Reviewer’s report

Title: Acupuncture for ankle sprain: Systematic review and meta-analysis

Version: 2 Date: 6 November 2012

Reviewer: Klaus Linde

Reviewer’s report:

The manuscript reports a systematic review and meta-analysis of 16 (+ 1 without data for meta-analysis) randomized trials from China investigating the effects of acupuncture in patients with ankle sprain. Overall, the review is well conducted (although I have one major problem with the methods – see below) and reported. Yet, it leaves me (not the first time with a review on Chinese trials) a bit at a loss: The quality of the primary studies seems so questionable that I really wonder what can be done with the results. The authors correctly point to most of these problems (primarily lack of blinding when using a highly subjective and problematic outcome measure such as global assessment in a condition like ankle sprain; also quality of randomisation, extremely asymmetric funnel plot). In addition, some of the results are hard to believe: on page 22, last line means 1.32 SD 0.42 vs. 6.55 SD 1.76 are given for a study. If this is transformed in a standardized mean different the effect size is simply huge….

Major compulsory revision

1. While I do not think that this will change the conclusion a lot, I find it problematic that the authors analyze trials in which acupuncture is used as a treatment alternative (e.g. should I treat with acupuncture OR NSAIDs) together with trials where acupuncture is added (should I ADD acupuncture to NSAIDs or not). In my view these are fundamentally different questions which should not be analyzed together (although it does not seem to explain a lot of the heterogeneity). Also, you had several trials with more than two arms which addressed both questions.

Minor comments (discretionary revisions)

1. page 2 line 6 to 7 and page 28 line 7 (“Acupuncture’s benefit was negated/nullified…”): (Although I am so skeptical about the primary studies) I do not think that this comes out of the data. In fact, the point estimate for the RR in adequately randomized studies is very similar to that for all studies. The confidence intervals are wide due to the small number of studies included. I would say “are not significant” or something similar.

2. Page 10, line 8 “considering countries in which acupuncture has been widely used”: I would suggest to delete this as other countries in which acupuncture is widespread (such as France and Germany) would be excluded (but authors from these countries often publish in English). You might argue that you cover relevant languages.
3. Page 10, selection criteria: I am surprised you considered quasi-RCTs.

4. Page 17, selection process, “37 studies did not satisfy acupuncture and control group criteria”: this is somewhat unclear – give at least examples.

5. Page 20, control intervention: describe how many comparisons with acupuncture as add-on and as alternative.

6. Page 25, sensitivity analysis: in the largest study most studies were “responders”; this seems to apply to a number of other studies. This is somewhat tricky. Might it be better to use the OR or the number of non-responders?

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests