Author's response to reviews

Title: Young adolescents’ independent mobility - Related factors and association with transport to school. A cross-sectional study.

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

Thank you very much for giving us the opportunity to revise our manuscript entitled “Young adolescents’ independent mobility - Related factors and association with transport to school. A cross-sectional study.” We feel that the comments of the reviewers strongly improved the article.

Although an outline of modifications is attached, we would like to draw your attention to the following major changes.

1. As all reviewers more or less mentioned that the link between the background of the study and the research questions needed to be more self-evident, the introduction has been fully revised and the research questions clarified. It is also much shorter now.

2. The methods section has been revised so as to make clearer the link between the variables studied in the background. We have also shortened slightly the description of the statistical methods used. We had different signals from the reviewers in that respect but we hope the amendments introduced facilitate without complexifying the comprehension.

3. The discussion has been completely restructured, based on comments from two reviewers that the discussion was too much of a repetition of results.

4. The abstract was revised based on comments from reviewers.

5. We made some revisions to figure 1, to better reflect the order in which the variables are now presented in the methods section.

Changes have been marked in the manuscript with yellow colour.

We hope that in its revised form you will find the article suitable for publication and we look forward to hearing from you.

Yours sincerely,

Klara Johansson
Marie Hasselberg
Lucie Laflamme
Outline of modifications

The email from Natalie Pafiits:

Please also do the following:
(1) Include all co-authors' email addresses within the title page of your manuscript.
This has now been done.

(2) Document, within your manuscript, the specific name of the ethics committee which approved your study.
The name in English and Swedish is written on page 5, rows 1-2.

(3) Although you mention parental consent, we also require that you document whether you received consent from your study participants and whether this was verbal or written.
This has been clarified on page 4, rows 23-24 and page 5, row 1.

(4) Include a copy of the questionnaire administered in your study as an additional file to your manuscript.
An English translation of the questionnaire is attached.
Reviewer #1

Minor essential revision – definition of ”categories”, ”class” and ”joints”

Authors:
We have revised the manuscript to make these concepts more understandable. Class is synonym of cluster, and we have revised the manuscript to only refer to “clusters” instead of “classes”, hopefully making it easier to understand. The concept of “joints” is a technicality of the methodology which we have now withdrawn from the manuscript (reviewer #3 thought this part was too long).

Category: a category describes one aspect (nominal value) captured by a given (e.g., male for sex of child)

Categories: different categories describe either several aspects of one single variable (e.g., Monday and Tuesday for day of week) or several aspects of more than one variable.

Reviewer #2

Major issues

1. The introduction is currently not very coherently written and lacks a proper justification for the research questions. Although it gives an overview of the research performed to date, I feel it doesn’t clearly identify the knowledge gaps and how this study will and will not contribute to that (i.e. the authors suggest that there may be differences between urban and rural areas, but this is not addressed in their study).

The introduction has been fully rewritten to better address the comments made by two of the referees concerning the link between the introduction and the research questions, which we have made every effort to clarify. The literature included has also been revised so that the references are more directly concerned with young adolescents than with children in general.

We are grateful that you alerted us to this and feel that the introduction and the manuscript as a whole have benefited from the changes and are now more coherent.

2. On p6 the authors mention that they present ‘selected’ data. It is unclear what this means and on what basis the data was selected. In addition, very little information is provided on the development and validity/reliability of the measures and whether they have been used previously.

We have clarified the choices of variables in line with the introduction (pages 3-4) and why these variables were relevant this specific study (pages 3-4; see also discussion, pages 13-14). In general terms, the variables selected have been identified in previous studies as related to child or adolescent mobility (often in
qualitative of univariate studies). We also made some revisions to Figure 1, to clarify the variables used and better reflect the order in which the variables are now presented in the methods section.

We also comment in the discussion on the development of the variables used and their coding, see page 15, rows 19-25 and page 16, rows 1-6.

Below are some details which we felt there was not space to include in the manuscript:

- Questions on fears and coping strategies were created for this questionnaire. Categories were based on literature review and suggestions from pilot testing.
- Questions on traffic environment were inspired by Mullan (2003) but formulated specifically for this survey.
- Child/parent opinions on mobility were adapted from a questionnaire by Sandqvist & Kiström (2000) and then treated in a new way by using the consensus between parent and child opinion.
- The question on commuting to school was based on literature review of similar studies, but the exact wording of the question was formulated for this survey, and in agreement with the Swedish version of Health Behavior in School-Aged Children, which agreed to include a similar question in the HBSC survey for that year.
- Questions on walking or bicycling in traffic were formulated for this survey, since one of the aims of the survey was to explore exposure to traffic risk.
- Questions on gender, birth country, type of housing, living in one/two places and car ownership were formulated similar to other studies, for instance the Health Behaviour in School-Aged children or Statistics Sweden’s survey on living conditions among children 10-18.

3. The description of the clustering technique applied is extensive but it leaves me with the question whether this the most appropriate way to address the research questions posed. It may be helpful to introduce the statistical technique briefly in the introduction and state how this will contribute to our existing knowledge. The authors do refer to ‘significant overrepresentation’ (p8) and ‘more than expected by chance’ (p11) but no statistical data is presented, was any additional statistical testing performed?

Thank you for making this point. The choice of method was based on the research question and on expectations that the mobility-related factors studied co-vary. We clarify the link between the research question and the methodology, both in the introduction section and in the methods section.

As described in the methods (page 7, rows 1-25) and results (page 8, rows 11-15) the categories which we present in the text (which are also in bold and italic print in table 1) are those which significantly contributed to formation of each class (p<0.5; based on the chi2 metric). This is reflected in the table of results when one
compares the percentages by class to the percentages all respondents aggregated (last column in table 1). For this reason, statistical testing is redundant.

4. Did the authors consider studying the correlation between the variables before including them in the analyses? Some of the conclusions are based around variables that I presume are highly correlated (such as parents not from Sweden, living in an apartment and having no car) and I am unsure if this is a correct representation of the data.

We have reduced the material before the analysis started, by excluding variables with a close to complete overlap. On the other hand, that some variables are correlated is precisely what we aim to explore, i.e. which aspects are connected or interact and which affect the adolescents (see manuscript page 6, rows 21-24).

5. The results section is currently very long and appears to be a repeat of the tables. Moreover, the discussion does seem to simply repeat many of the results as well and I miss a reflection on how these analyses fit in with and add to the current literature and how to move forward from here.

Few minor changes have been introduced in the results section besides a brief summary at the end. We feel it is important to highlight the categories of variables that have contributed to the formation of each class (in decreasing order).

The discussion however has been completely re-written to lift the findings, and shortened to avoid a repetition of the results section.

Minor issues

- The results and discussion section of the abstract currently both seem to present results. Please re-write.

Authors: done

- p5: it is unclear what the authors mean by ‘mobility in the traffic environment’.

We have removed the formulation “mobility in the traffic environment”, and clarified this variable on page 8, rows 4-5.

- p5: did all 1009 students provide full data (the tables describe 1008 participants) or was data imputed?

We have now clarified this in the methods section (page 6, rows 1-4)
The category “I don’t know/no response” was included as a specific category in the cluster analysis (as shown in Table 1), for those variables where such responses (or non-responses) existed. One respondent was removed from the data set after careful consideration, because both closed and open responses were inconsistent, suggesting that the person had only clicked boxes without reading the questions. This is additionally described in Johansson et al. (2009).

- **discussion**: please include some comments about the representativeness of the sample.

The discussion section now includes these comments (page 15, rows 12-16):

- **could it be that cluster 5 consists of adolescents who were not very interested in completing the questionnaires (i.e. missing answers, incoherent responses). Seeing that this is an adolescent population, I would expect some degree of disinterest even in those who do participate.**

Thank you for making this observation. We checked this against a question regarding respondents’ opinions on responding to the survey, and indeed - in this cluster more than half of the respondents (above average) were less than positive to the survey. See page 14, rows 14-20, in the manuscript.

**Reviewer #3**

**Major issues:**

1. ** Operational definitions of the three levels and "actual mobility" are weak.** Especially, family defined by foreigner status, home ownership and basic home environment (house/appartement) is unconvincing. The manuscript did not convince me to believe that "actual mobility" is more or less well defined by the mode of transportation to school and the Saturday walking or cycling activity.

The family variables were included in the analysis due to the fact that they have previously been shown as associated with reports of fear (both on this data material (Johansson et al., 2009) and in another study in Sweden (Jonsson & Brolin Låftman 2001) and with other factors relevant for mobility such as adolescents’ licenses for mobility. The variable of Swedish-born parents does not refer so much to foreigner status as to if one or both of parents were born and grew up in Sweden, which we assume may give them a sense of familiarity and belonging in the cultural context which they may also transmit to their children and which can lead to feeling more confident and independent in public space.
Type of housing is not so much about ownership as it is a proxy of socioeconomic status, and it also indicates something about the type of environment the respondents live in.

We have rewritten the introduction in response to comments from all reviewers, and chose to remove the division of variables into “individual, family and environment”, and rather highlight why each of the variables included are relevant for the study of adolescents’ independent mobility.

Concerning the point about “actual mobility”, we have changed the manuscript to talk about “active commuting” instead, which is a more narrow concept and is closer to the operationalization.

2. Overall, the overall, concrete intention of the manuscript has to be clarified. I would agree that point 1) of the authors research question (factors influencing freedom to move) can be somewhat addressed with their study. If it is the mayor topic, the public health relevance of the potential findings has to be clearer than in the present form.

The introduction has been fully rewritten to better address the comments made by two of the referees concerning the link between the introduction and the research questions, which we have made every effort to clarify. The literature included has also been revised so that the references are more directly concerned with young adolescent than with children in general.

The public health relevance of the findings are to increase understanding about adolescents’ unequal possibilities for physical activity, access to public space and freedom to move around in their neighbourhood. Though not the main focus, fears in public space, as well as safety and security – whether perceived or “objectively” measured – is a public health issue, not least because it can significantly impede independent mobility.

3. The text states that written consent was solicited from parents. Unless, there was also written consent from the children/adolescents I would have ethical problems.

We have now clarified this in the methods section (page 4, rows 23-24 and page 5, row 1).

4. The methods should be shortened and reduced to the major points necessary to get a basic understanding of their approach.

We have shortened the part of the methods section concerned with the cluster analysis (page 7 in the manuscript). However, based a comment from another
reviewer, we have expanded slightly the section on selection of variables and data treatment.

5. Figures/Tables should be self-explanatory, for instance in Table 1 the authors should state
   a) Classes should be named

   We have labelled the clusters at the end of the results section and use those names in the discussion. However, we have not included the names in Table 1 to avoid too general interpretations of the clusters at an early stage in the manuscript.

   b) state what research question you are addressing in the figure 1

   This has been dealt with (see figure 1)

6. The discussion is redundant in many passages with repetitions of results.

   The discussion has been completely re-written to lift the findings, and shortened to avoid a repetition of the results section.

7. The criteria how the five classes were actually chosen was elusive to me.

   The determination of the number of classes to be retained is arbitrary but rests on criteria presented in the methods section. Using those criteria and given our research questions, we felt that five classes were the best resolution.

Minor issues:
1. The introduction of term "freedom to move" is awkward.

   In re-writing the introduction, we chose to not use the term “freedom to move”, and instead talk about potential limitations to mobility.

2. page 8: "Class 1:..." The meaning of the percentages and the variance is unclear.

   We have clarified in the results section what is meant by this (the percentage is the share of all respondents which are found in this cluster, and the variance is the intra-cluster variance, i.e. the combined differences between the respondents in that cluster as concerns the categories in the analysis).

3. Please describe Stockholm County - does it include Stockholm city?

   Yes, Stockholm County is the larger administrative unit, encompassing all of the Stockholm municipalities – the city as well as all the suburbs. We have clarified this on page 4, rows 16-17.