Reviewer’s report

Title: Reliability of a two-probe ultrasound imaging procedure to measure strain in the Achilles tendon

Version: 0 Date: 07 Jun 2019

Reviewer: Lisa Newcombe

Reviewer's report:

Thank you to the authors and the editorial team for the opportunity to review this interesting article, which provides an interesting, technical and novel contribution to the field. I enjoyed reading this paper. Overall, the article is very well written and many of the decisions surrounding the methods are justified. There are some clear areas where improvements can be made with some minor amendments, mostly around points of clarity, prior to being suitable for acceptance for publication. I have provided my point-by-point responses below to assist with the review and revision processes.

1. Is the question posed by the authors new and well defined?

Yes. A clear aim is provided for the reader to understand the novel contribution of this work.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?

The methods are appropriate, and in most part very detailed with support from the literature, but would benefit from further refinement/clarity around methods of ultrasound image acquisition. This includes details of the sonographer/researcher undertaking the scans, standardised B-mode settings between scans and further clarity around the probe positioning and measurements for Achilles tendon elongation between ankle joint positions.

3. Are the data sound and well controlled?

The data appears sound and well controlled.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
Yes, but there could be more information in the figure legends for the ultrasound images and regions of interest identified (figure 5). The percentages reported for tendon strain could perhaps be explained next to normal/reference values, if available, to show clinical significance of the level of strain reported.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

The discussion is well-written and provides an interesting contribution to the field. The discussion appropriately discusses limitations of the study, particularly around the hand held positioning of the probe at the Achilles tendon. There are some points which could be explained in more detail, such as decisions to scan in longitudinal planes versus transverse.

6. Do the title and abstract accurately convey what has been found?

Yes, the title and abstract both convey the nature of the study and findings. The only comment I would make is that from the title and discussions of tendon stiffness in the abstract, I wondered if the study might have included strain elastography in measuring tendon stiffness & elasticity. I wondered if this approach had been considered as not mentioned in the introduction or discussion, although I understand not of direct relevance to measurements of tendon excursion.

7. Is the writing acceptable?

Yes, the writing standard is to a high standard.

Point by Point Minor Revisions:

* I think that this is an interesting study with valuable information for the field in testing the reliability of this technical technique. However, I think that this paper would benefit from some further detail and clarity around the significance of the probe positioning in terms of the measurements taken for measuring differences in tendon length and what this means for the regions of interest captured and overall measurement of tendon excursion. I understand that the software motion analysis captures the tendon excursion using the ROIs, but I am not too sure of the significance of the measurements of the probe on the skin or the regions of interest shown in Figure 5. The methods section (ultrasound image acquisition) may benefit from some more explanation of the importance of probe positioning in relation to the different regions of interest on ultrasound (figure 5) and measurements of length taken between ankle positions to measure tendon elongation.
* Methods (ultrasound image acquisition): In terms of the probe positioning overall, I apologise if I am misunderstanding the methodologies discussed in the paper, but I am unclear how the same level of calcaneal bone in the right side of the screen at the Achilles tendon insertion could be standardised between scans (in each ankle joint position). Could this have perhaps been measured in terms of tendon distance from calcaneal bone? Was the probe held consistently on the line that was drawn at the distal aspect of the probe, or was this able to move as per tendon elongation and a second line was drawn? I am unclear if the probes were held in situ as the ankle moved through range of motion in the biodex and motion was captured for use in motion analysis software? I wonder if some further detail could be included to clarify, following on from information presented at line 153?

* Methods (ultrasound image acquisition) (144-147): Clarity was provided for placement of the first probe at the medial aspect of the myotendinosus junction, but was the Achilles insertion scanned in the most central portion of the tendon (rather than more medial/lateral?).

* Methods (line 167): Details are provided in the expertise of the second rater (RE) in terms of expertise with the motion analysis software, but no details are provided for the first rater or expertise of the researcher placing this probe on the Achilles tendon. Does this rater have a qualification/training in ultrasound? Does the second rater have this training also?

* Methods: Sufficient detail is provided for the ultrasound machine and transducer utilised between scans, as well as images and timing of cineloops, but there could be more detail provided in the standardisation of B-mode settings/pre-settings between scans, including depth, focal zones, gain, frequency between scans.

* Results. Table 1: To be clear, are these measurements between trials taken from a mean of both raters or first rater?

* Discussion (lines 323-324): There is a statement that Achilles tendon measurements were undertaken in longitudinal planes rather than in transverse, contradictory to another paper. It is stated that this is because the transverse planes may influence Achilles tendon strain measurement but there is no explanation of why this might be the case, which could be included.

* Discussion: I agree that it is important to acknowledge limitations of the hand held probe versus the probe held in the device, as movement could occur in terms of probe positioning at the Achilles insertion. The authors have acknowledged this appropriately.
Discretionary Revisions

* Out of interest, in the exclusion criteria, were there any ultrasound features in this Achilles tendon that caused participants to be excluded (i.e. subclinical inflammatory changes/tendinopathy, calcifications)?

* Discussion (limitations) (lines 370-371) - should these sentences follow on from one another?

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