Reviewer's report

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

Version: 1 Date: 15 April 2014

Reviewer: 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.

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.

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).

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).

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

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.

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.

Results

Page 8: line 186 (FIGURE 1 – Flowchart). According to the last paragraph of the introduction, the extensive search without language restriction is one of the strength of this review. So, I would be interested to see in the flowchart the break down data for each database (number of records found per database: CENTRAL, MEDLINE, Embase, CINAHL, PEDro, OSTMED.DR, and OSTEOPATHIC WEBRESEARCH).

Also, in the method section the authors mentioned that ongoing trials were also screened (Page 6 – line 138). However, there is no information in the results about the number of registered trials found.

Page 9, 10 and 11: “Effect of Intervention”. In this section, the authors mentioned several times that the results were clinically relevant. According to the definition used in the methods (page 7: line 177-180), most of the comparisons showed significant but “small” clinically relevant effects. Authors need to include the magnitude of clinically relevant effects in all pooled effects.

Page 11: Line 266-272. There is no information in the methods that Adverse Events data would be extracted.

Discussion

Page 10: line 245. Please include a brief explanation of the method recommended by Hozo et al in the method section.

Page 13: line 314. It is unclear to me what the authors considered ‘grey literature’ (page 5 – line 109). According to the Cochrane Glossary, ‘grey literature’ means the kind of material that is not published in easily accessible journals or databases. It includes things like conference proceedings that include the abstracts of the research presented at conferences, unpublished theses, and so on. If the authors have searched this type of literature, I would be inclined to see in the flowchart the numbers of records retrieve from this source.

Page 13: line 323-333. Although I understand that the definition of clinical relevance was provided in the methods, I’m not totally convinced that the results found can be considered clinically meaningful. Although Cochrane advocates for the use of mean difference and standard mean difference when interpreting clinical relevance, the 95% confidence interval were quite imprecise for most of the comparisons and the lower bound for most of them fell into the category of small clinical effects. In my opinion, the use of the GRADE approach to assess the quality of evidence would contribute to a better interpretation of the results found in the meta-analyses of this review. For instance, despite of the significant and clinically relevant effects found in this review one question that remains is how confident the authors are with the results of the meta-analyses. The GRADE approach will help authors to clarify this point.

The authors noticed that there is a great source of heterogeneity (higher I2) but
failed to evaluate the ramifications of this issue. In the GRADE approach, heterogeneity is related inconsistency and sample size is related to precision. If GRADE approach is to be used in this review, it will probably reveal that the results found in the meta-analysis are based on low or very-low quality of evidence.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

none