Author's response to reviews

Title: Physical therapies for Achilles tendinopathy: systematic review and meta-analysis

Authors:

  Samuel P Sussmilch-Leitch (samleitch@netspace.net.au)
  Natalie J Collins (n.collins@unimelb.edu.au)
  Andrea E Bialocerkowski (A.Bialocerkowski@uws.edu.au)
  Stuart J Warden (stwarden@iupui.edu)
  Kay M Crossley (k.crossley@uq.edu.au)

Version: 2 Date: 30 March 2012

Author's response to reviews: see over
Response to Reviewers' Comments

We thank the Associate Editor and reviewers for their helpful comments on our paper. Below we provide a point-by-point response to the concerns raised by each reviewer. Reviewer comments are displayed in bold, and our responses given immediately below. Within the manuscript, changes have been highlighted in yellow.

**Reviewer 1:**

1. **The abstract and perhaps title, need to be more explicit that insertional and mid portion AT were studied.**

   The studies included in the systematic review utilised various cohorts with respect to the site of their Achilles tendinopathy (AT). While the majority of studies utilised midportion AT, some used a mixed cohort, one was isolated to insertional tendinopathy, and a few did not report the location of the tendinopathy. As such, we feel that it is misleading to define the location of tendinopathy in the title.

   A sentence has been modified in the results section of the abstract stating that the majority of studies utilised participants with midportion AT:

   “Four studies were excluded due to high risk of bias, leaving 19 studies, the majority of which evaluated midportion AT”

2. **Although this paper is a systematic review, please use the structured abstract format rather than the unstructured, narrative review abstract style.**

   The abstract has been edited to comply with the structured format.

3. **I agree that meta-analysis supports laser therapy, individual studies support eccentric exercise and one study supports microcurrent therapy. However, I believe more caution needs to be expressed when recommending shock wave therapy as this intervention was not effective when compared against a sham. In studies where it was effective the comparator group received eccentric exercise or a wait and see approach. Factors such as ‘resentful demoralisation’ or placebo may be responsible in these studies?**

   The conclusion section of the abstract has been amended to address this, and reflects updated findings.

   In addition, a sentence has been included in the fifth paragraph of the discussion:

   “Overall findings regarding SWT need to be interpreted with caution. Although SWT appears more efficacious than no treatment [1], findings of two studies suggest that SWT is no more effective than a sham intervention [2, 3], suggesting a placebo effect associated with pain and function outcomes.”

4. **The initial paragraph of the introduction may be improved if there is elaboration that AT can be classified as insertional and midportion.**

   This has been amended as suggested:

   “Symptoms can occur at the midportion or insertion of the tendon….”
5. Are pharmacological interventions (e.g. NSAIDs, topical rubefacients) used only when physical therapy interventions fail? This needs to be rewritten.

This has been clarified in the second paragraph of the background:
“Conservative or physical therapies are generally accepted as the first line approach for managing AT..and can be used in isolation or in conjunction with pharmacological and injectable agents.”

6. For the reader’s benefit, can the author’s, elaborate in more detail on the types of physical therapies that have been reported for AT rather than stating ‘...to name a few...’. Perhaps the interventions used as subtitles in the results could be included here.

This has been amended as suggested:
“Physical therapies for AT include exercise, electrotherapeutic modalities, soft tissue therapies, braces and splints.”

7. It is acknowledged by the authors that insertional and midportion AT may represent distinct clinical studies (and is also acknowledged as a limitation). Is it valid to pool analyses for an intervention that may be reported for these two conditions? Would a valid approach be for the results to be presented for midportion and insertional AT separately and studies that not specify the location of symptoms excluded?

As stated above, the current RCT literature does not permit grouping of study findings based on the location of AT. It is currently not clear whether the treatment pathways for insertional symptoms differ from midportion. Importantly, examination of study SMDs for Rompe 2007 and Rompe 2008 showed similar effect sizes despite one investigating SWT for midportion AT and the other investigating SWT for insertional AT. As such, we deemed that pooling of data for these two studies was valid. We included both types in this systematic review, and highlighted the different locations as something to be considered when interpreting findings (discussion):

“While we did not restrict inclusion based on AT location, the majority of included studies that reported AT location utilised midportion tendinopathies only, followed by mixed midportion and insertional cohorts...”

8. Quality Assessment: The PEDro criteria were used to assess study quality. Is the first item normally considered in the cumulative score? Can the authors provide more elaboration on the score given for each criterion, as well as the total possible score?

A modified version of the PEDro scale was used, which has been utilised in previous studies ([8-10]). The first item is generally not included in the total score when the standard scale is used, as this relates to external validity while criteria 2-11 relate to internal validity. However, with the addition of other methodological quality items, we considered it important to include item 1 in the overall score. This has been clarified in the method section, and the modified PEDro checklist attached (Additional File 1).

In the methods section of the revised manuscript:
“One point was awarded for each criterion that was clearly satisfied according to prespecified guidelines, and the 14 items summed to give a total methodological quality score out of 14.”
9. **Data extraction and analysis:** There is some inconsistency in the number of decimal points (i.e., 0 should be 0.0). The authors used a fixed-effects analysis in their meta-analysis. Can they please elaborate/justify this approach and was any statistical approach (such as I^2 statistic) used to determine statistical heterogeneity amongst studies included in the analyses?

The decimal points have been edited as suggested to ensure consistency.

Upon revisiting the meta-analysis methods used, the I^2 statistic identified statistical heterogeneity for the majority of pooled studies (> 50%). As such, the methodology has been amended and meta-analyses re-run using a random effects analysis, and results, discussion and conclusion sections amended to reflect this.

In the methods section of the revised manuscript:

“Where studies had sufficient homogeneity in participant characteristics, interventions, outcome measures and follow up times, a meta-analysis of outcome data was performed. Meta-analyses were conducted using a random effects model, as all pooled studies had a heterogeneity greater than 50%, which was determined using an I^2 statistical assessment of inconsistency [11].”

As changes in outcomes were seen when random effects models were used, the discussion and conclusions sections of the manuscript have been updated to reflect this.

10. **The authors state that if insufficient data were presented then the corresponding author of the study was calculated. Did any authors respond to the emails? Did any studies present dichotomous data for pain and function that were excluded from the analysis and could this be analysed/presented? How did the authors deal with any studies that may present multiple outcomes for pain and function?**

Three authors replied to correspondence. This is has been addressed in the outcome measures section of the Results section:

“Additional data was requested for six of the 19 studies, with three authors replying to correspondence and two providing sufficient data for further evaluation.”

As stated in the methods section (eligibility criteria), studies that evaluated outcome on measures of pain and/or function were included. None of the included studies presented dichotomous data for pain or function that was excluded from analysis.

Where studies used multiple outcomes for pain and function, measures with known validity and reliability were considered to provide a higher level of evidence.

11. **It is unclear why the individual quality score of the Silbernagel et al. paper is included in the text, but not for the other included studies. Please review this. Please review the in-text referencing on line 4 of paragraph 2 (page 12). Please capitalise “F” in Figure (i.e. figure 2) (and review entire manuscript). Page 13, paragraph 3: should ‘or’ be ‘versus/compared to’? Page 15: When referring to the ‘two studies evaluating SWT...’ can the authors include references.**

The individual quality score for Silbernagel et al has been removed from the text.

The in-text referencing for line 4 has been corrected.

Capitalisation for “Figure” has been amended throughout the manuscript.

Page 13, paragraph 3 has been changed from ‘or’ to ‘compared to’ as suggested (now page 14, paragraph 1).

The passage on page 15 has been edited and is now referenced appropriately.
12. As stated previously, I believe more caution needs to be expressed when recommending shock wave therapy as an effective intervention (in paragraph 1) as this intervention was not effective when compared against a sham. In studies where it was effective, the comparator group received eccentric exercise or a wait and see approach. These two groups were not treated the same. Factors such as ‘resentful demoralisation’ or placebo may be responsible in these studies? The studies of laser did at least use a sham/placebo. Please also apply this to the Conclusions section.

The conclusions for SWT have been altered to account for new effect size calculations (see our response to point #3 above).

13. Page 21: Can the authors include foot orthoses as another recommended intervention for AT that need investigation.

This section has been edited to include foot orthoses as an intervention for further future investigation.


Amended as suggested.

15. The manuscript has not been presented using the journal template. The template can be found at: http://biomedcentral.com/download/templates/BMC154d.dot.

The formatting has been altered to comply with this guideline.

16. JFAR only accepts two hierarchies of headings/subheadings. Please ensure that the font sizes clearly indicate the level of heading.

The format of headings/subheadings has been altered to comply with the requirements.

17. Please correct the formatting of the reference list (see: http://www.jfootankleres.com/authors/instructions/research#formatting-references)

The referencing style has been edited to comply with the requirements.

18. There are some areas with errors in spacing between words/references.

This has been addressed throughout the manuscript.

19. Please review manuscript and replace ‘et al’ with ‘et al.’.

This has been amended throughout the manuscript.

20. Rather than referring to Appendix 1 and 2, the authors should refer to ‘Additional file 1’ etc.

The titles of additional files have been altered as required.

21. Remove the word 'review' on page 5.
22. Please crop all excess white space from the figures.

This has been amended

23. Figure 2: There is an asterisk next to the word ‘loading’ but I am unsure if it is meant to be there?

The figure legend has been amended to explain the meaning of the asterisk.

24. Forest Plots (Figures 2 to 4): Can the authors add a caption at the top of each figure showing the direction of effect (e.g. ‘Favours intervention of interest’ against ‘Favours alternative intervention’)?

This has been added as suggested

Is it possible to add a reference [number] beside each author’s name so that the reader can cross-match to the reference list number?

This has been amended

Appendix 1: Is the ‘3’ beside ‘orth*’ a typographical error?

This error has been amended

Appendix 2: Should ‘inter-reliability’ be ‘inter-rater reliability’?

This has been amended

25. Table 1: The PDF version that I have access to doesn’t show any highlighting of cells for which the item for each study has been satisfied. In addition, can the authors check that the table confirms with the journal guidelines (i.e. no vertical lines or shading)? Can the word ‘papers’ be changed to ‘studies’ within the title?

Table 1 has been edited to comply with the journal guidelines and ‘papers’ has been changed to ‘studies’.

26. The issue of the histopathological nature of overuse tendon conditions should be better characterised. Though the hypotheses put forward by Cook et al seem to indicate that the end stage pathology may well be degeneration, the main pathological appearance is of a failed healing response (see Arthroscopy 1998; 14(8):840-3 and 2009;37(9):1855-67). Also, the issue of inflammation as a primum movens of the tendinopathy process has been put forward by Murrell, expanded by Cook et al, and formalised in other recent work (Int J Immunopathol Pharmacol.2011;24(1 Suppl 2):45-50; Sports Med Arthrosc. 2011;19(3):213-7). The authors should briefly expand on the above issue, and include the appropriate references.

The Background has been edited to better describe the pathology with appropriate references:
“Symptoms can occur at the midportion or insertion of the tendon, with the underlying pathology reflecting a failed healing response [12, 13], where both inflammatory and degenerative pathologies exist.”

27. Mention should be made that, in studies performed out of Scandinavia (see the RCTs by Rompe et al), the rate of success of eccentric exercises is nowhere near as high at those performed in that part of the world. This is confirmed by other longitudinal studies which, given the exclusion criteria adopted by the authors, were not included in the manuscript.

Only two of the nine studies evaluating eccentric exercise that fit the eligibility criteria for this systematic review were conducted in Scandinavia. Neither study provided sufficient data for effect sizes to be estimated and only one study reported between-group comparisons. As such, Scandinavian studies represent only a small contribution to the overall conclusions. We believe that caution should be exercised when comparing the non-RCT designs of longitudinal studies with the RCT studies performed by Rompe and colleagues, as their differences in study design may impact on their methodological quality. Differences in exercise protocols and monitoring of participant compliance are discussed in the Discussion section of the manuscript as factors that may contribute to the differences in findings across studies evaluating eccentric exercise.