Reviewer's report

Title: Representative survey on idiopathic environmental intolerance attributed to electromagnetic fields in Taiwan and comparison with the international literature

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Reviewer: Martin Röösli

Reviewer's report:

This paper describes the result from a survey about electromagnetic hypersensitivity (EHS) in Taiwan which was done in the same manner as 5 years before. In addition, a meta-analysis on previous EHS surveys is presented. All these data are unique and merit publication. However, the manuscript has to be substantially improved. Neither, structure, language nor methods fulfill quality criteria for publication. In the following I just comment on the major aspects to be considered in a potential revision. But I encourage the authors to critically revise all aspects of their manuscript.

Title: a more informative title is needed and not just a result statement, which may be even questioned (see my comment below). My suggestion would be something like: "Representative survey on idiopathic environmental intolerance attributed to EMF in Taiwan and comparison with the international literature by means of a meta-analysis".

Abstract: Mention the conduct of meta-analysis also in the methods section, make clear to differentiate between results from own survey and from the meta-analysis.

Introduction is not well structured and selection of references is unclear with some key references missing like systematic reviews of Rubin and Röösli or Baliatsas BMCPH, 2012. Also the first paragraph of the introduction is not related to EHS at all. I encourage to present more details on the results of the first survey in Taiwan as you cannot expect the reader to have read it.

Methods: Mention the exact wording that has been used to ask about EHS in your survey.

The chapter "literature review" should be labeled as meta-analysis. Again, one should extract the wording which has been used to ask about EHS in all papers including in the meta-analysis. Since there are no objective diagnostic criteria, several methods have been used and this may affect the prevalence rate.
The statistical analyses are not clearly described. Chi-square needs to be done on original data and cannot be done on adjusted values as the description implies. How the weighting has been done is not clear to me.

Results: Report the participation rate.

I do not understand table 2. How exactly has the comparison between survey 2007 and 2012 been done. All the columns appear to be a within survey comparison but some of the OR are presented as a between survey comparison (e.g. line 148 in the manuscript). I feel that either interpretation or analysis of data has not been done correctly. It also seems to be very implausible that OR for very poor health is 0.2 for EHS individuals. All previous studies report the opposite and also internal consistency is not given if the OR for impairment in daily activities is 2.2. Thus, correctness of data analysis needs to be thoroughly checked and if correct the reason for seeming inconsistencies should be made transparent. I propose to make 2 separate tables: one for within 2012 survey comparisons and one for between surveys comparison.

In table 1 column 3, row "excellent" confidence interval is not correct.

Line 150-174 needs to be restructured according to figure number. First start with selection of studies and then pooled prevalence rates and then at the end gender differences and not the other way round. It seems that line 156 to 161 do not refer to the meta-analysis but to the own survey. This is confusing. Regarding the time trends analysis, no statistical tests have been made whether there is a time trend or not. Thus, one should be a little bit more modest in interpretation.

Nevertheless, evaluation of time trends is interesting and in particular the results from repeated surveys in the same countries are relevant and useful for interpretation. However, more details are needed, whether the same collective was asked twice in the same manner or whether different questions, methods were used in different collectives. A table might help to clarify. For instance in Switzerland, there is a repeated survey in the same collective described in the cited paper Comptes Rendus Physique but this does not appear in the graph.

Discussion: Discuss limitations and strengths of the survey. E.g. does the phonebook includes both mobile and landline phone numbers or could there be selection bias (also with respect to changes since 2007).

The discussion about the cause of potential time trends in the prevalence is not very substantiated but just guessing without reference to any other publication. One should refer to relevant studies that have been conducted in this context.
Personally I also do not find it very relevant (and surprising) that women seem to be somewhat more likely to report EHS than men. Thus, would be good to discuss the motivation for this analysis and its implication for public health. But maybe with more detailed data extracted from the surveys one could think about other sub-group meta-analysis than gender effects. E.g. the prevalence rates for different diagnostic criteria used in different surveys.

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