Reviewer’s report

Title: Centre of pressure characteristics in normal, planus and cavus feet

Version: 0 Date: 18 Dec 2017

Reviewer: Ruth Barn

Reviewer's report:

This is an interesting article summarising alterations to Centre of Pressure (CoP) characteristics in normal, planus and cavus feet. I enjoyed reading the article and it will be of interest to some of the readership of the Journal of Foot and Ankle Research. The study was well conducted and results clearly articulated however I have some specific comments which I would recommend are addressed prior to publication. My main comment is around the discussion section which did not fully discuss potential mechanisms for the results shown.

For clarification, the PDF had no page numbers so I have numbered the pages 1-30 from the start of the PDF document.

Introduction

P6 L52 It is stated that there are 3 studies which have 'compared CoP in all 3 foot postures' and this is attributed to refs #5-7, this is misleading as ref #7 only compared planus and cavus foot types.

P7 L5 'The studies' at the start of the sentence is confusing as it appears to refer to the most recently cited references #8 and 9. This could be modified to 'The foot posture studies' for example in order to make it clear you are referring to those papers.

P7 L30 add 'the' after 'of'

Methods

P8 L3 by which mechanism were volunteers contacted? It is not clear what 'general call' for volunteers refers to? Email? Social media? Posters?

P8 L27 I was unable to access the supplementary file

P8 L33 Some clarity is required here where foot posture was not bilateral, for example if one foot was cavus and the other planus then a unilateral presentation may influence the outcome variables and progression of CoP? There is description of how one limb was selected if both met
the selection criteria but not how it was managed when foot posture was markedly different from the contralateral side.

P8 L40-44 numbers and gender in each group should be presented in results when describing the sample.

P9 L16 do you have a reference to support the assertion that the 2-step method will reduce the influence of walking speed?

P10 L5 the last sentence of the paragraph needs clarification regarding which variables the average/min/max/range were calculated for as it simply states 'for each phase'.

P10 L18-20 what is the significance level with the Bonferroni adjustment applied?

Results

P10 L55 Assumed it is the average of the VCoP being presented in the figure?

P10 L60 P11 L1-3 It might be worth stating here that these results were when the cavus foot was compared to the planus foot (where effect sizes are reported for stat sig differences).

P11 L5 add full stop

Discussion

Overall, I felt that the discussion did not explore in full the potential mechanisms for the results found with regard to the velocity of the centre of pressure. The discussion focused on possible locations of force vectors and generation of forces (which were not measured in this study). There was no discussion of the windlass mechanism or the sagittal plane rocker function which would be relevant during the latter part/propulsive period of the stance phase. In terms of the section on study limitations, there should be some cognisance of the potential impact of recruiting participants without bilateral foot posture and possible influence on outcome variables.

There should also be some recognition of the decision to divide the stance phase into 4 periods based on the work of Perry et al in terms of interpreting the results. Some of the literature on lower limb EMG (e.g. Murley et al) divided the stance phase into contact, midstance and propulsion rather than separating the latter part of stance into terminal stance and pre-swing. Furthermore, the discussion section did not tease out the specific functions occurring during these subdivisions of the stance phase (based on Perry) and linking that to the results found. I appreciate there are various ways to analyse pressure data and dividing the foot into functional regions is one way in which the velocity of CoP has been shown to provide significant results.
between normal and disease populations (Semple et al 2007). It is my impression that there is ultimately minimal differences between the groups in terms of the CoP and perhaps there needs to be recognition that the variables measured are more sensitive to detecting change in disease states compared to normal as opposed to variations in foot posture (I appreciate this is clearly related to walking speed in painful/symptomatic feet). When factoring in the Bonferroni adjustment, the results are not significant for a number of the findings (p-values are not presented so it is difficult to tell exactly which variables are significant).

P12 L15 add 'in terminal stance' after 'fastest COP velocity'

P13 L47 Add 'limb' after 'development of lower'

P14 L33 'influence of COP', amend to 'influence of foot posture'

Tables/figures/appendices

Unlike the tables, I could not see a corresponding title for each figure. With that in mind figure 2 should be clarified if it is the 'average' of the VCoP.

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