Reviewer’s report

Title: Meta-analysis of acupuncture for relieving non-organic dyspeptic symptoms suggestive of gastroparesis in diabetes

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Reviewer: HYANGSOOK LEE

Reviewer’s report:

Gastroparesis is known to affect approximately 40% of patients with type I diabetes and up to 30% of patients with type II diabetes. As symptoms of diabetic gastroparesis (DGP) such as nausea, vomiting, early satiety, bloating, and postprandial fullness can cause poor quality of life, and the substantial proportion of these patients do not respond to the usual treatment options such as dietary modification, prokinetics, and antiemetics, it is welcome to seek other management options for this troubling condition. This manuscript reports a meta-analysis of 14 randomised controlled trials (RCTs) of acupuncture for DGP. The risk of bias in the primary studies is high in important domains and the authors drew a careful conclusion considering this. However, there are some problems which I believe should be clearly answered and revised accordingly.

Major Compulsory Revisions:

1. Although the authors assessed the risk of bias using the Cochrane ROB assessment tool, they combined 8 RCTs without considering the ROB of each study. As shown in the funnel plot, we suspect that the studies reporting negative or neutral outcomes are missing or it is highly likely that the high risk of bias in the included trials exaggerated the effect estimate of acupuncture. Then, the authors should have dealt with this issue e.g., using sensitivity analysis, to determine whether this finding is robust. Only 8 out of 14 RCTs reported how they randomised participants and only one sham-controlled trial (Wang 2008) which was given low risk of bias for randomisation and allocation concealment, apparently did not report how they adequately concealed group assignment. Randomisation by lot is one thing and adequate allocation concealment method is another. Given that inadequate random sequence generation and allocation concealment result in overestimation of treatment effects, the authors should re-analyse the data considering the high risk of bias of the included studies. In addition, the outcome assessors in most studies seem to be participants who were not blinded. The authors gave unclear risk of bias for outcome assessment blinding which should be given a second thought. The outcome measures are mostly patient-reported ones, then risk of bias for outcome blinding would better be given high in medication-controlled trials of acupuncture.

To summarise, the authors should re-analyse the data particularly in the main analysis of response rate in 8 RCTs and revise the manuscript accordingly in consideration of adequately evaluated risk of bias for the included studies.
2. In the Discussion section, it would be better for the readers to suggest clinical and research implications of this review’s findings.

Minor Essential Revisions:
1. There are a number of grammatical errors and misspellings in the text, figures and tables. The manuscript should be shown to a native English speaker to receive language editing service.

2. Regarding inclusion criteria, studies testing a combination of acupuncture and Chinese herbal medicine were excluded. If so, the authors should give a clear reason why Shen 2010 and Wang 2010 were included. The term ‘acupoint application’ needs additional explanation.

3. In statistical analysis, the authors should state pre-specified classification for pooling studies, e.g. according to the control type or outcome measures.

4. The whole ‘risk of bias’ in the results section should be revised as mentioned above.

5. Regarding response rate, did the all studies report the outcome on a 3-point Likert-type scale? I wonder whether there were any studies reporting response rate on a 4-point Likert-type scale. If so, it should be reported how these were dichotomised and analysed.

6. Page 15, lines 322-3: twice daily acupuncture does not seem to be an easily acceptable practice outside China. A more generalisable suggestion would be reasonable. Moreover, it is controversial how long acupuncture’s effect lasts although the authors state 4-6 hours. It would be necessary to provide evidence for this.

7. Page 15, line 317: HAD scale – is it a quality of life measure?

8. Table 1: For reporting acupoints, I suggest WHO standard nomenclature for acupoint be used for the readers. (E.g. RN12 should be CV12)

9. Table 1: For control groups, dosage and regimen of pharmacological medication should be provided (e.g. bid, mg, etc.)

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

**Quality of written English:** Not suitable for publication unless extensively edited

**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.