Reviewer's report

Title: Mobile phone use and mental symptoms among young adults - a prospective cohort study

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Reviewer: Anke Huss

Reviewer's report:

Re: Mobile phone use and mental symptoms among young adults – a prospective cohort study

The authors describe associations between various aspects of mobile phone use and mental health in a group of Swedish young adults. A follow-up was performed to assess effects of exposure at baseline on the incidence of symptoms after one year. This is an interesting topic, but I have some reservations regarding the analysis and presentation of results and conclusions in its present form.

Major comments:

- The authors present tables of associations of exposure and mental health at baseline and of exposure at baseline and incidence of symptoms at follow-up. It would be good to see an additional analysis that looked at it the other way around: What happens to the people who report symptoms at baseline and who decrease their exposure? Does their mental health improve? It would considerably strengthen the conclusions if this effect could also be shown.

- In the analysis, adjustments are made for some potential confounders. Please explain what the effect of these confounders was on the effect estimates. In addition, accessibility stress (as well as the other categories of exposure) might be related to other, as yet unmeasured factors, such as some personality traits. These traits could also be related to developing symptoms over time. This issue should be discussed as a limitation of the study.

- While I don’t think that the use of Cox proportional hazard models is fundamentally wrong in this context, I wondered about the reasons to use it. The authors might wish to reconsider and use e.g. logistic regression, where it is no problem that there is no person time of follow up at baseline, and exactly the same interval of follow-up time for every study participant. Also, the associations would be expressed simply as OR which is sufficient in the context of this analysis. This would also make terminology slightly simpler (because what is now called prevalence ratio at follow-up is actually the incidence ratio).

- Tables 6 and 7 list the total n of the groups and percentages of the symptomatic population. It would be easier to understand if also the n of the symptomatic group was added. Also, some of the numbers do not add up. For example, the n
in Table 7 should be n of Table 6 minus symptomatic group (minus some missing data). This is fine for the women with current stress (second line), where 2695 minus 29% (=782 persons) symptomatic population equals the 1913 women in Table 7. However, the same calculation for the symptoms of depression yields 2672 minus 34% (=908), so 1764 women would be expected in Table 7, but not 692. Either there are some bigger mistakes in the Tables or the authors should explain and discuss in some more detail where this difference comes from and what 61% of missing data means for the interpretation of the results.

-The main conclusions in the manuscript relate only to mobile phone use. The way it is written now it could be misinterpreted to be related to the biophysical aspects of exposure (non-ionising radiation dose). This should be rephrased to make it clear that only psychosocial aspects of mobile phone use were assessed. In addition, accessibility stress had a more consistent effect on symptoms. This should be presented in the conclusions in a more balanced way.

Minor comments:

- Figure 1 could be omitted from the manuscript, as it does not add to understanding of the context.

- In its current form, the presented data do not inform about changes about exposure and outcome reporting at the two time points. How many persons report symptoms at baseline but not at follow-up and how many persons change their exposure status? It would be nice to see such a table added.

- The drop out analysis could be moved to the results part.

- Reduce the length of the introduction. Some parts of the introduction could instead be used in the discussion section.

- The discussion section could benefit from some shortening and restructuring, for example into the more conventional format of a short sub-summary of the main results, the strength and limitations, the comparison with other studies, the mechanisms that might be at work, and the conclusions.

- The manuscript might benefit from some English language editing. Currently there are some sentences where the meaning is somewhat unclear (e.g. discussion section p15: “It can of course be argued that this variable is close to the outcome. We find the variable of interest, though, since how we value the exposure probably has an effect on how we react to it”).

- The wording “mental symptoms” is unclear. Maybe change to “symptoms of reduced mental health”?

- The headers of Table 6 and 7 are misleading since these Tables deal with psychosocial aspects related to mobile phone use including frequency of use.

- In Table 2, the “overuse of mobile phone” is the only category that does not include the n for the reference (low) group.
**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.

**Declaration of competing interests:**

I declare that I have no competing interests.