Author's response to reviews

Title: Factors associated with stunting among children according to the level of food insecurity in the household: a cross-sectional study in a rural community of Southeastern Kenya

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Author's response to reviews: see over
Reviewer 1:

• Major Compulsory Revisions

1) The title doesn’t tally with the results. Should be “Factors associated with stunting among children .......”

Methodology:

According to the suggestion, we changed the title to “Factors associated with stunting among children according to the level of food insecurity in the household: a cross-sectional study in a rural community of Southeastern Kenya”

2) Sampling method is not clearly described – although the authors mentioned that the selected households were within 2.2 km radius, there was no information on the total number of households and what was the response rate. It was also not clear what is the total number of children from those households and what was the response rate.

We added a description of the areas of this study in lines 108-110.
Regarding response rate, we added it in line 171, Results section.

3) Sample size calculation should be carried out to justify the total sample size achieved (404).

We added description of the sampling size calculation in the designing stage of the cohort study in Line 99-103.

4) The exclusion criteria should be described more in detailed ie: severely ill.

We added exclusion criteria in line 145-148 and described the details of the children excluded from the study in the results section, line 169-170.

5) Statistical analysis –
   a) what does “multiple logistic regression clustered by household” mean?
Cluster option was used for the analysis, because some children in the same household may be correlated and not independent. The point estimate would be same, but confidence intervals would be modified with this option. We modified the description of the statistical methods in the text (Line 151-155)
b) Why was the analysis stratified by food secure and insecure when the aim of the study was “to examine the influence of household environmental factors on the nutritional status of children”?

First, we thought that food security level was the most influential factor affecting stunting, but according to the results, it was not related to stunting. Even in households with enough food, stunting still occurred. Why? To investigate the reason, we conducted the stratified analysis. Second, because household environmental factors might differ according to food security level, they could have a different effect on the nutritional status of children; therefore, we added the stratified analysis by food security level.

c) Should present the details of how was household wealth quartile index determined by estimating asset factors through principle component analysis (PCA).

Actually, we explained about PCA-based asset index in the first manuscript, but we changed the description to improve understanding (line 126-129). Also, we added a reference (ref number [23]) in this sentence.

Results:

6) Why was “number of sibling not attending public school” instead of “total number of children in the family” reported?

When we considered the intra-household environment, we realized that children in the school-age group go to school or go outside to play, but preschool children stay around the house and mothers need to tend to them. This might have some adverse effects on feeding and care of other small children in the same household. Therefore, we focused on the number of preschool children in the same household, as stated in the Discussion section.

7) Should not describe the results which were statistically insignificant as though they were significant.

We changed the wording according to this suggestion.

8) The crude OR in Table 1 should be inserted in Table 2 together with the adjusted OR.

As suggested, we inserted the crude ORs into Table 2.
9) Not sure which factors were adjusted – should present in the footnote, below the table. As suggested, we added a description regarding factors adjusted in Table 2 (revised).

10) As food security was not associated with stunting, the analysis should not be stratified as presented in Table 3.

Even though not significant, the factors related to stunting in food secure and insecure groups are quite meaningful. Although food is secure, there are children with stunting. Why? For food insecure households, which factors are related to stunting? From a public health perspective, this information is useful in rural regions of Africa. Furthermore, some variables become significant after stratification. For these reasons, we would like to keep Table 3 in our manuscript.

11) Wrong interpretation of OR – “Children between 2 and 3 years old had about a 3.5 times increased risks of stunting compared with those aged 0 to 5 months (OR: 3.58; 95% CI: 1.33-20.10)” – is not increased risk – should report as “children .. had 3.5 times odds for stunting compared with …”

We changed the wording of this sentence according to the suggestion as well as other parts regarding ORs.

Discussion:
12) Discussion was not conducted according to results ie:- results which were not statistically significant were discussed as though significant
- Assumptions made not based on results – ie: assumed up to 2 years old, a caretaker or mother gives mainly breast milk and complementary food to children; last- or next-to-last-born children are less likely to have sufficient meals.

We removed discussion about the points above and left those with significant results only.

13) Results which were insignificant were reported to be “marginally significant” in the abstract.
As suggested, we revised the abstract. In the revised version, we added more detailed description.

- Minor Essential Revisions
  The style of reporting needs to be extensively edited.

  We think the style is following the instructions for authors. If not, please specify the parts that need to be edited.
Reviewer 2:

Major Compulsory Revisions:

1. Is there any scientific way of determining sample size used? Is the sample adequate enough to address the objectives of the study? The assumptions and the procedures used to determine the sample size need to be described well.

As suggested, we added a sample size calculation.

2. Moreover, the adequacy of the sample size for further sub-group analysis by food insecurity level is important. The study will lose power by doing so, mainly for non-sever food insecure group (n=108). Sample size could be one of the reason for some of the non-significance of known factors for stunting. Hence, interpretation of the results should be with caution.

Thank you for your suggestion. As we note, sample size is part of our study’s limitation. We added this issue in the limitation part of the Discussion section.

3. Data quality issue is very important. About 12 children are flagged in the analysis i.e. children with more than +/- 4 SD. There is a need to document well about the quality of the data and how measurement errors were minimized. Why 12 children have values beyond the expected limit? Did the authors standardized the data collectors for taking anthropometric measurements? If so, what was the agreement level? It is good to describe well how the inter and intra-observer errors have been minimized.

We added exclusion criteria in the Methods section (line 145-148). Also, we revised the Results section (line 168-170). Regarding children with more than +/- 4 SD, we recognized this as birth date recognition errors by caregivers because most children in this study area were born at home. Date of birth was not always recorded accurately, although most of them can be identified by immunization records. However, some remain unclear.

Regarding the standardization of data collection, we used the same team for all measurements and data collection by questionnaire was performed after intensive training. Specifically, one person measured height and weight; another person administered the questionnaire survey, etc. Therefore, we do not think our data are biased by the data collectors.

Minor Essential Revisions:
4. Since the study design is a cross-sectional design, risks may not be able to be estimated. Better to revise the title to associated factors instead of different risks. 
   As suggested, we change the expression from “risk” to “association”.

5. How were the sample children selected? Well description of the sampling procedure is important for external validity of the study.
   We added a description of the households in the study area. Actually, this study was not a sampling study. We registered all children in the households within a radius of 2.2 km from the health center. We described this in the Methods section.

6. Describing well the time of measurement of the anthropometric measurements in the HDSS cohort and the additional survey conducted is important to link the outcome with the exposure. It is also important to describe how frequent information is collected in the HDSS?
   Thank you for your suggestion. Although this study was conducted in a relatively small area compared with the HDSS program area, we used HDSS data to retrieve households for the study. We added information about the frequency of surveys in the Methods section.

7. The possible explanation given for result on food intake and stunting: children who had tea/porridge with milk in the previous 24 hours had 1.65 times higher stunting compared with those who did not have tea/porridge with milk is very, is not convincing. If information on the feeding habit of those children who did not get tea/porridge is now collected, the explanation given in the discussion section will be very weak. Lack of adequate information on feeding habit shall be documented well as one limitation of the study.
   We discuss the interpretation of the association of ingestion of tea/porridge with milk with stunting. However, the actual situation is still unclear regarding feeding practices by the mother and household. To understand the situation, we should conduct more detailed nutritional surveys with enough participants.
   But, due to limitations our study can not evaluate this issue in detail, so we added some text on the limitations.

8. Instead of interpreting odds ratios as risks, it is important to interpret it as the odds. The two are different and the current study employed a cross-sectional study design, where risks cannot be computed.
Thank you for the suggestion. Instead of using “risks” we used “associations” in the revised manuscript.