Author's response to reviews

Title: The role of tibialis posterior fatigue on foot kinematics during walking

Authors:

Michael B Pohl (mbpohl@ucalgary.ca)
Melissa Rabbito (mrabbito@ucalgary.ca)
Reed Ferber (rferber@ucalgary.ca)

Version: 2 Date: 26 March 2010

Author's response to reviews: see over
March 26, 2010

Dear Sir/ Madam,

Based on the recent reviewer comments concerning our manuscript, “the role of tibialis posterior on foot kinematics during walking” we would like to submit a revised version for publication within the Journal of Foot and Ankle Research. We would like to thank the reviewers for some excellent questions and providing us with a thought provoking review. We have attempted to address as many of the comments/suggestions as possible and believe this has greatly improved the paper. Once again, each of the authors was fully involved with the study and concurs with the content in the final manuscript. We look forward to hearing back from you shortly.

Sincerely,

Michael Pohl, Melissa Rabitto, Reed Ferber.

Running Injury Clinic
KNB135
Faculty of Kinesiology - University of Calgary
2500 University Drive NW
Calgary, AB  T2N 1N4
403 210-7091
mbpohl@ucalgary.ca
Response to Reviewer #1 Comments

Discretionary Revisions:

“It is possible that the calcaneus is not the best bone to monitor to see if fatigue of the PT alters motion. Since the PT has direct anatomical attachment to the navicular bone, perhaps this would be a better bone to measure. However, looking at forefoot transverse and sagittal plane motion could be argued that they provide an indication of what the midfoot is doing. Perhaps you could include a brief discussion of how measurement of the forefoot is reflective of the midfoot and that fatigue of the PT still did not alter its motion.”

While it is possible that the calcaneus may not be the best bone to study, patients with PTTD typically display excessive rearfoot eversion during gait. However, given that PTTD also display altered forefoot kinematics we decided to adopt a multi-segment foot model approach to increase our chances of detecting kinematic changes following fatigue of the tibialis posterior. We agree with the reviewer that forefoot transverse and sagittal plane motion would provide an indication of the behaviour of the midfoot. Indeed, there is some literature to show that the medial arch angle is well represented by sagittal plane forefoot motion. Moreover, we expected that there would have been a greater chance of detecting kinematic changes between the forefoot and rearfoot (multiple joints) as opposed to just the navicular (single joint).
Response to Reviewer #2

MAJOR COMPULSORY REVISIONS

1. Please clarify which phase/s of the gait were assessed in the kinematic analyses. Were participants gait data simply analysed wherever peak values occurred for each parameter, or did the authors focus on specific phases of the gait cycle.

   We apologise that this was not clearer in the methods and have added some text to the methods to clarify (page 8, line 22). The peak values analysed were defined as the peak value during the entire stance phase. Some multi-segment foot papers have selected to analyse kinematic variables in specific sub-phases of stance, the idea being to tease out subtle differences that might be missed when simply looking at one peak value. However, inspection of the kinematic curves in Figure 3 should reveal that no differences were evident in any of the subphases of the stance phase. Moreover, the timing to the peak value and the standard deviation of this measure did not change between pre and post trials. This indicated that the peak value was not achieved at substantially different times in the stance phase when comparing the pre-post value. We have added some text to the discussion section to draw the readers’ attention to the additional information provided by the kinematic curves (page 11, lines 12-14).

2. The authors should include 95% confidence intervals and effect size calculations (i.e. Cohen’s d or standardised mean difference) for the data. Even though few significant findings were detected in this study, these statistics are routinely reported for clinical and biomechanical research.

   We have included the mean difference between the pre-post measurements in addition to the 95% confidence intervals and Cohen’s d for all the variables in interest. These can be found in Table 1.

SPECIFIC COMMENTS:

Methods section:


   We agree that this sentence was a little confusing and have attempted to clarify accordingly. (page 7, lines 5-6).

4. Page 8. Line 1 to 6. The MVC procedure is confusing and it would be very difficult for the reader to repeat this protocol. It would be ideal if the authors could include a diagram illustrating how the MVC were undertaken with specific reference to the sequence of rest periods, sets, fatigue thresholds, recovery periods.

   We think this was a good idea and thank the reviewer for this suggestion. We have added a new figure to describe the sequence of events surrounding the fatigue protocol (Figure 2).

5. Page 7. One of the key issues with this study is whether the participants were sufficiently fatigued, particularly as more than ¼ of the participants were not fatigued beyond the pre-specified threshold. While it is made clear in the methods that previous work by Kulig et al have demonstrated this apparatus selectively activate tibialis posterior, there is no mention of its reliability. In addition, has the fatigue protocol been published elsewhere? If so, can the authors indicate whether it is reliable/valid?

   While it is true that 8 subjects did not experience a 30% decrease in terms of force output, they were unable to complete two consecutive sets, which was also specified as criteria for stopping the protocol (page 8, line 5). Moreover, these 8 subjects did experience at least a 21% drop of force output. However, we do acknowledge that there was no mention of the reliability of the device developed by Kulig et al, since this was not quantified in the literature. We had already mentioned in the final paragraph of the discussion that a limitation of the study was the inability to quantify the level of tibialis posterior fatigue (validate). Therefore, we have expanded this section to acknowledge that
there is no literature to indicate the reliability of the device in activating tibialis posterior (page 14, line 17-18).

With respect to the fatigue protocol, this has not been published previously but the threshold was selected based on Christina et al. While the protocol did not cause a 30% decrement in force output for all subjects, it was successful in fatiguing the 8 subjects to a level where they were unable to complete more sets. We have acknowledged that the lack of EMG data made it difficult to directly quantify the degree of fatigue that was achieved in the tibialis posterior (page 14, lines 18-20). We have also added an extra line highlighting that future work is needed to validate how successful the protocol was at selectively fatiguing tibialis posterior (page 14, lines 20-21).

6. Page 8. Line 6. Please state what the average time was between the fatigue exercises and the gait trials and or the static rearfoot measurements.

Upon completion of the fatigue exercises all subjects mounted the moving treadmill within 15 seconds. We have added text to indicate this (page 7, line 9).

7. Page 9. Line 4 to 6. Some words are missing and the sentence does not make sense. ‘….a pearson product moment correlation was performed’? Please revise the whole sentence.

We apologise for this disjointed sentence and have amended it (page 9, lines 6-7).

Results section:

8. Page 9. Line 19. ‘Eight subjects did not drop below….’. Please revise this sentence

We have revised this sentence and hope that it now reads much clearer (page 9, lines 20-21).

9. Page 9. Line 21 and 23. ‘strength’ versus ‘fatigue’ versus ‘force output’. Is ‘strength’ the correct term here? The participants ‘strength’ technically did not increase or decrease, rather their ‘force output’ had decreased with fatigue; or conversely that had recovered, were ‘less fatigued’ and had a greater ‘force’

This is a good point and we agree that using consistent terminology would avoid confusing the reader. Therefore, we have revised our manuscript to use the term “force output” throughout.

10. Page 10. The ‘kinematic’ results are reported awkwardly. A) The second half of line 7 should start as a new paragraph. B) For the ‘forefoot data’ (line 11), the actual data has been included but this is not done for the ‘rearfoot data’. Please be consistent here. C) For line18/19, what is the difference between ‘within-day reliability’ and ‘precision error’? D) Line 18 is poorly worded (i.e. ‘exceeded the reliability’ does not make sense). I suggest saying something like ‘the changes detected were smaller than the magnitude of error’.

Based on the reviewers’ suggestions we have amended the kinematic results section (page 10, lines 6-23).

11. Page 11. Was the mean rearfoot angle (i.e. 6.8°) ‘inverted’ or ‘everted’ relative to the vertical?

The standing rearfoot angle was defined as the rear foot relative to the tibia. We have added some text to the methods section to clarify this point (page 6, line 16). We have also indicated that the mean rearfoot angle was recorded as 6.8° of eversion (page 11, line 6).

Discussion:


The word variables has been removed (page 11, lines 14-15).


The word musculature has been added accordingly (page 12, line 15).
14. Page 13. Line 16. I think the authors need to be careful not to over speculate the results of the correlation here because tibialis posterior EMG was not recorded. It is possible that in fact tibialis posterior was working harder, but the authors have very little evidence to support or refute this statement.

After reviewing the discussion we agree that the conclusions may have been a little too strong for the data collected. Indeed, without EMG data it is difficult to know what compensation strategies may have been employed by other muscles to offset the decreased tibialis posterior force output. Therefore, we have added some text in this section of the discussion to remind readers that “compensation strategies” may also limit the conclusions of this secondary aim (page 14, lines 1-4).

The word “to” has been added to the sentence (page 14, line 22).

16. Another limitation that requires some discussion is related to the reliability study that comprised only 5 participants. Given that most reliability studies include at least 30 participants, the use of only 5 participants may have caused an inaccurate estimation of the true error. This is an issue because the authors have qualified the significance of the results by referring to the magnitude of error detected in this study. Perhaps the authors could also refer to other related work to tease out this issue (i.e. McGinley JL, et al. The reliability of three-dimensional kinematic gait measurements: a systematic review. Gait & posture 2009;29:360-9).

While we acknowledge that five subjects is a small sample size for a comprehensive reliability study, this was not the primary aim of this present study. Although we do discuss the pre-post changes in kinematics with respect to the reliability values, we were careful not to perform any statistical analyses in relation to this issue. Rather, the purpose was to remind the reader that the pre-post changes were potentially smaller than the precision error associated with a within-day gait analysis. Therefore, we feel that an in-depth discussion of reliability would detract from the focus of the paper.

17. Could the authors also clarify whether the unit measurement for the error (RMSE) is same as the unit of measurement used for kinematic analyses?
The unit of measurement for the RMSE is quantified in degrees, the same unit of measurement used for the pre-post fatigue kinematic analyses. We have added some text in the methods section to clarify this point (page 9, lines 14-15).