Reviewer’s report

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

Version: 2 Date: 13 May 2005

Reviewer: Yves Martin-Prevel

Reviewer’s report:

General

The topic of the research, as indicated by the title of the paper, is to assess the role of some determinants of child nutritional status in Cameroon. These determinants are mostly socioeconomic and environmental and are considered at the household and at the community level. The data come from two DHS respectively conducted in 1991 and 1998. Between the two studies, the nutritional status of young children deteriorated. Unfortunately, it becomes unclear, when one read the article, if the main purpose of the article was:

(i) to highlight the effect of some determinants on the nutritional status in each of the studies, on a purely cross-sectional basis;

or (ii) to assess the changes in the effects of those factors in the 90s, as stated in the abstract;

or (iii) to assess how these changes occur in response to macroeconomic factors, as stated at the end of the background section in the text;

or even (iv) to assess the role of some determinants on the deterioration of the nutritional status between the two studies, as the authors tend sometimes to interpret their results, somehow abusively, in the discussion.

However, it is important to say that the statistical analyses performed by the authors can mainly respond to the point (i) above and, to a certain extent only, to the point (ii). There is also an important point about statistical analyses, regarding how the interaction between economic status and child age has been treated (which is not correct, or at least not correctly reported).

As far as cross-sectional analyses only are concerned, the results are quite trivial; they give no really new information about the determinants of the nutritional status in Cameroon, especially as compared to a recent article which used the same DHS data (article by Fotso & Kuate-Defo, in press in Health & Place; please note that this paper is quoted by the authors: cf. n 17 of the references).

However, it would be very interesting to use the DHS data to look for the determinants that are the most associated with the deterioration in the nutritional situation. Therefore, the authors should consider to re-analyze the data with this goal in mind.

More detailed comments are given in the following paragraphs.

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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 use of multilevel modelling to account for the hierarchical structure of the data is a good choice. However, the authors should give more detailed information: What piece of software was
used? What models were fitted? (please give the general form of the equations indicating which effects were fixed effects and which ones were random effects; please indicate also which interactions terms were introduced). Were the bivariate associations tested using also multilevel modelling? (and, if not, why?).

1.2 The sample size in multivariate analyses is not given: we only know the number of children with anthropometric data, from the text and from table 2. No indication is given in table 3 about the number of observations with complete data (to allow multivariate analyses with all the independent variables).

1.3 The choice of weight-for-age as the indicator of the nutritional status is not very well justified. Its true that WAZ is often used in epidemiological studies, as stated by the authors, especially when mortality is to be analyzed in relation to undernutrition. However, as far as economic and environmental determinants are concerned, it is well known that height-for-age (which is also available in the DHS data) is the most appropriate indicator. The author should give stronger arguments to justify the choice of WAZ instead of HAZ, especially because of the kind of determinants they analysed.

1.4 The authors used a synthetic index of the economic status of the household divided into quintiles in their bivariate analyses. To divide such a continuous index into quintiles is usual and allows to alleviate some problems that could occur with outliers; this allows also to study the relationship between the quintiles and the dependent variable (here WAZ) even if this relationship is not linear. In fact, from the bivariate analysis it can be seen that the relation is not linear (figure 2b). Exactly the same situation occurs for the MHSB synthetic index (figure 3a). Therefore I wonder why the socioeconomic index and the MHSB index are introduced as continuous variables in the multivariate model. The authors argued that these indexes were highly correlated with their quintiles, but this is obvious (by construction) and, in my opinion, does not justify to use continuous variables.

1.5 - In addition, the authors said that the quintiles were determined using cut-off points of the pooled data (i.e. data from both surveys pooled together, I guess). It would then be interesting to see whether the sample was equally distributed, according to the quintiles, within each survey. In particular, the authors stated that there were economic changes from the 1991 to the 1998 survey. We are therefore interested in knowing if these changes are visible through the distribution of the economic index.

1.6 Apart from the fact that I would rather have introduced quintiles of the economic index (and of the MHSB index as well) in the multivariate model, that makes no sense to introduce interaction terms in a multivariate model unless the main effects are also included in the model. According to table 3, it seems that interaction terms between the age category of the child and the continuous economic index have been introduced in the multivariate models but without introducing also the main effects of both childrens age category and economic index. This point strongly needs clarification.

1.7 As a general commentary about interaction terms, the authors did not satisfactorily explain why they choose to introduce an interaction term between the age category of the child and the continuous economic index, nor what was their general strategy about interactions testing. I can understand that a strong interest is given to the economic index, regarding the general context of the comparison between the two studies. However, were the effects of the interaction between the economic index and other variables also tested? For example, it would be interesting to see if there was an interaction between the economic level and the education of the mother (which is often the case), or an interaction between the economic level and the rural/urban status or the region of the country.

1.8 The pattern of changes in the nutritional status of children according to age in developing
countries is very well known: usually, the indices start to decline at 6 months or so and reach a plateau around the age of 2 years. The results that are shown here are therefore in accordance to what is expected. In my opinion there is no need to describe this relationship so extensively, unless age-specific effects could be really demonstrated (which remains unclear here: cf. point 1.6 above).

1.9 I strongly question the way in which the breastfeeding duration variable was built. First of all, this variable mixes information about the current status (still breastfeeding or not) and retrospective information (duration of past breastfeeding). Secondly, this variable is likely to be very age-dependent. Thirdly, the current classification leads to put in the same category some children with very different patterns of breastfeeding (for example, a child aged 7 months who just stopped breastfeeding belongs to the same group as a child age 35 months who also just stopped breastfeeding). In addition, the total of percentages in each category does not reach 100% in 1991 (table 2).

1.10 In the result section, especially when bivariate analyses are commented, there are numerous assertions that are not really supported by the data (at least not supported by any statistical test). Let us give just one example: it is said that decline in WAZ was concentrated in children of uneducated mothers (fig. 2b). One can see from figure 2b that, from 1991 to 1998, the mean WAZ of children of uneducated mothers fell from -1.08 to -1.35 z-scores (i.e. a deterioration of 25%) whereas in children of the most educated mothers the mean WAZ fell from -0.38 to -0.45 z-scores (i.e. a deterioration of 18%). Is this difference (25% vs 18%) statistically significant? A way to test this could be to pool the data of both surveys and to test if there is a significant effect of the interaction term between the year of survey and the level of education of the mother. Even the fact that, in the multivariate model, there is a significant relationship between the nutritional status of children and the level of education of the mother in 1998, whereas this relationship does not appear in 1991, does not imply that the decline in the nutritional status was stronger among uneducated mothers. Unless the data of both surveys are pooled, the question of the importance of the changes in one category of an independent variable versus the others categories cannot be answered.

1.11 As a result of the point 1.10 above, the interpretation of the results in the whole paper should be more cautious that it is actually.

1.12 As a result of points 1.6, 1.8 and 1.9 above, the long paragraph in the discussion section about the age-specific effect of the economic status and the possible role played by breastfeeding is purely theoretical and is not supported by the data. For the same reasons, I cannot agree with the first two sentences of the conclusion.

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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 At the end of the first paragraph in the background section, the figures given for the under-5 mortality rates are not from the same source (DHS in 1991 and Unicef Statistics in 2002). There is no warranty that the mode of calculation used by the two sources is the same, so the figures may be not really comparable.

2.2 In the background section, page 4, it is said that region of residence(is) a notable confounder of the SES-malnutrition relationship in the country. However, no reference is given to support this assertion.
2.3 The last sentence of the background section is somewhat misleading since the authors give no details about the very important changes in national economy, nor about the macroeconomic factors that are supposed to have influenced the effects of the determinants that are studied (cf. also point 1.5 above).

2.4 - There is no indication of the sample size in the legend of the figures.

2.5 It could be helpful to give the p-values associated to the tests of parameters in table 3 (or at least to give an indication of the statistical significance with * for p<0.05, ** if p<0.01, *** if p<0.001).

2.6 The authors said that the East province should be distinguished from the South and Center provinces, because of food availability. So I wonder why they havent disaggregated the data according to this?

2.7 The authors indicate that source of non-drinking water wasnt available in 1998 and that this may lead to differences in the assessment of the effect of the community environmental status. So, why havent they simply omitted this variable when they built the community environmental status in 1991?

2.8 Some results seem to interestingly deserve commentaries or discussion that is lacking. For example, why the decline in the nutritional status seems to be more important in the major cities (as far as this could be confirmed by appropriate analyses)? Why a decline among boys and not among girls?

2.9 The reference by Ferguson et al. is not correct. The right title is estimating permanent income using indicator variables and not estimating permanent economic status (please note that I havent check all references so carefully; I found this error by chance).

Discretionary Revisions (which the author can choose to ignore)

None

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: No

Declaration of competing interests:

I declare that I have no competing interests