ from those found in the previous models.

2 Minor Essential Revisions

2.1 Software used (page 9)

The software used is mentioned in the text (page 9)

2.2 Reference to figures in the paper (pages 12 and 13)

We have referenced figures in the paper (pages 12-13). We also select only the most important figures for presentation (pages 37-39).

2.3 Table 1 and percentage of rural households (page 27)

We have corrected the figure (page 27).

2.4 Total sample size of clusters (page 27)

We have corrected the total sample size of clusters among regions (page 27).

2.5 Change in urbanization rate

This information is not readily available and criteria for deciding for urban/rural status are not always explicit. A population census is now being conducted in the country, which will provide much accurate information.

3 Discretionary Revisions
3.1 Numbering of tables (pages 27-36)

We have partly followed the reviewer’s comment on the numbering of tables (pages 27-36).

3.2 Referencing of tables in the methods section (pages 10-11)

We reference tables in the methods section for clarity (pages 10-11).

3.3 Long footnotes below the tables (pages 27-36)

We have simplified the footnotes below the tables (pages 27-36).

3.4 Correlation between WAZ and HAZ (page 7)

We have taken out the sentence stating correlation between WAZ and HAZ (page 7).

Conclusions

We have addressed all the issues raised by Reviewer 1, especially those regarding (a) the deterioration of nutritional status of children between 1991 and 1998 and how it relates to the shift of the sample towards rural children during this period, (b) the inclusion of interaction terms, and (c) the interpretation of changes in the effects of factors associated with child nutrition (in 1991 and 1998) especially as we used pooled 1991 and 1998 DHS data for this purpose. We thank both reviewers for very helpful comments and look forward to the editor’s response.

Reviewer No. 2
No items requiring response