Reviewer's report

**Title:** Tendon lengthening and fascia release for healing and preventing diabetic foot ulcers: a systematic review and meta-analysis

**Version:** 1  **Date:** 5 March 2015

**Reviewer:** Trevor D Prior

**Reviewer's report:**

This is an important subject and the authors have identified a need to review the evidence for this type of intervention.

In my opinion, there are major concerns with the range of papers used for this analysis and thus the conclusions drawn. Generally speaking, this type of review would focus on high-quality research, generally RCTs/prospective cohort studies.

Furthermore, the authors are trying to identify outcomes in terms of specific procedural interventions; namely gastrocnemius recession, Achilles tendon lengthening and selective plantar fascia release.

**Major Compulsory revisions**

1. There were only 2 RCTs identified, one of which had sub optimum power and only one prospective cohort study which unfortunately included a wider range of procedures. The variability in the studies raises significant questions as to the value of the results.

   For instance, in outcomes, time to healing of the ulcer, page 10, paragraph 2, the authors note a range of papers that could not be included in the meta-analysis due to different study designs and they produce a range of results. One of the studies included subjects without diabetes. It is difficult to see how this range of results provides any particular guidance / have any value.

   Perhaps of more concern is the pooling of the rates for ulcers healed and the rate of ulcer recurrence. In the latter, the pooled rate of recurrence was 11%. However, in the RCT with adequate power the rate was 38.5% and 20% in the other RCT. In my opinion, the 11% value is misleading due to the nature of the papers included.

2. I accept that there is limited information available but do not feel all of the papers selected can be included. At the very least, I would recommend the following considerations:

   a. The RCT results are reported separately to the other studies. However, in the paper by Allam (2006) the intervention group had either a gastrocnemius recession or Achilles tendon lengthening. The authors should describe how the decision was made as to which procedure was chosen, whether the rates actually varied between the procedure (if reported although note if not reported)
and certainly consider if the complications were different as the Achilles tendon lengthening would provide a theoretically higher risk of rupture for example. The numbers of complications in the summary indicate these are small but they may all occur with one procedure.

b. The papers by Laborde (2009) and La Fontaine et al (2008) could be reported separately with the weaknesses in the study design acknowledged but they do cover specific procedures and therefore give an indication towards outcomes.

c. Similarly, the paper by Kim et al (2012) could be reported separately. This is the only paper to include selective plantar fascia release and is at least prospective. This highlights a potential option for management but would be qualified by the need for further research.

d. All of the remaining papers (26, 27, 28, 29, 30, 33) should be excluded as they either score very low on the quality assessment or include multiple procedures. The authors should justify why they would include papers with a low quality assessment given the other limitations.

4. In the discussion (page 12 last paragraph) the authors state that the rate of healing for the procedures is comparable to current evidence on total contact casts. However, they reference papers that have not been included in the selected papers /results reported and suggest these papers were used for comparison. Were these included in the statistical analysis? At the very least, they should document the papers that have been used, evaluate the quality of the papers and incorporate the appropriate results. However, given the comments above, the RCTs provide their own control and analysis and thus it would only be relevant for the other 3 papers. One option would be to compare the results reported from the papers on total contact cast alone (assuming appropriate quality) against the results for the total contact control groups in the RCTs. They could then report whether these are comparable and this data can be used for the remaining 3 studies.

Minor essential

5. Quality assessment, page 7

The authors state that they modified the scoring for item 27 relating to study power. Could they clarify whether or not this changed the possible overall score for this item and thus the total achievable school? I assume not as additional file 3 indicates a maximum score of 32 which is the same total score. As a result, I feel the authors should qualify the difference in the modification and the reasoning.

6. Outcomes

When the authors describe the outcomes and provide the references, it is difficult for the reader to cross-reference against the tables. In the tables the first column lists authors/year publication but not the reference number. I would recommend that the reference number is provided in the tables after the authors to facilitate quick reference between the text and the tables.
Level of interest: An article of importance in its field

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