Author’s response to reviews

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

Authors:
Po-Chang Huang (pcrainy@gmail.com)
Meng-Ting Cheng (dream1990@livemail.tw)
How-Ran Guo (hrguo@mail.ncku.edu.tw)

Version: 1 Date: 10 Oct 2017

Author’s response to reviews:

Response to Reviewers’ Comments

Reviewer #1

Background

1. Other than reported by the authors, the cited study by Linet et al. (1997) (p. 4, l. 45) did not come to the conclusion that residential exposure to electromagnetic fields (EMF) increased the risk of lymphoblastic leukemia in children

Response: As suggested by another Reviewer, we restructured the Background and appended some valuable studies as references. The study by Linet et al. was omitted in the revised manuscript.

2. Could you insert a reference, proving your statement on p. 5, l. 66/67?

Response: As suggested the Reviewer, we added some references (References 6 to 11) to support the statement.

3. It might be appropriate to critically reflect on the approach and methods used in the cited letter to the editor (Hallberg & Oberfeld, 2006) (p. 5, l.74-p. 8, .1), predicting a prevalence of 50% of IEI-EMF in 2017
Response: As suggested the Reviewer, we added a comment on the method of the literature “However, the most recent data included by the review were from as early as 2004, and the prediction was based on the assumption that the prevalence would have the same trend of increasing afterwards.”

Methods

4. Missing word (p. 6, l. 85)

Response: We corrected the description “the target sample size of in region” to “the target sample size in each region.”

5. Why was the target sample size 1,100 in every region?

Response: To obtain a representative sample of the whole population, we divided the administration districts in Taiwan as northern, middle, and southern regions; the regions have similar population sizes. The target sample size was set to obtain about the same number of participants with IEI-EMF as in the 2007 survey (170) in each region. As an increase in the prevalence was predicted, we used the upper bound of the 95% confidence interval of the estimated prevalence in the 2007 survey (15.3%) to estimate the target sample size (170 ÷ 15.3% = 1111) and then rounded off the number to 1,100. In response to the Reviewer’s comment, we added “which was determined to obtain about the same number of participants as in the 2007 survey in each region” to the revised manuscript.

6. During which time interval did the telephone survey take place?

Response: In response to the Reviewer’s question, we added the information “between December 2012 and March 2013” to the revised manuscript.
7. How people were contacted, how many declined to participate?

Response: In response to the Reviewer’s question, we added the following statement in the Questionnaire survey session: “The response rate was 23.6%, which was compatible to the 22.2% response rate in the 2007 survey.” to the revised manuscript.

Results

8. The results section would be better structured, if the authors would separate results of the telephone survey/comparison with the 2007 study and the results of the literature review/meta-analyses.

Response: As suggested by the Reviewer’s, we restructured the contents of the Result in the revised manuscript.

Discussion

9. How can the cited paper (Claassen et al., 2012) partly explain the declining trend in the prevalence of IEI-EMF? Claassen et al. (2012) come to the conclusion that "media often suggest a negative relationship between electromagnetic field exposure and health" and that information "was mostly framed in terms of precaution and concern" - in my opinion this would not speak in favor of a decreased prevalence; cf. Eldridge-Thomas & Rubin (2013) and Huiberts et al. (2013) for similar content analyses in Britain and Norway

Response: The analyses by Claassen et al. in 2012 showed that the media content often suggested a negative relationship between electromagnetic field exposure and health, and so if the public’s perception is affected by media reports, when the number of media report decreases, the prevalence of IEI-EMF should decrease consequently. Because the study by Claassen et al. found that the number of newspaper articles decreased from 87 in the first year (March 2008 to March 2009) to 68 in the second year (March 2009 to March 2010), their results speak in favor of a decreased prevalence over time, as observed in our study. In fact, in the Netherlands, a decline in the prevalence of IEI-EMF from 7.0% in 2009 (van Dongen et al., 2014) to 3.5% in 2011 (Baliatsas et al., 2014) was observed, which further supported our argument that media reports affecting public awareness may partly explain the trend. The studies mentioned by the Reviewer in UK (Eldridge-Thomas & Rubin, 2013) and Norway (Huiberts et al., 2013) also found that media articles tended to report negative health effects of EMF on human’s health, but no corresponding studies on the prevalence of IEI-EMF were available. In response to the
Reviewer’s comment, we added the description “For example, a study in the Netherlands found that the number of newspaper articles decreased from 87 in the first year (March 2008 to March 2009) to 68 in the second year (March 2009 to March 2010), and a decline in the prevalence of IEI-EMF from 7.0% in 2009 [28] to 3.5% in 2011 [30] was observed (Figure 2).” and added the two references provided by the Reviewer to support the argument “The media reports on IEI-EMF have focused on precaution but lack of scientific evidences, which could influence reader’s perception.” in the Discussion of the revised manuscript.

Figure

10. Fig. 1- Google scholar is missing - Could you insert the specific numbers for reason of exclusion?

Response: As suggested by the Reviewer, the Google scholar searching results and the specific numbers of exclusion were added to Figure 1.

11. Fig. 2 - New data on the prevalence of Germany (2009, 2013) is available https://doris.bfs.de/jspui/bitstream/urn:nbn:de:0221-2014022811170/3/BfS_2014_FM8854.pdf

Response: We thank the Reviewer for the information and added the prevalence data to the Figure 2. On the other hand, data from a study conducted in Japan were excluded because the study limited the participants to females.

12. Fig. 3 & 4 - Please check the spelling of the names (e.g., Scherottner, Levallosis)- It would be convenient, if you could insert the reference [x] - Please explain whether the year refers to the publication or to the year of the assessment.

Response: As suggested by the Reviewer, we added the names of the authors and the references to Figures 3 and 4 in the revised manuscript. We also indicated in the figure legends that the years are the years of investigation.
References

1. Please check the guidelines for the reference style (e.g., [12] Typos, spelling mistakes, grammar.

2. P. 4, l. 46 have -

3. P. 4, l. 53 a -

4. P8, l 112 meta -

5. P. 12, l. 185 interviews -

6. P. 13, l. 186 2007 survey -

7. P. 13, l. 193-194 too many words -

8. P. 13, l. 194 observe -

9. P. 14, l. 204 &

10. p. 16, l. 235, 237 prevalence -

11. P. 16, l. 235 the -

12. P. 16, l. 235 globally

Response: In response to the Reviewer’s comment, we checked and corrected the format of references, typos, spelling mistakes, and grammar.
Reviewer #2:

1. 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".

Response: In response to the Reviewer’s comment, we change the title as “Representative survey on idiopathic environmental intolerance attributed to electromagnetic fields in Taiwan and comparison with the international literature”; we spell out EMF because abbreviations should be avoided in the title and omit “by means of a meta-analysis” because that will make the title too long.

2. 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.

Response: As suggested by the Reviewer, we added the statement “and a meta-analysis” to the abstract in the revised manuscript.

3. Introduction is not well structured and selection of references is unclear with some key references missing like systematic reviews of Rubin and Rsl or BaliatsasBMCPH, 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.

Response: As suggested by the Reviewer, we restructured the Background session and added the important references as mentioned by the Reviewer. In addition, we deleted the first paragraph of the Background session in the original manuscript and added the following information of the 2007 study to the Method session: “The 2007 survey targeted at the households equipped with a telephone. The participants were randomly selected from the phonebook of Chunghwa Telecom, which is the only wire telephone provider in Taiwan, using the Computer-Assisted Telephone Interviewing System (WinCATI 2000, Sawtooth Technologies) [21]. To ensure the sample was representative, the survey applied a two-stage, geographically stratified systematic sampling scheme. Households were randomly selected from each of 25 geographical areas of Taiwan according to proportional population size, and a respondent above 18 years of age was enrolled from each household. The interview consisted of questions regarding demographic variables,
presence of catastrophic illness, self-reported health condition, and risk perception of various environmental agents, impairment of daily activities, and medical utilization. Self-perceived health status was evaluated with a five-point Likert scale. The EMF sources listed include mobile phone base stations, mobile phones, electric towers, and high voltage cables. IEI-EMF was identified by the question “While being near EMF sources such as mobile phone, electrical devices, or computer, will you feel allergic or sensitive?” Because the same method was used in our survey, we deleted some descriptions of our survey consequently.

Methods:

4. 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.

Response: As suggested by the Reviewer, we described the exact wording that has been used to ask about EHS in our survey as “While being near EMF sources such as mobile phone, electrical devices, or computer, will you feel allergic or sensitive?” We also changed the session label to “Literature review and meta-analysis” in the revised manuscript.

5. 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.

Response: In response to the Reviewer’s comment, we added Table 4 to summarize the references we cited in the meta-analysis. However, many of them did not report the exact wording, and so we put the definition of IEI-EMF instead.

6. 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 be done is not clear to me

Response: We did not use chi-square tests on adjusted values; we used 95% confidence intervals to determine the statistical significance. In fact, we did not report p values from chi-square tests.
In response to the Reviewer’s comment, we deleted the statement from the statistical analysis in the revised manuscript. Weighted chi-square tests can be conducted by using weighted percentage of each variable. For example, the test for impairment in daily activities can be done as the following:

Original data
IEI-EMF: 37 “yes”; 118 “no”
Control: 358 “yes”; 2790 “no”
P<0.001

Weighted data
IEI-EMF: 37.68 “yes”; 115.38 “no”
Control: 378.67 “yes”; 2771.27 “no”
P=0.006

As we stated in the original manuscript, this can be performed by using the SVYCHISQ command of the survey package of R Version 3.3.2.

Results
7. Report the participation rate.
Response: In response to Comment 7 from Reviewer 1, we have reported the response rate as “The response rate was 23.6%, which was compatible to the 22.2% response rate in the 2007 survey.” in the revised manuscript.

8. 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.

Response: As suggested by the Reviewer, we separated the original Table 2 into two, one presenting within-survey comparison of the current study (the new Table 2) and the other presenting comparisons between the 2007 and the current studies (the new Table 3) with tests for homogeneity. The errors identified by the Reviewer were from mis-coding the IEI-EMF and the non-IEI-EMF groups. All the data have been rechecked and corrected in the revised manuscript.

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

Response: We thank the reviewer for identifying the error and have corrected the confidence interval as 3.5 (1.3, 8.1).

10. 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.

Response: In response to Comment 8 from Reviewer 1, we have re-structured the section. Further re-structuring was done according to Reviewer 2’s suggestion.

11. 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.

Response: As stated in the original manuscript “We performed a meta-analysis of the 11 studies identified in our literature review that reported sex-specific prevalence rates and obtained a summary OR…,” this part is referring to the meta-analysis. To avoid confusion, we rephrased
the sentence as “Our meta-analysis also showed that women were more likely to report IEI-EMF than men, with an summary odds ratio of 1.20 (95%CI: 1.00—1.43) (Figure 4)…” in the revised manuscript. We made the statement about the decreasing trend on the basis of Figure 2. Whereas we did not perform statistical tests, the graph showed a decreasing trend after 2007. In response to Comment 1 of the Reviewer and to be more conservative, we have changed the title of the paper to “Representative survey on idiopathic environmental intolerance attributed to electromagnetic fields in Taiwan and comparison with the international literature.” To be more conservative, we changed the statement to “Overall, there was an increasing trend in the prevalence after the first report in 1994, but the recently, the prevalence seemed to be in decline. Specifically, all the three countries with more than one estimate after 2006 in our meta-analysis had declined: from 13.3% in 2007 to 4.6% in 2013 in Taiwan, from 7.0% in 2009 [28] to 3.5% in 2011 [30] in the Netherlands, and from 10.0% in 2009 [26] to 7.0% in 2013 [26] in Germany (Figure 2).” in the revised manuscript. We also made changes to other statements to make the interpretation more conservative.

12. 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.

Response: We have added a table (Table 4) summarizing the previous studies in response to other comments. In the study in Comptes Rendus Physique, 130 of the participants (94 women and 36 men) reported to be electromagnetic hypersensitive either in 2008 or in 2009. As the study also reported data in 2008 and 2009 separately, we redrew Figure 2 to plot the data from 2008 and 2009 separately, instead of plotting the 130 IEI-EMF altogether as in the original manuscript, in response to the Reviewer’s comment. Figure 3 was also redrawn.

13. 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.
Response: According to the Reviewer’s suggestion, we added the statement in the discussion: “The phonebook of Chunghwa Telecom does not include mobile phones, and therefore our survey might over-estimate the prevalence rate because sufferers of IEI-EMF are less likely to use mobile phones and thus more likely to subscribe landline phones due to the fear of EMF. Since the 2007 survey also used the phonebook and we observed a decrease instead of an increase, our conclusion of a decreasing trend should still hold even if the sample was biased.” We also discussed about the strengths of the study. Furthermore, we added an example from the Netherlands to support our speculation of a decrease in media coverage being one of the contributing factors for the decreasing prevalence (Reference 39) and another reference to support the possibility of desensitization (Reference 43).

14. 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.

Response: Our motivation of studying the sex difference came from Leitgeb and Schrottner, who reported that women had the lower perception threshold then the men in detecting a 50-Hz electric current. While other sub-group analyses might be interesting, the focus of this paper is on the time trend. Therefore, it might be better to leave other sub-group analyses to separate papers.