Author's response to reviews

Title: Influence of Socio-Economic Status on Habitual Physical Activity and Sedentary Behavior in 8- to 11-year old Children

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Author's response to reviews: see over
Please find enclosed the newly revised manuscript and point-by-point response to the reviewer comments. In the manuscript, the latest changes have been highlighted in red. As has been requested by the reviewer the suggestion for revisions are stated prior to the response to these comments. We hope that you find the changes satisfactory.

Reviewer: Verity Cleland

Reviewer’s report:

The authors have addressed most of my comments and questions adequately, but there remain a few outstanding concerns, detailed below. Can I suggest to the authors that in future when responding to reviewers’ comments they specify where (e.g. page, paragraph, sentence or line) in the manuscript the changes have been made? This makes it much easier for the reviewer to clearly see the changes and ensures that changes are not accidentally missed. It is also helpful to include the reviewer’s original comments in responses to reviews so that reviewers can clearly see the original comment and how the author has responded in one document.

1. Major Compulsory Revisions

Background

1.3 I still feel that my point about the rationale for SB has not been addressed adequately. Although the authors state that ‘It has been suggested that sedentary behavior needs to be considered separately from physical activity’, they don’t say why this is so – a reference to another paper is not adequate. If this is stated clearly in the rationale, the discussion that flows will make more sense. Currently the link between the two is lacking.

Response: The independent influence of sedentary behavior on health is now more elaborated on page 5. Suggested physiological mechanisms that independently increase mortality risks are mentioned and additional references are provided to support the concept of sedentary behavior being different than simply lack of physical activity.

1.3 Also, the authors state that they do not wish to include the Ball article in the rationale because of its longitudinal study design – I am not sure that I understand that argument. I am not fussed whether the authors choose to cite this article or not, but my understanding is that longitudinal studies provide a higher level of evidence about associations than cross-sectional studies. Can the authors clarify their argument, or perhaps clarify that in
paragraph 2 of the Background that ‘recent CROSS-SECTIONAL studies using objective measurements....'

Response: The Ball (2009) reference is now also used in the background section, when equivocal findings of the relationship between physical activity and SES are addressed (p.4). Indeed, longitudinal studies provide a higher level of evidence and thus should be acknowledged.

Methods
1.5 & 1.6 Despite saying that they have included the response rate in both studies, I am unable to find this information. It is important to include the number of children that were invited to participate (not just the number of children that did participate) so that readers (or the authors) can calculate a response rate. The response rate has important implications for the interpretation of results.

Response: The total number of subjects invited to participate in Study 1 is included in the methods section. The total number of subjects invited to participate in Study 2 is more difficult since the recruitment did not occur directly thru schools but through newspaper advertisements, etc. Hence, it could be estimated from census data that about 2000 3-8 year old children reside in Kearney, NE. This is stated in the methods.

Results
2.18 Figure 3: I can see now what the authors are trying to express, but feel it is still a little confusing. I think it is because the significant differences between groups are indicated in different ways (a * for TV and a line for screen time). Also the * is not centred directly above the ‘TV watched per week’ column. Perhaps the authors can think of a clearer way to show this, such as a single * above ‘TV watched per week’ column and another symbol (e.g. ^, ~) above the ‘total screen time per week’ column with a similar footnote?

Response: Two separate captions (a, b) are now shown above the bars of interest. Underneath the figure a description of these captions is provided.

Discussion
1.9 The authors appropriately argue that there is not sufficient evidence to support either opinion, which therefore highlights the importance of presenting both sides of the argument – currently only one side is acknowledged. I think it is important that the authors acknowledge that an alternative hypothesis exists.

Response: To acknowledge an alternative hypothesis it is now mentioned that physical activity has been shown to be successful in prevention or treatment of obesity (top of p. 15), which is in contradiction to the hypothesis by Tou and Wade that weight gain is a precursor of low physical activity.

1.12 As per 1.5 and 1.6, I was unable to find information about the number of children invited and the subsequent response rates.

Response: Please see comments on 1.5 and 1.6 and the according changes in the methods section.
2. Minor Essential Revisions

Abstract
1.2 The authors have added sex as a covariate to the abstract, but state that it made no difference to the results – it is unusual to include in a model a covariate that makes no difference.

Response: The utilization of sex has been deleted in the abstract and is now only addressed in the main paper.

Title & throughout
2.1 Screen time is still used in the title and abstract.

Response: Screen-time has been changed to sedentary behavior in the title and in the abstract.

Methods
2.10 First paragraph: which measure of SB was used, child or parent report? Please clarify.

Response: Given the age of the subjects, parent report was used for younger children and the child report for older children in study 2 but all children reported own screen time in study 1.

Results
2.16 Table 2: It is still not clear why total screen time doesn’t appear in this table.

Response: In the methods section (p. 9) it has been added that low-income children reported only limited access to computers. The sample size in the low income group would have been very low, and in the results it is shown that no significant differences in computer time occurred between groups. Therefore, time spent watching TV is used as a measure of sedentary behavior. In the discussion it will also be addressed that time spent watching TV seems to be of bigger concern regarding sedentary behavior.

2.21 Page 11, Study 2: In the second sentence the authors have not clarified whether these differences were statistically significant?

Response: These results were meant to be descriptive only, but it is now stated that there were no significant group differences concerning sex distribution (p. 12).

Discussion
2.24 It is not clear where in the text the authors are referring to their further discussion of environmental constraints on physical activity.

Response: The last paragraph on page 13 addresses possible environmental aspects – i.e. accessibility. Further, results from a study by Kantomaa et al. are mentioned, which showed that engagement in club sports is higher in high-SES children.

2.25 The authors have not clarified in the text whether the Blanc study was in human or animal models.

Response: As has been mentioned in the last response the Blanc study used human subjects. This is now specifically stated in the paper as well (p.16).
Level of interest: An article whose findings are important to those with closely related research interests. Quality of written English: Acceptable. Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.

As requested by the editor, the specific university review boards granting approval for the studies are now also listed in the manuscript (Methods p. 6 & 7).