Reviewer's report

Title: Headache features and associated symptoms of headache associated with mobile phone use: a cross-sectional study in university students.

Version: 1 Date: 26 May 2011

Reviewer: James Rubin

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Thank you for sending me this interesting paper to review. The authors have conducted a questionnaire survey of 247 medical students, identifying 40 who attribute headaches to mobile phone use. Using a semi-structured interview, they then characterise the symptoms reported by that subgroup. I think the paper presents a useful addition to the literature, but I do have some reservations about it and some suggestions for improvement.

Major Compulsory Revisions

1. In at least two places in the paper, the authors mention that “the headache features and associated symptoms of HAMP were not reported yet” (abstract) and that “to the best of our knowledge, this is the first report on the headache features and associated symptom of HAMP” (page 10). In fact, several previous studies have attempted to characterise the headache features and associated symptoms of people who report symptoms that are associated with mobile phones. From memory, these include:

Hocking 1998 (Reference 3 in the authors’ manuscript)
Roosli et al 2004 (Reference 5)
Stovner et al Nocebo as headache trigger: evidence from a sham-controlled provocation study with RF fields. Acta Neurol.Scand. 2008; 117 (suppl 188); 67-71

I think it would be useful to acknowledge these papers and discuss the results in relation to them.

2. The authors say that “recent randomized double-blind provocation studies have indicated that there is no causal relationship between MP use and hypersensitivity symptoms” (Discussion, third para). First, I think this reads better if the word “hypersensitivity” is dropped. More importantly, this is not quite accurate. These studies do not generally deny that there could be causal relationship between MP use and symptoms. But they do call into question what the mechanism is. In particular they suggest that MP use causes symptoms through mechanisms that are not related to the radiofrequency fields produced by the MP, and they suggest that psychological mechanisms (including a nocebo effect) may provide the causal mechanism. I realize that this is mentioned later on, but this particular sentence isn’t quite accurate as it stands.
3. The authors provide a description of the requirements we might look for before assuming a causal role between MP use and headaches. They then go on to say (Discussion, p12, second para) that “Rare headache provocation by regular telephones suggests that HAMP is related to the use of MP per se, even if the nocebo effect does play a role in HAMP provocation.” Why “even if”? Surely satisfying the criteria listed for assuming causation merely tell us that use of a mobile phone seems to trigger headaches. It says nothing about why use of mobile phone triggers headaches. It could be purely due to psychological mechanisms, purely due to radiofrequency fields, purely due to heat or sound, or some other factors or combination of factors. The causation criteria by themselves really tell us nothing about the mechanism involved.

4. Later in the same paragraph, the authors say that “The lack of HAMP occurrence when using hands-free equipment also suggests that certain factors nullified by hands-free equipment are related to HAMP provocation. In light of these recent reports using RF provocation and findings in our study RF may not be the sole cause of MP-associated headache, and the nocebo effect could play a role in provoking headache. However these findings do not exclude the possible role of MP in headache provocation.” I think there are several comments to make here.

First, I am concerned about the use of the word “sole” in “RF may not be the sole cause…” This takes for granted that RF is indeed one of the causes. But as the authors know, this is not supported by the large body of experimental data in this field.

Second, the following juxtaposition is not appropriate: “RF may not be the sole cause… However these findings do not exclude the possible role of MP in headache provocation.” It may be that this is just poorly worded, but the implication seems to be this study supports the assertion that RF fields are a cause of symptoms. I do not believe that the data presented here support that. The authors’ data could be said to be entirely consistent with a nocebo-based mechanism. Likewise, their data might also be seen as largely consistent with RF being the cause (although the single participant who gets symptoms from landline phones would still need to be explained), or heat from a phone being to blame. Making causal statements with these data is difficult.

Third, by saying that these findings “do not exclude the possible role of MP in headache provocation” the authors again do not clearly differentiate between an understanding of whether mobile phone use can trigger headaches (they can) and an understanding of what the mechanisms behind this might be. This distinction is important.

Minod Essential Revisions

1. The authors note in the first paragraph of the introduction that “Several epidemiological studies have suggested that MP use may be related to the
occurrence of these symptoms.” While I agree I am not entirely convinced that references 3, 4 and 5 support this assertion. As a review paper, reference 6 is probably sufficient on its own to support the statement.

2. Similarly, in the first paragraph of the introduction, the authors say that “a series of double-blind, sham controlled provocation studies have reported no significant difference in headache provocation between control and HP radiofrequency fields among participants who experienced some of the aforementioned symptoms during MP use.” References 11 to 15 are cited in support. Of these, references 11 and 12 are not provocation studies while reference 14 did not assess participants who experienced symptoms in connection with MP use. As far as I know, the most up to date review on this area is still that by our group. It cites several additional studies which do support the statement.


3. In the “questionnaire survey” section of the methods, the authors note that “we defined HAMP if headache developed during MP use or within 1 hour after MP use and … if he or she experienced HAMP more than ten time during last one year in this study.” Yet in the “current headache status of participants” section of the results, the authors say that only 37 of 40 participants with HAMP had experienced a headache in the past year. I am having trouble understanding why the remaining 3 participants were placed in the HAMP group if they had not had a headache in the past year. Could the authors clarify this?

4. The authors report “phonophobia” as being more prevalent among people who experience symptoms which they attribute to mobile phones. I found this interesting, as it might indicate a possible mechanism through which use of a mobile can cause symptoms. But as a psychologist I was slightly unsure whether I interpret the term “phonophobia” in the same way as the authors. It might be useful to see a translation of the exact wording of the question used by the authors. This would prevent any ambiguity for other readers.

5. I liked the suggestion that a different mechanism (or possibly it is a complementary mechanism that both initiates the nocebo effect and then exacerbates it?) is the noise and heat produced by a mobile phone. However, the authors then suggest (p 11) that this theory could be explored by using “more refined provocation tests that include all actual mobile phone conditions” and “conditions similar to actual MP use” (conclusions). Would it not be better to have a test in which these various proposed causal elements are separated out? E.g. a noise condition, a heat condition, an RF condition, and then combinations of them. Would that not provide a better way to demonstrate whether these are responsible for triggering the symptoms?
Discretionary Revisions

1. The manuscript is well written, but there are a few places where it would benefit from being read through by someone who is a native English speaker. Note also that in the Results (clinical features of HAMP) it should presumably be thirty eight participants from 40, rather than thirteen eight participants as currently appears.

2. I thought the use of pie charts (Figure 3) was very helpful. I wondered whether Figures 2 and 4 might also best be expressed in the same format, for consistency? I don’t have particularly strong views on that however.

3. I note that one participant mentioned that a normal telephone also triggered headaches. Did the authors ask about any other electrical triggers for symptoms? They will be aware of the broader condition of idiopathic environmental intolerance attributed to electromagnetic fields and that symptoms attributed to mobile phones makes up a possible subgroup within this condition. It might be worth broadening the discussion to include this? See, e.g.,


4. The authors note in the discussion that “discrepancies in the proportion of MP users who experience HAMP may be due to differences in MP types, and demographic features.” It may also be due to differences between different countries in levels of concern about mobile phones or media reporting about them (see, e.g. Mortazavi et al, Bioelectromagnetics 2007;28:326-330).

5. Unless I missed it (and apologies if so) no funding statement is provided.

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: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests