Author’s response to reviews

Title: Neighbourhood socioeconomic status and cardiovascular risk factors: a multilevel analysis of nine cities in the Czech Republic and Germany

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Author’s response to reviews: see over
Reviewer: Frank J van Lenthe

Major Compulsory Revision

- **(overcrowding)** We agree that the theoretical background of the variable was not sufficiently explained. We choose the second option proposed by the reviewer: to keep the variable and explain it more convincingly. A paragraph has been inserted in the methods section where the rationale for both variables is briefly explained [page 6]. Overcrowding is an indicator which is part of important deprivation measures such as Townsend, Carstairs or Jarman composite indices of deprivation (Morris & Carstairs 1991). In general, it is operationalised as the number of persons per room. The choice of mean living space per inhabitant as an indicator for overcrowding was due to data constraints from statistical offices in both countries (number of rooms per flat not assessed). In our view, the reference to square meters instead of the number of rooms should be a good approximation. Concerning the etiological concept underlying overcrowding we refer to the new paragraph including new references.


- **(physical activity, different aspects)** The reviewer pointed out that our measure of physical activity in this study does not include all aspects of physical activity, because only sports (during leisure time) is covered. We accept this point. However, since only comparable variables from both datasets could be used, it was not possible to test a more specific operationalisation of physical activities in all its dimensions. Both studies contained more detailed questions, e.g. about garden work, but the variables were not identical and could not be used in comparative analysis. Apart from this practical limitation the comment of the reviewer is of a more general importance. We conducted additional analyses in the German dataset, which contained a detailed assessment of sports, recreational activities, daily walking, etc. (see also ‘answers to reviewer 2’ for description of the instrument). In addition to the original variable which covers sport only, we constructed two new variables: a) non-sportive physical activities - i.e. working in the garden, walking with the dog … - and b) walking less than one kilometre a day (independent if recreational, at work or else). Area-level unemployment was the independent variable and covariates were the same as in model 3 in table 3. There was no association between neighbourhood unemployment and non-sportive physical activities and a weak association with the daily walking distance. We intend to continue this preliminary work and try to understand the patterns of physical activity in these cities in a future work.
To inform the reader about this limitation of the current paper, we added a paragraph to the discussion and referenced the van Lenthe et al. 2005 paper in Social Science and Medicine [page 12].

- (variance) We agree with the reviewer that the variance components are informative. Therefore, we added a new paragraph in the results section [page 9]. The main conclusion from our analysis is that in Germany the geographical variance was lower than in the Czech Republic. Moreover, the former was largely explained by the socioeconomic neighbourhood indicators. In view of the complexity of respective analyses and given space limitations, however, we decided not to include additional results for the readership of the journal.

Minor Essential Revisions

Abstract

- (response rates, risk factors) The response rates are included and the risk factors are named

- (association) We replaced ‘influence’ by ‘association’

Background

- (self-rated health) We interpret self rated health as an indicator for morbidity (although it is a predictor of mortality). To clarify this point we changed the formulation in the first paragraph of the background section.

- (reference for ‘...although not in all cases’) The respective reference is No. 16 Pickett & Pearl M. Nonetheless we deleted this part of the sentence in order to avoid confusion. The problem, that not in all cases an area level effect persists after adjustment is not the focus of the paper and we think it is justifiable not to explicitly mention this point.

- (term area level variation) The term is replaced by ‘area-level inequalities’

- (extend the rationale for the study) This advice was very helpful and we extended background section and added new text to the discussion of the inter-country differences [page 4-5,11].

Methods

- (adjusting for social isolation) First of all we wanted to use the same set of control variables for all four response variables in order to keep the analyses simple. In the case mentioned by the reviewer this might be a problem. But as tables 3 and 4 show, an additional adjustment for social isolation did not substantially change the estimators. Although the argument that social isolation might be a mediator is plausible, it is also possible that social isolation is a confounder. This would be the case if clustering of socially isolated people in low SES-areas depends on factors other than the area-level social characteristics itself (e.g. smaller apartments in deprived areas lead to a higher number of single inhabitants with higher levels of isolation). It is not easy to decide which position in a pathway this variable has, but given the small impact of the adjustment, we would like to keep the adjustment in
order to counter the argument, that the association between area-level SES and risk factors is a hidden effect of individual level social circumstances.

- **(gender and obesity)** The reviewer is right. The association between area level indicators and obesity was more pronounced in women at least in Germany, whereas the results for the other risk factors are comparable for men and women. We were aware of that difference, because we calculated all analyses for the whole group and for men and women separately. Nonetheless we decided not to show gender specific results because a) the relative differences between men and women were small and b) a distinction of the country and indicator specific tables by gender would have further complicated the interpretation of the results. As a solution, we propose to mention in the texts of the results section that there is a stronger association in women for obesity and will also include the respective odds ratios in the text [page 9].

**Discussion**

- **(epidemiologic transition)** To our knowledge the morbidity and mortality patterns of both countries are comparable nowadays. Although some reports exist, that cardiovascular diseases have already reached the peak and are declining in Germany as well as in the Czech Republic, this finding is not yet validated. Due to a lack of scientific information it is not easy to include overall disease trends into the interpretation of the findings.

**Conclusion**

- **(first sentence)** Sentence is revised.

- **(prevalence of obesity)** High prevalence of obesity in the Czech sample is consistent with previous findings. Silventoinen et al. (2004) reported mean BMI between 27.9 and 28.3 in different rounds of MONICA study in 35-64 years old population. WHO Technical Report (Geneva 1995) says that central Europe is characterised by the highest prevalence of obesity in Europe. The figures in the report suggest that 30% of men and 40% of women had BMI>30 in rural areas of the Czech Republic in 1995. Cifkova et al. (2004) report that BMI>30 was found in 29.5% of men and 27.8% of women in middle aged sample of general population in 2000/1. The prevalence is also high in Germany (26%), a result which is in line with other findings from Germany, e.g. from the Federal Survey of Health. The high number of obese persons may be a result of the typical diet in the Ruhr Area, where people consume a high amount of meat and fried food, while fresh vegetables and fruits are ‘underrepresented’ on tables.

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Reviewer: Basile Chaix

General comments:

- **(Lack of adjustment for income)** We thank the reviewer for this important comment. To address this issue, we conducted subgroup analyses in the German dataset where such information exists and where it is possible to assess the effect of the proposed adjustment. We found that the adjustment for income further reduced the area-level – outcome association, but the reduction was not substantial and the odds ratios remained significant. For example: If income is introduced into the model 2 in table 3 for the outcome smoking, we get the following results:

<table>
<thead>
<tr>
<th>Smoking (3800)</th>
<th>OR [95%CI]</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. lowest unemployment</td>
<td>1.00</td>
</tr>
<tr>
<td>2. 1.25 [1.04-1.48]</td>
<td></td>
</tr>
<tr>
<td>3. 1.13 [0.94-1.37]</td>
<td></td>
</tr>
<tr>
<td>4. highest unemployment</td>
<td>1.55 [1.25-1.92]</td>
</tr>
</tbody>
</table>

Moreover a part of the reduction of the estimators can be explained by a lower number of cases (over 100) due to item nonresponse to the income question. We added a short comment about this analysis to the respective paragraph in the discussion section [page 12].

- **(Size of the units)** Again it is possible to conduct a sensitivity analysis to assess the possible bias introduced by the different area unit sizes in the two countries. In fact, area-level information in Germany is available for smaller units (‘statistical districts’) than those investigated in the present paper. The 106 larger geographical units in the three cities can be further divided into 482 smaller districts. Their size is comparable to the size of the Czech units (2000-4000 inhabitants). Nonetheless we decided to use the larger units, because there was a considerable number of statistical districts with only one or two study participants. Such a small sample size might be problematic when linear multilevel regression models are calculated (e.g. with the unemployment rate). Although we only show results for the categorized area-level indicators (quartiles) our aim was to demonstrate that these results could be replicable with the continuous information (which is the case). To gain an impression of the risk of a possible bias we repeated our analyses with the smaller district information (unemployment rate on the level of statistical districts) for Germany. The results were similar to those based on larger units presented in the manuscript. In our view this indicates that the different unit size does not limit the comparability between these two countries.

- **(Cholesterol)** Unfortunately the cholesterol data were not available for the Czech Republic at the time of the statistical analysis.

- **(Education a mediator?)** This is a helpful comment, as we were in fact not aware of the possible confounding effect of education. As suggested, ‘mediator’ is deleted from the abstract, result section and conclusions. Currently we are attempting to assess the residential history of participants in the HNR study. With such data, it will be possible to further investigate migration trends in relation to education.
Specific comments:

- **(Page 3 references)** Thanks to the reviewer for this advice. Two of the new articles are now additionally cited for coronary heart disease (Winkleby et al. 2007; Chaix et al. 2007).

- **(Background section – number of studies)** According to van Lenthe 2006 only a few comparative studies exist (Stafford et al. 2004 and van Lenthe et al. 2005). We added respective sentences at the end of the background section [page 4].

- **(Methods – ‘data section’ date of examination)** Dates are inserted.

- **(Page 5 – questionnaires)** The HNR Study used the Paffenbarger College Alumni Physical Activity Questionnaire to assess all aspect of physical activity and the smoking interview of from Federal German Health Survey. In HAPIEE questionnaires were shorter, but contained some identical (smoking status) or almost identical questions (physical activity), which could be used to construct comparable variables.

- **(Page 5 – interaction tested)** We selected this additional control variable due to theoretical considerations. Especially when area level unemployment was analysed it seemed important to adjust for the individual occupational status of the participants (economically active and working, economically active but unemployed, retired, other inactive…). But in our analyses we handled this item as a confounder, therefore we agree with the reviewer, that our wording is misleading and the respective sentence was deleted [page 6]. A possible interaction of economic status and area-level influences of health was not in the scope of our paper, but would be interesting to come back to this point in future analyses.

- **(Page 5 – social isolation)** We added a sentence, describing the operationalisation of social isolation in our study [page 6].

- **(Page 7 – multilevel methods: correlated random effects?)** This is indeed a mistake and we apologize for this. Thanks to the reviewers comment we have corrected the error (random effects were assumed to be not correlated). The reason for the mistake was, that we have used for some time a library in R (nlme) in which the default was correlated random effects. In the recent calculations for the manuscript a newer version of the multilevel library in R (lmer4) with uncorrelated random effects was used, but we omitted to correct the manuscript.

- **(Page 7 – multilevel methods: fixed effects for cities?)** Here we were imprecise in describing our methods. Fixed effects for the cities were calculated as well. To better explain our model building approach, we added a few sentences to the methods section [page 7].

- **(Table 1 – overcrowding)** please see comments to reviewer 1 about the concept of overcrowding and its theoretical background.

- **(Table 1 education as risk factor?)** The table heading was changed in order to clarify the table.

- **(Page 8 third quartile)** Sentence deleted.

- **(Page 9 psychosocial characteristics)** This was a mistake, the right term is ‘education’.

- **(Page 11 vague statement about prevention)** Two examples are introduced to briefly illustrate the combined approach of prevention in a community setting.

- **(Page 11 homogeneity)** We deleted the paragraph.
- *(Page 12, census data vs. aggregated data)* The reviewer is right, the paragraph is misleading and was revised.

- *(References: mistakes ref 17,43)* Corrected