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

**Authors:**

- Mads F Hjorth (madsfiil@life.ku.dk)
- Jean-Philippe Chaput (jpchaput@cheo.on.ca)
- Kim Michaelsen (kfm@life.ku.dk)
- Arne Astrup (ast@life.ku.dk)
- Inge Tetens (intet@food.dtu.dk)
- Anders Sjödin (amsj@life.ku.dk)

**Version:** 5  **Date:** 27 August 2013

**Author's response to reviews:** see over
Dear Editor,

We are very pleased that the reviewers think that we have done a good job addressing the comments and that the manuscript is much improved. We are grateful to the last minor comments and suggestions raised 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

I would also request that the discussion clearly highlights in the limitations the potential misclassification of MVPA due to the selection of 60 second epochs. Finally, the authors need to make clear why they did not separately examine VPA and MPA as well as MVPA.

We have now clearly highlighted the potential underestimation of MVPA (time spent ≥2296 cpm) when using an epoch length of 60 sec compared to shorter epoch lengths (See line 365-369). We did not separately examine moderate PA and vigorous PA as accelerometers have a limited capacity to correctly classify these (especially) when using an epoch length of 60 sec. Instead, we decided to only use MVPA because it can be compared with the PA recommendations (See line 369-371).

Reviewer: Alex Rowlands

The authors have done a good job of addressing my previous comments. I have a few remaining comments:

1. The addition of MAPE and bias in Table 2 and the discussion is informative. Please also refer to these in the results (lines 190-195).

We have now added a sentence about bias and MAPE in the result section (See lines 193-194).

2. Line 113. Bed time and wake time is mentioned here but this does not appear in the methods until the final paragraph. It needs to be clear where this data comes from.

Thank you for this comment. We did not use self-reported bed time and wake time here. We have removed the wording bed time and wake time from this paragraph. Please also see answer to your question four below.

3. I commented on the selection of 15 minutes of consecutive zeros to determine non-wear time (comment 18). I appreciate the authors’ response but it is still not clear why 15 minutes was selected. Was it empirically determined? Based on examination of the data? Based on an earlier study?

It was selected based on examination of the data. It was a trade-off between wanting to determine non-wear time during the day (desired min of consecutive zeros was 15 or more) and at the same
time wanting to remove sleep (desired min of consecutive zeros was 15 or less). Please also see answer to your question four below.

4. I am confused as to why data were removed for consecutive periods of wear time shorter than 60 minutes during reported sleep time (lines 113-115). I do not follow the rationale for removing periods of movement during reported bedtime? If significant movement occurred here presumably the reported bed times were erroneous and this would be important information.

Thank you for this comment. We see that this is still not explained well enough. Self-reported sleep time was not used to derive physical activity and sedentary time. We removed consecutive wear time periods of less than 60 min during the entire day in order to remove non-awake time (outside midnight to 6am) as sleep for most children is characterised by minor periods of movement that we did not want to include in our analysis of physical activity and sedentary time (See lines: 110-115).

5. I am still unclear about the analysis of the parents outside Denmark. The authors state this is a proxy for ethnicity (line 155) and in the reply to my previous comment (20) state that this was included as they hypothesised that lifestyle indicators would differ by ethnicity. However, I can’t find this hypothesis or rationale for it or discussion of it in the text.

We did not have any specific hypothesis of which lifestyle indicators that would differ according to ethnicity, but merely included ethnicity as we speculated there could be some interesting differences to report. We have now incorporated this in the revised version of the paper (See lines 323-326).

6. CRF (lines 137-142): State the validity and reliability of this test.

Validity and reliability of this Andersen test were r=0.64 and r=0.84, respectively. We have now incorporated this in the revised version (See lines 140-141).

7. Lines 190-192: Sed time, MVPA and CRF differences are given in %, sleep in minutes. Present all variables as % differences or as absolute minutes.

We have now changed sleep duration from minutes to percent change in the abstract and result sections (See line 38 and line 192).

8. Lines 250-255: Are the recommendations from the National Sleep Foundation based on time sleeping or time in bed? As with comparison to MVPA guidelines, differences and similarities between the measures used to create the guidelines and the measures used to estimate prevalence will affect our interpretation of them.

We agree that it is difficult to compare both our self-reported “time in bed” and our accelerometer determined sleep duration directly with the recommendations from the National Sleep Foundation, and we merely want to highlight the large difference between self-report and accelerometer determined sleep. As far as we know, the recommendations from the National Sleep Foundation
are not really evidence based but mainly the result of expert consultations. The National Sleep Foundation call it “hours of sleep” and not “time in bed”.

As for MVPA, guidelines are based on studies using self-reported data which tend to overestimate activity. The number of children fulfilling the physical activity recommendations might, therefore, be underestimated in this and other studies that use accelerometers. We have now incorporated this (line: 359-362).

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

Reviewer: LUIS GRACIA-MARCO

The quality of the manuscript is improved after the corrections. Please, consider this comment to include as an additional minor limitation:

- Questionnaire data: time spent video games. There are other video consoles in which you do not need to move to play. Therefore, the authors might be missing some time spent in video games. The addition of a comment on this would be recommended in the limitations section.

We appreciate you comment and agree that our definition of screen time could be misinterpreted; however, we already included the non-active video consoles in screen time. In order to avoid misunderstandings we added a couple of words in the revised version (See line 157).

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'