Author’s response to reviews

Title: Socio-demographic and Facility-Based Determinants of Perceived Quality of Nutrition Services of Pregnant and Lactating Adolescent Girls in Trans-Mara East Sub-County, Narok County, Kenya

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Author’s response to reviews:

27 June 2019

To
Dear Sir/Madam,

Re: Resubmission of the manuscript ID NUTN-D-19-00001R2 titled, ‘Socio-demographic and Facility-Based Determinants of Perceived Quality of Nutrition Services of Pregnant and Lactating Adolescent Girls in Trans-Mara East Sub-County, Narok County, Kenya’

We thank the editorial team and the reviewers for providing useful comments that has aided in the improvement of the manuscript. We hereby provide point-by-point responses to the reviewers’ comments. In addition, we have attached the manuscript with the changed areas highlighted in red. It is our pleasure to re-submit the manuscript for consideration in BMC Nutrition.

Reviewer reports:

Reviewer 3:

Comment 1. My comment was on consent process.

That 40% of pregnancies happened between 14-19, and the age of consent in Kenya is 18 years. You mentioned that those underage had their parents provide consent. This is an issue because she may not actually come with her mother or adult, and may come to clinic with her partner instead.

Response 1: As previously pointed out in our manuscript, the legal age for consenting in Kenya is 18 years and above. It is clearly stated that for those below the consenting age, if they are interested in being included in the study, and they provide assent, then we have to additionally obtain consent from the parents/guardians. In the current study, since most of the study participants were still staying with their parents/guardians, these provided additional consent.

Comment 2: the recommendation of providing farms to the youth sound nice, but I think it is far-fetched and too ideal.

Response 2. We thank the reviewer for raising this point. However, we wanted to emphasize that this is not far-fetched as the Government in Kenya is geared towards implementation of the
youth-oriented programs, of which farming is included. This is part of addressing the demographic dividend in the country.

Reviewer 4:

Comment 1: Please include all comments for the authors in this box rather than uploading your report as an attachment. Please only upload as attachments annotated versions of manuscripts, graphs, supporting materials or other aspects of your report which cannot be included in a text format.

Please overwrite this text when adding your comments to the authors.

Response 1: All our responses were provided in the text box which had to be filled in with our responses. We did not attach our responses as additional documents.

Reviewer 5:

Comment 1: Abstract

The general idea of the abstract is that it should be understandable without reading the article. This has been a challenge. The following issues were not clear on an initial reading of the Abstract.

p2, line2, What is IEC?

p2, line 10, The goal of assessing "the socio-demographic and facility-based factors as proxies to access to perceived quality of nutrition-specific and nutrition-sensitive services in the context of adolescents' access and utilization of nutritional advice and services" is rather wordy and difficult to understand. In the Conclusion of the Abstract, the goal is more succinctly phrased "critical facility-based determinants of utilization of nutrition services".

p2, line 41, Is "environmental hygiene (80.7%) and basic personal hygiene (69.4%)" referring to the examination facility, the subject, or the subject's home, or something else altogether?

p2, line 49, Public health centers compared to what? We have to know what the alternative of the public health center is in order to make sense of the AOR.

p2, line 51, The same is true for the distance. Can we assume that the comparison is with a longer distance?
Response 1. All the issues raised above in the Abstract section have now fully been addressed.

Comment 2:

Introduction

p3, line39, Suggest changing "in the access and utilization of nutrition services" to "in the access to and utilization of nutrition services." Without that, the suggestion is for "access of nutritional services", which would be awkward and incorrect.

Response 2. This has been implemented as suggested by the reviewer.

Comment 3: Methods

p5, line 51, Change 'relevance" to "relevant".

p6, line 38, This formula is a bit confusing. In line 28, p is set at 50%. Where did this come from? Statistic earlier indicated that 7.4% of adolescents are pregnant with their first child. We might estimate from there the number of adolescents who are pregnant with any child or are lactating. It might reach 15-20% but unlikely to reach 50%. But to return to the formula, q= 1-p, so why is the formula of pq written as 0.5x0.8283? This might suggest that p=1-0.8283 =0.1717 or 17% which fits nicely with our estimate above. But clearly the authors used p=0.5 and q=0.5 because that is the only way to yield n=384.

Response 3: We really appreciate the reviewer’s perspective of the sample size formula. However, since our focus was access to nutrition services the prevalence for sample size calculation was based on access rather than status of sampling unit. Since there was no existing prevalence on access, we opted to set p @ 0.5 to get the basic minimum of the sample size required if we were to set alpha at p=0.05. In this case, the exact prevalence value would only give us sample size less than 384. So we still feel there was no statistical harm in being explicit on the road map to determining sample size.

Comment 4: p6, line 49, But all of this begs the question of why bother with any of that since that was not the formula that was used. The authors tell us that z was actually set to 1.64 for p<0.10. Why bother to include lines 21-48 at all? It is only confusing. Is p<0.10 the statistical criteria for the study for significance instead of the usual p<0.05?

Response 4: Again, we thank the reviewer for pin-pointing this critical aspect. We estimated the sample size based on z=1.64 to demonstrate non-effect on sample size on our results given that
our sample size was 337 as opposed to 384 which could have been the minimum. Any hypothesis rejected or accepted would find explanation for validity within the range of significance set at alpha=0.01 and alpha=0.05. In some cases, authors opt to display the 90% confidence interval, which is acceptable in surveys. As such, we still request to be allowed to display the two ranges of significance as a better option in statistics than reporting rate.

Comment 5: p8, line 24, What is a FGD?
p8, line 27, What is a CHV?
p8, line 41- p9, line8 Questionnaire-Interview Method. This is a very critical section since it is the proxy end-point of the study. These personal health practices are "assumed to have high chances of accessing quality of nutrition services." Subjects were scored as high or low utilizers based on these personal practices. By my imperfect count, there are 7 practices enumerated here. I would appreciate it if the authors would specifically enumerate these personal practices that went into this important score. Also, very importantly, these scores are never reported in the results section. Please report, probably in table 1, how many girls were high and low utilizers. In fact, as a suggestion, it might be interesting to create a whole new table of the 7 constituent variables that make up the proxy measure and describe how many girls had each, together with the Batt-scores and final proxy scores. This is an important variable. Just a suggestion.

Also, it would be very helpful and would strengthen your manuscript considerably if you could validate this proxy measure of utilization somehow. This jump from personal health practices to nutritional consult utilization seems quite a stretch and much more related to girl's mother's education, urbanization, and family wealth. Who else has used or devised such a proxy measure or directly compared personal health practices with utilization among women or girls?

Response 5: These are great suggestions which we believe are still beyond the scope of the current manuscript. We ideally explained the methodology used to get the Batt values which were loaded into the various models following validation. As such, we request to maintain the information as it is currently.

Comment 6: p9, line 49 Quantitative data analysis. Tables 1 and 3 have extensive Adjusted Odds Ratios, but the method for these is not described. What variables are adjusted for?

Response 6: The approach that we used in the current manuscript is pegged on the fact that Odds ratio or Crude Odds ratio are obtained when you are considering the effect of only one predictor variable i.e. your equation consists of only one independent variable. The data we presented are actually on crude odds ratio and NOT adjusted odds ratios.
We have now included the following statement to address the reviewer’s concern: ‘Bivariate analysis based on odds ratio were done to determine how each of the socio-demographic and facility-based variables relate with perceived quality of nutrition services among pregnant and lactating adolescent girls.’

Comment 7: p10, line 12-13, I suggest changing, "Diet diversity score tested the ability of simple dietary diversity scores to predict micronutrient adequacy of diets of women of reproductive age." to "A simple dietary diversity score was used to predict micronutrient adequacy of diets of women of reproductive age."

Response 7: This has been changed as suggested by the reviewer.

Comment 8: p10, line 17, What is WDDS?

Response 8: This is now defined as Women Diet Diversity Score (WDDS).

Comment 9: p10, line 17, Change "intake10" to "intake" or "intake [10]".


Response 9: These have been corrected as suggested.

Results

Comment 10: p12, line 27 Change 18.4% to 37.4% and 21.0% to 2.1% to conform with Table 1.

Table 1. The fact that the AORs are against the proxy measure is not at all clear here. It needs to be described in the title and footnote fully, especially if there is adjustment for confounders. The AOR values for Education and Religion are especially meaningless since the references group is astonishingly small.

Response 10: These issues are addressed under Reviewer 5, Response 6.

Comment 11: What are the reference groups for Adolescent status and Source of Food?
I suspect an error in the Religion since 95% of girls apparently have a religion but only 14 apparently declare one. Rather odd.

However, the text indicates that 95% of the girls indicate they are Christian.

I note that the AOR = 0.624 in Table 1 is not the same as the AOR=0.61 on p12 for Dependency on parent or guardians.

Also, why is the first AOR on the table for 0.490 for Adolescent status not marked as statistically significant?

Response 11: All these issues have been currently addressed in the revised manuscript.

Comment 12: Why is age not included in Table 1? This is a very important descriptive variable and probably a covariant in the logistic model.

Response 12: The age category has now been included on Table 1.

Comment 13: p13, line 5, This section and the Figure are a bit confusing. It would be helpful if the authors added a line or two at the beginning of this section on where these data came from and how they were compiled into this graph. I am not sure if they were from a survey or from a focus group, for example. If I understand correctly these are the responses of mothers of the girls when asked what nutritional services they found helpful. Also, I find the term "environmental hygiene" vague. What does this mean?

Response 13: Since the statements were obtained from FGDs, we have currently included the following statements to address this comment, ‘Further analysis to qualitatively explore the extent to which the adolescents were satisfied with nutrition services was mainly assessed by adolescents’ mothers in a focus group discussion (FGD).’

Comment 14: p 14—Table 2. Why are AORs not included in this table as with Table 1 and 3?

Response 14: The table only displays level two results. The variable under consideration was a measure of quality which attempted to display the nature of food groups. This component was included in the value labels for assessing level of utilization.

Comment 15: p.14, lines 15-20. There should be no statistical analysis here. The chi square for uneven distribution is meaningless. "Low mark (at most 3 food groups) was registered by 36.8%
which was significantly below the expected frequency of 33.3%.” 36.8% is not less than 33.3% and is meaningless anyway. Please rephrase.

Response 15: We thank the reviewer for his comment. However, we would like to state that the analysis in question was a simple z-test against expected frequencies and not a Chi-square.

Comment 16: Table 3. Same comment as above about include a description of the AOR being against the proxy. Include in title and footnote. It would be helpful here to have the frequency and percent data for this table as for Table 1. These table could be made consistent if the p-value was removed from.

Response 16: Given that we wanted to see only the extent of the facility-related determinants of overall utilization of critical services and products, we felt that the odds ratios are adequate. However, we still remain to be advised further whether the percentages would add value to the manuscripts.

Comment 17: Table 3. CBO, NGO, FBO, and IEC, need to be defined in a footnote. What are the reference groups? How can Methods of conveying messages have two reference groups?

p14, line 39, How can a participant score a 5 on 4 dichotomized items?

Response 17: All the abbreviations are defined below the Table. However, each item was measured on 5-point Likert scale and a participant would score a maximum of 5 out of 5.

Comment 18: p15, line20, "However, even though dispensaries were frequently used, it had no influence on overall utilization of adolescent's nutrition services." This is a case in point where the authors know about the frequency of a certain usage but readers do not because it is not in Table 3. Please add this information.

Response 18. In order to address this comment, we have re-phrased the statement to read, ‘The other types of health facilities other than public health centres had no influence on overall utilization of adolescent’s nutrition services (Table 3).

Comment 19: p15, line 27, How can both distances be statistically significant if there is not another distance in the questionnaire to compare these against? Please inform us.

Response 19: The reference for this is now provided.
Comment 20: p15, lines 34-44, Again, these AORs of 7.85 and 3.91 are interesting but no comparison is made. 7.85 times what? 3.91 times what? We need to know these things.

Response 20: Currently, clarity is provided as a footnote under Table 3 to take care of this concern. It now reads, ‘For the initial regression analyses, the factor marked # was used as the reference group. However, to derive the OR for the original reference group, all the categories under each section were collapsed to provide reference.’

Comment 21:

Discussion

Conclusion

p20, lines 51-59 Remove the reference of 36.6% being higher than 33.3%. Is 33.3% an acceptable number of malnourished people just because it is 100/3?

Response 21: This has now been removed as suggested by the reviewer.

We look forward to the publication of the manuscript in BMC Nutrition journal.

Sincerely,

Prof. Collins Ouma