Author's response to reviews

Title: Sensitivity to electricity - Temporal changes in Austria

Authors:

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Author's response to reviews: see over
Dear Editor! Dear Reviewers!
Thank you for your comments, we revised our manuscript and attached a point-by-point response to clarify the concerns.
Best Regards, J. Schröttner

(1) Ethics - Experimental research that is reported in the manuscript must have been performed with the approval of an appropriate ethics committee. Research carried out on humans must be in compliance with the Helsinki Declaration (http://www.wma.net/e/policy/b3.htm), and any experimental research on animals must follow internationally recognized guidelines. A statement to this effect must appear in the Methods section of the manuscript, including the name of the body which gave approval, with a reference number where appropriate.

The study was approved by the Ethic Commission of the Medical University Graz. The appropriate reference number is 19-277. A statement including the name of the body and the reference number was added to the method section.

(2) Informed consent must also be documented. Manuscripts may be rejected if the editorial office considers that the research has not been carried out within an ethical framework, e.g. if the severity of the experimental procedure is not justified by the value of the knowledge gained.

This was an inquiry study without any experimental load to the participants. Before starting the interview the persons were informed of the right to abstain from the participation. The inquiry was continued only after consent of the participants.

(3) Please include a ‘Competing interests’ section between the Conclusions and Authors’ contributions. If there are none to declare, please write ‘The authors declare that they have no competing interests’.

The Competing interests section is now included in the manuscript.

(4) Please include an Authors’ contributions section before the Acknowledgements and Reference list. For the Authors’ contributions we suggest the following kind of format (please use initials to refer to each author's contribution): AB carried out the molecular genetic studies, participated in the sequence alignment and drafted the manuscript. JY carried out the immunoassays. MT participated in the sequence alignment. ES participated in the design of the study and performed the statistical analysis. FG conceived of the study, and participated in its design and coordination. All authors read and approved the final manuscript.

Authors' contributions section is now included in the manuscript.

(5) Please also include the questionnaire you administered as a supplementary file to the manuscript.

File has been attached.

Reviewer: Martin Roosli

Major compulsory revisions
1. The study is described as a follow up. According to what is stated in the conclusions, the objective of the study was to investigate temporal change of the prevalence of EHS in Austria by comparing the new study results with two previous studies. I doubt that this aim can be achieved because this study is different compared to the two previous studies. One previous study used measurements of the perception threshold for 50 Hz field. The other previous study focussed on urban and rural clients of a power utility. Thus, not only temporal changes but also different collectives or different measurement methods may explain divergent results. This should be more explicitly discussed.
I also suggest that this study is not described as a follow-up study but rather an independent study.

The term follow-up had been avoided. Temporal changes of the prevalence could be estimated, anyway. The text was changed accordingly.

2. In my opinion the comparison of the risk perception with the established scientific knowledge is difficult to make. Firstly, the authors should be more specific to what kind of health risks and exposures they refer when they talk about the absence of established health risks. The scientific evidence is quite variable for different type of exposures and health effects. There is no doubt that EMF causes health effects if the exposure is large enough. Thus, I wonder what has exactly been asked in the survey.

As stated, the question was related to environmental field levels (the questionnaire is added as a supplementary file).

Secondly, it should also be mentioned that absence of health risk is not scientifically proven for many exposures and health effects. From this perspective, the risk perception of the population is not completely contradictory to the scientific knowledge.

We do not consider it adequate to include such a statement. The reason is that as a scientific principle, proving the nonexistence of a health risk is not possible for any risk factor at any level.

Thirdly, there are many more aspects that should be considered when making such a comparison (e.g. individual risk vs. population risk, etc.) Such a comparison should therefore discuss such aspects. However, in my view there is no need for such a comparison anyway.

For improved clarity the statement was revised. (see text)

3. I have some concerns with the method how the study participants were selected. The chosen method has the disadvantage that person that stay a lot of their time at home are markedly overrepresented. I would have preferred a random population sample with an attempt to contact all persons from an a priori chosen sample.

The aspects of sampling including a potential bias have been discussed. (see discussion section) The described procedure has been preferred over a preselected sample to overcome the more relevant bias due to poor response rates. A potential bias due to persons staying more or less time at home could not be avoided by either approach.

In the last paragraph of the result section was stated that there was a high correlation between age, education and place of living. In my view the applied stratified sampling method according to age, sex and living place should prevent from such an aggregation. The authors should explain how this could have happened.

Education and place of living were no parameters for the stratified sampling method. The different meanings of the terms “place of living” and “regional allocation” have been made clearer and the text had been changed accordingly.

In addition, one should report the participation rate and the number of phone calls that did not result in a contact. I guess that not everybody contacted was willing to participate in the interview. The authors should also provide the proportion of the population that is not registered in the
public phone registry. I guess that persons who have only a mobile phone are not registered. Such persons may be less likely to be EHS. All these points should first be clarified before the statement can be made that “care was taken to generate a representative sample and to overcome the problem of poor participation rate”. (discussion)

Care was taken to get a representative sample. Limitations and potential bias have been discussed. The participation rate and a potential impact of mobile phone contacts have been included in the discussion section.

4. According to the WHO factsheet No. 296 EHS is characterized by a variety of non-specific symptoms, which afflicted individuals attribute to exposure to EMF. According to this definition 30% of this sample may be considered as electromagnetic hypersensitive. The definition of EHS that was applied in this study (physician visit) is more strict than in any previous study. Nevertheless, the proportion of EHS in this study is similar to other countries. This is a somewhat surprising finding and should be discussed and explained in more detail.

Unfortunately, there is no common definition of EHS (see Leitgeb and Schrottner, Bioelectromagnetics 2003, 24, 387-394). We do not consider it surprising that our results are at the lower end of the span since different definitions lead to prevalence’s spanning from 1.5 to 9.5%. (see changes in the text)

Minor essential revisions
5. The formatting of reference in the text is not correct.

All references in the text were numbered consecutively, in square brackets, in order which they appear. (see reference section)

6. First paragraph of introduction: Not all of the cited studies are actually negative studies. This should be corrected.

The wording in this first paragraph of the introduction was revised. (see text)

7. In the introduction the authors refer to the conclusion of a study that self declaration is extremely unreliable and usually overestimated without giving any details. I would like to know the main arguments for this statement.

Text has been modified to clarify the arguments.

8. In the introduction is stated that increased electro-sensitivity is a necessary but not a sufficient precondition for EHS. In my view this statement cannot be made. So far, research has not identified any objective criteria or biological mechanism for EHS. Thus, one cannot know what is involved with being EHS. Moreover, why should a person who claims to be hypersensitive to mobile phone radiation, have a lowered perception threshold for extremely low frequency EMF?

This statement is based on extensive investigations at EHS groups with ELF- and/or RF-attributed symptoms, recruited by different strategies and comparisons with results gained at the general population (see references given in the paper).

9. It is well known that the exact phrasing of the questions has an impact on the outcome of a questionnaire survey. Thus, the exact wording of the most important questions should be given in the paper. Were symptoms asked in an open way or was a list given? If a list was given, was the sequence of the list randomized? (The questions that are already given in the figure captions should be referenced in the method section.)
Examples of the exact wording are given in the method section and the questions given in the figure captions were added to the method section. Symptoms were asked in an open way (see attached questionnaire)

10. Only one statistical test should be used either the Kruskal-Wallis or the median test. The authors should chose the test whose assumptions fits the data best.

We revised statistical analyses and used Chi square test, which was recommended by the second reviewer.

11. In my view meterosensitivity is not a health complaint but rather a self declared causal attribution (similar to EHS). I guess that meterosensitivity also includes headache.

This is a semantic discussion. In fact, “meteorosensitivity” is a term frequently used also by physicians.

12. It seems implausible that overall 57% is using a personal computer, but 56% uses mobile phone AND computer at the workplace. This would imply that there are almost no persons who are using a computer at home only.

We asked two separate questions regarding the use of electrical equipment. 1) “Do you use computers at home?” and 2) “Do you use computers at your workplace?”. The data showed that 56% of the responders use a computer at work and 57% use a computer at home. However, this does not mean, that all persons using computers at work also use computers at home.

Discretionary revisions

13. The authors may more explicitly discuss the public health implication if such a large part of the population is disturbed by EMF in the everyday environment.

It would go beyond the scope of the paper to discuss potential sociological or political aspects in required depth in particular since a causal relationship with EMF has become extremely implausible.

14. I encourage the authors to compare the three groups “EHS”, “disturbed” and “undisturbed” individuals more extensively. E. g. are there group differences in the health risk perception, in the avoidance pattern, in the willingness to accept health risks, or in the type of information used, etc?

Inclusion would considerably increase the length of the paper. A separate paper dealing with such aspects is in preparation.

Level of interest: An article of limited interest
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.
Reviewer: Girdhar Agarwal
Reviewer’s report:
As asked by the editors, I shall concentrate on statistical review of the
manuscript.

1. The nonparametric tests used are incorrect. The authors want to test equality
of proportions among different groups. Chi square test is used for this purpose.
Since cell frequencies are low, chi-square test with pooling should be used.

We revised statistical analyses and included Chi-square tests. Text was modified accordingly.

2. All the confidence intervals for proportions should be checked. Which formula
is being used? Mostly, confidence intervals are symmetric about the point
estimate. None of the intervals satisfy this here.

The confidence intervals were calculated in SPSS, where for percentage rates the calculation is based on
Binomial-distribution. Therefore the confidence intervals are asymmetric about the point estimate.
We changed calculation of confidence intervals: Based on normal distribution we used the following
formula: CI=p±1.96*(sqrt(p*q/N)). (CI…Confidence Interval; p…calculated percentage; q=1-p; N…sample
size). Text was modified accordingly.

3. I do not agree with reviewer’s concern about the following point:
“Furthermore, the authors state that self classification to EMFs is "considerably
increased" from the rate (~2%) reported in a 1994 Austrian survey by Leitgeb;
the reviewer argues that this may not have been statistically significant if the
previous sample size had been equivalent to that in this paper.”

Text in the discussion and conclusion section has been modified.

4. Leitgeb (1994) investigated 200 persons, whereas the present study is based
on 526 subjects (larger sample than previous one). The estimates should be
more precise in the present study. The comparison between two studies is fair
enough. For comparison purposes, the sample sizes need not be equal.

The aspects of different sample size have been included into the discussion section.