Author's response to reviews

Title: Does school-based physical activity decrease overweight and obesity in children aged 6-9 years? A two-year non-randomized longitudinal intervention study in the Czech Republic

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Author's response to reviews: see over
Response to reviewer's report to manuscript “Does school-based physical activity decrease overweight and obesity in children aged 6-9 years? A two-year longitudinal study in the Czech Republic” (MS: 7474852947082031)

We thank the reviewer and the Associate Editor for their constructive comments that improved our manuscript substantially. We have amended the manuscript to the satisfaction of all the comments. All our responses (in cover letter) and amendments (in manuscript) are in blue ink for easy identification of the changes.

Reviewer: Nicola Diane Ridgers

We agree with and thank the reviewer.

1. The use of the term longitudinal can also indicate cohort studies. Suggest using longitudinal intervention study in the manuscript for clarity.

This comment is similar to the first comment of the Associate editor. We have now changed the formulation of the title of the manuscript. We have now also employed the term ‘non-randomized longitudinal intervention study.’

2. The bullet points were removed, but the points were not integrated in to full sentences. I suggest that this is done.

This has now been amended.

3. Can it be clarified that the PA program implemented was part of the Healthy Schools Program? In addition, did the schools have to meet any criteria to be intervention schools?

The implemented PA programme was part of the Healthy Schools Program. The two chosen schools meet the same four criteria to be intervention schools. This has now been clarified.

4. Page 9, last sentence. How was the intervention sex-specific?

This comment is similar to the 4a comment of the Associate editor. We have now clarified it - a feature of this PA intervention was gender-specific – one of the teachers organised the PA programme for girls; and another teacher organised it for boys.

5. How did you control for clustering using an RM ANOVA?

We control the clustering for RM ANOVA according to the school attendance list and PA log book.

6. The finding that more girls than boys met PA recommendations is an unusual finding. Some discussion of this may be useful for the reader.

We have now inserted short part of discussion on page 18, lines 11-14.

The Associate Editor:

We agree with and thank the reviewer.

1. Title: It should be clear from the title that this is a "non-randomized longitudinal intervention study". Simply saying it is a longitudinal study means that some readers may think it is a longitudinal observational study (cohort study).

This has now been amended.
2. Abstract: The study method needs more description: please state that this was a non-randomized study and that intervention schools were selected on the basis of an existing pro-activity environment.

This has now been amended.

3. Introduction: revise the sentence: "however, this positive association on weekends PA is still not well investigated."

This has now been revised.

4a. Methods: revise the sentence for clarity: "Hence, children were free to play together with same or both girls and boys in couples, threesomes and small groups. However, the same gender playing will not be prohibited by research team."

This has now been amended.

4b. Methods: Revise the sentence for clarity: "co-education teaching a little more"

We have now clarified it. We put short characteristic of co-education teaching.

4c. Methods: Should this sentence read "not reset"?: "Monitors were no reset throughout the day"

Thank you for your thoroughness. This has now been amended.

Discussion: At the end of the sentence "This study has limitations. The intervention schools were selected in a nonrandomized manner, and the representativeness of our children to the wider population of children in the Czech Republic requires that caution is exercised when drawing generalisations." I would like to see an acknowledgement that not only was the selection of the schools non-random, it in fact may have biased the outcomes in that the intervention schools were those with environments (physical and cultural) that were already more conducive to physical activity. It is possible that this, rather than the intervention itself, may have resulted in the outcomes described. Other descriptive characteristics of the intervention and control students (socioeconomic position in particular) at baseline would also be helpful since it is also possible that

We underlined the physical activity conducive environment as a factor of selection of intervention school in limitations of the study. We also inserted suggested sentence of “Other descriptive characteristics of the intervention and control children (socioeconomic position in particular) at baseline would also be helpful to more complete assessment of the effectiveness of a school-based two-year PA intervention programme in reducing obesity and overweight in 6-9 year-old children”.

Thank you for your thoroughness. This has now been amended.