Author's response to reviews

Title: Associations between factors within the home setting and screen time among children aged 0-5 years: a cross-sectional study

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Author's response to reviews: see over
May 17, 2012

Dear Dr. Steven Allender,

Enclosed please find a revised version of the manuscript entitled “Associations between factors within the home setting and screen time among children aged 0-5 years: a cross-sectional study” that is under consideration for publication in BMC Public Health. We sincerely thank the reviewers for their time and constructive feedback. Please find the point-to-point response to the reviewers’ comments below.

Sincerely,

Ian Janssen, PhD

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This study investigates the association between demographic, parental and environmental factors and the television viewing time of children five years of age and under in Ontario, Canada. There is clear justification given for the research question, and the methods of the study are described quite thoroughly. The findings presented add to the research on the correlates and determinants of sedentary behaviour among young children, however the limitations of the study should be taken into account more fully in its interpretation.

We thank the reviewer for their positive comments and constructive feedback. We have addressed your specific comments about the limitations below.

One issue that deserves more examination is whether the participating parents were demographically and socioeconomically representative, and what implications this may have for interpretation of the findings.

Our sample is a convenience sample that was not designed to be representative. When examining parental education and income in Table 1 it is evident that our sample is of a relatively high SES compared to norms in our health region, province, and country. We state the lack of representativeness as a limitation on page 16 (lines 269-271) within the Discussion. We have also added a sentence on page 14 (lines 241-243) indicating that future research is needed in representative samples to examine if these relationships are consistent at the population level.

The lack of representativeness would certainly affect the descriptive information of the sample. For instance, because average screen time levels are higher in children from lower SES homes, the screen time levels in our sample are likely lower than what would be observed in a representative sample. However, our study was an etiological study and not a descriptive one, and it is less clear as to whether the associations we observed between factors within the home setting and screen time levels were impacted by a lack of representativeness. For such an issue to occur, the associations themselves would need to vary within the children who were studied vs. the children who were not. While this is possible, it is unlikely. In support of our belief, we examined whether there were SES interactions for our regression models, and we did not find any. Thus, we are reasonably confident that the lack of representativeness did not impact our main findings and observations regarding the relationship between factors within the home setting and screen time level.

Another matter that should be considered is that only one environmental measure was included, when the study has been based on an ecological model of sedentary behaviour. The presence of televisions in the home is important, but the nature of the environment external to the home will also present barriers or enablers of non-sedentary, active behaviours. Two environmental barriers (climate and safety issues) as examined as parental cognitions, but these were not treated as physical environment measures.

As noted in several places in the Introduction, Methods, and Discussion sections, this study
was guided by the Ecologic model of Sedentary Behavior. This model recommends setting-specific examinations of the multi-level factors that influence screen time (see pages 4-5, lines 19-26). This is important because the contextual factors that influence screen time from one setting (i.e., home versus neighborhood) are likely different and these different contextual factors may interact with the intrapersonal and interpersonal factors differently when examining the influence of screen time. Therefore, setting-specific examinations of sedentary behavior will likely lead to more successful interventions. Because the objective of this paper was to focus on the home setting, we focused on factors within the home. However, we note on page 15 (lines 259-262), future research needs to examine factors in other setting such as the neighborhood setting.

We recognize that two items in the parental barriers measure pertain to environment barriers. However, a strength of our parental barriers factor is that it is a multi-item scale with a good internal consistency (α=0.79). Thus, these items are being averaged together to measure one underlying factor “parental barriers to reducing screen time”. Therefore, we do not think it would be appropriate to break up this scale to measure different factors. Especially given the objective of the study is to focus on the home setting.

In response to comment 1.4, screen time is now our main dependent variable. As a result we added in “computers in the bedroom” and “video games in the bedroom” as physical environment variables in the analyses. Consequently, we now have three physical environment variables. Also, while we recognize the importance of the “physical” environment in the ecological model, the “social” environment (interpersonal level) is another important aspect of this model and we have numerous factors at this level.

1.4 It was not clear why, given that 38% of children participated in video/computer game play, that this was not added to television viewing time to get a total screen viewing time measure, as has been done in other studies of children’s sedentary behaviours. Adding video/computer game time may not have had a significant bearing on the main findings, but this issue could be considered by the authors.

The reviewer makes a good point as our introduction is framed around screen time. As a result, the revised manuscript now presents the total screen time results (television + video/computer games) instead of the television only results. Please note that the total screen time in the sample was driven in large measure by television time, and as speculated by the reviewer, the results for total screen time that are presented in the revised manuscript are almost identical to the results for television time that were presented in the original submission.

1.5 P7, line 82 - confirmation of what type of reliability was demonstrated for the self-efficacy measures in Campbell et al’s study, with the reliability statistic given;

In the Campbell et al. study the authors stated in the discussion section that the self-efficacy items had good reliability. However, they did not indicate the type of reliability or provide the reliability statistic. Therefore, we are not able to provide further details on the reliability of this self-efficacy measure. We note in the limitations paragraph of the discussion that “while the questionnaire was pilot tested before distribution to the participants and all of the independent and dependent variables were adopted from national surveys or previous research,
psychometric properties were not available for all of the variables.”

1.6 **P11, line 173-174** – it is not clear what is meant when it is stated that the linear and logistic regression analyses were repeated for screen time, when the logistic regression procedure for television viewing was just described;

These lines have been removed as the analyses now focus on total screen time. Please see response to comment 1.4.

1.7 **P12, line 185-188** – some explanation could be given for what the regression coefficients are saying about the nature of the relationships between the dependent and independent variables;

Based on this comment and other reviewer’s comments we have added more information about the nature of the relationships for all of the analyses presented in the results section of the revised manuscript.

1.8 **P21, table 1** – is it correct that 92% of respondents were male?

We appreciate the reviewer catching this mistake (92% of the respondents were female parents/guardians). This has been corrected in Table 1.

**Reviewer 2**

2.1 The manuscript notes clearly the recommendations on limiting screen time in the under fives in the first paragraph of the intro, but then goes on use just TV viewing as an outcome. Please reanalyse with a combined measure of all screen time (TV and video games). I suggest applying the cut-points of 2hrs, or using > or equal to 90th centile for this combined measure;

The reviewer makes a good point as our introduction is framed around screen time. As a result, the revised manuscript now presents the total screen time results (television + video/computer games) instead of the television only results. For the logistic regression analyses we predicted the top quartile of total screen time. Please note that the total screen time in the sample was driven in large measure by television time, and that the results for total screen time that are presented in the revised manuscript are almost identical to the results for television time that were presented in the original submission.

2.2 **p4, line 12-13**: you want to foster apporpiate screen time habits in parents for their children under 2, not children, as they don't determine how much they use at this age, please clarify

The objective of this study is to determine what factors to focus on when fostering appropriate screen time habits in children 5 years and younger. Our results do suggest that parents may play a key role, especially for the younger children (i.e., under 2 years). It is important to remember that the behavior (screen viewing) that this study is focusing on is occurring at the
child level. This study suggests that one method of modifying the child’s behavior or fostering good behavior is targeting the parents. Thus, fostering appropriate screen time habits in pre-school children is encompassing these parental factors.

2.3 **Parental cognitions should be renamed as parental attitudes.**

We respectfully disagree with this recommendation. Parental cognitions includes four different factors, only one of which is parental attitudes. Based on behavior change theories such as the social cognitive theory, we feel it would not be correct to put a factor such as parental self-efficacy under an umbrella term of parental attitudes.

2.4 **Clarify when parental predictors are television specific (e.g. self efficacy)**

None of the parental predictors are television-specific. They were all asked regarding screen time with television and video/computer games as examples. In the methods sections the specific questions used to assess the parental factors are given (see pages 7-9) and we hope this provides clarity to the reader.

2.5 **p12 line 199. Stronger estimates don't indicate a sex difference; I suggest including interaction terms in unadjusted analysis between your predictors and sex. If they are significant, please report all models and tables separately for boys and girls.**

We ran interaction terms for gender and none of these were statistically significant (see page 13, line 202). Furthermore, within stratified analyses for the linear regression the results were extremely consistent within boys and girls (see page 13, lines 203-204). Therefore, it was not necessary to present the results separately for boys and girls.

2.6 **Table 2: Include N, and type of correlation as footnotes;**

The “N” was added to the top of the table under screen time. “Pearson” correlations was added to the title of the table.

2.7 **Table 3: These are stepwise models**

Different research disciplines use different names for the modeling strategy used in our study. The psychology field typically calls the approach that was used in our paper a stepwise approach. However, in the epidemiology and public health fields stepwise refers to a specific modeling strategy for creating a parsimonious model that focuses on the statistical significance (p-value) between a confounder and an outcome. As this paper has been submitted to a public health journal, we feel it is most appropriate to use the terminology that is most accepted within the public health discipline. Thus, we feel “sequential” regression is more appropriate that “stepwise” as this is a more specific term that better reflects the modeling technique used in our study.

2.8 **Note limitations of measurement validity - you could have used accelerometers to measure sedentary time; and the potential for self report bias of parents -**
The objective of this study was to focus on the factors that influence screen-based sedentary behavior in pre-school children. While accelerometers can be used to measure total sedentary behavior time, they do not provide information on the time spent in specific sedentary behaviors such as screen time. Therefore, accelerometers could not have been used to capture screen-based sedentary behavior. Having said that, we note in our limitation section on pages 15-16 (lines 266-269) that parent-report measures of screen time are prone to information bias (i.e., self-report bias). This measurement error would likely be non-differential; therefore, the true associations may have been underestimated.

2.9  **p5.line 26: please reference the statement on most screen time occurs at home**

A reference has been added to this statement. Please see page 5 (line 27).

2.10  **How many imputations did you do?**

Five imputations were used (page 10 (line 151)).

2.11  **p11. line 159: do you mean p<0.05**

Yes, this has been corrected.

2.12  **Is the adjusted R2 adjusted for clustering?**

Yes, the adjusted R2 came from the SURVEYREG and SURVEYLOGISTIC procedures. These procedures account for the clustering.

2.13  **Is the best case a complete case analysis? How many participants in the complete case and how do the estimates compare?**

If we would have used a listwise approach (i.e., deleted participants who were missing information on any of the variables of interest) our sample would have been reduced from 746 to 550. The findings do not change when running the analyses in the 550 versus the 746 participants. However, the majority of participants with missing information were only missing information for one of the independent variables (page 10, lines 152-154). Therefore, we feel that the multiple imputation approach is superior to the listwise deletion approach because less data was lost.

2.14  **Mention the recent paper in JAMA on activity and sedentary behaviour**


We are aware of this recently published paper, which focused on total sedentary time measured with an accelerometer in 4-19 years olds. While we agree that this is an excellent publication,
its goals, research questions, and study population were substantially different than that of our study, which focuses on specific types of sedentary time (i.e., screen time) in 0-5 year olds. While consistent associations have not been found between total sedentary time (as assessed by accelerometry) and cardio-metabolic risk factors, consistent associations have been observed between screen-based sedentary behavior and a variety of health outcomes across different age groups. For the introduction and discussion sections we have focused on the literature regarding screen-based sedentary behavior and health outcomes in pre-school aged children, which we feel is most appropriate given the objective of this study and the sample used to address this objective.

**Reviewer 3**

3.1 **The abstract needs to say what the significant factors associated with screen time are in more specific detail - too general as written now.**

The factors associated with screen time were added to the abstract.

3.2 **This also applies to the discussion. While the modelling is framed about the ecological model, to make it more relevant for practitioners wanting to identify modifiable strategies for intervention, could the authors refer to the specific scales.**

More detail has been added to the discussion to identify modifiable strategies for interventions that are relevant for practitioners. Please see page 15 (lines).

3.3 **Table 3 - coefficient for family structure in 4-5 year olds would appear to be not significant based on the confidence interval, but shows an asterisk indicating P<0.05.**

We thank the reviewer for catching this error. The asterisk has been removed.

3.4 **Within some of the scales I wanted to know which of the items contributed most or would be the most important areas to focus on. Can the authors tease this information out more?**

The strength of the associations between the individual variables that make up the scales and screen time is shown in Table 2.

One of the main purposes of conducting the logistic regression models was to further help tease apart the importance of the specific items. The results in Table 4 show the association between each item while controlling for all of the other items that are part of the same and different scales. More information was added regarding the logistic regression in the results section. Please see page 13 (lines 215-220).

In terms of the linear regression, given that parental cognitive factors explain such a large portion of the variance, this may be the most important area to focus on. We have expanded on
this in the discussion section. Please see page 15 (lines 244-251).

| 3.5 | **Normative perceptions based on the question about maximum screen time may more realistically reflect awareness of various guidelines about screen time, and have some inherent positive response bias. Perhaps the label for this variable might be changed?** |
|     | At the time this data were collected in the summer of 2011, Canadian sedentary behaviour guidelines did not exist for children under the age of 5. There were screen time recommendations from the Canadian Paediatric Association for children of all ages. However, studies in the US among older children have showed that only a small percentage (~30%) of parents are aware of the American Academy Pediatrics screen time recommendations (Barradas, et al. 2007). This is also likely true in Canada. As a result we have decided to keep this factor labeled as descriptive norms. |