Author's response to reviews

Title: Correlates of Objectively Measured Overweight/Obesity and Physical Activity in Kenyan School Children: Results from ISCOLE-Kenya

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Responses to Reviewers and Editor Comments

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Title: Correlates of Objectively Measured Overweight/Obesity and Physical Activity in Kenyan School Children: Results from ISCOLE-Kenya

Reviewer 1

Manuscript needs to be shortened in some specific sections and redundant text.

• As advised, we worked on the length of the manuscript in order to make it more concise.

Major Revisions:

Background
- Page 4, 2nd paragraph. The classical and overall guidelines for 5-17 yrs old are 60 min of MVPA as the authors stated. However, other cut-offs have been suggested to be related and with age and gender-specific information (Andersen et al. 2006; Jimenez-Pavon et al. 2013). Then, this reviewer suggest to cite these different cut-off and to consider the possibility of performing sensitivity analyses using them.

• We acknowledge that there are other cut-offs as suggested by the reviewer; however, these WHO physical activity (PA) guidelines are very similar to those used in the UK, USA, Australia, and Canada. As such, we opted to cite these global guidelines keeping in mind that they may be suitably implemented in a country like Kenya, which has no PA guidelines of its own.

- Page 5, first paragraph. The authors mention that growing numbers of research studies are not nationally representative. Do the authors suggest that the current manuscript is nationally representative? Please explain that.

• One of the issues that we sought to highlight was the lack of nationally representative data on childhood PA and overweight/obesity in Kenya. While this current manuscript was not nationally representative, we suggested the importance of conducting comprehensive research studies such as the ISCOLE study at a national level. This was discussed on page 17.

- In overall, the introduction should be shortened and focused in the main aim.

• We thought that the introduction provided a good rationale for the study as is; however, we shortened the length of the discussion and conclusion sections.

Methods
- Page 6, 2nd paragraph. The sentence “Given that much of the knowledge.....in north America and Europe” needs a reference/s to support that affirmation before the next “there is...”.

- We have now included references to support this affirmation.

- Page 8, direct measures: the cut points used are based on Treuth et al validation; however, this reviewer wondered whether this validation is useful for PA GT3X. Moreover, I suggest performing sensitivity analyses by other cut-off as you know there are important differences by authors.

- The ISCOLE-study is an international study conducted in various countries around the world; therefore, for standardization and comparability, two cut-points were chosen and used to create activity levels based on intensity counts for all countries/sites: Treuth et al. (data were processed using 1 minute epochs) and Evenson et al. (data were processed using 15 second epochs). Both are validated for use in child and youth populations.

- We re-ran the analyses included in this manuscript using the Evenson et al. cut-point and found that the relationships determined remained the same; hence, most likely not a function of the cut-point used. We thought that this offers more credence to our findings, and hope that this will satisfy the reviewer (see attached tables file).


Results

- The 1st paragraph seems to be more appropriate of the sample description for the methods section.

- We have now moved this to the methods section.

- Page 10-11. Participant and parent characteristic: this big paragraph should be simplified as redundant information should be avoided.

- We have simplified this section to avoid redundant information.

Discussion

- Page 13. Second paragraph: I would suggest starting the discussion section by a summary of the main results of the paper.
We have now included a paragraph that includes the main findings of the paper.

- Page 16, parental factors paragraph. This positive association is quite curious as is in the opposite direction of expected based in developed countries findings (Jimenez-Pavon et al. 2010; Jimenez Pavon et al. 2010). Then, of relevance should be to discuss the different results found in undeveloped countries compare to developed countries. It is important to note that in the current study to have higher SES give the chance to better food availability and to sedentary behaviours. However, in developed countries higher SES provides the better access to sport facilities and healthy foods as well as the parent are more aware of the real necessity of doing PA for prevent against obesity.

- Page 18, 1st paragraph. AS mentioned before these results are also inverse to findings in Europe (Jimenez-Pavon et al. 2010; Jimenez Pavon et al. 2010), then please include this references and locations in the text.

We agree that this positive association is quite interesting, and went further to discuss possible reasons why this might be the case on page 16-17. Our team has also just concluded a systematic review that includes aspects of the influence of SES on overweight/obesity and PA in Sub-Saharan Africa’s children (submitted and under review). A similar positive association with SES was determined from this review. These findings are supportive of our affirmation that socio-cultural constructs and their implications on body weight and PA from developed countries may not always be reproducible in the Sub-Saharan context.

However, we included references (such as those suggested by the reviewer) to support the affirmation that the results are inverse to findings from developed countries.

Reviewer 2

Major Revisions:

My main concern is the use of “underweight” (classified by a ZBMI <-2SD) which is an indicator of acute under-nutrition and more easily reversed when conditions change. Authors should have used instead the indicator of “stunting” (classified as a Z-height-for-age <-2SD) which shows chronic malnutrition and has much greater health implications for the future of the children. When conducting studies on the overweight/obesity prevalence in low and middle income countries it is very important that the prevalence of stunting is known. BMI works extremely poorly when there is a high percentage of stunting and in some populations shouldn’t be used at all. Also, if there is a high prevalence of stunting all the physical activity data will need a different assessment because a stunted child cannot cope with physical activity guidelines that were set for well-nourished children. Stunted children also have considerable less percentage of muscle mass and overall much less energy. I advise the authors to read the following papers (Wilson et al., 2011; Bogin and Varela-Silva, 2012; Varela-Silva et al., 2012).
strongly advise the authors to remove the prevalence of underweight and replace it by the prevalence of stunting.

- While we respect and appreciate this reviewer’s comment/suggestion, we would like to point out that this was not the focus of our paper. We were mainly interested in correlates of overweight/obesity and PA and only reported on the healthy and underweight categories to offer a complete picture. Using BMI-for-age to obtain categories of underweight, healthy weight, overweight, and obese for 5-17 year olds is acceptable WHO methodology that is often used in this context. A change to height-for-age categories would require significant refocusing of the paper, inconsistent with the main aim of the paper.
- However, we have taken note of this recommendation as it may be implemented in future work with this data.

Minor Revisions:

1. Abstract. Page 2, line 1. Please replace the word “plague” with some other word such as “burden”

- We have made a change from “plague” to “burden”.

2. Background. - The opening sentence doesn’t read well. Instead of “Physical activity and nutritional transitions around the world have resulted in a shift towards more sedentary lifestyles...” Please consider using for example: “Nutritional and behavioural transitions around the world...”

- The concepts of a “physical activity transition” and a “nutritional transition” are well described in literature; hence our choice of the terms. Further, both are behavioural transitions. We therefore opted to leave the terms as is.

3. Background. Consider changing the expression “normal” weight to healthy weight”

- We have made a change from “normal” to “healthy” weight throughout the manuscript.

4. Background/Results. The first paragraph under the heading “Results” should still be included in the Methods.

- We have now moved this to the methods section.

5. BMI of the parents are based on self-reported height and weight. It is necessary to state in the paper the limitations of relying on self-reporting for height and weight because people tend to overestimate height and underestimate weight.

- We have included a brief statement indicating that there are limitations associated with use of self-report data on page 16. However, it is important to note that these relationships
have not been confirmed in the Sub-Saharan context. Our team recently submitted a manuscript exploring the relationships between self-report and directly measures PA and sedentary behaviours in children in Kenya which highlighted some similarities and some differences in the discrepancies between self-report and direct measures in the Kenyan context compared to findings in developed countries.

**Editor**

"As highlighted by Reviewer 1, the manuscript is very long. I am satisfied with the length of the introduction as this provides a good rationale for the study however the discussion is very long and quite repetitive of the results. I recommend condensing the discussion where possible. The conclusion section is very long and contains more of a discussion rather than conclusions. I recommend reducing this section into 1 paragraph which focuses specifically on the main conclusions of your study and the implications of your findings.

As advised, we worked on the length of the manuscript in order to shorten it and make it more concise. We left the introduction as this provides a good rationale for the study however the discussion is very long and quite repetitive of the results. I recommend condensing the discussion where possible. The conclusion section is very long and contains

It would also be nice to see more of a discussion of your study strengths and limitations in a paragraph preceding the conclusions. A further limitation for example of your work is the fact that accelerometers do not directly measure sedentary behaviour, they estimate sedentary behaviour through a lack of movement counts, however they cannot distinguish between sitting and standing postures. Therefore periods of standing still may be misclassified as sedentary time, this should be acknowledged.

- As advised, we worked on the length of the manuscript in order to make it more concise; particularly the discussion and conclusion sections. We also included the suggested additional limitations.

In the first paragraph of the conclusions where you compare your findings to the findings from studies conducted in high income countries (last 2 sentences of paragraph), does the second to last sentence here refer to adult data? This isn't clear in the paper. You may in fact choose to remove this sentence from the discussion if it refers to adults as the comparisons with child data (last sentence) would be more relevant for your paper."

- We appreciate this comment and have removed these adult findings and replaced them with references supporting our affirmation that the results are inverse to findings from developed countries.

**Additional Changes**
• We have updated the ISCOLE protocol reference [22] with a recently published ISCOLE methods paper.
• The reference numbering was changed to reflected additional references.

We wish to thank the reviewers and editor for their comprehensive review of the manuscript. It is our hope that they will be satisfied by our attempts to address their concerns.