Author's response to reviews

Title: Parent and child physical activity and sedentary time: Do active parents foster active children?

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Author's response to reviews: see over
Dear Associate Editor,
We would like to thank you for the opportunity to respond to your comments and those of the two reviewers. Please find below a point by point response (in italics) to each comment below. We have also used the yellow highlighter option in Word to show all of the changed sections.

**Associate Editor Comments**
Reviewer 1 made several good suggestions to help improve the clarity of the paper. This reviewer also specifically highlighted a number of important issues that need further attention in the discussion: for example, discuss why you think that associations were found only for girls.

We have addressed each of the comments from Reviewer 1. A point by point response is listed below.

This reviewer also highlighted the fact that the analysis was simplistic—what else can you control for? Neighborhood safety?

All of the analyses have been performed again in light of the comments of Reviewer 2 and the Associate Editor. We have also controlled for all of the factors that were suggested for which we have relevant data.

Reviewer 2 was particularly concerned about the simplicity of the analyses. This reviewer suggested that perhaps you could try to unpack some of the findings in terms of whether the association was greater among certain subpopulations such as those at risk for obesity. This reviewer also raised some questions on the methods—please address these.

We thank the reviewer for raising such issues. For all models we have tested whether there was an interaction between the key exposure and parental BMI. As there was not it does not seem to be appropriate to perform analyses by parental sub-group. We have also looked at the nature of the association between parent and child accelerometer behaviours and the scatterplots and correlations indicate a weak linear association and as such our analyses are appropriate. Further clarification of these methods has now been added to the paper. Please see pages 9 and 10.

Finally, in addition to addressing the points raised in the reviewers reports, in particular those highlighted above, there are two other issues that I would like to have addressed. First, I also feel that the empirical work is simplistic in the sense that it does not control for important individual, parental, household or neighborhood characteristics. Indeed, data may be lacking on such items. The footnotes to the tables note that the estimates control for a home related index of multiple deprivation - please provide more details on that.

The IMD variable provides detail on the education and health of residents within the area. The IMD is an area level measure of deprivation produced by the UK government that includes income, health, educational and employment status relative to a small geographical area around the participant’s home address. IMD score therefore provides a global indicator of socioeconomic status and provides information on a number of the factors suggested by the Associate Editor. We have therefore provided more detail on this measure in the text. Please see pages 5-6.
Unfortunately, we do not have data on the geographical location of participants address but recognition of this limitation has been added to page 14.

Second, what does the distribution of TV viewing hours look like? Instead of just a dichotomous definition of greater than 2 hours/day, what if you use a trichotomous indicator that assesses a heavy use category of greater than 4 hours per day? This may help to identify further nonlinear effects that come from a higher threshold of heavy viewing.

We thank the Editor for this very helpful suggestion. We have now changed the analysis of the TV viewing data by using a multi-nominal regression model in which both parent and child TV viewing are categorised as < 2 hours per day, 2-4 hours per day, and >4 hours per day, thereby providing an indication of the extent to which the participant met (<2 hours per day), exceeded (2-4 hours) or greatly exceeded the Academy of Pediatrics guideline on television viewing for children. This approach has significantly changed the study findings by showing that higher parental TV viewing is associated with a higher relative risk that the child will be in the highest TV viewing group when compared to the lowest. Greater detail on this modelling approach and the resulting findings is provided on pages 10 and 12. We thank the Associate Editor for this very useful suggestion which has improved our understanding of this issue.

Referee 1
Thank you for your time doing this study and preparing this manuscript. This is an interesting and timely study that examines the influence of parental PA and screen behavior on their children. This is a very well-done study and nicely written manuscript. All comments are in the category "minor essential revisions".

We thank the reviewer for the very supportive comments.

Abstract:
2 hour AAP – This is a little confusing and tricky since AAP recommends no more than 1-2 hr/day and your cut points were 1-2 or 2-3, so those kids or parents that select 1-2 could be at the 2 hr which is exceeding the recommendation. Just to be careful in phrasing and make explanation and categorization.

We have now reworked all of the analysis into 3 TV viewing groups. These are explained on page 7.

Background:
1st sentence: specify children or adults

The first sentence related to adults. We have now added a new sentence that relates specifically to children. Please see page 3.

“lower levels of childhood obesity” – that is confusing. To me this sentence reads that TV is associated with lower obesity, which is not the case in the majority of studies.

We mis-spoke and thank the reviewer for correcting us. The sentence has now been changed to highlight that high TV is associated with higher levels of obesity. Please see page 4.
Methods:
Adjusting for daylight – unique and interesting. You may be starting a trend. Can you provide in results if this was significant.

*We have not reported this result in the tables as daylight was considered a potential confounder and not a key variable of interest. Therefore, if we report the findings for daylight we would need to report model effects for all confounders which we believe would draw attention from the key associations of interest. However, for the reviewers interest we found that overall daylight was significantly associated with girls CPM (t = 2.90, p = .006) but was not significant in any of the other models.*

Good defense of cutpoints selected.

Thanks.

Analysis:
Were there any data on neighborhood safety? That might explain some variance since kids, girls particularly, have shown higher TV with unsafe or perceived unsafe neighborhood.

*We do not have these data. A statement of possible confounders that are not included in the paper has been added to the limitations section on Page 14.*

Discussion:
You will need to include that during this age (10-11 yo) kids are becoming more autonomous and maybe less influenced by parents. Parental behavior may have greater influence on younger children.

*We thank the reviewer for this suggestion. This has now been added to page 15.*

Need to explain why you think parental sedentary time is only associated with girls sed time rather than boys and girls?

*A statement of possible causes and the key research gap has now been added to page 12.*

Can you explain why sedentary counts were predicted more variance than sedentary minutes. Could this be a function of the difference in epoch length?

*In the revised models there is little difference in overall variance so this has not been added.*

You could remove all non-mother parents and see if results are strengthened. This would be an interesting side analysis in the results and discussion.

*We thank the reviewer for the suggestion but we feel that due to the clustered nature of the data and the number of confounders for which the models are adjusted we have insufficient power to perform these kind of sub-groups analyses. We have, however, recognised this limitation on page 14.*
Referee 2:
The authors have focused on an important, yet understudied, topic of the association between parent and child activity and sedentary activity patterns.

We thank the reviewer for highlighting the importance of our work. Please see detailed responses to issues raised below.

Major compulsory revisions
The analysis is overly simplistic, thus it is unclear whether there is a stronger association among certain high risk subsets.
* It would be helpful to see whether the associations varied by caregiver's BMI. Are the associations stronger among overweight and obese parents?

We have tested whether there is an interaction between each of the key exposures and parental BMI for all of the outcomes. We therefore feel that it would be inappropriate to conduct the type of sub-group analyses proposed. However, we have highlighted in the methods on page 9 how we tested for this interaction and have added parental BMI as a confounder to all models.

* Is the association stronger among very sedentary or very active parents? Using a linear model makes a strong assumption. At a minimum, it should be explored whether the association increases in a linear manner vs. a threshold effect. It is likely that the association might be stronger among those with higher values (in either being sedentary or active).

We thank the reviewer for the suggestion. In the preliminary phases of the analyses we used scatterplots and correlations to examine the associations between parent and child accelerometer variables. These analyses indicate weak linear associations and there is no evidence of a threshold effect. This information has been added to the paper on page 9.

* A logistic model isn't appropriate for an outcome with an approximately 50% prevalence. It may make sense to either predict a higher threshold or to use prevalence ratios.

At the Associate Editor suggestions we have now created trichotomous TV viewing outcome and exposure variables and then used a multi-nominal regression model to examine associations. These models have highlighted that higher parental TV viewing is associated with the likelihood that the participant watches more than 4 hours of TV per day when compared to a <2 hours per day reference group, thereby indicating higher parental TV viewing is associated with high TV viewing among children. Details of the new model are provided on page 10 and the findings are shown on page 12.

Minor Essential Revisions
* the differences between those and included and those retained is somewhat troubling. It would be good to discuss the possibility that the observed associations might be stronger in less affluent populations, who tend to be at higher risk of obesity.

This has now been added to page 13.
were Spearman correlations used or Pearsons? The former would be more appropriate. As we have changed the way that we have analysed the TV viewing data (see above) we have dropped the TV viewing correlations from this paper. The Pearson correlations between child and parent accelerometer variables are still shown in Table 3.