Author's response to reviews

Title: The prevalence of underweight, overweight, obesity and associated risk factors among school-going adolescents in seven African countries

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Author's response to reviews: see over
June 4th, 2014

Editorial Board, *BMC Public Health*

RE: Resubmission of “The prevalence of underweight, overweight, obesity and associated risk factors among school-going adolescents in seven African countries” manuscript for review by *BMC Public Health*.

We would like to resubmit the above-named paper to *BMC Public Health*. We have revised the manuscript in order to address the comments provided by the reviewer. Most of the revisions were minor clarifications and corrections for omissions about the method of analysis. One significant change is the addition of a sensitivity analysis to determine if our grouping of individuals during regression analysis produced similar results to an alternative method of analysis, splitting individuals into only distinct overlapping groups. Our analysis did not suggest that there were significant differences in results between these two methods of analysis. We have attached our responses to the reviewer comments to this letter as requested.

All authors contributed to the project and can take responsibility for its contents. We have no conflicts of interest to declare.

I will function as the corresponding author and can be reached at the address below or by e-mail (Jason_Randall@cpe.umanitoba.ca). Thank you in advance for your consideration of this submission.

If there are any further requirements, please do not hesitate to contact me.

Respectfully submitted,

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Reviewer's report
Title:
The prevalence of underweight, overweight, obesity and associated risk factors among school-going adolescents in seven African countries
Version: 3
Date:
16 May 2014
Reviewer:
Rahim Moineddin
Reviewer's report:
Comments on ‘The prevalence of underweight, overweight, obesity and associated risk factors among school-going adolescents in seven African countries ‘

Reviewer comment:
The authors used cross-sectional data from the Global School-Based Student Health Survey (GSHS) collected using a clustered sample design. The authors selected a subset of data from the following African countries: Benin, Djibouti, Egypt, Ghana, Mauritania, Malawi, and Morocco, as part of the GSHS. The data are freely available, and I downloaded the data from the public domain. There is sufficient information on the sampling such as stratum and weights that must be used in the course analysis. Ignoring sampling weights and stratum will produce biased estimates and incorrect standard deviations (and standard errors).

I haven’t seen anywhere in the paper that the authors treated the data as a non-random sample and used these sampling information in their analysis. The Stata software that authors used for data analysis has excellent features for analyzing complex survey data. It is not clear whether reported unadjusted results such as percentages and other rates are weighted or not (for example table 2).

Reply:
We did utilize the survey options in STATA to adjust for the complex survey data. A sentence has been added to the end of the analysis section to clearly state that the design of the sample was taken into account. Thank you for noticing this omission in our methods description.

Reviewer comment:
In table 1 it is not clear what code 0 implies and what code 1 implies.

Reply: This is the numeric value that was assigned to the responses for the analysis. A note has been added to the table to clarify what these values mean and how they are interpreted by STATA during analysis.
Reviewer comment:
There is a section on assessment of heterogeneity of the risk factors. In the method section authors wrote ‘There is the possibility of heterogeneous effects of these factors between the countries in these data. To test this, the regression analyses were done using a dummy variable for each country and risk factor combination. A Wald test was used in post-estimation after the regression to determine if estimates varied significantly between these dummy variables. A significant result would indicate that the effect of the variable was heterogeneous among the countries’. It is unclear what this is meant to accomplish. Then authors used Stata Statistical Software: Release 13 as a reference for their method. I recommend authors provide a pre-reviewed book or article for their approach.

Reply: A previous reviewer had expressed some concerns over whether there would be heterogeneity between the countries analyzed, so we added a brief analysis to show that this was not a significant issue in these data. I think a lot of readers would not be interested in this information but a few, such as the previous reviewer, would be very interesting. Therefore I think it makes a useful contribution to the paper and the subject area.

The reference noted was intended for a sentence that was missing. It must have been accidentally deleted during the editing process, somehow. This mistake has been fixed so that it is clear that the reference is for the Statistical package used for the analysis.

Reviewer comment:
The authors used logistic regression to investigate associations between risk factors and over/under-weight status. It is unclear what the outcome is. My take is that the authors combined under/over-weight into a single category and estimated the probability of this dichotomous outcome versus the probability of being normal weight? An alternative methodology would be to use multinomial logistic regression (where the 3-level outcome is underweight, normal weight and overweight) and estimate the impact of country and other factors on the probability of falling into one of these 3 mutually exclusive categories.

Reply: We did not combine under/over-weight in the analysis. I’ve changed the first sentence of the paragraph on the logistic regression analysis because it did seem to suggest that we were combining those groups. We did combine overweight and obese for the regression analysis since there were comparatively few obese individuals in several of the countries. Our analysis compared those who were underweight to those who were not underweight in one regression analysis. Then those that were overweight/obese were compared to those that were not overweight/obese. Finally, the obese were compared to rest of the population.

Using multinomial regression would be another way to assess the data. However, I have had some issues describing the use of multinomial regression with STATA. STATA’s multinomial regression outputs relative risk ratios, rather than odds ratios. Although these approximate odds ratio they are slightly different. Explaining this difference and preventing
readers/reviewers from interpreting the output as risk ratios was problematic in my previous paper utilizing this type of regression.

I prefer to analyse restricted sub-sets of the population using logistic regression instead. Therefore, in response to your comment, I have added a brief discussion of a sensitivity analysis for analyzing these groups as entirely separate groups as would be done in multinomial regression (i.e. underweight vs normal weights, overweight vs normal weight). The results are essentially identical. The OR for male sex is no longer significant for being overweight due to the exclusion of the underweight males from the regression. A couple other marginally non-significant variables become very marginally significant, but this could just be noise rather than a true difference. The effect size estimates and their confidence intervals are still nearly identical between the different methods of comparison.