Author's response to reviews

Title: Active transport between home and school assessed with GPS: a cross-sectional study among Dutch elementary school children

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Author's response to reviews: see over
Dear editor and reviewers,

On behalf of the co-authors, We would like to resubmit our manuscript entitled: “Active transport between home and school assessed with GPS: a cross-sectional study among Dutch elementary school children (MS: 1348010730101726). We would like to thank BMC Public Health for the opportunity to resubmit our manuscript. We deeply appreciate the comments and suggestions made by the three reviewers. We have carefully revised the manuscript following each recommendation made by the reviewers. Our response to each point brought up by the three reviewers is listed in a separate Word document. The comments/suggestions of the reviewers were very helpful and have considerably contributed to improve the scientific quality of the study. We hope that the revised manuscript will be accepted for publication in BMC Public Health.

On behalf of the co-authors,
Yours sincerely,
Sanne de Vries

Reviewer 1
Reviewer: John Reilly

Reviewer's report:

General Comments

This is a generally well-written, though slightly lengthy, manuscript which describes an interesting study. The main strength of the study is the novelty arising from the use of GPS in active commuting to school research.

1.a I think the design and methods are essentially sound, and my main concern is over the extent to which the findings are supported by the results or are slightly overstated. Some statements are difficult to justify from my perspective (see specific comments below), and

In accordance with your comments we have been more careful in drawing conclusions from the study. It was not our intention to overstate the results in the study. We have thus adjusted our abstract, discussion and conclusion. For more detail see answer 5a and 5b. We are now more succinct in the Discussion while also taking into account the comments of the reviewers. This has resulted in deleting several sections in the Discussion. For example, we have deleted the description of the McMillan study[23], shortened comments about seasonality, and deleted the first section of the strengths and limitation.

1b I think there is a need for different (more cautious) terminology in places (e.g. ‘correlates’ better than ‘determinants’) – to me these are ‘Major Compulsory Revisions’.

We agree with the reviewer that based on a cross-sectional study design we cannot speak of determinants. Therefore we have followed the reviewer’s advice and this is now changed. For more detail see answer 2b.

1c I think that some additional caveats re. generalizability are indicated in the manuscript, particularly as the study is relatively small and cross-sectional, from one European nation with a relatively high prevalence of active commuting (particularly cycling), and conducted in one season only (Major Compulsory Revisions), and accuracy of various decisions and cutpoints for measurement of key variables in the analysis. The authors do acknowledge the main study
limitations in the Discussion though. Another general problem is the perception that the study simply states or confirms the obvious (that proximity to elementary school influences the probability of active commuting to primary school).

We agree with this reviewer that the results of the study might not be generalizable to the UK or US and have acknowledged these points more thoroughly in the discussion. For example, we have highlighted the differences between the Netherlands and the US and how these might be explanatory for the results (see also answers 2, 3, 4 for reviewer 2). Moreover, both in the Abstract and the Conclusion section of the revised manuscript (Conclusion, p.19) we started our conclusion with the words: “within the current research setting” to highlight the question of generalizability, and further elaborated on this topic by stating in the conclusion that “studies should investigate whether the results found in our study can be generalized to older children, children living in other countries, more rural areas or areas with different urban form.” (Conclusion p.19, line 354-356)

Furthermore, although the study has been conducted in one season only, we have acknowledged this (and other caveats) in the discussion and have explained what kind of consequences this could have had for the results of this study. For example, Discussion, line 291-292: “It is likely that this is an underestimation of the percentage of active transport trips in other seasons, since most of the measurements were conducted during the winter period. [37].”

Next, although previous studies have already investigated proximity to school this is one of the first studies that has used more objective and automated methods, i.e. GPS receivers, to quantify the relationship between distance and mode of transport. With these objective methods, better informed decisions can be made on school siting. Also, most of the previous studies on active travel have been done in the UK and the US, where cycling as a mode of transport isn’t as prevalent as in the Netherlands.

Specific Comments-Major and Minor Compulsory Revisions

1. Abstract
   a. ’physical activity’ better than ‘Energy Expenditure’ in background.

   We agree with the reviewer and have changed the sentence to: “Active transportation to school is associated with higher levels of physical activity.” (Abstract, p.2, line 29-30)

   b. Is first sentence of Results section of abstract really justified? – is ‘at least once per week’ an adequate criterion for defining dominant travel mode? (and this is repeated in the Conclusions section which is not necessary and possibly not justified).

   We agree with the reviewer that active transportation ‘at least once per week’ is not an adequate criterion for defining dominant travel mode, and have deleted the first sentence of the Results section of the abstract. However, out of all of the tracks included in the study (n=812), 79.2% of the tracks were classified as active transportation. (Results section, 3rd paragraph, p.13, line 238). We feel that this is an important result of the study but have now refrained from the use of the phrase ‘dominant mode of travel’. We have removed this phrase from the manuscript and now speak of “most frequently used mode of travel” (Abstract conclusion, line 52, Conclusion, line 373).
2. Introduction
a. Last line of first para.- active commuting ‘has declined’- where ? needs a brief expansion/clarification.
Thanks for the suggestion, the paragraph now ends with the following clarification: “… the proportion of students in the US that walk or bike to school has dropped from around 40% in 1969 to 12% in 2007 [16]. Similar numbers have been observed in Australia [17]. Although the proportion of children that actively commute is generally much higher in European countries, there is some evidence that suggests these trends can also be observed in Europe [18]”.

b. P2 is the literature on ‘determinants’ not better described as literature on ‘correlates’ or predictors. Determinants seems too strong a term and not consistent with the terminology proposed by Bauman et al in the Lancet series in 2012.
The literature that was used is cross-sectional and we agree it does not justify the use of the term ‘determinants’. Correlates would thus be a more appropriate term and we have changed the text accordingly. It now reads as follows: “…they are two distinct modes of transport that should be separated in the analysis, since they have been associated with different correlates in previous research” (Introduction, 2nd paragraph, p.6 line 106-107).

3. Methods
a. I was a little unclear as to how the statistical analyses were conducted and think this needs expansion/clarification.
We have further explained our choice for the multinomial logistic mixed-effects model in the statistical analysis paragraph (Methods section, p. 12, line 205-211): “This model was used to account for the clustered data structure where clusters of children lived in the same city and where individual children recorded a different number of tracks. Distance of the recorded track was used as the independent variable, mode of transport was the dependent variable. The analysis was done on the level of the GPS tracks. This way, the hypothesis was tested whether GPS tracks of relatively larger recorded distances had a higher chance to be classified as passive transport. Within this model, this meant that the tracks…”

b. There is no consideration of sample size- some justification of sample size and related issues (e.g. clustering), or justification of the decision not to mention them, would be advisable.
The present study was done in a subgroup of the larger SPACE study in which the main data collection methods consisted of questionnaires and activity diaries. Thus, the sample size of the subgroup was relatively small mainly because of practical issues, as there was only a limited availability of resources (e.g., financial, equipment, time). The way we dealt with the clustered data structure has now been further clarified in the Methods / Statistical analysis section (see also answer 3a, Methods section, p. 12, line 219-225). The small sample size is a limitation, as is now addressed on p. 19-20.

c. state in text the cutpoints for weight status based on BMI (e.g. Cole-IOTF if that is what was used), and clarify that ‘overweight’ includes the obese.
We have changed the text accordingly and have now stated in text that we have used the Cole et al, 2000 cut points. We also made sure it is now clear that both the overweight and the
obese children were classified as ‘overweight’. The sentence now reads as follows: “These measures were used to calculate BMI (kg/m²), and to categorize the children into ‘normal weight’ or ‘overweight’ according to age- and sex-specific cut-off points for children by Cole et al. [31]. All children that were above normal weight, including the obese, were classified as “overweight” (Methods, Instrumentation, 2nd paragraph, p.9, line 153-156)

d. P9 has a stray word ‘Results’ in the middle of the paragr.- I think this is a typo. The stray word has now been removed. Also, we changed the heading ‘Results’ on P11 to **bold** instead of _italic_.

e. how representative is the sample of the parent sample or of the Netherlands? (e.g. in terms of proximity to school). The proximity to school seems very striking and this might well have influenced the results and is also relevant to the generalizability of the results.

In the Netherlands children are supposed, or in some cities even obligated, to attend a primary school within the neighborhood they are living in. Therefore, in most schools 90% to 100% of the children live within the same neighborhood as where their school is located. Average distance to an elementary school in The Netherlands is 0.71 km (CBS [http://kpvvdashboard-7.blogspot.nl/2011/12/5-km-gebruikt-64-van-de-middelbare.html](http://kpvvdashboard-7.blogspot.nl/2011/12/5-km-gebruikt-64-van-de-middelbare.html)).

Even in the least densely populated municipalities, the average distance to an elementary school is 0.83 km. In this study, only children that lived in the same neighborhood as their school were selected, this is also mentioned in the Method section, 3rd paragraph, line 145). This was because this study was part of an environmental intervention (restructuring) focusing on the neighborhood. This explains the even shorter average distance to school found within our study sample (average straight line distance to school in our sample: 0.36 km) compared to Dutch averages. For this reason, we have acknowledged in the manuscript that the results of the study are not representative for other settings (see also answers to general comments, answer 1c)

Moreover, the subgroup of children that wore GPS in the present study seem to be a good representation of the overall SPACE study. All of the 5 cities investigated in the SPACE study are represented in the subgroup. Also, the age, sex and BMI of the subgroup are not significantly different from the original study sample (see Table 1 below, tested with t-test and fisher’s exact test).

<table>
<thead>
<tr>
<th>Table 1. Difference with parent sample</th>
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<tbody>
<tr>
<td>Total (n=977)</td>
</tr>
<tr>
<td>Age (Mean ± SD)</td>
</tr>
<tr>
<td>Sex (% boys)</td>
</tr>
<tr>
<td>BMI (Mean ± SD)</td>
</tr>
</tbody>
</table>

4. Results

a. I am not convinced that figures 1, 3, and 4 are essential and would like to see a justification of their inclusion.

We believe these figures all support the reader in understanding the data. 

*Figure 1* shows an example of a child traveling from home towards the school building. It has been added to the manuscript to help the reader visualize the data that was collected for the
study. It supports in explaining the automatic detection of trips and in explaining the cluster detection mechanism (that has now been added/included in the manuscript).

Figure 3 shows the distribution of distance among all GPS tracks that were classified as active transportation. This figure shows how there are relatively few active transport tracks above 1000 meter and that within this setting, most of the active transport took place on distances between 300-600 meter. We agree that there is a considerable overlap between Figure 3 and Figure 4, so we decided to remove Figure 3 from the manuscript. If the editor wishes so we could offer the Figure as a supplementary file.

Figure 4 (Figure 3 in new manuscript) shows the mode of transport distribution in relation to the distance of the tracks (with categories of 100 meters). We believe this figure is essential for describing the reader how (even on such relatively short distances) there was a trade-off between different modes of transport and this would be much more difficult to describe in text.

5. Discussion

a. Is quite lengthy but very well written, and does acknowledge the main study limitations- my major concern is that the acknowledgement of these limitations does not seem to translate into sufficient caution around the main findings as noted above.

It was never our intention to overstate the results of the study. Thus, we have taken more caution in drawing conclusions from the study. We have thus rewritten the end of the Conclusion section which now reads as follows (with changes underlined): “Furthermore, it should be realized that, although distance between home and school seems to be an important correlate associated with children’s transportation mode, there are other correlates that are also associated with transportation mode, such as individual, family, school, social and physical environmental factors.” Also, to be more succinct in the Discussion, while also taking into account the comments of the reviewers, we have deleted some sections in the Discussion (see comment Reviewer 1, answer 1a).

b. Conclusions section ‘active commuting as the dominant mode of travel’ needs more of a justification for this reviewer, and I would also like to be convinced that this was not just a function of the sample and setting. The authors may wish to add the appropriate caveat to this sort of statement.

Of course, results within this cross-sectional study will be a function of sample and setting. To highlight this limitation we started our conclusion with the words: “within the current research setting” and have suggested that further research could see whether our results are generalizable to other settings (Conclusion, line 354-356). Moreover, we have been more cautious in drawing any firm conclusions from the study (see also answer 5a). We think that the use of the phrase ‘dominant mode of travel’ is justified because 79.2% of the school-trips are classified as active transportation and 92% of the children recorded at least one trip with active transportation. (See also answer 1b)
A paper on a topic of interest in the 'active transport' field. It is well written. The methods are what will be of interest here, not so much the findings, which are similar to those reported in previous studies of mode choice vs. distance from home for children and adults across a range of cities.

Major Revisions
1a The methods are only slightly innovative. GPS has been used to explore children's travel and in particular their journey to school for some time now. It is indeed useful for capturing actual routes used, and getting observed data on trip frequency, length, travel time, etc.. The innovation appears to be not that the tracks were coded automatically (this is now common - see Peter Stopher's work) but how they were coded. These methods seem to open up potential ways to use GPS tracks in future research using the now ubiquitous smart phones and other enabled devices, in studies where additional information from students cannot be obtained to isolate and code journeys-to-school as active or not. Such methods could be quite helpful in that context and this should be stated in the paper.

In the discussion we pointed out that our method offers a potential way to handle data analysis in studies that only use information from ‘smartphones’: “These methods could be helpful in future research, for example when information from GPS-enabled mobile phones is unaccompanied by additional information (e.g. travel diaries) from students to code journeys as active or not.” (Discussion, Strengths and Limitations, p.17 Line 323-325) Moreover, we now refer to the relatively innovative cluster detection method we have used to identify home and school locations (Methods, Data handling, p. 164-165).

1b However, the methods in this paper have a major limitation in that the GPS was not accompanied by a travel diary as occurred in the CAPABLE project (uncited - see Mackett, Banister et al. 2007 http://www.casa.ucl.ac.uk/capableproject/download/CAPABLE_finalReport.pdf), the PEACH study in the UK, the CATCH project in Australia and others. Alternatively, Danish researchers have already combined accelerometer data with GPS tracks to categorise travel as active or not. This matters.

Within our study we did have activity diaries available for most of the children but it proved to be extremely difficult to match these diaries with the actual GPS track (mostly because of the lack in temporal precision in the activity diaries). From the report of the CAPABLE study it seems like they encountered similar problems where it took them considerable effort to reconstruct and match times from diaries with GPS tracks. We have tried to match the diaries with the GPS tracks through recoding the diaries and GPS-tracks into three different segments of the day (morning - before 10:00, lunch 10:00-13:00 and afternoon- after 13:00) This way, we were able to match 38.9% (n=316) of the tracks to the diaries. Because there was such temporal imprecision and thus inaccuracy in the matching, and we could also match only a minority of the tracks, we decided not to include this data in the manuscript. In our discussion, we have now adjusted our comment on accelerometers and added the reference to the Danish group as we agree that this may be an important step to improve the automatic coding process (Strengths and limitations, final paragraph, line 367-373).
That the 'automatic' methods provided in the paper may be around 30% inaccurate in allocating a GPS track to a particular mode of travel (walk, bicycle, bus, etc.) is a major problem that can readily be overcome by supplementing the GPS units with basic travel diaries. I suggest a) making this limitation clearer, b) highlighting the role travel diaries can make, c) citing the relevant studies that have used this approach, and maybe d) highlighting that future research may also better refine these metrics to increase the accuracy of automatic coding, to void the need for travel diaries in future studies.

We did use activity diaries, but we were able to match only 38.9% of the tracks and have decided not to use this in the manuscript (see also answer 1b). We did make an estimation of the accuracy of our methods: When we consider activity diaries to be the gold standard, the following numbers could be calculated for sensitivity and specificity of our classifying methods.

<table>
<thead>
<tr>
<th></th>
<th>Sensitivity</th>
<th>Specificity</th>
</tr>
</thead>
<tbody>
<tr>
<td>GPS - Walking</td>
<td>68.9%</td>
<td>94.5%</td>
</tr>
<tr>
<td>GPS - Cycling</td>
<td>70.5%</td>
<td>78.7%</td>
</tr>
<tr>
<td>GPS - Motorized</td>
<td>82.5%</td>
<td>89.3%</td>
</tr>
</tbody>
</table>

Since these numbers are the result of only a limited number of observations, we have decided not to include this in the manuscript. If the reviewer or editor wishes so, we can add the information about the sensitivity and specificity to the manuscript. Of course, we are more than aware of the difficulties that accompany travel diaries, such as recall bias and the notion (also made in the CAPABLE project) that there is a general underreporting of trips in activity diaries. The numbers above seem to correspond with the numbers reported by Bohte and Maat where they were able to correctly classify around 70% of the journeys (this is now mentioned in the manuscript on p.17 Discussion, strengths and limitations, line 333-334). We agree with the reviewer that it would be an ideal scenario if in future studies on transport behavior of children, the children wouldn’t have to deal with the time-consuming process of keeping a travel diary. Because of previous mentioned issues we don’t think that activity diaries are the best way to validate automatically generated GPS-trips. Instead, we think that an automated validation system, such as proposed by Bohte and Maat holds more promise for the future. This was already written in the manuscript (page 19, Line 359: “… might be further improved by using such automated validation system”). In our discussion, we have now cited the Danish group (see answer 1b). Furthermore, we have adjusted our discussion to highlight the need for more accurate automatic coding methods (Strengths and limitations, final paragraph, line 336-340).
2. There is some interest in the Netherlands angle given that Dutch mode shares and cities are quite different to either US or many Asian cities in key ways (lots of small schools, short travel distances to schools, high cycling rates), but this is not given sufficient focus in the paper at present. The Netherlands is highlighted by Pucher and others as a place that offers vulnerable road users ‘safety in numbers’. Given the slim contribution of the work at present, the question of mode share decline by distance in cycling cities, such as most Netherlands’ cities, should be highlighted.

We have now put more focus on the European angle and also referred to the work of Pucher in the introduction (Introduction, page 5, line 88-90). “.. it is expected that changes in mode of transport will be different in European countries where cycling is much more common (e.g. Denmark, Germany, the Netherlands (Pucher et al., 2008). “ Furthermore, when discussing the higher prevalence of cyclists in the Netherlands in the Discussion, we have put more focus on the design of Dutch cities which possibly influences active transport behavior. (Discussion, line 297-300) “…..Results from the present study thus might not be applicable to populations of schoolchildren outside the Netherlands. Key to the high levels of cycling in the Netherlands seem to be the separate bicycle lanes, combined with traffic calming measures in residential neighborhoods (Pucher, 2008). Moreover, schools in the Netherlands are relatively close to their residents. Even in the least densely populated municipalities, the average distance to an elementary school is smaller than 1 kilometer.”

Discretionary revisions
3. Rep14, final para, when discussing the role of safety in determining mode share. I suggest you include here mention of safety in numbers in terms of children both living in close proximity to school, and having such high proportions of active travel.

We have now mentioned the role of safety in numbers in the end of the discussion. Discussion, Line 309-313: “In the Netherlands, with a high prevalence of vulnerable road users such as cyclists, road safety is also influenced by the phenomenon of ‘safety in numbers’ (Jacobsen, 2003). In the present study, this means that because of the high percentage of active transport among school children, motorists would have adjusted their behavior to increase road safety”.

4. It would also be preferable to mention issues of perceived traffic danger and parental attitudes to ‘stranger danger’ (threats against a child’s person by outsiders) in Dutch cities at some point in the paper, which appear to be very different to that in the US, and may also be explanatory factors in these results.

We have now mentioned both aspects of safety in the discussion. Discussion 305-308: “Parents are thought to find it convenient to drive to school and drop their children on their way to work, but cannot drive their children back home. Also, parents may worry about traffic danger and ‘stranger danger’ on the road to school. The darkness in the morning influences both road safety and social safety, making cycling or walking to school more dangerous [39]”
Reviewer 3

Reviewer: Dorien Simons

1. Major Compulsory Revisions

1.1. Introduction, first paragraph: the authors state that “it is essential to prevent a sedentary lifestyle at an early age: studies have shown that children that are more physically active early on in life are also more physically active as adults.”

This is an incorrect conclusion as a sedentary lifestyle and physical inactivity are not the same. One can have a sedentary lifestyle but still reach physical activity recommendations by sitting 10 hours each day and doing 3 times a week one hour of sports. It is very important to make correct use of this terminology and to make a clear distinction between these two concepts.

We thank the reviewer for the suggestion. Although being physically active and a sedentary lifestyle are closely related concepts, we agree that there is a difference between the two. The paper focuses on promoting physical activity and we have removed the sentence about sedentary lifestyle in the Introduction section of the revised manuscript.

1.2. Throughout the paper, the authors mention that ‘the aim of the study was to analyze the relationship between the distance between home and school and the children’s willingness to actively travel to school’. But the willingness of the children to use active transport was not measured (no questions were asked about feelings, opinions, choices, preferences, etc.). Furthermore, young children often do not choose their travel mode, because parents decide it for them. Could the authors please change or remove this term from the paper?

We agree that the term willingness appears to refer to a psychological construct, this is not what we intended. We have not measured ‘willingness’ with the use of questionnaires and agree that using the term willingness could cause unnecessary confusion for the reader of the manuscript. We have removed the term from the manuscript and decided to rephrase the sentences containing the word ‘willingness’. We now use the phrase: “the proportion of children that use active transportation”. For example, see abstract line 46.

2. Minor Essential Revisions

2.1. Throughout the paper, both ‘active transportation’ and ‘active transport’ are used. Can the authors please be consistent in the use of terminology and always use the same term?

We decided to use ‘transport’ and have removed the word ‘transportation’ from the manuscript.

2.2. Introduction, second paragraph: can the authors please clarify in more detail why the use of GPS is better than the use of GIS? Or perhaps use a reference to substantiate this statement.

We feel that the use of GPS offers some clear advantages over the calculation of the shortest route within GIS. Calculation of GIS shortest route will depend on the quality of the available information on road networks. Moreover, the actual route traveled is not always similar to the shortest route (Duncan, 2007). We have now revised the paragraph and substantiated our statements by citing the Duncan article (Introduction, 2nd paragraph, line 99-100).
2.3. Methods, instrumentation/measures: it is not clear why BMI was calculated and why children were categorized into ‘normal weight’ and ‘overweight’. This measure was reported in table 1, but not used in any analyses, so it seems unnecessary to report this measure.

We report the proportion of overweight children as it provides insight in the population of the study. Since the work is cross-sectional, we feel this is vital information for the reader, as this will facilitate the comparison of the current study with other studies. Furthermore, only reporting the BMI of the population is not sufficient as cut-offs for being overweight (or being obese) are different between age groups. The difference in age within our study sample would make it hard to interpret the ‘crude’ BMI data. (For example: a 6 year old boy with a BMI of 18 would be considered to be overweight, whereas this is considered a healthy BMI for an 11 year old).

2.4. Methods, data handling: can the authors remove the word ‘Results’ in the middle of this paragraph?
The stray word has now been removed. Also, we changed the heading ‘Results’ on P13 to **bold** instead of *italic*.

2.5. Methods, statistical analysis: Could the authors please expand and make it clearer how the data were analyzed exactly? Can the authors also clarify why they used a multinomial logistic mixed-effects model?
See answer to Reviewer 1, section Methods 3.a.

2.6. Table 1: this table is not very clear. Can the authors please rearrange the elements within the table? The words ‘Boys’ and ‘Girls’ do not seem to be at their correct spot, as it does not matches the columns underneath. Furthermore, the horizontal lines should be removed, except the ones at the top and bottom of the table, and the one underneath the title words. Also all vertical lines should be removed.
We have verified that the words ‘Boys’ and ‘Girls’ are in the correct spot. We have removed the blank rows and columns from the table. We followed the instructions for authors of BMC Public Health that state that ‘Columns and rows of data should be made visibly distinct by ensuring that the borders of each cell display as black lines’. Furthermore the instructions suggest not to generate columns by using tabs or separate text. Tables will be typeset and displayed in the final published form of the article. From these instructions, removing the vertical lines, horizontal lines, and changing the lay-out of the table does not seem necessary in this version of the manuscript.

2.7. Table 2: the horizontal lines should be removed, except the ones at the top and bottom of the table, and the one underneath the title words. Also all vertical lines should be removed.
We did not make any changes to this table, see also answer 2.6: “The instructions for authors of BMC Public Health state … From these instructions, removing the vertical lines, horizontal lines, and changing the lay-out of the table does not seem necessary in this version of the manuscript.”