Reviewer's report

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

Version: 1 Date: 26 June 2007

Reviewer: Frank J van Lenthe

Reviewer's report:

General

This is an interesting and important study. There is substantial evidence of the role of ‘place’ for health, but too my knowledge no such studies have been conducted in more Eastern European countries. This is particularly interesting in the light of the past structures, which may have shaped countries in terms of the distribution of health-enhancing facilities and residential segregation. An international comparison is therefore important, but can only be done with essentially similar data. This study used very similar data from both countries. I do have a number of suggestions that in my perspective could improve the paper. Some issues are crucial for the study, other are minor details.

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

Perhaps the first main crucial issue is the use of overcrowding as a measure of area-level socioeconomic environment. Although there is no general consensus what should be measured to reflect area-level SES, this variable is not commonly used. There should be either more theoretical or empirical support from other studies to justify this measure of area SES, or the factor should be left out of the analyses. I would guess that city centers may be highly dense, but are also relatively affluent. There is some literature suggesting that a highly dense neighborhood reflects stress. For understanding of the findings, this indicator should be better justified.

Another important issue is the recognition that analyses between area-level characteristics and health and health-related behaviours should be conducted as specific as possible (see for example Giles-Corti et al, 2005). This may be crucially for physical activity. The importance of the neighborhood environment may substantially differ for walking to shops as compared to cycling to work or for doing sports. In order to be sure that there is no association between area SES and physical activity, specific elements of physical activity need to be used as dependent variables.

Finally, the results of the multilevel analyses only include the associations, but not the variances at the city level and the neighbourhood level. I would urge the authors to include the variances in the Tables as important information can be
obtained from these variances (see papers by Merlo and Chaix in the J Epidemiol Comm Health).

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Abstract:

- Methods: the response rate should be included. Instead of writing ‘established risk factors’, these factors could be described concrete.
- The conclusion states that area level SES influences health related lifestyles. This cannot be concluded from a cross-sectional study, there is an association.

Background:

- First paragraph: poor self reported health is considered an indicator of morbidity, shouldn’t this be mortality?
- Second paragraph: “..although not in all cases…” should have a reference.
- Third paragraph: the term area-level variation is little confusing in connection to multilevel analyses. I would prefer area-level differences or inequalities.
- In general, I think the rationale for the study could be extended little, by mentioning that there are (almost) no studies in Eastern Europe, and by elaborating little bit more on why this is so important. It could be stated, for example, whether the authors believe area level inequalities to be larger or smaller in either country.

Methods:

- I doubt whether it is good to adjust associations for social isolation. It is known that social isolation is related to physical activity. If isolated persons are less likely to be physically active, and if isolated persons cluster within neighborhoods (as the result of poor social cohesion), social isolation is on the pathway, and adjustment for social isolation may result in an over-adjustment.
- It is little inconsistent that there was no gender interaction for obesity. Some studies suggest stronger associations between area SES and obesity in women as compared to men. Is there any explanation for the absence of such an interaction?

Discussion:

- To what extent are Germany and the Czech Republic in different stages of the epidemiological transition. Can this be included in the interpretation of the findings? For example with regard to the finding that only neighbourhood level effects were found for obesity in Germany only?

Conclusion:
- I believe that the first sentence of the conclusion is too strong formulated, the study shows little more detailed findings, at least for obesity.

Table 1: the percentage of obesity is extreme high. Is it really 30% of the population with a BMI above 30 in the Czech Republic?

Discretionary Revisions (which the author can choose to ignore)

**What next?:** Accept after minor essential revisions

**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'