Reviewer's report

**Title:** Osteopathic Manipulative Treatment for Low Back Pain: A Systematic Review and Meta-Analysis of Randomized Controlled Trials

**Version:** 3  **Date:** 22 June 2005

**Reviewer:** Elmer Villanueva

**Reviewer's report:**

**General**

I thank the Authors for a comprehensive and considered response to the my comments. I agree that the manuscript has been improved both by adopting changes suggested by the Reviewers as well as the elaboration of justificatory statements about contentious issues. This major strength of the open review process is wonderfully demonstrated in the previous (and current exchange).

My comments appear below.

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**Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)**

None.

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**Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)**

I do not accept that the issue of (non) head-to-head comparisons between OMT and NSAIDs has been clarified. The Authors persist in using the Discussion section as an editorial base from which to launch statements that have very little supportive evidence precisely because no head-to-head comparisons have been conducted. To wit, while the authors claim that "... OMT provides an analgesic effect comparable to nonsteroidal anti-inflammatory drugs, including cyclo-oxygenase-2 inhibitors" (Manuscript p13), they cite evidence not from the use of NSAIDs in low back pain but in osteoarthritic knee pain. The use of NSAIDs in lateral elbow pain results in a comparative weighted mean difference in pain of about -1.88 versus placebo (see SE Green's Cochrane Review). It would be prudent to conclude, using their reasoning, that OMT produces only a fraction of the effect of NSAIDs. Where they choose to compare results focusing on back pain, they only comment on the differences in study durations.

My original advice was simple: in the absence of information from a head-to-head information, cease making comparative claims. I trust the Authors to eliminate such statements, for which I do not need to be consulted.

I accept that the Authors may not need to back-transform effect size estimates into their original scales. However, the Authors make an extraordinary claim the implication of which I am unsure whether they fully realise. This relates to their belief that pain measures, at least as used in the source studies, are not "readily interpretable from a clinical perspective" (Response to Reviewers p8). I am certain experts in the arena of pain management will have views about this. My concern, however, is more basic. First, if they claim that these measures are not readily interpretable, why
use them (as they have done in Ref 46) in place of more patient-relevant measures such as the number of days pain free or ability to perform activities of daily living? Second, if these measures are not readily interpretable, then their summary is even more so. Thus, what is the utility of a meta-analysis of outcomes with little clinical meaning?

Discretionary Revisions (which the author can choose to ignore)

What next?: Accept after minor essential revisions

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

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests