

Author's response to reviews

Title: Diet, physical activity, and adiposity in children in poor and rich neighborhoods: a cross-sectional comparison

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Author's response to reviews: see over

Reviewer's comments,

General

Overall this is a high quality publication, well written, and with well stated aims from a fresh, new and needed perspective. The methods are appropriate, although clarifications are needed to explain some terminologies and to describe/discuss in more detail. Methodologically my main concern is why the authors chose to classify children in terms of above or below the median (BMI z-score=0) and the limitations of this approach to showing differences in relation to obesity.

I found that the question posed by the authors is new, well defined, and is of key importance. I hope that publication of this article will stimulate others to publish similar research so we can gain a better understanding about the determinants of overweight and obesity in children. The methods are appropriate and reasonably well described, additional clarifications are requested in the methods (see compulsory revisions points for methods). The data is sound, but there is an explanation needed for why one group of children filled in the questionnaire at home, with help from the parents, but the other group did not.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

These suggestions are not major, but are needed clarifications for some terms. These might be placed in the category of "minor essential revisions" as indeed they are not major issues. However, as these suggestions include elaboration of the methodology and discussion points, I place these comments here.

We thank the reviewer for the constructive comments. The revised document is substantially improved. The reviewer's comments are bolded and our responses appear in normal font. In the manuscript the changes are highlighted in yellow.

1. The title implies a focus on obesity, although obesity was not measured according to references that are commonly used. I would recommend either changing the title, taking out the word "obesity risk factors" as this was a comparison of BMI and diet and activity behaviors. Risk factors for obesity were not assessed at the individual level.

We have modified the title appropriately. It now reads, "Diet, physical activity, and adiposity in children in poor and rich neighborhoods: a cross-sectional comparison."

2. Methods, Study population, page 5 Why did children from School B fill in the questionnaire at home with help from parents but not children from school A? Could this introduce any bias in the results, if so what direction. Further clarification about WHY this happened would be helpful,

and in the discussion section some further description is needed about what impact this might have had, if any, on the results.

We asked the principals of both schools for permission to administer the children questionnaires and carry out the measurements at the respective schools. The principal of School A agreed but the principal of School B only permitted us to conduct the measurements at the school, possibly because there was a teachers strike at the school when we were collecting the data. We therefore sent both the child and parent forms home. In the instructions we requested the children complete the child questionnaires. Children in grades 3 and higher were able to do so without help. In School A we helped the younger children with the questions and at School B we requested the parents to do so. As a result the responses from School B may have been biased towards being socially desirable, or “healthier”. However, the children reported differences in the types of foods they ate; those at School A reported eating more cruciferous vegetables, while those at School B more green leafy vegetables. As green leafy vegetables are generally not perceived to be more “healthy” than other types of vegetables it seems that bias did not impact these responses substantially. Similarly, children at School A reported being more physically active than those at School B, and there were differences in the perception of the walkability the neighborhoods reported by the parents. Based on these observations the bias was probably not substantial. We have explained this in the discussion. (page 11)

3. Methods, P. 6. Three factor eating questionnaire, TEEQ, what are the three factors that are the basis of this questionnaire? Later you describe scores in relation to green vegetables, fries, and sugar intakes. Are these the three factors? Are all green vegetables included, or is it more specifically assessing green LEAFY vegetables?

The 3 domains of the TEEQ are cognitive restraint, uncontrolled eating, and emotional eating. We have clarified this in the methods (page 6).

4A. Methods BMI z-score. Given the plethora of methods used for determining overweight/obesity it is important to briefly justify the choice of BMI z-scores over alternative methods (for example BMI %iles and/or the IOTF measures). Why choose the median as the cutoff point, as opposed to +2zscores, or something equivalent to the 85%ile or 91st percentile as other methods have done?

We initially chose the median BMI z-score as the cutoff to get two groups of equal sizes, which would be more statistically efficient than having unbalanced groups. However, as we are not making any comparisons of risk factors and BMI in this paper we do not need to do this. We have instead classified children as overweight or obese if they were at or over the 85th percentile of z-score, as suggested.

5B. Results BMI z-scores. I would recommend showing the whole distribution of the BMI-zscore. How is this cutoff point used? "No differences in BMI

z-scores of teh children studying at the two schools were detected..." this appears only to be an assessment of mean differences. What about prevalences? What about associations between overweight/obesity and behavior results? Even if the means are not statistically different (not surprising given the sample size) you might find differences in terms of prevalence. Also, given the small sample size, you could find meaningful results using ORs testing associations with key behaviors (the ones that are significantly different). Even if you don't have the statistical power to get statistically significant results in these associations, the strength of association could be informative to future studies.

We have provided the prevalence of overweight and obesity using the 85th percentile of BMI z-scores in the results. (Table 1, Results, page 9) As the risk factors that could potentially impact BMI were substantially different in the two schools, combine the data would mask this. When we stratified by school we were constrained by the small sample size, particularly in School A, in which there were just 16/48 cases. Because of low power and the potential for alpha error due to multiple testing we felt that these results could be misleading. Instead, we focused on the differences in the prevalence of risk factors at the two schools, which were able to demonstrate, even in this sample because the differences with respect to these factors between the two schools was so large.

6. Discussion, related to point 1, it would be nice to see a discussion of the possible impact of having parents help their children fill in the questionnaire in one school and not the other.

This is elaborated on page 11.

7. Discussion: Why not more discusion of the BMI results for children? The finding that BMI is not statistically different but behaviors are is a key point. What does this mean? There may be meaningful differences in the BMI of the two populations, even if there is no statistically significant difference (limited power due to small sample size). In any case, the issue warrants some discussion.

We have elaborated this point in the discussion on pages 10-11.

8. Discussion: Do you have any ideas why sitting time is greater in high SES groups... and what are they doing if not watching TV and playing video games? Are they reading more? Some discussion would help, even if you don't have the data, to describe your thoughts that might explain theses results in school children from Hamilton.

School B had better standardized test scores than School A. It is plausible that children in School B were more sedentary because they studied more even though they watched less TV than those at School A. We have mentioned that point in the results on page 9.

9. Discussion, p. 11 lines 8 & 9, minor clarification needed to specify what "these" refers to in the sentence "As these foods are generally"... which of the school's foods (A or B) are referred to here by "these". I realize that space is limited and you may not have the space to address all of these issues in full. However, some further descriptions would immensely strengthen this paper. If associations were tested, but not reported because of limited statistical power, it's worth noting in the text.

We have clarified that point in the text (page 11).

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

There are a number of minor clarifications needed:

1. It looks like there was a spell check replacement of "gra" with School B that went awry, this would explain some odd words/phrases in the text such as "grSchool Bin" and "demogSchool Bics". I would recommend a spell check on "School B" to fix these. Other examples of problems potentially related to what I am assuming is a spell check replacement: School Bdes (=grades), Bde 1 (=grade 1?), Other grades 1-8 given as School Bde1.... School Bde8, Post School Bduate I assume was a spell check error meaning post graduate?

We apologize for this oversight. The corrections are made.

2. Term "School Aerful" in the text, I don't know what this term means or where it comes from. Maybe another spell check issue?
3. p. 5. related to the child questionnaire "food intake was ased"... of course should be "assessed"

These also have been corrected.

Discretionary Revisions (which the author can choose to ignore)

I know this review process is not blind, so I am writing in the first person under the assumption that you already know my identity. A review I wrote with colleagues about school based interventions supports many of the points that you raise in this article. Our review differs from the Summerbell review in that we include more small studies than the Summerbell article does. Using a different set of criteria we found a more positive results (more successful interventions). I would be happy to send a draft of this paper as an email attachment, or the authors can find the article at: Obes Rev. 2006 Feb;7(1):111-36. The prevention of overweight and obesity in children and adolescents: a review of interventions and programmes. Doak CM, Visscher TL, Renders CM, Seidell JC. Of course, I'm not stipulating that you include reference to my own article as a required revision. However, in the article you may find

further support the importance for your own results. Related to your points, we found that single interventions were more effective, although it may be that the single interventions were more effective simply because these were usually more direct interventions (such as introducing PA in the schools or limiting TV viewing). In contrast, multiple message interventions tended to be education based with no direct change in the physical environment. In our review we did not see a clear link between longer duration and greater effect, but smaller scale studies that are tailored to the local do appear to be more effective.

Given the current body of evidence showing a growing number of effective interventions that are small scale and tailored to local needs, it is clear that more small scale studies, like this one, are needed.

The quoted review is indeed insightful, and its conclusions are similar to ours. We have therefore quoted it in this paper.

Quality of written English: Needs some language corrections before being published