PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Preschool children's context-specific sedentary behaviors and parental socioeconomic status in Finland: a cross-sectional study
AUTHORS	Määttä, Suvi; Konttinen, Hanna; Haukkala, Ari; Erkkola, Maijaliisa; Roos, Eva

VERSION 1 – REVIEW

REVIEWER	Katherine Downing Deakin University, Australia
REVIEW RETURNED	22-Mar-2017

GENERAL COMMENTS	Thank you for the opportunity to review this manuscript. Overall, this is a well-written manuscript investigating the associations between measures of socioeconomic status and sedentary behaviour in preschool-aged children. The inclusion of a number of different parent-reported sedentary behaviours in addition to objectively assessed sedentary time is a particular strength. I have a few minor comments below for the authors to consider. General comment: I suggest that the authors reconsider the abbreviations used for the different types of sedentary behaviour investigated. Sedentary time (i.e., objectively assessed sedentary time) is often abbreviated as SED, while screen time is often abbreviated as ST; the use of ST as an abbreviation for sedentary time in the current manuscript may cause some confusion. Abstract: Page 2, line 42: "Otherwise, parental SES tended not to relate to objectively measured ST." I would suggest rewording this; significant associations are either present or not and the word "tended" implies that there may have been other associations. Introduction: Page 3, lines 27-29: Please include here (and throughout) "years" after the ages, i.e., "defined here as aged 3 through 6 years"). Please also add the word "behaviour" after sedentary, i.e., "spend most of their waking hours in sedentary behaviour". I would also suggest rewording the definition of sedentary behaviour. The definition in the letter to the editor cited by the authors is "any waking activity characterized by an energy expenditure ≤ 1.5 metabolic equivalents AND a sitting or reclining posture"; therefore, I would suggest removing "are mainly conducted". Otherwise, the introduction is well-written and provides strong rationale for the study.

	Methods: Page 6, lines 30-34: The measurement of a range of different screen-based sedentary behaviours is a particular strength of this study; to my knowledge few studies have investigated tablet and smartphone use in children of this age. I would suggest the authors consider reporting associations for individual screen behaviours separately – there may be different associations between SES and TV viewing compared to SES and smartphone use for example. The authors include some discussion of the use of tablets and smartphones in the 3rd paragraph of the discussion – investigating associations for individual screen behaviours would be interesting in light of this discussion. Results: Page 7, line 49: "A total of 771 children filled in the diary properly" – should this be parents? Throughout results section please change any instances of P=0.000 to P<0.001. Discussion: Page 13, lines 17-20: This seems repetitive of the first paragraph – the association between parent education and screen time has already been stated above.

REVIEWER	Bill Heerman Vanderbilt University Medical Center
REVIEW RETURNED	05-Jul-2017

GENERAL COMMENTS	Määtä and colleagues report on a cross-sectional analysis of an ongoing study. They evaluate the associations between parent socioeconomic status with 1) child sedentary time (measured by accelerometry) and 2) child sedentary behaviors (measured by parent report) among children in Finland. Overall this manuscript provides incremental but important contributions. The major contributions include 1) that in general parent socioeconomic status is not associated with preschooler sedentary time and 2) that there are small but potentially important associations between lower parental socioeconomic status with increased child screen time and decreased child reading, highlighting the importance of measuring specific sedentary behaviors, instead of just sedentary time. The manuscript could be strengthened by addressing the following issues: Abstract: 1) The conclusion in the abstract does not match that in the discussion. While the authors have accurately interpreted their results in the discussion, they seem slightly over-stated in the abstract. Including a statement about how in general SES was not associated with ST would be helpful. Introduction: 2) The authors provide adequate context for the study, but I would encourage them to focus the first paragraph of the introduction more carefully, addressing more fully the question of why measuring sedentary time in preschoolers is clinically meaningful.

	Methods: 3) Page 4, lines 46-48: The authors state that the purpose of the main trial is “to diminish socioeconomic differences in preschoolers’ energy-balance-related behaviors.” Were any of the children in the present analysis exposed to an intervention prior to data collection? And if so, how was this addressed? 4) The authors list “major” recruitment criteria. Were these the only criteria for the study? Or were there others, perhaps associated with the original trial? 5) The authors mention that data were available on 24% of those invited. The authors should address this as a potential limitation in the discussion section, recognizing the potential non-responder bias and how it might impact results. If at all possible, providing basic demographic characteristics on the non-responders could strengthen the argument that their sample is representative of the children in their target population. 6) The authors should report whether the Evenson cut-point uses the vertical or vector magnitude (i.e., tri-axial) axis on the accelerometer. 7) Page 6, line 15. “We did also modifications” should be corrected. Perhaps the authors meant, We made modifications to the original version… 8) It would be helpful to provide an appendix of the screen time questions asked, as it was a modification of a previously published tool. 9) How were children who lived in the same household accounted for in the analysis (i.e., siblings)? Results 10) The authors should provide the average wear time for the accelerometers. 11) The authors comment that “Those who did not produce valid accelerometer data…were more likely to have a mother with a lower level of education…” This should be addressed in the limitations with an explanation of how this type of non-response could bias the interpretation of the results. 12) This is a matter of preference, but the authors should consider including in the narrative the key numerical results for the data they highlight. It is helpful to see the confidence intervals and p-values for the key results, even though it is a minor duplication of data already presented in the tables. 13) This is a matter of preference, but the authors should consider adding leading zeros on decimals in the tables. 14) In the caption for Table 3 it states that the regression was controlled for research time. This is the first mention of this. Please provide additional detail in the methods. And please address why the analyses reported in Table 2 were not controlled for research time.

	Discussion 15) It would be helpful to provide some context for the reader about the impact of a 17 minute difference in screen time. Based on previous research, does this amount lead to other clinically meaningful outcomes? 16) In the discussion how these results fit into previous literature, I would invite the authors to consider the following manuscript. Vorwerg Y, Petroff D, Kiess W, Bluher S. Physical activity in 3-6 year old children measured by SenseWear Pro(R): direct accelerometry in the course of the week and relation to weight status, media consumption, and socioeconomic factors. PloS one. 2013;8(4):e60619. The authors should consider adding to the limitations section two additional considerations (in addition to the ones mentioned previously). 17) First, could social desirability bias impact the parent-report of child screen time behaviors, and could this be differentially associated by socioeconomic status (i.e., parents with higher education might be more likely to under-report screen time)? 18) Second, a statement of the generalizability of the results should be included. Do we have reason to believe that these results are relevant for the preschool population in Finland? And what about outside of Finland? 19) The article cited by the authors in defense of the Evenson cut-points only includes children between 7-13 years of age (reference #48). The authors should include citations that are consistent with the age of the children in the present study.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Katherine Downing
Deakin University, Australia
Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

Thank you for the opportunity to review this manuscript. Overall, this is a well-written manuscript investigating the associations between measures of socioeconomic status and sedentary behaviour in preschool-aged children. The inclusion of a number of different parent-reported sedentary behaviours in addition to objectively assessed sedentary time is a particular strength. I have a few minor comments below for the authors to consider.

General comment:
I suggest that the authors reconsider the abbreviations used for the different types of sedentary behaviour investigated. Sedentary time (i.e., objectively assessed sedentary time) is often abbreviated as SED, while screen time is often abbreviated as ST; the use of ST as an abbreviation for sedentary time in the current manuscript may cause some confusion.

Response: Thank you for this comment. We have corrected the abbreviations in our manuscript. We use now abbreviation SED for objectively measured sedentary time instead of using ST.

Abstract:
Page 2, line 42: “Otherwise, parental SES tended not to relate to objectively measured ST.” I would suggest rewording this; significant associations are either present or not and the word “tended” implies that there may have been other associations.

Response: We have now modified this sentence. The new sentence goes ‘ Otherwise, parental SES was not related to objectively measured SED.’ (please, see the change in page 2, last line in results-part)

Introduction:
Page 3, lines 27-29: Please include here (and throughout) “years” after the ages, i.e., “defined here as aged 3 through 6 years”).

Response: We have double-checked our manuscript and added ‘years’ after the ages (please, see the change in page 3, first lines in introduction).

Comment: Please also add the word “behaviour” after sedentary, i.e., “spend most of their waking hours in sedentary behaviour”.

Response: We have added word ‘behavior’ after the ‘sedentary’ word (please, see the change in page 3, first lines in introduction)..

Comment: I would also suggest rewording the definition of sedentary behaviour. The definition in the letter to the editor cited by the authors is “any waking activity characterized by an energy expenditure ≤ 1.5 metabolic equivalents AND a sitting or reclining posture”; therefore, I would suggest removing “are mainly conducted”.

Response: We have corrected this sentence (please, see the new sentence in page 3)

Comment: Otherwise, the introduction is well-written and provides strong rationale for the study.

Response: Thank you for this comment.

Methods:
Page 6, lines 30-34: The measurement of a range of different screen-based sedentary behaviours is a particular strength of this study; to my knowledge few studies have investigated tablet and smartphone use in children of this age. I would suggest the authors consider reporting associations for individual screen behaviours separately – there may be different associations between SES and TV viewing compared to SES and smartphone use for example. The authors include some discussion of the use of tablets and smartphones in the 3rd paragraph of the discussion – investigating associations for individual screen behaviours would be interesting in light of this discussion.

Response: Thank you for this comment. Your suggestion is relevant. We have now modified our manuscript so that we have added results of specific screen behaviors separately. This suggestion therefore caused multiple changes into manuscript. We added a new table 4 plus a result section explaining the significant associations (please, see the pages 14 and 15). In addition, we needed to modify the following sections in manuscript: abstract, measures, statistical analyses and discussion (please, see the modified lines in red in these sections). We hope that these new parts of manuscript make our study even more relevant.

Results:
Page 7, line 49: “A total of 771 children filled in the diary properly” – should this be parents?

Response: Yes, you are right about this comment. We have now corrected this part (page 8).

Comment: Throughout results section please change any instances of P=0.000 to P<0.001.

Response: We have now corrected and replaced instances of P=0.000 to P<0.001. (please, see the results section)

Discussion:
Page 13, lines 17-20: This seems repetitive of the first paragraph – the association between parent education and screen time has already been stated above.

Response: Thank you for this comment. We agree that this sentence is repetitive in relation to the sentences in first paragraph. We have now re-structured this sentence so that it is not too repetitive. We however would like to keep this sentence in our manuscript, because it illustrates the SES differences in minutes and provides therefore a concrete illustration of our results (please, see the page 16). Another reviewer also hoped to have clinical meaningfulness into our discussion section. Therefore, we have re-structured this paragraph.

Reviewer: 2
Bill Heerman
Vanderbilt University Medical Center
Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

Määtä and colleagues report on a cross-sectional analysis of an ongoing study. They evaluate the associations between parent socioeconomic status with 1) child sedentary time (measured by accelerometry) and 2) child sedentary behaviors (measured by parent report) among children in Finland. Overall this manuscript provides incremental but important contributions. The major contributions include 1) that in general parent socioeconomic status is not associated with preschooler sedentary time and 2) that there are small but potentially important associations between lower parental socioeconomic status with increased child screen time and decreased child reading, highlighting the importance of measuring specific sedentary behaviors, instead of just sedentary time. The manuscript could be strengthened by addressing the following issues:

Abstract:
Comment 1) The conclusion in the abstract does not match that in the discussion. While the authors have accurately interpreted their results in the discussion, they seem slightly over-stated in the abstract. Including a statement about how in general SES was not associated with ST would be helpful.

Response: Thank you for this comment. We have added a new sentence in conclusion section to illustrate that in general, SES was not associated with ST (please, see the page 3, conclusions paragraph). Due to these additional words, we also needed to modify some other parts of the abstract so that it does not exceed word limit of 300 words.

Introduction:
Comment 2) The authors provide adequate context for the study, but I would encourage them to focus the first paragraph of the introduction more carefully, addressing more fully the question of why measuring sedentary time in preschoolers is clinically meaningful.

Response: Thank you for this comment. We are not sure if we understood your comment right, but we noticed that our first paragraph mixes words 'children','preschool children' and ' early childhood'. Therefore, we decided to unify the words in this chapter. We hope that this modification makes it easier to understand why measuring sedentary time in preschoolers is important. We have also modified our first paragraph in introduction so that it is clearer to understand why measuring sedentary time in preschoolers is clinically meaningful (please, see the changes in page 3 and 4).

Methods:
Comment 3) Page 4, lines 46-48: The authors state that the purpose of the main trial is "to diminish socioeconomic differences in preschoolers' energy-balance-related behaviors." Were any of the children in the present analysis exposed to an intervention prior to data collection? And if so, how was this addressed?

Response: The DAGIS project consist of multiple data collection phases, and the data used in this manuscript is from a cross-sectional study (without any trial/intervention study design). Based on the results of this cross-sectional study, intervention trial is conducted later this year (with new participants and new municipalities). We acknowledge that this part of the methods is not clearly stated, and we have now modified it (please, see the page 5).

Comment 4) The authors list "major" recruitment criteria. Were these the only criteria for the study? Or were there others, perhaps associated with the original trial?

Response: Thank you for this comment. Our recruitment had two steps. Firstly, we had 3 criteria for preschools to be included, and if a preschool gave willingness to participate, we secondly expected that more than 30 percent of families in at least one of the groups consented to participate before actual data collection was conducted in the preschool.
Our main recruitment criteria was to include preschools with at least one group of children aged 3-6 years, because this age group is our focus in this study. We also excluded all the pre-primary education classes (only 6-year old children). In addition, we excluded the preschools that are open for 24 hours a day due to practical issues for conducting data collection in these preschools (e.g. it would be difficult to have all the children in preschool setting at the same time during the week to deliver accelerometers and collect them back). In addition, our selection criteria for preschools were that working language in preschools is Finnish or Swedish. Total contacted 16 preschools were excluded due to these above-mentioned reasons. We have added detailed information about recruitment criteria into our manuscript (the new information is found on page 5).

Comment 5) The authors mention that data were available on 24% of those invited. The authors should address this as a potential limitation in the discussion section, recognizing the potential non-responder bias and how it might impact results. If at all possible, providing basic demographic characteristics on the non-responders could strengthen the argument that their sample is representative of the children in their target population.

Response: You are right that our participation rate of families were low, and it might have influence in generalizability of our findings. We have added about this limitation into our discussion (page 19). We have no information about the families, who declined to participate in our study. Therefore, we cannot provide any basic demographic characteristics on the non-participants.

Comment 6) The authors should report whether the Evenson cut-point uses the vertical or vector magnitude (i.e., tri-axial) axis on the accelerometer.

Response: We have added this information in the methods section (page 6).

Comment 7) Page 6, line 15. “We did also modifications” should be corrected. Perhaps the authors meant, We made modifications to the original version…

Response: We have corrected this sentence as you suggested (page 6).

Comment 8) It would be helpful to provide an appendix of the screen time questions asked, as it was a modification of a previously published tool.

Response: We have added our diary as an online supplementary material showing how the screen time and reading time in our study was measured. We hope that this supplementary material provides a good picture of diary that we used (please, see the supplementary file).

Comment 9) How were children who lived in the same household accounted for in the analysis (i.e., siblings)?

Response: We conducted our analyses with three different cluster options – that is preschool, preschool group and family. All these three sets of analyses produced similar results. Since preschool group was a primary sampling unit in the DAGIS study, we then decided to choose it as our cluster variable.

Results
Comment 10) The authors should provide the average wear time for the accelerometers.

Response: We have added this information into the manuscript (page 8). The average wearing times were similar despite the variable used. The total daily wearing time (despite the context) was 773 minutes. The wearing time during weekends was 768 minutes. The wearing time in preschool time was 419 minutes on average, and at home 348 minutes (combined 767 minutes). Therefore, we decided to add only the information about total daily wearing time in this manuscript.

Comment 11) The authors comment that “Those who did not produce valid accelerometer data…were more likely to have a mother with a lower level of education…” This should be addressed in the limitations with an explanation of how this type of non-response could bias the interpretation of the results.

Response: Thank you for this comment. We have added more about this limitation into our discussion together with your previous comment (comment number 5) (please, see the page 19).

Comment 12) This is a matter of preference, but the authors should consider including in the narrative the key numerical results for the data they highlight. It is helpful to see the confidence intervals and p-values for the key results, even though it is a minor duplication of data already presented in the tables.

Response: We have added the confidence intervals for the key results. We hope that this modification improve the understanding of the key results that we highlight (please, see the results section).

Comment 13) This is a matter of preference, but the authors should consider adding leading zeros on decimals in the tables.

Response: We have added the leading zeros on the decimals in the Tables.

Comment 14) In the caption for Table 3 it states that the regression was controlled for research time. This is the first mention of this. Please provide additional detail in the methods. And please address why the analyses reported in Table 2 were not controlled for research time.

Response: Thank you for this comment. We noticed that we have had a wrong word in Table 3 for describing season. The research time was used in our old manuscript drafts, and we did not pay enough attention to replace this word with season in our Table 3. We have corrected this word in Table 3 (on page 13). Now, the word 'season' is used throughout our manuscript.

Discussion
Comment 15) It would be helpful to provide some context for the reader about the impact of a 17 minute difference in screen time. Based on previous research, does this amount lead to other clinically meaningful outcomes?

Response: Thank you for this comment. The comparison to other studies is a bit difficult because most of the other studies have either dichotomized or categorized the screen time making it hard to do comparison. We found only very few studies with limited sample size that had also mean values to be included but we felt that we would give too strong statement in the introduction about the difference. However, we modified this part of the discussion so that we refer more to higher risks of exceeding the recommendations among children with low SES backgrounds. We also refer to a new meta-analysis that concludes how associations of SES and children's sedentary behavior might be country-specific. When discussing about the clinical importance, we added some sentences about this topic, and raised more the public health importance of this result. In addition, another reviewer suggested some changes to be made in our manuscript, and due to these requests, we have also written a new paragraph into discussion about practical relevance of our results. (please, see the re-written sections in discussion on pages 16 and 17).

Comment 16) In the discussion how these results fit into previous literature, I would invite the authors to consider the following manuscript.
Vorwerg Y, Petroff D, Kiess W, Bluher S. Physical activity in 3-6 year old children measured by SenseWear Pro(R): direct accelerometry in the course of the week and relation to weight status, media consumption, and socioeconomic factors. PloS one. 2013;8(4):e60619.

Response: Thank you for this tip. We have familiarized with the manuscript, and added it as a reference into our manuscript.

The authors should consider adding to the limitations section two additional considerations (in addition to the ones mentioned previously).

Comment 17) First, could social desirability bias impact the parent-report of child screen time behaviors, and could this be differentially associated by socioeconomic status (i.e., parents with higher education might be more likely to under-report screen time)?

Response: Yes, you are right that social desirability bias can also have influence in reporting the children's sedentary behaviors. We have added a sentence about this into our manuscript (page 19).

Comment 18) Second, a statement of the generalizability of the results should be included. Do we have reason to believe that these results are relevant for the preschool population in Finland? And what about outside of Finland?

Response: We have added a statement of the generalizability of our results. It is true that our participation rate is low, and participants might be biased (page 19).

Comment 19) The article cited by the authors in defense of the Evenson cut-points only includes children between 7-13 years of age (reference #48). The authors should include citations that are consistent with the age of the children in the present study.

Response: Thank you for this comment. We have changed the reference so that it is consistent with the age of the children in our study. This new reference (Janssen et al, 2014;:Predictive Validity and Classification Accuracy of ActiGraph Energy Expenditure Equations and Cut-Points in Young Children.) has studied the accuracy of different cut-points among 4-6-year-old children (page 19).

VERSION 2 – REVIEW

REVIEWER	Katherine Downing Deakin University, Australia
REVIEW RETURNED	15-Aug-2017

GENERAL COMMENTS	I thank the authors for their responses to my previous comments and for making the suggested changes in the manuscript. In particular, the inclusion of the individual screen behaviours as additional outcomes is welcome; however, I have some concerns with respect to the way in which the additional analyses were performed. Firstly, it is not clear why the authors have dichotomised the individual screen behaviours (TV viewing, DVD/video watching, computer use and tablet computer/smart phone use) into the highest 25% of using/viewing time compared with the lowest 75%, particularly given that the other outcomes (sedentary time, total screen time and reading time) were examined as continuous variables. I would suggest that dichotomising the variables in this way does not provide meaningful results. If there is a strong rationale for this please include, otherwise I would suggest re-running the analyses with these outcomes as continuous variables. Secondly, the increased number of statistical tests performed increases the potential for Type I error. I would recommend the authors control for this appropriately, e.g., by using an approach such as the Benjamini-Hochberg Procedure (Benjamini Y, Hochberg Y. Controlling the False Discovery Rate: A Practical and Powerful Approach to Multiple Testing. Journal of the Royal Statistical Society Series B (Methodological). 1995;57(1):289-300.)

REVIEWER	Bill Heerman Vanderbilt University Medical Center United States of America
REVIEW RETURNED	03-Aug-2017

GENERAL COMMENTS	The authors have addressed previous comments appropriately and I have no further concerns.

VERSION 2 – AUTHOR RESPONSE

Thank you for the comments and additional questions. We appreciate the comments and questions because these changes in manuscript improve the quality of our manuscript. Hopefully, our changes in the manuscript will further clarify the manuscript. More detailed answers are written below. The changes made in the manuscript are written in red. The possible removals are marked with strikethrough and red colour.

Comment: I thank the authors for their responses to my previous comments and for making the suggested changes in the manuscript. In particular, the inclusion of the individual screen behaviours as additional outcomes is welcome; however, I have some concerns with respect to the way in which the additional analyses were performed. Firstly, it is not clear why the authors have dichotomised the individual screen behaviours (TV viewing, DVD/video watching, computer use and tablet computer/smart phone use) into the highest 25% of using/viewing time compared with the lowest 75%, particularly given that the other outcomes (sedentary time, total screen time and reading time) were examined as continuous variables. I would suggest that dichotomising the variables in this way does not provide meaningful results. If there is a strong rationale for this please include, otherwise I would suggest re-running the analyses with these outcomes as continuous variables.

Response: Thank you for this comment. We dichotomized these individual screen behaviors due to non-normal distribution. We also checked that any possible correction for distribution would not help. There are quite many children, who had zero minutes user time in DVD and computer, for instance. Therefore, we considered that logistic regression analyses as the best solution in this case. We anyhow noticed that we have not pointed out this clearly in our manuscript, and we added the clarification in our manuscript (please, see the page 7).

Comment: Secondly, the increased number of statistical tests performed increases the potential for Type I error. I would recommend the authors control for this appropriately, e.g., by using an approach such as the Benjamini-Hochberg Procedure (Benjamini Y, Hochberg Y. Controlling the False Discovery Rate: A Practical and Powerful Approach to Multiple Testing. Journal of the Royal Statistical Society Series B (Methodological). 1995;57(1):289-300.)

Response: Thank you for this comment. We conducted the Benjamini-Hochberg procedure for our analyses, and added information about this procedure in our manuscript (please, see the page 8 and 14).

VERSION 3 – REVIEW

REVIEWER	Katherine Downing Deakin University, Australia
REVIEW RETURNED	19-Sep-2017

GENERAL COMMENTS	Thank you to the authors for making these changes. I have no further comments.