

Author's response to reviews

Title: Exposure to mobile telecommunication networks assessed using personal dosimetry and well-being in children and adolescents - design, methods, non-response-analysis and descriptive data of the German MobilEe-study

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Author's response to reviews: see over

First of all we would like to thank the reviewers for their critical reading of the manuscript and their helpful suggestions

Reviewer's report

Title: Mobile phone exposure assessed using personal dosimetry and well-being in children and adolescents - design, methods, non-response-analysis and descriptive data of the German MobilEe-study

Version: 1 **Date:** 9 September 2008

Reviewer: Joachim Schüz

Reviewer's report:

Thomas et al. describe design and methods of a cross-sectional study on RFR and well-being in children and adolescents and provide first descriptive results.

As this is a very comprehensive study and also the first study on environmental RFR exposures in children which will result in quite a number of articles, it is a good idea to give a more detailed description of the methodology in a separate paper that can be used as a reference for all follow up articles.

Major compulsory comments:

1. The self-reported distance to the near base station is a major concern, as it appears that the authors plan to use it for a number of analyses (page 10). However, as shown in reference 16, the agreement between self-reported distance and true distance is abysmal. There is also some concern that persons concerned about health effects from base stations perceive them to be closer than they are. If the authors plan to use distance as one of their exposure variables (which is also not a very good choice, as distance to a base stations only weakly predicts exposure (see reference 33)), it is surprising why they don't attempt to calculate the distance based on geographical coordinates. This is not a huge effort, as they have the addresses of all participants and coordinates of base stations can be obtained from the network operators. The authors need to add self-reported distance as a limitation in their discussion and should also justify why they plan to use it and why they do not attempt to assess the true distance.

Answer: For the main analysis we used measured exposure to investigate a possible association between mobile phone exposure and well-being in children and adolescents. As most of previous studies used self-reported distance to the next base station as proxy for exposure, we decided to use this information in a second analysis to compare our results to previous findings. As it was only a secondary analysis, the effort of using geographical coordinates would not have been justified. We appreciate that self-reported distance has its limitation therefore, we discussed it in the manuscript on page 13 (1st par.): “Overall, personal dosimetry is considered a better measure of exposure than stationary measurements or estimation (self-reported or calculated) alone.” and also on page 3 (4th par.): “One main drawback of these studies was that the exposure assessment had to rely on self-reports of the participants.”

2. Even though participation rates of 50% were to be expected, they pose a major problem as participation is related to many factors, like better education or higher level of concern. Hence, the non-responder analysis is very useful, but also comprises only half of the non-responders. This has to be more clearly stated in the discussion of limitations, together with a more critical assessment of what potential biases are to be expected.

Answer: This aspect is discussed in the revised version of the manuscript on page 13/14: “There are still missing information about non-responder as we could not reach all of these subjects for the non-response-analysis. As primarily those participated who were concerned about possible health effects caused by exposure to mobile telecommunication networks a selection bias is possible. Furthermore concerned participants could overestimate subjective exposure and symptoms. Due to the objective exposure assessment a differential misclassification seems to be unlikely.”

3. The choice of exposure quartiles for the main analysis needs to be more justified.

Answer: We a priori decided to divide exposure in quartiles to determine possible dose response relationships. We could not take exposure as a continuous variable, because too many values were below the limit of determination. (page 15, 3rd par.).

As illustrated in figure 2, the exposure variable appears to be highly skewed, i.e. the range of exposures within the highest quartile seems to be larger than differences between quartiles.

Answer: We thank the reviewer for this comment and have changed the revised version of the manuscript accordingly (page 15, 4th par.) “In a sensitivity analysis we divided exposure at 90% percentile to compare those 10% of the participants who had the highest exposure levels to the remaining participants. One reason to use also a binary cut-off was that the range of exposures within the highest quartile seemed to be larger than differences between quartiles”.

The secondary type of analysis, i.e. the dichotomization at the 90% percentile, seems to be more appropriate. The authors should give the absolute values of both the categorizations and add a justification for their statistical approach.

Answer: We did not give the absolute values, because the binary cut-off was only used in a sensitivity analysis. The values for exposure during waking hours for the children were: low exposure: 0.13-0.25; high exposure: 0.25-0.92 and for the adolescents: low exposure: 0.13-0.26; high exposure: 0.26-0.78. The values are given in table 3 in the revised version of the manuscript (page 23).

Minor essential revisions:

1. Whole article. The term “high frequency” should be replaced by “radio frequency”. The frequency range should be defined in the beginning of the article.

This term has been changed throughout the revised version of the manuscript.

2. Page 3, 2nd paragraph. Reference 3 is from 1998 and only a preliminary report. More up-to-date references should be used or even a good review on this topic.

Answer: A review of Rööslı et al. (2004) was added in the revised version of the manuscript.

3. Page 3, 3rd paragraph. It should be mentioned that recent studies also indicate a higher SAR in children compared to adults.

Answer: This aspect was added to the revised version of the manuscript on page 3 (3rd par.): “In addition, recent studies indicated higher SAR values for children in comparison to adults.”

4. Page 3, 4th paragraph. The Danish replication of the TNO study (Riddervold et al, 2008) should be added.

Answer: We did not cite the above mentioned paper or also those of Haarala (2005) or Preece (2005), because these were experimental studies and we decided to cite only epidemiological studies that investigated health outcomes.

5. Page 5. Add some information on how long an average interview lasted.

Answer: A sentence concerning the duration of the interview is given in the revised version of the manuscript on page 5 (3rd par.): "The duration of the interview was about 30 minutes."

6. Page 7, bottom. Describe in more detail on how distance to the nearby base station was assessed. Give the phrasing of the question and the reply options, i.e. whether it was an open question or you provided reply categories?

Answer: There is an extended sentence in the revised version of the manuscript on page 7 (3rd par.): "The questions were taken from the questionnaire of the annual survey of the Institute for applied sciences (infas) and provided different reply categories". The questions are also given in the revised version of the manuscript on page 7.

7. Page 8, 2nd paragraph. For readers not so familiar with these technologies, please define "uplink" and "downlink" and give the exact frequency ranges for GSM, UMTS, DECT and WLAN.

Answer: This information is given in the revised version of the manuscript on page 8 (2nd par.).

8. Page 8, 4th paragraph. It is not entirely clear, whether the values below the detection limit were set to 0.025 V/m after or before the summation. If the latter, this should be justified in the text, as the exposures from GSM 900 are higher than from the other technologies.

Answer: This information was added to the revised version of the manuscript on page 8: "To classify the exposure all measured values that were below this limit were replaced previous to the analysis by half of the limit (0.025 V/m)."

It should also be mentioned of what proportion of measurement values was below the detection limit, preferably by technology. As not all participants may have had the respective technology, there may have been a true zero for, let's say, DECT?

Answer: The information regarding the proportion of the values below the limit of determination was added on page 11 (4th par.): “The majority of measured values were below the limit of determination (82% of the measured values during waking hour for children and adolescents).”

As we were not able to differentiate between the frequency bands we could not give these information by technology.

9. Page 11, 1st paragraph. The statement that participating parents could better declare the distance is not substantiated. It is only shown that they more often declared the distance to the next base station. This should be phrased accordingly.

Answer: The sentence was changed accordingly in the revised version of the manuscript on page 11 (2nd par.): “Furthermore participating parents more often knew the distance to the next base station (don’t know: 3%) than those who did not want to participate (don’t know: 22%).”

10. Page 11, Objective exposure. It would be very interesting to see these results for uplink and downlink separately. According to fewer base stations in rural areas, uplink exposures are assumed to be higher in rural areas, as APC is less effective (see e.g. Lönn et al.). Downlink may be lower in rural areas, as there is less communication traffic. The authors also need to comment on this.

Answer: As the exposimeter has a low selectivity to differentiate between the up- and down-link channels we were not able to give these results. This problem is described in the exposure assessment on page 8 (2nd paragraph) and in the discussion on page 13 (2nd paragraph)

11. Table 3. The absolute values of the exposure quartiles should be added to the table. Please add to the text of the average agreement would be for the dichotomization at the 90% percentile.

Answer: More detailed data on exposure to mobile telecommunication networks is given in the revised version of the manuscript in a new table 3 on page 23.

12. Figure 2. Please add the boxplot for the combined data for children and adolescents.

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

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests.

Reviewer's report

Title: Mobile phone exposure assessed using personal dosimetry and well-being in children and adolescents - design, methods, non-response-analysis and descriptive data of the German MobilEe-study

Version: 1 **Date:** 18 August 2008

Reviewer: Kjell Hansson Mild

Reviewer's report:

Major Compulsory Revisions need for Thomas et al: Mobile phone exposure assessed... First of all a very similar paper has just been published by Joseph W, Vermeeren G, Verloock L, Heredia MM, Martens L. Characterization of personal RF electromagnetic field exposure and actual absorption for the general public. Health Phys. 2008 Sep;95(3):317-30. This paper should of course be read and cited. Furthermore regarding childrens use of mobile phone I am also missing a reference to the paper by Söderqvist F, Hardell L, Carlberg M, Hansson Mild K. Ownership and use of wireless telephones: a population-based study of Swedish children aged 7-14 years. BMC Public Health. 2007 Jun 11;7(147):105.

Answer: The paper of Joseph et al. was cited in the revised version of the manuscript on page 12 (4th par.) in the discussion.

Regarding the paper of Söderqvist et al.: We only cited those papers where an association between mobile phone and health was considered. Therefore only the paper of Söderqvist from 2008 was cited.

The paper is a population based study the exposure to electromagnetic fields with mobile phone frequencies (not as it is said on page 4: exposure to mobile phone frequencies..) and to assess the level of exposure in a general population. The study also wants to find out about health effects and describes the questionnaire BUT then those results will be published elsewhere!! If so then the details about that part can be deleted.

Answer: As the paper gives a description of the methods of the study we felt it is necessary to enclose the questionnaires that were used in the field study.

However the use of ESM-140 dosimeter (what is the dose hear??) is strange in view of their aims since they say on page 8 that the instrument has a low selectivity between up- and down-link channels. Thus, they are not able to distinguish between what comes from a base station and what comes from the person's use of a mobile or DECT phone. Furthermore it is not clear how the instrument is responding to WiFi signals and digital radio/TV signals and other RF/microwave sources in our near environment.

Answer: As we described on page 8 and also in the discussion in the manuscript we took into account the low selectivity of the Maschek-Dosimeter. There is only one other personal exposimeter (Satimo, past Antenessa) available, which does not show this deficit. As the results of our pilot study showed, this exposimeter was not applicable for children and adolescents due to its weight and size and therefore we had decided to use the Maschek-exposimeter despite its low selectivity. The Maschek-Dosimeter does not react to radio/TV signals and other RF/microwave sources in the near environment.

There is no rational given for the summation formula on page 8 as to why the square of the E field is given and not a linear sum. Clearly at these low level the concept of SAR is not relevant.

Answer: After consultation with experts of the Federal Office for Radiation Protection we a priori decided to use the formula given on page 8 for the analysis.

The results on page 11 is hard to read since they are now given in percentage of ICNIRP. Better to give values in field strength for a general comparison and not lock it to just ICNIRP values. This of course also is valid for Figure 2, where also a better legend is needed to understand the figure with all its symbols.

Answer: It would make sense to give values in field strength for the different frequency ranges. As the exposimeter has a low selectivity to differentiate between the three frequency ranges we were unfortunately not able to give these results.

On the discussion part I am missing how much of the exposure did occur in school with the use of WLAN and laptops etc. In offices often a LAN net operating at 5 GHZ is used and this is mostly the dominant exposure except of course for that from handheld phones towards the head.

Answer: As we used only the overall exposure to multiple frequency fields for the analysis we do not have the information in WiFi (WLAN) alone.

Furthermore it is not common in German schools that Laptops and WiFi (WLAN) are available, only few schools Germany are equipped with this technology.

The reference list is not clear in many of its citations. For instance no 1 and 2 are very hard to get for other people. Ref 16 only gives first author and then et al whereas others have the full list. Ref 18 unclear. Ref 31 misspelled author and journal.

Answer:

Reference 1 is changed into a paper published in English (Hutter et al. 2004)

Reference 2: This report is given as a citation for the actual proportion of Germans that are concerned about EMF exposure. You can find the information in English via the following link:

http://www.emf-forschungsprogramm.de/forschung/risikokommunikation/risikokommunikation_abges/risiko_021.html .

This link is also given in the revised version of the manuscript

Regarding ref 16 (ref 20 in the revised version of the manuscript): All authors are given in the revised version of the manuscript

Ref. 31 (ref. 36 in the revised version of the manuscript): The author's name is given in the correct form in the revised version of the manuscript

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 interest.