Reviewer’s report

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

Version: 3 Date: 14 October 2005

Reviewer: Yves Martin-Prevel

Reviewer’s report:

General

The authors have responded to most of the previous comments. They also made the necessary additional work to help the reader in the understanding of the results. However, I still do have some questions about the statistical analyses, of which some remain unclear to me. The help of a statistician could be necessary for this point. Also, I still do think that there is a tendency to « over-interpret » the results in terms of evolution (worsening) of the nutritional situation, mainly because the given results do not allow to be sure that the nutritional situation in Cameroon deteriorated over the 1991-1998 period. Please see point 1 below.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1- Major Compulsory Revisions

1.1- The authors clearly stated in their response to previous comments that the objectives of their paper are (i) to assess the effects of socioeconomic and environmental factors on child nutritional status; (ii) to assess changes in these effects in the 90’s; (iii) to examine age-specific effects of household economic status. The title and the abstract of the paper are in accordance with these objectives. As far as these objectives are concerned, I agree that the paper now clearly adresses an interesting question, mainly in showing changes in the effects of some determinants in two similar surveys at a quite short time-interval.

Unfortunately, starting at the conclusion of the abstract, then continuing in the comments of the results, the authors introduced two non-explicit objectives, i.e. (iv) to assess whether the nutritional status of children deteriorated between 1991 and 1998; (v) to assess which determinants could explain the (probable but not proved) changes in this nutritional status. Therefore, they interpreted their results in a way that is not correct and not supported by the given results.

My main concern is that one can see from table 1 that the two samples (1991 and 1998) are not comparable, at least regarding some very important determinants of the nutritional status, I mean the region and place of residence of the surveyed people. Looking at children’s distribution (and not at households’ or clusters’ distribution, since children represent the statistical units of the analyses) it is obvious that from 1991 to 1998 there is a strong shift of the sample towards rural children (72.4% vs 60.4%). There is also a shift, though less pronounced, in the repartition of the sample between regions. As a result, the changes observed in the children’s nutritional status may be related to these shifts in the repartition of the sample, at least in part, rather than to the economic crisis (the reality of which is not contested). The question is: what would be the differences in the nutritional indices and indicators of children of the 1998 survey as compared to those of the 1991 survey if region and place of residence were taken into account in the comparisons? The nutritional status of
children during a period of economic crisis is likely to worsen, but the authors did not show to what extent the decreases in indices may be due to the shifts in the samples or to other factors (including economic situation). I acknowledge that place of residence and region are taken into account in the adjusted models (tables 3b, 3c and 3d), but these models are run separately in 1991 and 1998, and therefore do not answer the above question. In addition, the authors stated that their analyses were not weighted because of software limitations. This remain questionable (why not use another piece of software?) and do not warrant that the distribution of the samples have been corrected.

Also as a consequence of the differences between the sample, I question the interpretation of the changes in some other characteristics of surveyees between 1991 and 1998. For example, it is amazing that 37.3% of households had flush toilets in 1991 and only 20.4% in 1998. It is also amazing that the percentages of people owning a radio or a TV set decreased so much (even in a period of economic crisis).

I would suggest to add to table 2b the results of the comparisons of nutritional indices/indicators across years of survey, adjusted for place of residence and may be also for other factors that could not be related to the economic crisis (such as age and sex of children). Possibly, the figures given in table 2.a should also be adjusted for the same variables. Depending on the results of these adjustments, comments of the results and sentences such as “the nutritional situation worsened” should be (i) kept as is; (ii) modified (more cautious); or (ii) removed.

1.2- I do not understand what is exactly done regarding the interaction terms (last paragraph of page 10). In the sentence 2, I do not understand why there is a dummy variable for the year 1991 and another one indicating the year 1998. In my opinion, there should be only one dummy variable (with the value 0/1 indicating year 1991/1998). In addition, independent variables themselves should be included together with year of survey and the interaction term. Therefore, I do not understand the last sentence of this paragraph.

1.3- I do not support the interpretation that is drawn from the results presented in table 3d. Firstly, I am not sure to understand how the “changes in the coefficients of variables” have been calculated (since data from the 2 years are not pooled in this analysis, as far as I understand the corresponding part of the methods section). Secondly, I do think that differences in coefficients from two separate models cannot be directly compared and interpreted as an increase or a decrease of the effect of the factor over time.

The help of a statistician could be necessary to elucidate the two above points (1.2 and 1.3).

1.4- Maternal education and urban/rural place of residence are variables that are included in the models as part of the MHSB index, as part of the economic index, and also as full independent variables. This could lead to colinearities and subsequently to an inflation of the variance of estimators. The authors should consider either to remove these variables from the construction of both indices, or to remove the full variables from the models.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
2- Minor Essential Revisions

2.1- The piece of software used is mentioned in the response of the authors but not in the text of the paper.

2.2- Figures are not referenced in the text of the paper.

2.3- Table 1, Households 1998 column, percentage of rural households should be 67.3% rather than 20.9%.

2.4- The total sample size of clusters among regions (142) does not fit the same total among types of place of residence (149). Please carefully check figures and totals in the other tables.

2.5- In the discussion section, page 16, the authors give some information about changes in GDP to illustrate the severity of the economic crisis. This is fine. I would suggest to add also figures about the urbanization rate during this period (if available; this is obviously related to my main point 1.1 above).

Discretionary Revisions (which the author can choose to ignore)

3- Discretionary Revisions

3.1 – I would rather have given numbers 2, 3, 4, 5… to the tables, rather than 2.a , 2.b, 3.a, 3.b etc.

3.2- It is not usual to reference tables in the methods section.

3.3- There are quite long foot notes below tables that may not be necessary for some details that are given in the methods section.

3.4- Page 7: the sentence “WAZ and HAZ were positively correlated in our data” is useless, since this is always the case.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes

Declaration of competing interests:
I declare that I have no competing interests