Reviewer's report

Title: Prevalence of psychiatric disorders in relation with urbanization in Germany

Version: 1 Date: 31 January 2007

Reviewer: Christian Meyer

Reviewer's report:

General

It is my pleasure to review the paper of J. Dekker et al.. The authors conducted a secondary analysis on an existing cross-sectional nationwide general population survey from Germany. The aim of the study is to investigate the association of urbanization and major psychiatric disorders. Strengths of the study are a large and nationwide sample and the use of state of the art assessment instruments. Differences in prevalence and admission rates of psychiatric disorders between rural and urban areas have been frequently analysed. However, refined analytic approaches focusing on non-psychotic disorders are sparse and only little is known about Germany. My major concerns about this study refer to the statistical analyses and the description of the methods.

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

1. To me it is not clear how and if the authors recognised the complex sampling design of the GHS-CS (multistage cluster sampling) and the subsequent GHS-MHS (disproportional cluster sampling including GHS-CS screening positives and 50% of screening negatives). Ignoring such design factors can result in underestimation of standard errors and biased statistical inference. Although the authors cited some papers giving more details of the study design (some of them are German language papers), it would be necessary to briefly describe the sampling design and to describe how this is considered in the statistical analyses.

2. The authors reported a “conditional” response rate, which is very high. However, the total response rate including the primary recruitment in the GHS-CS is much lower. This should be reported and discussed as a limitation. Further it would be helpful to have some response rates stratified for the different categories of urbanization. As the primary recruitment in the GHS-CS included a personal assessment in a local examination centre, burden of participation and subsequent non-response can be higher due to larger travel distances in rural areas. This is important because some studies on survey-methodology found that (psychiatric) morbidity is associated with non-response.

3. It is mentioned that nicotine dependence was assessed. Since other substance use disorders were included, I wonder why nicotine dependence was not further considered in the study. Nicotine dependence is often ignored by psychiatric epidemiology but it is the most prevalent mental disorder in Germany and highly relevant for public health in general. Including this diagnosis would be a strength of the study.

4. A table showing the distribution of the “other “ risk factors across the different categories of urbanization would be good to initially evaluate possible confounding/independence.

5. In Table 1 the authors reported a very high number of statistical tests (I estimate 10 pairs of categories of urbanization * 19 disorder categories = 190 tests). It seems that now measures have been used to adjust for multiple testing. Contrary, the authors additionally report “trend difference” (I assume this is a borderline significance using a p level of .1 for the same single comparisons – it can be confused with a test for linear trend, which could be more meaningful here). The categories of urbanization are defined arbitrarily. Therefore, I would suggest to drop this kind of single comparisons. Instead it would be useful to give the unweighted n’s in each cell. It seems that there are some cells with less than 5 observations which is also problematic for statistical tests. In my opinion there is no surplus in separately reporting the very rare diagnostic sub-categories.

6. Co-morbidity or co-occurrence of mental disorders is very common and associated with higher impairment and disorder severity. It would be interesting to see if prevalence of two or more disorders is also more common in urban areas. Additionally co-morbidity has implication for the final analyses of the
urbanisation-disorder-association. The current analysis which were repeatedly done for each diagnostic category, did not allow separating the effect of the analysed diagnosis from the effects of possibly co-occurring disorders. One solution would be to use ordinal logistic regression analyses with the urbanization categories as “dependent” variable and simultaneous inclusion of all diagnostic categories as “predictor variables”. In further steps, models including the other risk factors and interaction-terms can be developed, to elegantly test the independence or interaction.

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

7. I am not used to the term accumulation of risk in this context. In life course epidemiology this means an additive effect of risks. The authors may refer to synergistic effects (combined effect that is more than the sum of the single effects). The terms independent risk factor and interaction may be less confusing.

8. In the abstract is a typo: “significantly significant”.

9. In Table 1 proportions are certainly no percentages as indicated by the column heading.

Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article whose findings are important to those with closely related research interests

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