PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Subjective Symptoms Related to GSM Radiation from Mobile Phone Base Stations: a cross-sectional study
AUTHORS	Gomez-Perretta, Claudio; Navarro, Enrique; Segura, Jaume; Portolés, Manuel

VERSION 1 - REVIEW

REVIEWER	Hanns Moshammer Inst. Env. Health, Med Univ Vienna, Austria
REVIEW RETURNED	13-Sep-2013

GENERAL COMMENTS	In 2003 the authors published a paper [their ref. 4] that reported higher prevalence of a list of (unspecific) symptoms in persons living in closer proximity to a base station. Now they submit another paper presenting practically the same data and reaching very similar conclusions. I do not find that this new paper adds any substantial new and relevant information. Therefore I would not recommend publishing the paper in its current form. Nevertheless when I read the 2003 paper several years ago I wanted to ask the authors several questions. If they could re-write their paper to answer these questions this might in fact make the new paper worthy for publication. I do not remember all the details of the 2003 paper. In short the authors used two approaches to analyze the data then. In the first approach they divided the respondents to the questionnaire in those living in close proximity to the base station and those living farer away (I believe the cut-off distance was set at 150 m) and compared the two groups. They found the people living closer to the base station had higher exposure (average exposure approx. 1 mW/m^2) than those living far away (0.1 mW/m^2). They found that the people closer to the base station reported on average more and more frequent symptoms. In this analysis they simply treated the symptom categories “never-seldom-often-very often” as a linear variable and they calculated the average over both groups for each symptom. In the second approach they applied a linear regression assuming again that the categorical variable was indeed linear and normally distributed. I do not remember if they included any potential confounders in the model. I can understand that their original approach has been criticized for several reasons and that the authors tried to respond to the critique. One methodological critique certainly concerned the assumption of

	linearity and normal distribution of the outcome variable. In the new paper the authors deal with that issue in two ways: (a) by creating a new binary variable and by performing a logistic regression and (b) by applying a bootstrapping method to produce more accurate confidence intervals. A logistic regression is a perfect way to circumvent the issue of non-normal distribution. I do not entirely see the need to additionally do the bootstrapping approach. Indeed the two approaches provided very similar results. Alternatively the authors might have considered doing an ordinal regression. They used SPSS and this program – as far as I know – does offer that option. By doing this they could have circumvented the loss of information by collapsing the categories into two. On the other hand results of ordinal regression are not as easily interpreted as those of simple logistic regression. Another certain point of critique was about possible confounders. The authors themselves in their 2003 paper pointed out that measured exposure and observed distance were highly correlated. Therefore it could be possible that the increase in symptom frequency is (partly) caused not by the fields but by the perceived (psychological) threat. Indeed a base station might affect people in different ways. There is the visual impact of a towering mast close by, there might be acoustic phenomena (e.g. continuous sounds from machinery etc., infrasound has been discussed as a possible mechanism of effect), and also the fields might have a biological effect. Even with fields it is not clear if the electric or magnetic component or if the combination of both (the flux density or the energy uptake) would be more relevant, or if it would rather be the long-term average exposure or the exposure above a certain threshold or the very peak exposures that are indeed relevant. I do not really find that any of these exposures “confounds” the effect of base stations but rather these are different pathways by which base stations can affect the health of the neighbors. Disentangling the causal pathways would be better done with experimental study designs. Nevertheless in this new paper the authors undertook to evaluate the possible confounding by “fear”. In 2012 respondents were asked if they worried about the masts back in 1997. I would have expected that the answer to that question cannot be very sensitive because of the long time between 1997 and 2012. Surprisingly the answer correlated well with sleeping problems reported in 2000 or 2001. This association is plausible and therefore indicates that the responses in 2012 were still somewhat reliable. But of course some reverse causality (people with sleeping problems recall stronger worries) cannot be excluded. In the new study the authors controlled for fear as a possible confounder and they found that it did not confound the association between exposure and symptoms. Living near a base station does not make more people generally fearful, but people that generally worry about the fields would express stronger fears when they live close by. But since they were not asked about strength of fears it is

not very surprising that the binary variable (fear yes/no) is not closely linked to distance or exposure and therefore does not confound the association.

In 2010 the authors also performed new measurements of the EM field. I simply do not understand why this was done. Was there concern that the original spot measurements were not representative of long-time exposure? In that case there would have been a (non-differential) misclassification of exposure that would rather lead to an overestimation of the confidence intervals and eventually also to an underestimation of any true effect. But I am not sure how measurements taken so many years later could amend the old problem, because after 10 years the whole grid of base stations must have changed and exposure in 2010 would not have any influence on symptoms in 2000.

The new measurement campaign was reported in detail in the methods section and it resulted in the loss of power because some participants could not be reached again or valid measurements were not possible because of rain. But the results of that campaign are not reported. We do not hear if the exposures (2001 and 2010) were correlated with each other nor if there was any upwards or downwards trend in exposure over time. So the relevance of the whole enterprise remains obscure.

When I read the 2003 paper for the first time I did have quite different concerns. These concerns mostly deal with two possible selection biases. There is a possible individual selection bias: Of 215 originally randomly selected persons only 101 finally participated in the 2003 study. If persons living closer to the base station AND having more symptoms were more inclined to participate this would have led to a selection bias. The authors could analyze among those persons that filled the questionnaire (150) if those that permitted an exposure measurement (101) and those with missing exposure data (49) differ in symptom scores. They could check if those with exposure data (101) and those without (114) differ in distance to the base station. This information would inform about possible selection bias.

I am even more concerned about collective selection bias: I simply do not know and could not deduce from the 2003 paper (nor from the new one), why the authors chose to study the population of La Nora. It could have been simply a random decision or a quasi-random decision (meaning that the selection was not based on an a priori information on symptoms and concerns). But if there was an a priori concern about health effects due to the base station the findings are no longer representative for the general population: In a large country like Spain there will always be some “clusters” of people with higher symptom rates simply by chance. But people living in a neighborhood observing an increased rate of certain symptoms will “always” seek to find a causal interpretation for that. Whatever serves as a plausible reason for their symptoms will be blamed, be it a base station, a waste dump, an industrial plant or

	something else. Always that “thing” will be blamed that seems the most likely cause based on its central location. If in such a setting a study will be conducted the study is bound to find significant “effects” but I would hesitate to interpret these findings causally. Still the finding would be interesting for the generation of hypotheses. I would want a new paper dealing with the same data-set to discuss these two kinds of selection bias. I would want a new paper to discuss the old findings in the light of current knowledge. The authors give a detailed history of epidemiological studies in the field but they should reflect the results in the light of experimental studies. I do understand that there is still no clear concept of how the base stations should cause this poorly defined “microwave syndrome”. But for this paper to add to the existing literature it should at least set out and try to propose some plausible mechanisms. The 2003 paper only reported exposure by mentioning the mean exposure of the two groups. A more detailed report about the exposure, its variation and change over time, would be interesting. Also of course it would be interesting to know if there is a threshold of effect. With a ROC analysis the authors could at least calculate the most distinctive cut-of value (at which concentration are sensitivity and specifity both optimized?). Instead they report a cut-off based on 50% of persons reporting each symptom (table 3). So for rarer symptoms they report higher cut-offs. But this is not relevant for defining limit values or for considering a threshold that could be further evaluated in future studies. The current abstract does not inform the reader what the study really is about. From that text the paper could be a study of new data, a literature review, a meta-analysis, or the re-analysis of old data. The English language could be improved throughout the paper.

REVIEWER	Michael Carlberg Department of Oncology, University Hospital, Örebro, Sweden
REVIEW RETURNED	27-Sep-2013

GENERAL COMMENTS	Abstract: Some of the most important results from the logistic regression should be added here (OR + 95 % CI in "Results"). Methods - Statistical analysis: Page 11, line 23-25: "The dependent variables health-related symptoms) given in four ordinal categories (0=never, 1=sometimes, 2=often; 3=very often) were dichotomised (0,1 = 0 versus 2, 3 = 1)." An alternative way to analyse the data would be to use ordinal logistic regression instead of binary logistic regression. With this method the ordinal categories (0-3) could be kept and no information would be lost due to the dichotomisation. See for example

	Söderqvist et al: Use of wireless telephones and self-reported health symptoms: a population-based study among Swedish adolescents aged 15–19 years. Environmental Health 2008, 7:18. Did the authors try to analyse using this method? If so, were the results similar to the binary logistic regression? To visualize the results, the authors might be interested in using restricted cubic splines (OR symptom vs exposure in µW/m2 as a continuous variable) for some of the symptoms. In that way it would be possible to see if the relations between symptoms and exposure are linear or not. See for example Hardell et al: Pooled analysis of case-control studies on acoustic neuroma diagnosed 1997-2003 and 2007-2009 and use of mobile and cordless phones. Int J Oncol 2013, 43:1036-44. Please add information about the version of SPSS used for the analyses. Results: Page 12, line 32: "The sample age distribution was 42, 17 ± 17, 61 and [15–81] interval..." It is not clear what this means (mean, SD, interval?) - please rephrase. Page 12, line 43-48: "The univariate logistic regression indicated that age was inversely associated with irritability [OR =0.97 (0.95-0.99)] and that the oldest had the greatest difficulties hearing [1.03 (1.01-1.06)] and walking [1.04 (1.01-1.07)]. Please indicate that the intervals in the parentheses corresponds to 95 % CIs. Discussion: Only 88 of the 215 subjects were included in the final analyses. The authors should discuss how this might have affected the reliability of the results. Tables: Table 2: Please indicate what the ORs for GSM exposure represents (increase in risk per increase in log µW/m2?). Table 3: Please explain the meaning of SSV, SPF and AV (SSV=sensitivity, SPC=specificity, AV=average?). Table 4: It would be more clear if adjusted results (and covariates used in each analysis) could be shown for all symptoms according to Table 2. In that way one can compare all unadjusted and adjusted results directly (an alternative would be to combine Table 2 and 4 in one table but perhaps such a table would be too wide).

	Again, please indicate what the ORs represents. Table 5: Explain that "B" represents the beta coefficent (=log OR). In general: Throughout the manuscript, change "significant" to "statistically significant" when appropriate.

VERSION 1 – AUTHOR RESPONSE

Answers to Professor Hanns Moshammer's comments:

Authors are very grateful to Professor Moshammer, his comments will contribute to improve the quality of this article. The manuscript was revised attending his comments, changes are introduced in red colour in the revised manuscript, and explanations are given in the following paragraphs:

The answers to comments are ordered following the text of Prof. Moshammer.

1) This manuscript is a re-analysis of old data: We modified the abstract attending reviewer comments, to clearly inform the reader that this manuscript is a re-analysis of old data published in 2003.

2) In the former paper (2003 paper) the correlations were calculated between non continuous variables (symptoms). Thus, the former paper described the association through Pearson correlation when variables did not follow a normal distribution. In the present work the binary logistic regression was the chosen method since there is not a requirement to the variables being normally distributed. Ordinal logistic was discarded since the small size sample and consequently many empty cells (4 health status) produced higher standard errors and consequently anomalous confidence intervals.

The results of ordinal regression are not as easily interpreted as those of simple logistic regression. We found many empty cells since ordinal regression needs a large number of cells to classify subjects. Our sample was not sufficient to complete the minimal number of cells.

We agree with Professor Moshammer in applying a bootstrapping method to produce more accurate confidence intervals. Bootstrapping was considered a good method to control the reliability of our results especially with such small sample, and fortunately Bootstrapping showed acceptable bias (beta coefficient) with respect the normal analysis. We think Bootstrapping is an important contribution in this article.

We introduced a new paragraph at the beginning of the Discussion: "In the present re-analysis a more robust statistical method was employed, indifferent to the assumption of normality. In order to reduce the limitation of sample size effect and extrapolate our results to the entire population from which the sample was obtained, re-sample method or bootstrapping was used."

3) In 2003 we compared both groups related with two extremely separated distances (< 150 m and > 250 m). We never did a correlation between measured exposure and observed distance (Only with Sommers´d or similar could have been done), since distances were interval measures. Moreover correlations were between non continuous variables (symptoms).

4) The comment about causal pathways: We agree with the reviewer, the causal pathways would be better done with experimental study design. This is introduced in the revised manuscript at the end of the Discussion. Also 5 new references are introduced in the revised manuscript. The new references [3]-[6] and [38] are introduced and discussed in the introduction and discussion of the revised manuscript. The older references are re-numbered in the revised manuscript.

5) We did not introduce potential confounders in our paper of 2001. We introduce potential confounders in this new manuscript, confounders such as age, gender and use of cellular phone are considered in the new model.

We initially agree with the reviewer's comment about the fear as a confounder. In this new study we controlled for fear as a possible confounder. The subjects were asked about strength of fears, however there was not a gradient it their answer. The subjects were totally categorical expressing their opinions about this issue. Remarkably, their perceptions about the possible risk of the antennas were mostly categorical, answering yes or not to this issue, avoiding intermediate positions.

We think that dichotomized fear confounds the association between sleepless and GSM exposure since fulfill the *sine qua non* condition of being statistically significant in the univariate model (Sleepless vs. fear) and fear and GSM were not associated, fulfilling the second condition of no association between predictors.

6) We did not perform any new measurement in 2010. The measurements were performed in 2001. This manuscript is a re-analysis of old data. This is clearly written in the first line of Exposure-assessment. However we clarify this point in the revised manuscript. In 2010 it was performed a new statistically approach, introducing confounders and eliminating 2 subjects because illness and 11 because the 2001 measures (RF) were made under raining condition.

7) A participation bias is extremely difficult to avoid, but differences between participants (88) and no participants (26) were not due to symptoms status or subjective distance to the BSs. The age was introduced as covariate in the analysis of the symptoms between groups. This is explained in last paragraph of the Results and Discussion of the revised manuscript.

The 49 residents with not exposure data were due because external causes: refusal to give admittance for taking measurements (16), residents not being at home for the scheduled measurement appointment (10); and methodological restrictions such as having serious health problems (stroke, neurodegenerative, cancer, rheumatological disorders and severe depression) (23). In the new article we added to this list a pregnant woman and a subject with alcoholism.

Following your comments, we analyzed if those who permitted an exposure measurement (88 in our last model) and those uninterested in RFs measures (26) (excluding those with serious illness) differed in symptom scores. We performed an analysis of variance (ANOVA) and an analysis of covariance (ANCOVA) adjusting with age, since this variable was correlated with irritability, headaches, walking difficulties and hearing loss:

7.1) ANOVA showed that the group with EMF measures were more prone to complain of memory loss (F = 5,07; p= 0,027); while subjects without EMF measures showed more skin problems (F = 10,66; p = 0,001). Therefore, health status was overall no different between those groups, the group with EMF measurements and the group without EMF measurements.

7.2) There was not relationship between subjective distance to the BS and willingness to participate (Pearson Chi- square = 2,80, df = 1, p=0,094).

7.3) There were not differences between groups related with age, being the group without measurements younger (mean =36,38 years; SD = 15,94) than the group with measurements (42,17,17,61).

7.4) There was not a measurement effect when using age as covariate (square partial eta = 0 % to all symptoms (irritability, headaches, walking difficulties and hearing loss).

8) We explain the reasons to select La Ñora, this town was representative of a small urban-rural area, free of pollution and bias related with health or education, as well as accessibility to information related to cellular phone towers. This is introduced in “Methods” of the revised manuscript.

9) Comment about the exposure description: We introduce this paragraph the revised manuscript (Exposure assessment) to provide a better view of the exposure: “The broadband measured exposure was almost invariable during the time interval of the measurements. It changed with the position or place but it did not change in time, this could be related with a low intensity of the traffic channels (a very low number of phone calls) related with the higher and constant intensity of the broadcast channel [10].”

10) Comments about the threshold of effect and ROC: We introduced Goodness-of-fit of the outcome binary response variables related to GSM exposure (log). Table 3 was modified in the revised manuscript to show cut-off values of exposure according to ROC analysis.

The cut-offs were determined from ROC analysis (best point which predicted maximum sensitivity and specificity from the classification table - table 3). A new paragraph is introduced at the end of Methods- Statistical analysis:

“With the predicted probability scores derived from the regression analysis, receiver operating characteristic (ROC) curves were constructed for all symptoms or modalities in order to analyze

sensitivity and specificity levels. For each curve, the best cut-offs for GSM exposure that maximizes (sensitivity + specificity) were also calculated."

Answers to Professor Michael Carlberg's comments:

Authors are very grateful to Professor Carlberg, his comments contributed to improve the quality of this article. The manuscript was revised attending the comments, changes are introduced in red colour in the revised manuscript, and explanations are given in the following paragraphs:

1) The abstract was modified to include some of the most important results from the logistic regression (OR + 95 % CI in "Results").

2) The comment about the use of ordinal logistic regression instead of binary logistic regression: We did ordinal analysis but SE (ordinal) was higher than SE binary; but results were similar. SE for ordinal (irritability 0,35-0,38.0,51) and SE for binary (0,10); and change in -2 logLLK = 22,94 (ordinal) and 19,36 (binary) Nagelkerke was similar (0,26 for binary vs. 0,22 for Ordinal). We prefer binary instead of ordinal to make an easier interpretation.

To visualize the results, we used restricted cubic splines (OR symptom vs exposure in μW/m2 as a continuous variable) to see if the relations between symptoms and exposure are linear or not. We follow the work of Hardell et al: Pooled analysis of case-control studies on acoustic neuroma diagnosed 1997-2003 and 2007-2009 and use of mobile and cordless phones. Int J Oncol 2013, 43:1036-44.

3) We give information about the version of SPSS used for the analyses at the end of the section "Statistical analysis".

4) In "Results": Page 12, line 32 was rewritten in the revised manuscript giving more details: "The mean age was 42, 17 years (standard deviation ± 17, 61, interval 15–81)".

5) SD is Standard Deviation.

6) In "Results": Page 12, we show the intervals corresponding to 95 % CIs.

7) Only 88 of the 215 subjects were included in the final analyses. We discuss how this might have affected the reliability of the results in the last paragraph of "Results". This comment overlaps with a comment from Prof. Moshammer.

8) We indicated the meaning of ORs for GSM exposure, it represents the increase in risk per increase in log μW/m2, (log=ln natural logarithm).

9) In Table 3 we explain the meaning of SSV, SPF and AV: SSV=sensitivity, SPF=specificity, AV=average.

10) We introduced adjusted values from Table 4 in Table 2 and eliminated Table 4 in the revised manuscript. In that way one can compare all unadjusted and adjusted results directly. The caption was also modified to indicate that ORs represent the increase in risk per increase in ln GSM (µW/m2).

11) Table 5 was re-numbered Table 4 in the revised manuscript. The caption of the new Table 4 explains that "B" represents the beta coefficient (=log OR).

12) Throughout the manuscript, "significant" was changed to "statistically significant" when appropriate.

VERSION 2 – REVIEW

REVIEWER	Hanns Moshammer Med Univ Vienna, Austria
REVIEW RETURNED	06-Nov-2013

GENERAL COMMENTS	The English langauge still needs improvement. I recommend the authors ask a native speakerfor help. e.g. the first sentence of the abstract: a paper is not a study! Proposal: "We performed a reanalysis of the data from Navarro et al, 2003, in which health symptoms related to microwave exposure from mobile phone base stations (BS) were explored including data obtained in a retrospective inquiry about fear of exposure from the BS." (by the way: I am not sure that reference numbers are helpful in the abstract because the abstract often is a stand-alone text.) Apart from the language there is also the issue of English versus continental number format. I am happy with either decimal point or comma, but not when even in the same paragraph (first para of results) the authors switch from one to the other. This version of the paper is considerably improved. Apart from langauge check I still suggest some (minor) revisions. - please check the percentages in the descriptive statistics of the first paragraph in "results". (a) I'd rather report % of women (using PC or mobile phones), i.e. % of 45 persons, not % of female phone users, i.e. of 88 persons. (b) Even accounting for rounding errors the percentages do not add up to full numbers. Table 2: OR is provided for a 2.72-fold increase. This is difficult to interpret. A better measure would be e.g. 10-fold increase (re-calculation would be easy) The late query about concerns (as a possible confounder) might render the results less valid. That should be discussed under "limitations". ROC curves: a cut-off with small SE or SP is not very meaningful. The possible values should be restricted to meaningful values (e.g.

	>0.5) A figure showing the distribution of EMF measurement results (e.g. histogram,...) should be provided. Dichotomisation of the symptoms variable should be explained before the binary variable is first introduced (approx. 2 paragraphs earlier). I am willing to do another review but I do not think it is necessary. I think the authors can respond to my proposals. My English is not good enough to evaluate the langauge quality of an improved version.

REVIEWER	Michael Carlberg Department of Oncology, University Hospital, Örebro, Sweden
REVIEW RETURNED	05-Nov-2013

GENERAL COMMENTS	The authors have replied to all of my questions from the previous review; the manuscript has improved and the tables are much more clear. There are just a few points that needs attention before the manuscript can be published: In general: The type of decimal mark is not consistent throughout the manuscript. In all tables, the period sign (".") has been used but in the text sometimes the comma sign (",") has been used (for example "The mean age was 42, 17 years (standard deviation ± 17, 61, interval 15–81)..."). This was also the case in the previous version of the manuscript but I didn't notice it then - I apologize for that. Please change to period sign in the whole text (eg "The mean age was 42.17 years (standard deviation ± 17.61, interval 15–81)..."). Abstract - results: Here, some of the ORs and CIs are presented with only one decimal accuracy (for example "Lack of appetite OR=1.6, 95 % CI=1.2-2.0) while in the rest of the manuscript two decimals are used. Please use two decimals also in the abstract (eg "Lack of appetite OR=1.58, 95 % CI=1.23-2.03"). Page 15, line 3-10: "There were not health differences between those who permitted an exposure measurement (88 in our last model) and those uninterested in RFs measures (26). ANOVA showed that the group with RF EMFs measures were more prone to complain of memory loss (F = 5,07 ; p = 0,027); while subjects without EMF measures showed more skin problems (F = 10,66; p = 0,001). There were not health differences between those groups when using age as covariate."

	Shouldn't this read "There were health differences between those who permitted an exposure..." since you found statistically significant results according to the ANOVA? Then, when using age as a covariate the results were no longer statistically significant? Finally, in the answers to my previous comments, you write: "To visualize the results, we used restricted cubic splines (OR symptom vs exposure in μW/m2 as a continuous variable) to see if the relations between symptoms and exposure are linear or not. We follow the work of Hardell et al: Pooled analysis of case-control studies on acoustic neuroma diagnosed 1997-2003 and 2007-2009 and use of mobile and cordless phones. Int J Oncol 2013,43:1036-44." However I find no such figures in the manuscript so I assume this comment was added by mistake. As I wrote previously using restricted cubic splines would be interesting but it certainly is not necessary. Also it would be difficult since this method has not been implemented in SPSS (I did not know that when I wrote the previous review).

VERSION 2 – AUTHOR RESPONSE

Answers to Professor Hanns Moshammer's comments:
Authors are very grateful to Professor Moshammer, his comments contributed to improve the quality of this article. The manuscript was revised attending his minor comments, changes are introduced in blue colour in the minor corrections (red colour corresponds to the first revision), and explanations are given in the following paragraphs:
The answers to comments are ordered following the text of Prof. Moshammer.
1) The first sentence of the Abstract was changed following your comment.
2) Typing mistakes were corrected substituting comma by decimal point.
3) The percentages in the descriptive statistics of the first paragraph in "results": Since no statistically differences due to gender were observed, we omit the results about % of electronic devices according to gender. These lines were rewritten in the revised manuscript.
4) Table 2: OR is provided for a 2.72-fold increase. This is difficult to interpret. A better measure would be e.g. 10-fold increase (re-calculation would be easy): We introduced column 3 and modified Table 2 caption accordingly. Log is natural logarithm =Ln, and Log10 is base-10 logarithm.
5) The late query about concerns (as a possible confounder) might render the results less valid. This comment has been introduced as a limitation.
6) In ROC curves we report only those cutoffs with SSV or SPF above 0.5
7) A figure (histogram) showing the distribution of EMF measurement results is introduced as Figure 1.
8) Dichotomisation of the symptoms variable was explained.
9) English was revised

Answers to Professor Michael Carlberg's comments:
Authors are very grateful to Professor Carlberg, his comments contributed to improve the quality of this article. The manuscript was revised attending his minor comments, changes are introduced in blue colour in the minor corrections (red colour corresponds to the first revision), and explanations are given in the following paragraphs:
The answers to comments are ordered following the text of Prof. Carlberg.
1) There were typing mistakes and the decimal point "." was used in the whole text.
2) We introduced two decimals accuracy.

3) In Page 15, lines 3-10 are modified to improve comprehension:
“There were not globally health differences between those who permitted an exposure measurement (88 in our last model) and those uninterested in RFs measures (26), and these results were unaltered when using age as covariate. Square partial eta measured 0 % contribution of the willingness participation variable to symptoms that correlated with age such as: irritability, headaches, walking difficulties and hearing loss. Also, there was not relationship between subjective distance to the BS and willingness to participate (Pearson Chi- square = 2.80, df = 1, p=0.094).
However, ANOVA showed that the group with RF EMFs measures were more prone to complain of memory loss (F = 5.07 ; p = 0.027); while subjects without EMF measures showed more skin problems (F = 10.66; p = 0.001).”

4) In the answers to previous comments, we wrote erroneously: "To visualize the results, we used restricted cubic splines (OR symptom vs exposure in µW/m2 as a continuous variable) to see if the relations between symptoms and exposure are linear or not. We follow the work of Hardell et al: Pooled analysis of case-control studies on acoustic neuroma diagnosed 1997-2003 and 2007-2009 and use of mobile and cordless phones. Int J Oncol 2013,43:1036-44."
This was a mistake; we did not use cubic splines since is not is not provided by SPSS.