Reviewer's report

**Title:** Non-specific physical symptoms in relation to actual distance and perceived proximity to mobile phone base stations and powerlines.

**Version:** 1  **Date:** 24 January 2011

**Reviewer:** Martin Röösli

**Reviewer's report:**

This is an interesting analysis on a public health relevant topic. The sample size of the study is large and allows investigating the proposed topics. Nevertheless, some issues have to be clarified prior the manuscript is suitable for publication.

- **Major Compulsory Revisions**

  The hierarchical structure of the data is not clear for readers who are not familiar with the Dutch system. What exactly are PC4 and PC6? What is meant by SES? I assume it is socioeconomic status. Is this included in PC4/PC6 or do we deal with cross classification? (This could be easily clarified with a graph.) What is the reason for not including SES as a fixed effect in the model? The whole paragraph about statistical analysis is difficult to understand and important issues are missing: description of the outcome variable (continuous4DSQ score?), type of random effect model (random intercept, random slope?), how cross products were calculated and what is the implication for interpretation of the coefficients. I am also wondering whether interactions should be tested for the most important factors. It might be possible that the relation between symptoms and geo-coded distance to BS is different for those reporting sensitivities and those who do not.

  Only significant effects are listed in table 2. In my opinion also non-significant association are important and interesting and I would like to see them. Moreover, such a table would make transparent which factors are included in the model. In any case, it makes no sense to list only significant categories within one variable as it was done for occupational status (p. 15, 2nd paragraph). In this case significance just depends on the choice of reference category. Thus, a likelihood ratio test is needed to check whether the variable (as a whole) is significant or not. If so, coefficients for all factors have to be listed and the reference category has to be indicated.

  It is quite striking that the effect of distance on symptoms increased, the more variables were included in the model and reached borderline significance in the most comprehensive model. Such a pattern is usually an indication of residual confounding and might be interpreted as indication for a relation. (Even if interpreted in favor of a relation this must not be a causal relation. E.g. selective outcome reporting may be an issue.) This aspect and possible other explanations should be considered and discussed.

  The result section is not very reader friendly. Basically, it reproduces the history of the model development. I think the manuscript would profit if the relevant
pattern of the results would be described in a concise manner instead of going chronologically through the results of the 5 models.

Only in the discussion (p. 19) becomes clear that the data were collected in 2006 whereas exposure was assessed based on the base station distribution in 2008. This is a severe limitation and just to state “the number of base station increased only moderately” is not enough. Such a statement has to be based on referenced figures.

It would also be interesting to see whether geo-coded distance to power line is related to symptoms or not.

- Minor Essential Revisions

P. 8: No information is given about the participation rate for non-responder interviews. I assume that more than 255 people had to be contacted (or tried to be contacted).

P. 9, 2nd paragraph: Was there any information available on emitted power of the base stations. If so, one should differentiate between macro and micro cells. Similar considerations have to be made for the level of voltage of power lines (p. 10).

P. 13, last paragraph: As perceived distance was only a binary variable, I consider a non-parametric group test as more appropriate than calculating spearman correlation.

P. 19, line 1: In the view of the relatively low participation rate (37%) I do not agree on the statement “an important strength is the limited possibility of response bias.”

Table 1: I am very puzzled that 57% of the participants worked less than 20 hours per week and only 7% more than 20 hours. Is this representative for The Netherlands? If not explain, what type of selection bias happened.

Figure 1: The meaning of “clusters” is not explained (or I missed it).

- Discretionary Revisions

P. 6. I am not sure whether the term “multilevel confounding” is correct and widely used.


Figure 2: Y-axis needs a non-logarithmic label. Otherwise, it is difficult to read. I propose to include a graph for perceived and real distance to power lines (similar as for mobile phone base stations).

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

Quality of written English: Needs some language corrections before being published
Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
I declare that I have no competing interests.