Author's response to reviews

Title: Osteopathic manipulative treatment for nonspecific low back pain: A systematic review and meta-analysis

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Author's response to reviews: see over
Author responses (in red) to reviewer reports

We would like to thank the reviewers for their thorough review of our manuscript and their insightful comments and suggestions. The manuscript has been thoroughly revised and copyedited. We believe we have amended the manuscript according to all recommendations and the manuscript is now much improved and clearer.

Title: Osteopathic manipulative treatment for nonspecific low back pain: A systematic review and meta-analysis

Version: 1

Date: 7 April 2014

Reviewer: Simon French

Reviewer’s report:

General comments
This manuscript describes a systematic review of osteopathic manipulative treatment for low back pain. It is well conducted and reported, but I have a few concerns as described below that, if adequately addressed, will hopefully improve the paper.

Major revisions
1. It does not appear that the PRISMA checklist has been used for guidance in reporting this review, eg structured abstract, PICO outcomes, etc. The authors should resubmit a revised version with improved reporting and the checklist completed.

The manuscript has been revised to improve the reporting of methods and outcomes in accordance with the PRISMA checklist.

2. Abstract: outcomes are not mentioned in the Methods section of the abstract, nor is the duration of follow up upon which the conclusion is based.

The abstract has been revised and outcomes and duration are included.

3. Search strategy (Abstract & Methods): the search strategy identified 307 reports, not 31. Thirty one was the full text papers that were retrieved.

This has been corrected.

4. Abstract Conclusion: clarify that the conclusion relates to 3 month follow up (and not other time points).

The abstract has been revised and the follow-up period has been included in the Conclusion.

5. Background: needs some data reported here on the percentage of people with low back pain who visit an osteopath (eg see Walker BF et al. J Manipulative Physiol Ther 2004;27(5):327-35)
The Background has been amended as requested to provide some indication of percentages of LBP people seeing osteopaths. The suggested paper has been included.

6. Background: need a section on LBP in pregnancy with prevalence, etc. And some justification for why this should be included in the category of “non-specific LBP” (see below for more about this).

The Background has been amended to include a paragraph on LBP in pregnancy and postpartum. The reasons for inclusion are justified in the inclusion criteria of the Methods section.

Background: needs some more information on OMT. What treatment components does this typically consist of? Perhaps some history of the development of OMT would also be helpful.

More information on OMT has been included in the Background.

7. Methods: Inclusion and Exclusion criteria more information needed:

a. To make this clearer, this section should be described in terms of PICOS (see PRISMA for suggestions), perhaps with the use of sub-headings, as per Cochrane systematic review reporting.

This section has been restructured with subheadings, as recommended.

b. more description needed about what constituted “authentic OMT intervention”, ‘Authentic’ OMT intervention has been more clearly explained.

e.g. was this based on training of the practitioners, whether the authors of the RCT identified the practitioners as osteopaths, whether they were registered health professionals, etc

This has been more clearly explained in the revision: practitioners were required to be identified as osteopaths or osteopathic physicians.

c. and when you say “authentic” do you mean “pragmatic”, in that practitioners could choose from a range of treatments under the broad category of OMT?

Yes, and this has been more clearly explained in the revised text.

d. In some of the back pain literature, back pain in pregnancy is not classified as non-specific LBP, eg some LBP RCTs specifically exclude women who are pregnant because this may be seen as a different condition, and in the Rubinstein Cochrane systematic review (see citation below) this category of low back pain was excluded. The authors of this review include this under the category of non-specific low back pain. I would suggest the authors provide justification for this, or else change the title, objective, etc of the review to remove the term “non-specific” to reflect that this category of low back pain was included in this review.

The inclusion of pregnant and postpartum women has been more clearly justified in the Methods. This condition can be considered as nonspecific LBP because no specific pathology is typically present. It should be noted that these studies were not included in the general analysis but were analysed separately from the studies that did not include these types of subjects; the conclusions are also specific for these two groups.
e. Justification is needed for why only RCTs of >10 participants were included. If studies meet all inclusion criteria and provide relevant outcome data the sample size should not be relevant. The study that was excluded based on this criterion (Kirk et al 2005) could possibly have been excluded because the purpose of the study was to determine feasibility, not efficacy.

This criterion has been removed.

9. Search: full search strategy including MeSH terms used should be included as an additional file, as per PRISMA

This has been done and is presented in Table 1.

10. Risk of bias: the authors’ determination of a study being at “low risk of bias” is unclear. They state “In line with recommendations from Cochrane Collaborations…” however no reference is provided here. Do you mean based on the Cochrane Back Review Group recommendations (Furlan 2009), or on the Cochrane Handbook? (Note that these two sources provide different recommendations). I would suggest the authors provide more detail here, for example that which is reported in Rubinstein et al Cochrane Database of Systematic Reviews; 2011, Issue 2. Art. No.: CD008112

This has been clarified to indicate the criteria from Furlan 2009.

11. Methods line 174 & 175. Definitions needed here (with references) for acute and chronic.

This has been reworded to define chronic duration and the durations of each study listed in Table 2.

12. No information is provided in the Methods about how heterogeneity of included studies was determined. Please provide. Also, results of heterogeneity analysis are provided in the forest plots, but this is not discussed in appropriate detail in the results.

This has been done.

13. Results: in the Risk of Bias section, it would be helpful to have more description of the different biases of the studies, eg how many had no allocation concealment described, blinding, etc?

This information has been included in Table 4

14. Line 215: sentence commencing “According to the recommendations…” should be moved to Methods

This has been done.

15. Results, section “Effect of Interventions”. It would be helpful when reporting the results that a comment on the Risk of Bias of the studies discussed is included, to give the reader a sense of the strength of the findings. See how Rubenstein et al did this in their Cochrane Review (cited above)

Table 4 gives detailed information about the Risk of Bias assessment. In the Results, we have described two important aspects of the Risk of Bias. This is further discussed in the Discussion section.
16. It is a shame the data from the largest included RCT could not be included in the meta-analysis, especially considering the study was done so recently. The alternate method used by the authors (best and worst) seems to give some strange results. For example, for pain the best and worst results are the same. I am not familiar with this method, but not sure how useful it is when the results for best and worst are the same? Please clarify. Also, the methods description this should be in the Methods, not the Results section – and more detail is needed so reader can understand how this was done. We have been able to include the data from this study into our review and analysis and have described the statistical procedure that allowed us to include this data.

17. Also, in the relevant Forest plots I would suggest removing the Licciardone 2013 study where the results are entered as 0. Confusing when you look at Forest plots without text explaining this. We have included this study.

18. Discussion: how do the results of this review compare to results where other professional disciplines have been evaluated for the treatment of LBP, eg physical therapy or chiropractic? Minor compulsory revisions. We have added a paragraph that discusses the results of other reviews of manual therapy for LBP, such as spinal manipulation and chiropractic.

19. A few typographical errors throughout, eg line 103 term “language” missing, line 244 & line 291 “data was…”. Needs a thorough proof read and correction before resubmission. The manuscript has been thoroughly proofread.

20. Line 171/172 sentence commencing “If different time interval…” is a duplication of earlier information. This duplication has been removed.

21. Competing Interests. At least two of the authors have osteopathic backgrounds, so I would suggest that this is declared in this section. This has been done.

Reviewer 2: Rafael Pinto

Reviewer’s report:

This is a systematic review investigating the efficacy of osteopathic manipulative treatment (OMT) for non-specific low back pain. The authors concluded that the effects of OMT for reducing pain and disability are clinically relevant for chronic nonspecific LBP and for LBP in pregnant and postpartum women. The authors should be congratulated on the rigorous methods used in the meta-analyses. Having said that, I believe that there some part of the reviews need to be clearer. Also, I would suggest the use of GRADE to assess the overall quality of the evidence which is likely to influence the conclusion of this review.

The manuscript has been revised to improve the reporting of methods and outcomes in accordance with the PRISMA checklist. GRADE assessment has been used to assess the
quality of the outcomes.

Minor Essential Revisions

Introduction

Page 4: Line 92-93. Authors need to define what Osteopathic Manipulative Treatment (OMT) is at this stage. Some readers will not be familiar with this terminology. I notice that more information is given in the methods/discussion but the definition should be stated upfront as well.

More information on OMT has been included in the Background.

Page 5: line 106. The objective provided does not follow the PRISMA statement. Authors mentioned the intervention, participants and outcomes of interest. Please also provide information about the other components of the PICOS (that is, the comparisons and study design).

The manuscript has been revised to improve the reporting of methods and outcomes in accordance with the PRISMA checklist.

Methods

Page 5: line 116. This sentence is not necessary unless the protocol of this review was registered in a publicly available register (e.g., Prospero).

This sentence has been deleted.

Page 5: line 124. The term clinical judgment in this sentence is quite broad. Please explain the rationale for using OMT.

The use of ‘authentic’ OMT and clinical judgement are now better explained in the Methods and discussed in the Discussion.

Page 6: line 129-130. Also, give the rationale for excluding single manual technique. This should be in line with the definition of OMT to be added in the introduction.

OMT is now better explained in the Background and the rationale for ‘authentic’ OMT intervention and why single techniques are excluded is clearer. Treatment is characterised by a holistic approach to the patient, and OMT may be applied to many regions and tissues of the body, sometimes remote from the symptomatic area and at the clinical judgement of the practitioner [20-22]. Single techniques or protocols do not allow for a holistic approach or clinical judgement.

Page 6: line 6. The list of search term provided is not enough for readers to assess the comprehensiveness and completeness of the search performed. Authors should adhere to the PRISMA statement and provide a full electronic search strategy for at least one database, including any limits used, such that it could be repeated.

This has been included in Tables 1 & 2.