

Author's response to reviews

Title: Household food access and child malnutrition: Results from the eight-country MAL-ED Study

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Author's response to reviews: see over
June 25, 2012

Dear Editor:

Thank you for the opportunity to provide a revised version of our manuscript “Household food access and child malnutrition: Results from the eight-country MAL-ED Study.” Attached please find a point-by-point revision to each of the reviewer’s questions.

Sincerely yours,

William Checkley, MD, PhD
Assistant Professor of Medicine
Johns Hopkins University
Responses to Reviewers’ Comments

Reviewer 1: Sera Young

1. The HFIAS scale goes from 0-27, but the scales in the figures and the text go from 10-37. Your data will be more easily interpreted by those familiar with HFIAS if you keep scale consistent.

   In an earlier version of the manuscript we used the scale in the same form as other HFIAS work, but we changed it based on a consensus decision between co-authors. However, based on the reviewer’s recommendation, we have decided to change it back because we understand and agree that it is best to be consistent with previous work using this scale.

2. At lines 184-6, it’s written that the scores were skewed right “indicating low food insecurity” but in typical use of the scale, the higher the score, the more food insecure. So, the interpretation about food insecurity is exactly opposite here. This is important to clarify.

   The interpretation is now consistent with the direction of the scale.

3. I would encourage analysis of HFIAS by none, low, medium and high, as outlined in Coates, J., Swindale, A., & Bilinsky, P. (2007). Household food insecurity access scale (HFIAS) for measurement of food access: Indicator guide. Washington DC: Food and Nutrition Technical Assistance Project. NB that this procedure is more complicated than quartiles or even cut-offs. This would be particularly useful for Figure 1.

   We used the guidance in Coates et al. 2007 to form categories of food access insecurity. The results are below, and have been integrated into the manuscript. We have updated the text, and Tables 1 and 2 using these categories. In accordance with the guidance in Coates et al. 2007, we have continued to include food access insecurity as both a continuous and categorical measure.

<table>
<thead>
<tr>
<th>HFIA Category (Coates et al. 2007)</th>
<th>Frequency</th>
<th>Percent (HFIA prevalence)</th>
<th>Cumulative Percent</th>
</tr>
</thead>
<tbody>
<tr>
<td>Food Secure</td>
<td>296</td>
<td>37.5</td>
<td>37.5</td>
</tr>
<tr>
<td>Mildly Food Access Insecure</td>
<td>90</td>
<td>11.4</td>
<td>48.9</td>
</tr>
<tr>
<td>Moderately Food Access Insecure</td>
<td>217</td>
<td>27.5</td>
<td>76.4</td>
</tr>
<tr>
<td>Severely Food Access Insecure</td>
<td>186</td>
<td>23.6</td>
<td>100.0</td>
</tr>
<tr>
<td>Total</td>
<td>789</td>
<td>100.0</td>
<td></td>
</tr>
</tbody>
</table>
4. Using 4 as the cut off for food insecurity because it’s the sample median is one approach, but why not use the cut offs in Coates et al 2007? Your decision to not use standard cutoffs needs to be better justified.

*We changed this to use the cutoffs in Coates et al. 2007.*

5. Analyses of the impact of food insecurity sometimes control for SES and sometimes don’t. This paper is strong for differentiating the two. However the SES indicators that have been used are partly a measure of wealth and partly a measure of hygiene (see Humphrey in the Lancet 2009 about enteropathy). It would be useful to keep sanitation and water source separate from the rest of the characteristics in Table 3.

*We have separated water and sanitation out in Table 3 (now Table 1), and specified in the title that the table includes indicators of wealth and hygiene.*

6. In discussion, seasonality should be addressed. Are some of the variation in findings attributable to FI being measured at is peak or lowest points?

*We agree with this reviewer that seasonality is an important variable in the assessment of food insecurity. However, this feasibility study was not designed to assess the important role of seasonality in household food access insecurity. Additional information on the season of data collection at each site is included in the supplementary materials. We will be assessing seasonality more closely in the MAL-ED cohort study, in which we are measuring food access insecurity every six months based on child enrollment, not time of year. This approach will provide data on fluctuations within country between similar children due to seasonality.*

7. It would be helpful for the reader if two additional columns were added to Table 1: mean and SD HFIAS score and the mean SD SES score (as calculated using principle components analysis, ideally of those characteristics not pertaining to sanitation, per above ). This will prepare your audience for the interesting observation made around lines 254.

*We thank the reviewer for this comment and we have made these additions to Table 1 (now in supplementary online materials).*

8. At line 255 you call the observation a contradiction, but in the next paragraph you explain how it isn’t, because food access security is not merely an indicator of SES. Consider rephrasing.

*We have edited this sentence.*

More Minor Suggested Revisions
9. Was back translation of surveys done? How was their fidelity tested?

All MAL-ED sites were previously established research sites prior to the initiation of this study, with site-specific protocols for translation and back translation. Two sites chose to use English forms in the field (administered by field workers), three sites used the translated and English versions together, and the remaining sites used only translated forms. Some sites hired translators to conduct back translations, while others held field worker workshops to discuss the meaning of each question. In the case of translated forms, copies were shared with the Data Coordinating Center to validate the translations.

10. There is irregular use of acronyms, e.g. HFIAS is used then later spelled out. Same for SES, HAZ. Also, sometimes you use food access insecurity, other times food insecurity; consistent use of food access insecurity is more accurate.

*We have addressed these inconsistencies in the text.*

11. Please clarify the study design at line 99. Are you describing enrollment for the main study or pre-study pilot activities? Or are these participants part of the main study? How will the cases and the control children be selected? How many malnourished per site and at what age will they be enrolled? Etc. Alternatively, you could skip details on the controls and just be clear about participants in this current study.

*We thank the reviewer for pointing out this potential source of confusion for readers. The pilot study was conducted in preparation for the cohort study, not the case-control study. In the case-control study, cases are selected based on a severe underweight measurement (WAZ < -3). We chose to exclude reference to the case control study from the manuscript to simplify the study description for and not confuse our readers.*

12. Lines 44-45 phrase seems to be missing words.

*We edited this sentence.*

13. Lines 59 and 60 “may” should become “can”.

*We have made this change.*

14. 135 physical consequences include hunger, so “hunger” can be dropped.

*We made this change.*

15. 146 insert “WHO standard” before median. It could be interpreted as meaning the population median as it currently reads.

*We made this change.*
We made this change.

16. 154 typo

We fixed the typographical error.

17. 158 add “infant” before age

We have added the word “child” before age.

18. Line 232 which test for heterogeneity?

We clarified that we conducted a likelihood ratio test comparing a full model with interactions between food access insecurity score and country dummy variables with a reduced model without those interaction terms.

19. line 238 might --> “likely”

We have made this change.

20. 281 will you do repeated measures of FI?

We have clarified this in the text.

21. 292 may be clearer to refer to this as the pilot study

We have made this change.

22. Line 297 with current data about effect sizes, a sample size calculation could be done.

We have removed this comment about effect sizes in the manuscript. We are confident that the lack of significant relationship between WHZ is a true finding, and not a result of insufficient power.

23. Line 301 please give example of how some items are culturally dependent.

Researchers in the Pakistan study site expressed general concerns about bias due to cultural stigma against reporting food insecurity. However, they felt the questionnaire was robust to this threat. In addition, researchers in the Tanzania site noted that reporting bias around food access insecurity might be more serious in predominantly agricultural areas, such as the Tanzania site, than in more urban areas. We have added the Pakistan example to the text.

24. Line 409 reads awkwardly
25. Please spell out the names of the countries in all figures.

We would kindly request that the reviewer considers that we keep our current abbreviations for each country. Some names are long and may crowd the figures. We have provided a careful legend for each figure explaining the abbreviations. Moreover, these are standard abbreviations that we have chosen as a network for all future publications. We hope this is acceptable for this reviewer.

26. With your crowding metric, what type of room did you ask about? Sleeping rooms or any room in the house?

We have clarified this in a bullet under Table 3 (now Table 1). This is calculated as the number of people who usually sleep in the house divided by the number of rooms used for sleeping.

27. I find it distracting and unnecessarily wordy to call out figures and tables. For example, lines 97-99 could be streamlined by dropping the sentence that begins with “Table 1” and just putting (Table 1) at the end of the first sentence. This can be done throughout the MS, e.g. lines 174-5.

We have made this change in the text.

28. The recommendation to not use the Household Hunger Scale (the name of which should be mentioned in the text) is hard to defend. If results are similar with fewer questions, it can be appealing for many scientists. Line 274-- why do you want to characterize the full experience of food access insecurity for your study? Perhaps stating that it was not appropriate for your study would be more accurate. Also, please consider including results using HHS in online supplementary material (line 272)- it would be nice for readers to see them.

We agree with the reviewer that it would make more sense to use the simpler (smaller) scale if possible. Recent validation work in multiple countries (single country studies) has produced mixed results, leading investigators to suggest a shortened version of the scale, called the Household Hunger Scale, comprising only the final three items related to hunger. However, in our study, the adapted version of the scale did not achieve statistical significance, suggesting that the full scale may be a better measure of chronic malnutrition or that these two different scales capture different information. We have edited our statements in the discussion and included the HHS results for use in an Online Supplement.
**Reviewer 2: Lawrence L. Haddad**

**Major Compulsory Revisions**

1. The paper implies (not explicitly) that the magnitude and significance of the relationship between the HFAIS and ZHA is similar in all countries (lines 225-227: "did not vary significantly"), I assume an F test was conducted comparing a model with and without the 7 country dummies interacted with HFAIS (please be clearer), which rejects the interaction model at 0.17 (again I am guessing). This is quite a weak test-- would some of the interaction terms would be significant individually even if as a group they are not? Can we seem some more exploration of this or is the power too low? Can we at least see the regression with the interaction terms included? This is important because we want to know if the index works better in some contexts than others in identifying children at risk of growth faltering.

   *We conducted a likelihood ratio test comparing a full model with interactions between food access insecurity score and the eight country dummy variables with a reduced model lacking those interactions. The results of this test provided evidence of the extent of heterogeneity across countries in the relationship between food access insecurity and HAZ.*

   However, we agree with the reviewer that it is also a good idea to look at the individual interaction terms; however, none of the individual interaction terms achieves statistical significance at the 0.05 level:

<table>
<thead>
<tr>
<th>INTERACTION TERM</th>
<th>COEF</th>
<th>SE</th>
<th>Z</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>fsscore:factor(country_id)BR</td>
<td>0.009078</td>
<td>0.027745</td>
<td>0.327</td>
<td>0.74361</td>
</tr>
<tr>
<td>fsscore:factor(country_id)IN</td>
<td>-0.037632</td>
<td>0.029689</td>
<td>-1.268</td>
<td>0.20534</td>
</tr>
<tr>
<td>fsscore:factor(country_id)NE</td>
<td>0.003593</td>
<td>0.033940</td>
<td>0.106</td>
<td>0.91571</td>
</tr>
<tr>
<td>fsscore:factor(country_id)PE</td>
<td>0.018439</td>
<td>0.032711</td>
<td>0.564</td>
<td>0.57314</td>
</tr>
<tr>
<td>fsscore:factor(country_id)PK</td>
<td>-0.043168</td>
<td>0.030164</td>
<td>-1.431</td>
<td>0.15282</td>
</tr>
<tr>
<td>fsscore:factor(country_id)SA</td>
<td>-0.013320</td>
<td>0.031035</td>
<td>-0.429</td>
<td>0.66790</td>
</tr>
<tr>
<td>fsscore:factor(country_id)TZ</td>
<td>-0.042173</td>
<td>0.033788</td>
<td>-1.248</td>
<td>0.21235</td>
</tr>
</tbody>
</table>

   Thus, we found that the best way to summarize lack of heterogeneity was simply with a likelihood ratio test. We have clarified this in the Methods and Results sections of the paper. We have also added the results of the full model (with interactions) in an online supplement.

**Minor Essential Revisions**

2. Pls explain why the final model is better than the full model.

   *We removed SES indicators that were not statistically significant for the sake of*
parsimony, but this did not change the estimate of the relationship between food access insecurity score and HAZ. We have clarified this in the text.

Discretionary Revisions

3. I would also like to see some more discussion of why the ZWH estimated coefficients on HFAIS were not significant.

We have included the following text in the manuscript to explore this issue:

“Given different risk factors for wasting and stunting, and the weak correlation between these measures in our data, it is not surprising that food access insecurity is associated with faltering in HAZ but not WHZ. In addition to different risk factors, growth faltering in WHZ tends to occur at younger ages and result in higher mortality than faltering in HAZ (1). Given the age of children enrolled in this study (older than 24 months), they were more likely to be stunted or healthy than to be wasted. Further research is warranted on approaches to expanding this household food access insecurity measure to more effectively capture factors associated with risk of wasting.”

4. It would be good to get a better sense of the magnitude of the results on ZHA (elasticities perhaps?) and also in relation to other studies using some version of the HFAIS which aim to link to anthro.

We have included the following text on this issue in the manuscript:

- Line 222: “In pooled regression analyses, a 10-point increase in food access insecurity score was associated with a 0.20 SD decrease in HAZ score (95% CI 0.05 to 0.34), controlling for water source, mother’s education, and people per room.”
- Line 264: “Our results indicate that food access insecurity is not simply an indicator of SES, but is also independently associated with growth faltering. The effect of a five-point decrease in food access insecurity was roughly comparable to the effect of a five-year increase in mother’s education on HAZ, and was approximately equal to one-third the effect of access to an improved water source. Although our analyses reveal that food access insecurity is independent of these socio-demographic indicators, these relationships warrant further exploration. The complexity of these relationships underlines the utility of a simple measure, such as the HFIAS, that could potentially predict growth faltering in children.”

5. I did not find the diagrams helpful, I would drop them.

We felt that the other reviewer found the figures to be helpful, and have revised them based on other feedback. We find them useful for the following reasons, and have chosen to keep them:
- **Figure 1:** Shows the distribution of food access insecurity scores by country site. We believe this visual representation of the distributions is helpful in assessing within and between site variations in responses.

- **Figures 2 and 3:** Show the distribution of HAZ and WHZ by country site, again allowing the reader to understand the patterns of nutritional indicators at each site. Because the relationship between WHZ and food access insecurity score is not significant, perhaps we could remove Figure 3 if the editor and reviewer strongly feel that we should.

- **Figure 4:** This figure shows one of the key findings of the manuscript: the food access insecurity is linear related to HAZ.