Reviewer's report

Title: Foot kinematics in patients with two patterns of pathological plantar hyperkeratosis

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Reviewer: Kirsten Tulchin

Reviewer's report:

Thank you for the opportunity to review the manuscript “Foot Kinematics in Patients with Two Patterns of Pathological Plantar Hyperkeratosis.” The study aims to determine if there are kinematic differences of the rearfoot, midfoot, metatarsals and hallux during walking in patients with two distinct patterns of pathological plantar hyperkeratosis. The authors have a well-established line of research in foot kinematic testing and validation.

I have included my primary critique/comment as a discretionary revision, realizing it involves a continued area of controversy in multi-segment foot kinematic modeling. However I strongly urge the authors to add additional discussion of these points to their manuscript as I believe they may improve the strength of the research reported.

Major Compulsory Revisions
- None

Minor Essential Revisions
1) Methods section: paragraph 1 – there is a reference to figure 1 to display a callus. This was not included in the manuscript and the Figure 1 displays the marker set (and was referenced in paragraph 2.

2) Methods section: last paragraph – there is mention of ANOVA as two-factor interaction including ‘side’. While this is included in Table 4, there is no comment to these results in the text.

3) Results section: paragraph 1 and throughout – I suggest consistency in terminology for the transverse plane. The text reads external and internal rotation, but your graphs read abduction and adduction.

Discretionary Revisions
1) My primary critique/comment of this paper involves the use of relaxed standing to define the zero position. First, to note, the authors have made reference to this potential limitation several times in their discussion. However, more thought should be given to this topic. Stating (in the discussion) that some differences may be missed by use of the relaxed position of the foot as zero, leads me to question the abundant use of the angular orientation data, which is presented in Table 2, and heavily discussed throughout the manuscript. I can comment on
the continued debate of the “neutral” position in multi-segment foot kinematics for pages, however, my point is that this particular manuscript appears to be currently written and focused more on the angular orientation data than the ROM data (table 3), which I think weakens its quality. The differences between the two groups should be in both angular orientation and ROM, and therefore, with an acknowledged potential limitation to the orientation data the manuscript should focus more on the ROM data.

2) One other related issue to the relaxed standing “neutral position”: Rigid is often thought to implicate less motion. Is it possible that the “rigidity” of a segment can be refer more to the apparent fixed mis-aligned “neutral” orientation, even with proper range of motion? For example, according to the noted paradigm, the rigid foot should have a more inverted heel and the mobile foot a more everted heel. Perhaps the more rigid foot appears “rigid” because of its inability to each an anatomically neutral position— a fixed position of the heel, rather than its actual ROM. If that is the case, the relaxed standing position in a rigid foot would remain in a more inverted position (for example -5°), and using this as a zero position would subsequently cause the kinematic pattern to appear less inverted over the gait cycle (shifted towards eversion). The more mobile foot which may be able to assume a more anatomically neutral position during the static loading (during a less loaded, double limb standing posture) and therefore may not be offset as much during the dynamic loading of walking. My comment here is to emphasize that angular orientation angles of one group (rigid) may be more greatly affected by the potential limitation of using the standing position as neutral. (i.e. There is more “error” in representing the bone alignment in the rigid group compared to the mobile group.)

3) Discussion or comment should be added regarding some the differences seen in the ankle neutral to heel off region. In some cases, as in the rearfoot sagittal plane, the pattern and slope of motion seems to the identical between groups, and the reported differences in ROM appear to be solely due to the 8.4% difference in duration of this phase. In comparison, group 1 seems to demonstrate a slightly different pattern in rearfoot motion in the frontal plane compared to group 2, which may be more likely the cause of differences in ROM. It is recommended that this be mentioned in the text.

4) Figures 4, 6, 8 are referenced only briefly in the discussion. It is the opinion of this reviewer that these figures be removed from the manuscript. Their results are not discussed in the text (results section) and the commentary on these figures in the discussion is not critical to the points being made. The additional graphs may overwhelm the reader with non-essential data.

Minor Issues not for publication

1) TYPO Results Section: Paragraph 2: midfoot motion at time of maximum hallux dorsiflexion - 10.8 compared to 2.9 - Table 2 lists Group to have -7.9degs

2) TYPO - Table 2: Leg/Heel – FF sagittal –text in the results section indicates Group 2 had -3.3 degrees not +3.3degs.
Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.