Reviewer's report

Title: Acupuncture for the Treatment of Tinnitus: A Systematic Review

Version: 4 Date: 24 February 2012

Reviewer: Klaus Linde

Reviewer's report:

The manuscript reports a systematic review of randomized trials of acupuncture interventions for tinnitus. There are no major flaws and the subject is of interest. Still, the review leaves me somehow frustrated (as a number of other reviews from this team). In the last few years the approach to systematic reviews is clearly changing from summarizing from what’s available (often with a focus on methodological quality) to what the evidence tells us regarding patient-relevant outcomes (e.g., approaches such as summary of findings tables and GRADE). New high quality reviews try to pre-define outcomes that really matter to patients and then try to extract, assess and summarize the available studies accordingly. While the systematic review in this manuscript clearly shows that there is no solid evidence base for the use of acupuncture for tinnitus it tells us very little about the actual findings of the available trials and how to proceed further (apart from the common place that better trials are needed). The authors report whether group differences in trials are significant or not and then conclude that the available evidence is contradictory. For a reader it is completely impossible to check whether this conclusion is accurate. Counting p-values in a set of mostly underpowered (from all what we know from recent research effects of true over sham acupuncture are likely to be small at best), clinically totally heterogeneous trials is not very informative. In the end, taking the weak evidence of other treatments for tinnitus into account, what would the authors recommend to patients?

Below I make some suggestions for revisions of the current manuscript. For future reviews I would recommend the authors to follow current trends to make their reviews more informative.

Major comments:

• Personally, I would suggest to the authors to delete or revise their conclusion that the evidence in “contradictory”. Consider to include a clinical conclusion in the last paragraph of the main text.

• The authors should consider a table summarizing study findings in some more detail.

Minor essential revisions:

• Study quality (p 8): You write that three trials had adequate randomization. According to table 3 one study had low risk regarding sequence generation and another regarding concealment. Doesn’t that mean that not a single study was
adequate regarding the two randomization aspects?

Discretionary revisions

• Contrary to what the authors say in the PRISMA checklist (item #8) the FULL search strategy is not presented. Consider to provide an appendix giving the full strategy for at least one database (as Korean and Chinese databases were used, too, it might be interesting to see this for some of these databases, too?).

• Selection criteria (p6-7): I found the description somewhat difficult. Consider to follow the usual, more accurate approach – type of studies: trials in which allocation was explicitly randomized …, particularly, I found the wording regarding control groups and control for drug therapy not very clear (maybe it is because I am not a native English speaker)

• Risk of bias (p 7): in the Cochrane tool no “points” are given. As stated above current high quality reviews try to focus on patient-relevant outcomes. With the risk of bias tool one tries to assess whether the effect estimates observed are likely to be unbiased. It is a bit confusing to write that you used four criteria while the table has seven columns. Also, the ITT item is somewhat problematic. First, the use or non-use of an ITT approach does not per se imply a high or low risk of bias (depends on attrition, whether you go for difference or equivalence/non-inferiority etc).

• Discussion first and third paragraph: There are twice statements that small studies are less likely to generate reliable findings/exaggerate treatment effects. In the way it is written (particularly in the second statement) I find it a bit difficult as it mixes statistical (imprecision) and methodological (risk of bias) issues.

Level of interest: An article of importance in its field

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 interests