Author's response to reviews

Title: A repeated measurement study investigating the impact of school outdoor environment upon physical activity across ages and seasons in Swedish second, fifth and eighth grade pupils

Authors:

Peter Pagels (peter.pagels@lnu.se)
Anders Raustorp (anders.raustorp@ped.gu.se)
Antonio Ponce De Leon (Antonio.Ponce.De.Leon@ki.se)
Fredrika Mårtensson (fredrika.martensson@slu.se)
Maria Kylin (maria.kylin@slu.se)
Cecilia Boldemann (cecilia.boldemann@ki.se)

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Author's response to reviews:

Editorial comments:

"The authors are requested to respond to each of the reviewers' comments. In particular they should be much clearer about gaps in the existing literature and the specific contribution of this study. The specific aims of this study should be clearly stated and the discussion should be more focused on the extent to which the aims were met. Further, more detail is required on the processing of the accelerometer data including a more robust defence of only selecting 1 day as a minimum wear time (minimum hours per day?) as well as how differences in wear time were standardised. This is particularly important for estimates of sedentary time. Finally the manuscript would benefit from improvements in grammar."

Response to Editorial comments:

The literature has been supplemented and the text upgraded accordingly. The aims of the study have been specified as required by reviewer 1, items 1 and 2, and the abstract has been rewritten accordingly. The processing of the accelerometer data has been elaborated in the text, including a more robust defence of 1 day as a minimum wear time in line with comment 2 Minor revisions made by Reviewer 1, as well as the average number of valid days across the sample. Also, the language has been brushed up and the manuscript scrutinized to remove faulty grammar.

Reviewer 1

Discretionary Revisions

1. In the Background section, the following statement could be expounded upon: “Outdoor environment has been proven to be a strong predictor of PA, but the
evidence of its actual role for PA is conflicting or unclear." (Line 82)

We admit that we overdid the statement, we have changed it as follows: “The role of the outdoor environment may be a relevant predictor of PA”. (beginning line 78)

2. The research aims do not map easily onto the analysis plan and results section, making the paper harder to follow.

The research aims have been reformulated as follows (starting line 91) “… that PA decreases by age [Tudor-Locke et al 2006, Ridgers et al 2012], and that the outdoor environment impacts physical activity [Fjortoft et al, 2009]. This study will further investigate the impact of outdoor environment at compulsory school upon the pupils’ patterns of physical activity considering season, age and gender”.

3. The use of the Actilux technology is a new and interesting direction for the field. Though there are potential limitations to this approach, the authors reconciled by comparing to direct observation. The results of these comparisons could be explicitly presented and highlighted as a strength in the discussion section.

We made a point of that in the discussion section (lines 363-364): “Further, comparing the Actilux readings to the recordings of outdoor times by direct observation made the data of clocked outdoor time more reliable.”

4. Authors state, “The weather conditions were uniform within each measurement period and similar during all season…” There is no descriptive data presented here showing differences between schools for these variables.

We have added the following section in the results section, lines 237-239: “The weather during fieldwork was generally sunny with clear or partly cloudy skies, except for one rainy day in September at Forest 2, and one windy day with snowfall in March at Art Grass 1”.

Minor Essential Revisions

1. It is unclear how time spent at school (Line 241) and time spent outside (line 243) for each age group was calculated. Adding more detailed information about this measure would be important for repeatability.

We have added the following section in the results section, lines 256-259: “Time spent at school and time spent outdoors was the same as wear time of accelerometers. Time was calculated from the raw data stored in the accelerometers’ memory and outdoor time was separated from indoor time with data from the Actilux, described in the methods section.”

2. On Line 184, the authors state, “There was no significant difference between the total MVPA averages for one or more days of monitoring, thus all children with at least one day of data were included in the analysis…” The results of the statistical test should be presented, as well as descriptive data regarding the average number of valid days across the sample.
We have added the following section in the results section, lines 187-189: “The mean numbers of measured schooldays per child were 4.3, 4.7 and 4.2 in September, March and in May respectively. A one-way ANOVA test showed no differences between measure days (1-5 days) and daily minutes in MVPA, p > 0.05.”

3. Descriptive results for minutes of MVPA and %MVPA by grade and by school are not clearly described, and it is unclear whether or not statistical tests for differences between schools within a given grade were done.

These analyses were done in the mixed model that was also tested for sensitivity, as commented by reviewer 3. We have amended table 2 with relative MVPA data, which may also in parts cover the comment made by reviewer 2 regarding the results section.

4. There is a statement in the abstract referring to sun exposure that does not appear anywhere else in the manuscript.

“Sun exposure” has been removed from the abstract

5. A number of grammatical errors were found throughout the manuscript.

The grammatical errors have been found and removed.

Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

1. In the Background section, the authors note a number of previous studies that explore the impact of the outdoor environment on physical activity levels. These findings are not sufficiently summarized. There are also a number of important and more recent articles (including review articles) addressing the school environment and/or seasonality that are not included. Additionally, an obvious gap in the literature is not clear from the authors’ introduction. Finally, the introduction did not clearly lead up to the final paragraph stating the aim of the paper, which was broad and not well defined. Clearly outlining the aims (and hypotheses), building a case for the importance of the current study, and showing how the current study will build on previous research could improve the impact of the current manuscript.

This comment reviewer 1 shares with the editor and reviewer 2 (Major compulsory revisions). The comment also mirrors Reviewer 1’s comment no. 5. We have added 5 references (2 of them being the ones required by reviewer 2), and the background section has been upgraded accordingly. One reference has been removed (Ridgers et al 2007) as in our opinion that reference was covered by Ridgers et al, 2006, and 2012. Further, Table 2 has been amended with data, and Figure 1 has been added (same response to comment on Results by reviewer 2).
2. In general, details about measurements and data reduction techniques are minimal. More information about how measures were implemented and analysed would strongly improve the manuscript. Along with this, providing information about reliability and/or validity of measures used could be a great addition to the findings. A specific example would be to including more detailed information about how wearing and non-wearing time was distinguished in accelerometry data reduction.

Accelerometer non-wear didn’t apply as the pupils wore the accelerometers during their entire scheduled time during the school day and as long as they stayed at the school premises (2nd graders). We amended the text as follows, starting at line 259: “The pupils wore the accelerometers during their entire scheduled time during the school day and as long as they stayed at the school premises (2nd graders). Throughout the seasons, and during all days the pupils engaged in activities that enabled measurement by accelerometry, except of swimming lessons in 2nd grade at Art Grass 1 (40 min) and City 3 (40 min) in September, and at The Hill 4 (40 min) in May”.

3. The analysis section is also lacking in overall detail making future replication challenging. Because children were nested within schools and have repeated measures over time, the proposed mixed model is appropriate. But, more detailed information about the statistical models used to test the main outcomes is necessary in backing up the interpretation of the findings.

This comment has been responded to under the comment made by reviewer 2 about the Methods section.

4. The fact that only 4 schools were included greatly limits the ability draw conclusions about the impact of school-level (e.g. environmental) factors. The authors appropriately note this limitation, and state that due to budgetary restraints chose a smaller number of schools in exchange for higher quality measures. But do not fully discuss the assumptions being made in order to have confidence in the results. The authors might also address why they did not match schools on potential confounders (e.g. socio-economic status, population size), which could have decreased the severity of the assumptions being made.

A matching of confounders was indeed made but not explicitly described in the Methods section. We have amended the text with the following sentence (lines 110-114):

“Further, the schools were selected to reflect the socio-economic composition of the municipalities of the whole country, with a majority of the population living in medium-sized cities outside or on the outskirts of a metropolitan area (Statistics Sweden). The ISCO code was applied for socioeconomic classification (European socio-economic classification, ISCO, 1988).”

Regarding the assumptions being made in order to have confidence in the results we tried to include all possible components or behavior settings that are usually contained in, including the vegetation typical for the climate. We added a
sentence in the Methods section: “In selecting school outdoor environments we considered all typical behavior settings and combinations of behavior settings that are representative for Northern and central European school outdoor environments.” The passage was inserted as the second sentence (line 123).

5. The contributions this study makes to the field are not clearly delineated throughout the discussion and conclusions. By more explicitly describing the current findings in comparison to previous findings, the contributions made could be more obvious to the reader. Additionally, the authors should avoid overstating the findings, as the study design was experimental in nature. Potential confounding at the school levels seems to be a major limitation of the analyses and hinder the conclusions drawn. Finally, the manuscript could be improved by stating more fully the potential further directions or necessary next steps generated by the current findings.

We have upgraded the relevant parts of the discussion section and the conclusions. In the Discussion section we have referred to the findings of recent or fairly recent articles now included, namely Tudor-Locke et al, 2006, Ridgers et al, 2006, Ridgers et al, 2012 and Fjortoft et al, 2009, the two latter requested by reviewer 2 to be included. This upgrading was done in accordance with the upgrading of the aims (last sentence of the Background section). Finally, in the Discussion section we have rephrased our conclusions: “The school outdoor environment may thus be a potentially health promoting factor during school time”, in order not to overstate our findings.

Reviewer 2

Major Compulsory Revisions

The ‘why’ of the aim of this paper is not very clear. What exactly is ‘missing’ from existing studies (i.e. not known), and what does this study add to the field? Interesting data material, but unclear what this paper adds to the field. The presented findings seem a bit trivial, and the more interesting stuff is not shown. The analyses do not seem appropriate, or potentially, there appropriateness has not well been explained. Seeing that the theoretical explaining factors of this type of behavior are well described with an ecological model, a multilevel model with students nested in schools would seem appropriate. With school-level factors such as schoolyard size included at the school level, and individual correlates at the individual level. More specific, there are a series of issues with the methods, results and conclusion that need to be addressed.

We believe that we have responded to this comment by reformulating the aims of our study, as also has been commented by the editor and reviewer 1: “This study will further investigate impact of the outdoor environment at compulsory school upon the pupils’ patterns of physical activity considering season, age and gender.” (last sentence in the Background section).

Methods
How was the children’s actual use of schoolyard areas mapped in practice? Which criteria were used to identify accelerometer non-wear? Were school and/or classes included as a level in the analyses? A multilevel model with multiple measurements nested in students, nested in schools would seem appropriate. Were analyses adjusted for personal factors? LVPA, MVPA and VPA are not independent from each other; why were all 3 levels analyzed? The method section only describes MVPA. Were the accelerometers worn on the outside of clothing to be able to capture LUX? If so, how could that have affected the measurements? It seems that accelerometers on top of thick winter clothing would be less accurate in registering body movements.

Regarding the reviewers comment that LVPA, MVPA and VPA are not independent from each other we reconstructed Table 3 by removing LVPA and VPA.

Accelerometer non-wear didn’t apply as the pupils wore the accelerometers during their entire scheduled time during the school day and as long as they stayed at the school premises (2nd graders). We amended the text as follows, starting at line 259-263: “The pupils wore the accelerometers during their entire scheduled time during the school day and as long as they stayed at the school premises (2nd graders). Throughout the seasons, and during all days the pupils engaged in activities that enabled measurement by accelerometry, except of swimming lessons in 2nd grade at Art Grass 1 (40 minutes) and City 3 (40 minutes) in September, and at The Hill 4 (40 minutes) in May”

We believe that by “personal factors” the reviewer refers to confounders which were checked and found to be non-significant in bivariate analysis. We therefore amended the following sentence, starting at line 206: “Potential confounders related to PA and socio-economic status were non-significant and were thus removed.”

The accelerometers were worn outside the clothes and tightly strapped with elastic belts around the hip so that even thick clothes (applied only in March at Art grass 1 and Forest 2) that the fabric was compressed against the body. One of the purposes of our fieldwork was to constantly check that the accelerometers were correctly mounted and worn outside the clothes. We have added a sentence in the Methods section, line 176-178: “By ocular observation the accelerometers were constantly checked by the observer (one for each class) that they were correctly mounted and worn outside the clothes.”

Results
Results mentioned in line 271-280 are not presented in a table? They seem important.

These results have been partly integrated in Table 2, and Figure 1 has been added which we believe contribute to clarification. For further clarification text describing Figure 1 has been added in the Results section starting line 292:
“Compared to the other schools MVPA was significantly higher among 2nd grade girls at Forest 2, as well as the difference between boys and girls was non-significant. Nor was there a significant gender difference in MVPA at Hill 3 and City 4, but the overall levels were significantly lower. Relative MVPA, though high at Art Grass 1, differed significantly between genders (Fig 1). Among 5th graders MVPA was significantly higher at Art Grass 1 and Forest 2, compared to the two other schools. At Art Grass 1 relative MVPA was the same as that of the 2nd graders, but dropped among 5th graders at the other schools. The greatest decline in relative MVPA was observed among girls, particularly at City 4. Throughout the schools a significant gender difference prevailed, that was pronounced at City 4 (Fig 1)”.

Conclusions
The fact that there is a difference between the schools cannot necessarily be explained by the difference in how the schoolyards look. Based on this study it cannot be concluded that extended ball areas with ‘art’ grass (I assume this refers to artificial grass?) and green play areas enhance PA. Boys more active than girls is not new, nor is decline with age, nor is the fact that outdoor time is a good predictor for MVPA.

We have now stressed the fact that school outdoor environment may play a role, at least among younger school children; as such observations have been previously reported, both among preschool children and school children. We therefore added the following sentence in the Discussions section, lines 343 - 348: “Vegetation and woodland has previously been shown to increase physical activity among older preschoolers (Boldemann et al, 2006), and among school children (Fjortoft et al. 2009), particularly among girls. This is concordant also with our observations, showing that girls at Forest 1 spent significantly more outdoor time in MVPA than girls did at the other schools. Open spaces seem to contribute to maintaining MVPA among pupils as they grow older. Among girls, woodland seems to fulfill that function as well (Fig 1)”.

Discretionary Revisions
Useful additions to the reference list & background


The references have been added and referred to in the Background section.

Reviewer 3

Statistical reviewer:
The authors used mixed effects modelling to test for differences in MVPA between schools (with varying outdoor environments), seasons, grades/years and gender. Season was entered as a random effect to allow for the clustering of MVPA measures within individuals. This approach is frequently used for analysing longitudinal data as it accounts for the correlation between repeated measures made on the same people and is therefore appropriate for this study.

Minor though essential additional analysis:
The authors assumed that there was no pattern to the correlation/covariance between repeated measures by defining the pattern as ‘unstructured’. This can be problematic when there are many measurement occasions resulting in many parameters, but with just three occasions this dataset has clearly coped. However, the unstructured approach, whilst the most accurate, may have compromised their power unnecessarily and therefore I would recommend that the authors repeat the analysis trying a couple of possible/candidate structures to see whether it makes any difference to their key findings. If this ‘sensitivity’ analysis makes little difference then there would be no need to report this in the manuscript.

A ‘sensitivity’ analysis was run, and didn’t show any or only very little differences to our key findings, so we leave the text as it is.

The authors test for the main effects of season, school, grade and gender but do not formally test interaction terms of interest. For example, it seems intuitive to test the ‘school x grade’ interaction as presumably you would want to know whether the impact of ‘school outdoor environment’ on MVPA differed by age. If the ‘school outdoor environment’ only impacts on the MVPA of 11 and 14yr olds but not 8yr olds then this is an important message. ‘School x season’ may also be of interest.

For clarification figure 1 has been added.

Sincerely,

Peter Pagels