Author's response to reviews

Title: Body mass index and overweight in relation to residence distance and population density: experience from the Northern Finland Birth Cohort 1966

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Version: 3 Date: 28 August 2013

Author's response to reviews: see over
Dear Ms Rita Aguirre,

We thank the editors and reviewers for their positive comments and have revised the manuscript according to their advice. Please see the detailed responses below.

All revisions have been marked in blue.

Additionally, we have made some minor revisions in
- Figure 1 (shadings improved)
- Figure 3 (minor errors in gridlines corrected)
- Supplementary Figure S1 (one minor misprint corrected)
- Supplementary Figure S2 (minor errors in gridlines corrected)

We hope the manuscript would now be acceptable for publication, but we are happy make any further revisions if you regard it as necessary.

Sincerely,

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RESPONSE TO REVIEWERS

Reviewer's report
Title: Body mass index and overweight in relation to residence distance and population density: experience from the Northern Finland Birth Cohort 1966
Version: 2 Date: 7 June 2013
Reviewer: Markku Löytönen

Reviewer's report:
Authors should consider a stronger statement of the next step - to look into the causal factors behind the results as this might significantly improve understanding the spatial variation in obesity.

RI We have added a statement to Conclusions that individual-based measurements of daily activity are needed to strengthen the causal argument that variations in everyday physical activity explain why people are fatter in remote areas. In fact, such measurements have been done in the 45-year follow-up of NFBC.

Level of interest: An article of outstanding merit and interest in its field
Quality of written English: Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests:
I declare that I have no competing interests.

Reviewer's report
Title: Body mass index and overweight in relation to residence distance and population density: experience from the Northern Finland Birth Cohort 1966
Version: 2 Date: 24 June 2013
Reviewer: Marjaana Lahti-Koski

Reviewer's report:
This is a well-written paper with a unique dataset. It deals with an important issue on variation of BMI and overweight in well-defined geographical areas providing useful data for public health scientists and health promoters. There are, however, some concerns that I would like to point out.

Major compulsory revisions needed:

Abstract

1. In the abstract, results chosen to be presented in numbers are not that reasonable. Given that analyses showed curved patterns, I think it is not justified to give the difference in the mean BMI using maximum ranges in road distance and population density. The same applies for results on increases in overweight. It remains unclear, to which e.g. 35% increase for distance refers. The results should be revised. I think it would be more informative to pick up main results on crude associations from the first paragraph on page 9, supplemented with results on analyses allowing for confounders and curvilinear associations as presented
R2  The abstract text has been revised according to the referee’s advice.

Potential confounding factors
2. As concluded, the likely explanations include variations in everyday physical activity in different residential environments. Commuting activity is one of the components for physical activity. It seems to me that this component was totally missed in this paper as only leisure-time physical activity and occupational physical activity were controlled for in the analyses. Or were the respondents asked to include also commuting activity in leisure-time physical activity in the questionnaire? In all, I think commuting activity is a very important issue concerning urban planning, physical activity and overweight, and should be discussed in this paper.

R3  Unfortunately, commuting was not asked in the questionnaire. We have to assume that the road distance used here as a surrogate measure for daily physical activity also reflects the effects of commuting. This remains a weakness of our study and is mentioned in Discussion (3rd paragraph).

Minor points
3. It was said that population density increased with growing alcohol consumption. Nevertheless, it is not described in the methods how the information on alcohol consumption was collected. Similarly, one of the results was that heavy alcohol consumption was associated with higher BMI. However, a definition for heavy alcohol consumption is lacking.

R4  We have added a description on alcohol questions and how alcohol consumption was measured (subsection Potential confounding factors, 3rd paragraph). Alcohol consumption was classified as quartiles; we did not define “heavy” consumption separately. Thus the statement “Heavy alcohol consumption…” (subsection Crude associations of BMI…; 2nd paragraph) has been changed to “Subjects in the highest alcohol consumption quartile had higher BMI…”.

4. Given that data for the study was collected in 1997 I wonder why data on local government areas on page 3 and Figure 1 were based on year 1998.

R5  The year “1998” was just a misprint, we are sorry for that. All data refer to the year 1997, including the number of towns (105). The numbers have been corrected.

5. In Tables 1 and 2, it remains unclear whether Oulu as a region of residence stands for the city of Oulu or Oulu province. Please clarify.

R6  In Tables 1 and 2, “Oulu” has been replaced by “City of Oulu”

Discretionary Revisions
6. Some demographic factors were controlled for in the analyses. However, I
wonder whether issue on unemployment were properly taken into account. Could higher body weight among people living in remote and sparsely populated areas be partly explained by higher unemployment rate, i.e. in remote areas more people stays at home without work? In potential confounding factors, socioeconomic groups included entrepreneurs, higher administrative employees, lower administrative employees, workers and others (n=274), which I assume could include students, housewives and unemployed. In addition, there were 165 missing cases. For occupational class (office work etc.) number of missing cases were as high as 582 (> 10%). I highly recommend this issue to be discussed in the paper.

R7 The referee makes a good point. The socioeconomic group “other” includes students, pensioners, unemployed subjects and those whose socioeconomic groups remained unknown. This group is small (n=274), and can hardly cause any major influence on the results. As explained in the text (subsection Analysis allowing for confounders…, 1st paragraph, last sentence), socioeconomic group was tentatively entered to the regression model, but as it had no influence on the parameter estimates, it was not included in the final model. We can assume the same applies to those with missing data on occupational class. Some discussion on this has been added to Discussion (3rd paragraph ) and we also added footnote d to Table 1 to explain the contents of the socioeconomic group “other”.

7. I assume that there are limitations for the number of Figures presented in the paper. Yet, I wish that supplementary Figure S1 could be included in the paper since it is very informative giving data on sex differences in relation to the research questions.

R8 Apparently, the referee means Figure S2. In this sex-stratified analysis, the parameter estimates are less precise (wider confidence bands) than in the combined analysis, due to splitting of the data and the consequently lower statistical power. We therefore prefer to present the combined (more precise) analysis in Figure 3 and the sex-stratified (less precise) analysis in Figure S2.

Level of interest: An article of importance in its field
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.