Author's response to reviews

Title: Use of wireless telephones and self-reported health symptoms: a population-based study among Swedish adolescents aged 15-19 years

Authors:

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Author's response to reviews: see over

Reviewer's report
Reviewer: Neil Pearce

1. Is the question posed by the authors new and well defined? Not particularly. This is a continuation of a previous study, but the manuscript of the previous report has not been provided so it is not clear how much of the findings are new. The new findings appear to be: (i) those for wireless phone use; (ii) those for health symptoms. The former are of interest. The latter are so prone to bias and reverse causation that they are almost uninterpretable.

Our response (Re): Yes, the new findings are those mentioned above. The previous study is published in BMC Public Health and has now been attached (ref 13). We chose to report them separately for three reasons. First, the 15-19-year-olds received a questionnaire with additional question, which the 7-14-year-olds did not get. Secondly, we expected the older group to be quite different from the younger ones in terms of lifestyle factors and how they use the wireless technology. Thirdly, among the 15-19-year-olds, 200 subjects were selected from each age group and gender, while among the 7-14-year-olds only 125 boys and 125 girls were included.

Control for bias in this study is limited which makes the interpretation difficult. We have however tried to control for the most likely factors that might have biased the results e.g. watching TV, family income, for further details see manuscript.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?
Yes

3. Are the data sound and well controlled?
The findings for mobile and wireless phone use, yes; the findings for health symptoms, no.

Re: There are obvious limitations to the study design that we used, which we also discuss in the manuscript, see page 13 and last paragraph.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
Yes

5. Are the discussion and conclusions well balanced and adequately supported by the data?
The discussion and conclusions focus on the findings for health symptoms, but these are the least reliable part of the study.

Re: Partly we agree and we have added a couple of sentences on wireless phone use see page 11, first and second paragraph. The reason for the more lengthy discussion on the findings for health symptoms is because it is not as straight forward to present as the former part. Because we think that to some degree data on the health symptoms and

perceived health are interpretable, but also as the reviewer points out prone to bias, this needs to be discussed rather then just dismissed as uninterpretable.

6. Do the title and abstract accurately convey what has been found?
Yes

7. Is the writing acceptable?
Yes

What next?: Accept after minor essential revisions
Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Needs some language corrections before being published
Statistical review: Yes, and I have assessed the statistics in my report.

Reviewer's report
Reviewer: Michael Kundi

This is an interesting hypothesis-generating cross-sectional study of mobile phone use in adolescents. It substantially increases the evidence base on wireless phone use in this group and adds interesting questions about determinants of usage habits on one side and consequences on health and wellbeing on the other side. Study design and conduct were of good quality, analysis can be improved as detailed below.

Minor Essential Revisions:

Abstract: Age range of subjects should be included in the Methods section.

Re: Now included in the abstract.

Factors associated with frequency of mobile phone use should be restricted to subjectâ##s characteristics; sleep should be excluded (see below).

Re: We agree and this has now been deleted in text and tables.

Page 4, 1st paragraph: As there are only few data linking mobile phone use in children and adolescents to variables such as smoking and drinking habits and other health related life-styles as well as to social background, statements about such relationships should express greater caution and may also refer to possible dependencies on the socio-cultural background.

Re: True, changes have now been made to express more caution. We mention that a health related life-style might be of importance since this has been reported in one study (ref 10) regarding smoking and alcohol drinking; see page 3 and last paragraph, and another study regarding father's/guardian's educational level and family type, see page 14 and second paragraph (ref 8).

Page 5, 2nd paragraph: It is stated that the questionnaire comprised 27 questions. However, in the following description of the contents of the questionnaire much more than 27 items are mentioned. Obviously some questions comprised several items. As the number of questions is of minor interest rather the number of pages should be mentioned because the mentioned discrepancy might lead to confusion.

Re: Number of pages and items have now been added, see page 5 and second paragraph.

Page 6 & 7, Statistical Methods: Because authors mentioned in the introduction that investigation of the relationship between subjective symptoms and mobile phone use was exploratory it should be mentioned in this section that no correction for multiple endpoints has been applied. While it is not faulty to use

logistic regression with mobile phone and DECT use in one model as independent and in others as dependent variable this procedure might still cause confusion. Therefore, authors should think about applying the more general log-linear model for explaining mobile phone and DECT phone use and restrict application of logistic regression to analysis of symptoms. Alternatively, authors could apply ordinal regression for symptoms and general health ratings (because no dichotomization, which is always debatable, is needed in this case).

Re: We thank the reviewer for pointing this out. We now mention that no corrections were made for multiple endpoints. We have also changed to the ordinal regression analysis, which we agree is a more appropriate method for the symptoms and health ratings – especially if we also avoid causing confusion. However, we prefer to use logistic regression for explaining use of mobile phone/DECT (Table 2), since we want the results to be comparable to our previous study on children, 7-14 years old (ref 11).

Page 9, 4th paragraph: Analysis of sleep length as an explanatory variable for mobile phone and DECT phone use reveals the methodological insufficiency of the chosen procedure because rather the extensive use of these devices explains the sleeping behavior as the other way round.

Re: We agree, and have now excluded sleep from the text and Table 2. Instead we included a separate analysis on wireless phone use related to insufficient sleep using the latter as dependent variable, which gave somewhat higher OR than the other way around, see page 10, first paragraph. We mention in the discussion why this might be of some importance, see page 12 and first paragraph.

Page 10, 2nd paragraph: Analyzing the different categories of symptoms separately is not the best option. Either ordinal or if distinct differences in contribution of explanatory variables across symptom categories are assumed, multinomial logistic regression should be preferred.

Re: As mentioned we chose to use ordinal regression analysis instead, which did not change the results much; most ORs were similar but some of the borderline findings turned significant and some that were significant, became non-significant. In Table 3 this applies to Hay Fewer, Dizziness, Headache and Tiredness, all use > 15 min/day. In Table 4 it applies to Concentration difficulties and Stress for total use, and Anxiety, Tiredness and Body pain for use > 15 min/day. In Table 5 it applies to perceived health adjusted for sleep and tiredness for mobile phone use > 30 min/day and for DECT phone use > 15-30 min/day. Overall most ORs increased with use in min/day.

Page 11, 2nd paragraph: Interpretation of the relationship between sleep and use of wireless phones is not convincing. Age was controlled for in the analysis. This may not remove interaction of age with other independent variables; however, it seems that sleep is no independent predictor at all. Rather extensive use of TV, computers and wireless phones indicates a certain life-style in adolescents that now and then will lead to problems getting enough sleep. In addition, recent studies indicate that mobile phone use before going to sleep increases tiredness

(Sleep 2007;30(9):1220-3, Arnetz B submitted).

Re: We agree and this has been changed. A certain life-style is likely to be related to problems with sleep and day-time tiredness. There is for example a Belgian study by Van den Bulck of how mobile phone use after lights out is related to increased levels of tiredness (ref 12). Adjusting for watching TV and use of computers did not change the results in our study.

Page 12, 1st paragraph: Authors mentioned the possibility the significant results could be due to chance. Indeed, 23 symptoms have been analyzed in relation to mobile phone use, and assuming independence of occurrence of these symptoms and a 5% level of significance the probability of three or more of the symptoms being significant by chance is about 2.5%. What is more interesting is the fact that among the 23 symptoms only 2 had ORs below 1 for mobile phone use and only 1 for DECT phone use. Unless we assume that all symptoms are somehow related to mobile or DECT phone use this points to a confounding factor. The symptoms strongest associated with mobile and DECT phone use point to a socio-economic factor. Family income or education of parents or some other proxy of SES should be included in the analyses (although family income was only weakly related to mobile and DECT phone use). Another possibility would be to construct a variable from TV and computer use that could be included as well. Headaches as well as concentration difficulties could be related to a life-style of extensive media use and less outdoor and physical activities. Asthmatic symptoms have been explained to some degree by the hygiene hypothesis, which partly explains higher prevalence in higher categories of SES (fewer siblings, less contact to animals etc.).

Re: Point taken, the possibility of a chance finding is there, but is rather small and instead we should of course have mentioned how few ORs there are below 1 and what this points to. We have now modified the section about possibility of chance findings in the discussion and also added in the methods that no corrections were made for multiple endpoints; see page 14, first paragraph and page 7, last paragraph. There could be confounding e.g. by as the reviewer mention a socioeconomic factor, but it could also be due to response bias or perhaps more likely bias due to previous believes or opinions among the respondents as a result of the way the questionnaire was designed. We have added this to the discussion; see page 13 and last paragraph. In one analysis we have now also tried adjusting for watching TV and family income with similar results; see page 14 and second paragraph.

Page 13, 2nd paragraph: Analysis of hands-free devices is not clear. It is stated that excluding subjects reporting anytime use of hands-free kits had no statistically significant effect on results. As prevalence of headaches was lower in subjects using hands-free equipment, excluding them should increase ORs of mobile phone use. Furthermore, statistical comparison between results of analysis with and without subjects using hands-free kits is not admissible. Rather use of hands-free devices should be included as a dummy variable.

Re: Apologies if our wording was not the best – it was in the Singapore study that the prevalence of headache was lower among those who used hands-free. However, had there been an effect excluding hands-free users in our study we would of course have expected an increased odds ratio. We have now adjusted for use of hands-free, which gave the same results as for the unadjusted analysis.

Discretionary Revisions:
Page 3, 3rd paragraph: Conductivity of tissues at different ages, and in particular of human tissues, has not been extensively studied so far. Although there are theoretical reasons and some empirical data that support the statement about higher conductivity and increased absorption in brain tissues of children this is still an unresolved problem. In addition, even if absorption in sensitive tissues is higher in children, we do not know at which age absorption reaches the values of adults. Instead of referring to scientific controversies surrounding these issues authors rather should underline the insufficient scientific basis to resolve them.

Re: We agree; there is no point in presenting a balanced view of opinions that do not originate from scientific evidence. Instead we now focus on the gap of scientific knowledge. Paragraph 3, page 3 in the manuscript first submitted has now been deleted and paragraph 2 modified.

Page 11, 2nd paragraph: It is interesting that DECT phone use is less popular in sparsely populated areas. Do authors have data about landline phone use in these areas? Maybe phone use in general is less popular in these regions (although mobile phone use does not show a difference, which might still sum up to an overall less prevalent use of phones).

Re: It is interesting; unfortunately we only have information on whether a landline phone is used or not.

What next?: Accept after minor essential revisions
Level of interest: An article of outstanding merit and interest in its field
Quality of written English: Acceptable
Statistical review: Yes, and I have assessed the statistics in my report.