Author’s response to reviews

Title: Seasonal variation in objectively measured physical activity, sedentary time, cardio-respiratory fitness and sleep duration among 8-11 year-old Danish children: a repeated-measures study

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Author’s response to reviews: see over
Dear Editor

We are very pleased that both reviewers think that our findings are important and we thank you for the opportunity to resubmit a revised version of our manuscript. We are grateful for the comments and suggestions from the reviewers and we have revised our manuscript accordingly. Below you will find our point-by-point response to all comments from both you and the two reviewers.

Editorial Request

Please supply English language versions of all questionnaires developed specifically for use in this study, and upload them as additional files.

As this was a study only running in Denmark there exist no English versions of the demographic questionnaire. We have, however, uploaded the Danish version of the questionnaire.

Reviewer: Alex Rowlands

This paper uses a repeated measures design to assess the seasonal variation in physical activity, sedentary behaviour, fitness and sleep time in children. These measures are frequently taken using a single measure; this study allows the assessment of how representative one off measures of these variables are. I have a number of suggestions for the authors to consider.

Major essential revisions

1. The discussion could focus more on the effect of using a single snapshot measure to represent a given lifestyle indicator. This is an important message from this paper. For example:

   a. If a single measure has to be used when would be the best time of year to take a given measure? I.e. what was the most representative season? This is addressed in a study on adults for pedometer steps by Kang et al. (Measurement effects of seasonal and monthly variability on pedometer determined data. J Phys Act Public Health. 2012: 9; 336-43).

   This is an excellent suggestion. We have now included measures of both “bias” and “mean absolute percentage error” to address when would be the best time of year to take a given measure. We have, therefore, incorporated this throughout the paper (e.g. we have split the former table 2 into two separate tables).

   b. The relationship between activity measures and CRF was stronger when using the average of three measures. What about the relationship BMI Z score (or weight status)? Measures of health outcomes may be similarly strengthened if using more representative measure of activity and/or sedentary time.

   This is correct; however, the coop of this paper is not about health outcomes.
c. The prevalence of meeting the activity guidelines differed according to the season measured. This deserves more discussions. What are the implications of this? What are the authors’ recommendations?

We agree that this could be more highlighted in the discussion. We have added a comment about this (l. 299-304).

2. Descriptive information is given for boys and girls separately in the Table 1. However, the data for boys and girls is collapsed for other analyses (although sex is used as a predictor in the multiple regression). Consider analysing the data for boys and girls separately. This may be particularly pertinent given the age group. For example, the observed decrease in performance fitness with puberty is more likely in girls than boys.

Thank you for this comment. We now realize that our argument for collapsing the data was not present in the statistical section. The overall effect between our variables of interest and season or weekday/weekend day were the same among boys and girls (except sleep duration between weekdays and weekend days) and data for boys and girls were therefore combined to improve clarity and maximize statistical power. This is now stated in the statistical section, and sleep duration between weekdays and weekend days are presented separately for boys and girls in a footnote of table 3.

3. Statistical Analysis: Consider looking at the effect of season X weekday/weekend instead of collapsing data across seasons for the weekend/weekday analyses. Most studies take a single measure so the magnitude of the difference between weekdays and weekends during different seasons is of interest.

This is a very good suggestion. We now show weekdays and weekend days from all three seasons in table 3. Furthermore, we present the ratio between weekend days and weekdays and test if these ratios differ according to season. By doing so we now conclude that it is not only during winter and during weekend days that activity level is lowest, but specifically during weekend days during the winter (see also answer to question 1c above).

4. Statistical analysis: state how sedentary time was adjusted for wear time. Were the other activity variables adjusted for wear time? If not, why not?

Thank you for bringing up this question. Sedentary time was adjusted for wear time in the regression analysis in table 3 (included as a covariate), but not adjusted for in table 2 or 4. As our data include 24 hour monitoring and the fact that very little time was scored as non-wear time (see l. 134) we believe that normalising for wear-time would normalise for sleep duration. We therefore decided not to adjust for wear time in any of the tables. Instead, we mention whenever adjusting for wear time would have made any difference. The same applies for total PA and MVPA (see l.212-215).

5. Results: CRF was 4% higher in spring. How does this compare to the repeatability of the test? In 3rd paragraph of the discussion it is stated that the children ran for longer in the second test in a...
validation study. How much longer? And how does that compare to the differences observed over seasons in the current study?

In the validation study they ran 15 m longer in the second test. It translates into 0.5 ml/kg/min. In our study they improved 0.8 ml/kg/min from first to second test and 0.9 ml/kg/min from second to third test. It therefore seems as at least a part of the improvement could come from trying the same test multiple times and not due to season. We have written that more explicit in the text (l. 277-282).

6. Results: Present the accelerometer wear time.

We have now presented accelerometer wear time excluding sleep (see l. 134).

7. Discussion: Paragraph 4. What is meant by ‘Canadians are twice as sensitive to 10 mm rainfall...’?

We agree that this sentence was not clear and have now rephrased it in order to avoid misunderstandings (l.292-294)

8. Discussion: Paragraph 7. N.B. these variables don’t all add up to 24 h as light activity is not accounted for.

You are right. We have now changed the wording and say “that they are a part of the same 24 hour” and not that they add up to 24 hours (l. 330).

9. Conclusions: The proportion meeting MVPA guidelines changed according to season. This is key and should be stated here.

We have now stated this in the conclusion (l.377-378) and discussed it a bit more (see comment 1c).

a. If activity levels are lower in certain seasons they may be good times to intervene, but is it possible that these are difficult times to intervene as it may be harder to increase activity then?

See answer to comment 1c.

Minor essential revisions
10. My understanding of this paper is that repeated measures were taken of the same children at 0 months, 3 months and 6 months. If this is the case ‘a cross-sectional study’ is not appropriate in the title. This is also stated in the ‘Strengths and Limitations’ section. Consider the term ‘observational’ instead.

We agree. We have now changed the title to “…a repeated-measures study” and removed it from the “Strength and Limitations” section.
11. Background: ‘as the reliability and validity of habitual….’ Insert ‘estimates of’ or ‘measures of’ before ‘habitual ….’.

Thank you. This is now corrected (l. 54)


Done (l.105)

13. Physical activity and sleep assessment: The Evenson reference for the paediatric cut-point should be given as well as the reference for the Trost comparison study which demonstrated that it was the most accurate out of the ones they tested.

We agree. The paper is now sited (l. 119).

14. Anthropometric measurements: Were these taken at baseline only?

No, these were taken at months 0, 3 and 6, but for this paper we have only presented baseline anthropometrics in order to describe the population as this paper does not deal with changes in anthropometric measures.

15. Statistical analysis: Two way ANOVA. Presumably this analysed the control and intervention group from the original study which were collapsed for this study as there was no effect of intervention on the variables of interest. Please be explicit here – there has been no mention of the control group.

You are absolutely right. The control and intervention group from the original study were collapsed for this study. We have stated that more clearly now (l. 168-169).

16. Discussion: First paragraph. Specify the variables rather than healthier lifestyle (and elsewhere in paper). This only refers to activity measures.

We have now removed the wording from the discussion and conclusion, and specified the variables instead.

Discretionary revisions
17. Physical activity assessment: Why was a 60 s epoch selected? It is recommended to use shorter epochs when assessing activity in this age group.

To our knowledge our epoch (60 sec) is comparable to the majority of studies published in the literature. We used the recommendation by Trost et al. (2011) that recommended the Evenson cut points to estimate time spent in sedentary, light-, moderate-, and vigorous-intensity activity in children and adolescents. Their comparison study was based on 60 sec epoch, reinforcing our decision to use 60 sec epoch. We are well aware that shorter epoch might have resulted in more time in MVPA; however, the use of epoch adjusted activity count cut points for data collection at
shorter epoch lengths should be used with caution until validity has been formally established for shorter epochs. We have now discussed this (l.360-362).

18. Why was a period of 15 minutes selected to indicate non-wear? This will likely have included sedentary time.

This is a good point. The vast majority of studies to date have asked participants to wear the accelerometer during day-time only, and to take it off during the night. As the children in our study were asked to wear the monitor at all times (also during the night) we defined a non-wear criterion that was also a non-awake criterion. We are aware that the likelihood of including sedentary time in the non-wear time increases (and thereby underestimate sedentary time) with lower periods of time to indicate non-wear, however the non-wear criteria of 15 min was a compromise as it removed non-wear during the day and also removed non-awake between bedtime and midnight and 6am to wake up time. We have now rephrased the method section to avoid misunderstandings (l. 110-115).

19. Table 2: Consider presenting the distance run as well as the estimated VO2max.

We have added the distance run in the CRF-test as a footnote to table 2.

20. What was the rationale for investigating the effect of parents born outside Denmark? No explanations or interpretations appear to be presented for this.

Our hypothesis was that the lifestyle indicators would be different among children with different ethnicity. We used number of parents born outside Denmark as a proxy for ethnicity. This is now mentioned in the method section (l. 155).

21. Strengths and limitations: It is stated that the effect of school can be assessed. What was the effect of school?

We are not interested in the effect of school in this paper but to highlight that the effect of season that we found are independent of school and school neighbourhood. We have now highlighted this in the revised version (l.347-348)

22. It appears to me that the 3rd paragraph of the discussion is more key to the main focus of this paper than the 2nd paragraph. Consider re-ordering.

We believe this could be done both ways; however, we think that it is best to discuss “levels” before “seasonal variation” and, therefore, we decided to keep the ordering.

Minor issues not for publication

23. Abstract: Results, last sentence. Presumably this should read ‘intraclass correlation’?
Thank you. This is now corrected (l. 41)

24. Study population: ‘Children were excluded from the ....’ Presumably the criteria should read ‘3 weekdays and 1 weekend day’?

Thank you. This is now corrected (l. 99)

25. Results: ‘No inter-individual differences were found...’. This compares children meeting and not meeting inclusion criteria? No need for ‘inter-individual’.

Agree. We have removed “inter-individual”.

26. Results: 2nd paragraph. ‘4th graders accumulating 5.4 min/day less MVPA, being 32.4 min/day more sedentary...’ Should this be 28.8 min/day?

Thank you for pointing this out. By mistake we presented the estimate unadjusted for wear-time in the text (32.4 min/day) and in table 3 we presented it adjusted for wear-time (28.8 min/day). As we no longer adjust for wear-time in our analysis (see comment 4) the correct number is now 32.4 min/day (see comment 4).

a. Same paragraph: puberty: 12 min/day more sedentary. Should be 12.4 min.

Same as above. It should now be 12.0 min/day. 12.4 min have been changed to 12.0 in table 4.

27. Discussion: Paragraph 5. In this paragraph there are a couple of references to differences between weekends and between weekdays and variation during weekends etc. Should this be between weekdays and weekends?

We now see that this could be misunderstood. As we now calculate the ratio between movement behaviour at weekdays and weekend days we have deleted these three lines.

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Needs some language corrections before being published

The paper has been proof-read by an English proof reading service.

Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests: I declare that I have no competing interests

Reviewer: LUIS GRACIA-MARCO

The aim of the present article is to examine between-season and within-week variation in physical activity, sedentary behaviour, CRF and sleep duration in a large group of 8-11 year-old Danish
school children. Additionally, authors reported the level of these lifestyle indicators, identified correlates of the lifestyle indicators and investigated the connection between them. Overall the manuscript is well written but some comments must be considered to make it clearer and improve its quality.

**Background:**
- Is overweight/obesity one of your outcomes in this article? There are some allusions along the manuscript and you are not using body fat, BMI...as outcomes in your article.

No, overweight/obesity is not an outcome in this paper; however, knowledge derived from this study could potentially help tackling obesity; hence the paper is put into this context.

- “…accelerometer determined sleep duration...”- this is a bit confusing as in your methods it seems sleep duration was self-reported. Please, clarify.

*We now see that the description in the methods could be misunderstood. Only the duration of time spent in bed was self-reported. Based on accelerometry every minute of time spent in bed was analysed for sleep using the algorithm by Sadeh to estimate sleep duration. We have now highlighted that in the methods section (l. 124-127).*

- “...in northern Europe”- it would be useful to comment before this that your study has been done in northern Europe.

*We agree that it is important to state that this has been done in northern Europe as early as possibly in the paper; however, we believe that line 16 in the background is early enough.*

**Aim:**
The authors said that this is a cross-sectional study, however it is confusing as when I read the methods it seems you did a longitudinal study. In the methods you said “Children were excluded if they did not have valid measurements from all three seasons in either PA...”. Therefore, it seems longitudinal. Could you please clarify?

*Same comment as Rowlands comment number 10. We have changed the title to “A repeated-measures study” to take into account the 3 time points of assessment.*

**Methods:**
- Mean age of participants?

*Mean age of all participants is now included in table 1.*

- Clarify cross-sectional vs. longitudinal as stated above.

*This has been corrected*
- 60 seconds epoch should be acknowledged as a limitation of the study and discussed. Children’s PA is characterized by short periods of time and 60” epoch has been shown to underestimate vigorous PA in children (See papers Rowlands AV 2006, 2007)

*See Rowlands’ comment 17. Furthermore, we did not estimate vigorous physical activity, but only moderate-to-vigorous PA.*

- 60 minutes disregarded before analysis. Is there any evidence that support this?

*See Rowlands’ comment 18.*

- CPM expressed as total vertical counts. This is not clear as you used a triaxial accelerometer. Did you set the accelerometer to count only in the vertical axis? If yes, why? Please, clarify.

> We collected all three axes, however, we only used the vertical axis to be able to compare with other studies and to use validated cut-offs. We, however, reran our statistical analyses and it did not make any difference to the results whether using 3-axis vector magnitude or only the vertical axis. (l. 362-364)

- Difference in sleep duration...(page 5) is 3.8 min. The authors said this is a small difference however p<0.001. Therefore, although small it seems the two approaches significantly differ?

> This is of course correct; the two methods significantly differ by 3.8 min as stated in the method section. However, as only 4% of all sleep logs were missing and the fact that the bias was only 3.8 min for these individuals, we believe that this has not affected our results significantly.

- CRF- this is a suggestion, do not start a sentence using abbreviation (apply to the whole document), it is not formal. In addition, can you briefly give some more information about the test?

*Thank you for this suggestion. We do no longer start a sentence using abbreviations.*

- Questionnaire data: any reference that support not to include some active video games in screen time?

> We are not aware of any studies; however, we were only interested in estimating inactive screen time.

- Statistical analysis: Did the authors checked the normality of the variables? Did the authors checked if there was sex-interaction to justify why showing the data males and females together? Qui-square test should be Chi-square test. The authors talk about the effect of an intervention? Did you do an intervention? Could you please re-organize a bit this section using full-stops? Please clarify all this issues.
a) After dividing into weekday and weekend days at all three seasons (as suggested by Rowlands’ comment 3) weekday and weekend days of total PA and MVPA are no longer normally distributed. Therefore, we present these as median (IQR) and transform before testing (See statistical section)

b) Sex-interaction (see answer to Rowlands comment 2).

c) Qui-square is changed to Chi-square test.

d) As mentioned in the beginning of the methods section, the larger study is a cluster-randomised cross-over trial. There was no intervention effect on the variables of interest and, therefore, we were able to look at seasonal variation (l. 168-170).

Results:

- Table 1 shows results separately for boys and girls, but then all tables analyse participants all together. Would be interesting to include an additional column with the whole sample descriptive characteristics.

This is a good suggestion. An additional column with the whole sample is now included in table 1

- Table 3. Less PA, more sedentary time, less sleep time and more CRF? This needs to be discussed in the discussion.

We assume that this question refers to the beta-coefficient for age. After running all regressions again we found that the p-value for the association between age and MVPA was previously reported as 0.005, however it should be 0.046 (n=635). We then ran the regression analyses between age and MVPA again, however this time only with the individuals having a valid fitness test (n=413), and found that the association was no longer significant (Beta= -2.4, P=0.17). We have corrected this mistake and added this non-significant p-value to the footnote in table 3.

Discussion:

- last line of page 7; (e.g. cut-off and wear time criteria). What about epoch? There were differences in epoch setting? This needs to be discussed at some point in the discussion. Smaller epochs are more accurate specially when working with children.

Both studies used 60 sec-epoch as in our study. This has now been mentioned (l. 240). See also answer to Rowlands’ comment 17.

- page 8, last lines of 1st paragraph. “Using the objective measurement of sleep...”, in the methods the authors said that sleep time was self-reported?

See answer to this comment above (question to background).

- page 8 “studies looking at seasonal changes in sedentary time using accelerometers are sparse, inconsistent and cross-sectional of nature. Are cross-sectional studies in relation to these outcomes a limitation? Recent reviews have shown that both cross-sectional and longitudinal studies have provided consistent findings regarding seasonality and PA levels (Carson and Spence 2010; Rich et al 2012).
A cross-sectional study cannot be used to calculate the individual variation (ICC) and the mean average percentage error (MAPE) between seasons. By presenting these ICC, future studies can correct for these ICC and get an estimate of the real effect even if they only measure at one point in time. Furthermore, if it is only feasible to measure once the MAPE will give an indication as to when to measure a given lifestyle. Therefore, we see cross-sectional studies to be limited in relation to these outcomes.

- page 10, line 6. As these children did not accumulate more MVPA...maybe this is influenced by your epoch?

See answer to Rowlands comment 17 (l.360-362).

- page 10, paragraph 2. Our negative correlation observed between sleep duration...and viceversa. These lines are too repetitive. Re-write please. Why do you think there is a negative correlation?

Given our study design we can only speculate about this negative correlation; however it has been suggested that the inherent activity level could manifest itself in higher activity level both during the day and the night, which by definition results in shorter accelerometer determined sleep duration. We have now discussed this in the revised version (l. 332-335).

Limitations: We did not use information from diaries...However, using a diary to note down non-wear time is an effective... Both ideas seem opposite. First you said is ambiguous and then is effective? Please, clarify.

We now see that this is a bit confusing. We made the participants fill out diaries as this has been shown to improve compliance, however we did not use them as they are often ambiguous and imputation conflicts with the objectivity of accelerometer data. To avoid confusion and at the same time limit the number of words we have deleted these four lines.

Conclusions
Why do you talk about southern latitudes? In this study you have data from northern europe, so conclusions should be limited to this.

We assume the same issues apply at southern and northern latitude; however, we agree that it would be better to limit the conclusion to northern (l. 379-380).

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