Author's response to reviews

Title: "Tweaking" the IOR foot protocol to improve the diagnosis of pes-planus: application to the kinematics of feet in a 13 years old population

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Author's response to reviews:

REPLY point-to-point to Reviewers’ recommendations

Reviewer: Dominic Thewlis
Reviewer's report:

Major Compulsory Revisions

1) There appears to be some degree of incongruity between the title of the paper and the topic discussed. The title suggests that the paper will investigate the kinematics of the feet of children. However, the paper is much more focused on the modifications made to the IOR model than the output of the model. I suggest that the authors clarify if the manuscript is one focused on the kinematics with mention of the modifications made to the model or focus on the modifications made. If the latter were the case then it would be preferable for the authors to demonstrate that the modifications made to the model do not alter the output.

This is a sensible point: we wanted to investigate the multi-segmental kinematics of feet in a special children population – and this has now been explained much better - but in order to do so we had to make ‘minor’ changes to the original gait protocol. Both these aims were supposed to be of value for this readership. The authors, however, fully agree with the reviewer on that the title might have been misleading and is not summing up the content of the paper. In this revised version we hope the reviewer will appreciate that the title has been changed to: “Tweaking” the IOR foot protocol to improve the diagnosis of pes-planus: application to the kinematics of feet in a 13 years old population.

2) The aim states that the study was set to look at how the modifications to the model affected the kinematics of young health participants. I am unclear why children were selected, if the study was just investigating the modifications. I appreciate that this has some degree of repetition from point 1, but I think that it further emphasizes my point.
Yes, this is somehow connected to the point above. In our original submission the authors stated that the aim of the study was to apply a modified version of the IORfoot protocol to the kinematic analysis of feet in a young population of 13 years old children. We agree on that it was not sufficiently highlighted why a young population was chosen in this investigation. The main modification to the IORfoot protocol described in the paper concerns with an additional marker at the insertion of the Achilles tendon as to improve the tracking of the varus/valgus of the calcaneus. This angle is especially critical when assessing foot kinematics in children, since the diagnosis of pes-planus (flat-foot) is normally performed at the age of the subject’s cohort chosen for this study. Similarly, the medial longitudinal arch (MLA) angle is often used as a clinical parameter for assessing the degree of severity for the pes-planus. It is the authors’ belief that MLA angle and/or the arch index (ratio between midfoot and total foot area) are not suitable to classify the severity of pes-planus, and rather other more appropriate indexes - such as the varus/valgus of the calcaneus - should be tracked more precisely for an improved diagnosis. In other words, again, the choice of this special population has required modifications to the protocol. The aims of the study and the rationale for choosing the population have been better described in the Introduction of this revised version.

3) The authors state that calcaneal and forefoot markers were used to identify the gait cycle events. Could the authors provide more detail? Has the method been shown to be accurate and reliable?

The method relies on the analysis of the first derivative of the vertical displacement of the foot virtual marker as reported in O’Connor et al. (2007). A reference to the paper has now been added to the Methods of the revised manuscript.

4) The order of the methods seems a little odd. I would suggest that the modifications made to the model are first described before then going on to state what parameters were extracted for analysis.

We certainly agree with the reviewer on this point. What suggested by the reviewer was the intended order of the methodological section but apparently and erroneously this was not implemented in the submitted version. Correct order of the methods has been restored in this revision.

5) Throughout the results, the authors suggest that the data are ‘consistent’. How was this quantified? At this stage it appears as though this was simply a visual inspection of the data.

The authors concur with the reviewer on that “consistent” was based on the qualitative experience-based visual estimation of the bands for the inter-subject kinematic curves and no data were provided supporting what stated in the Results. The authors are also used to associate consistency with the standard deviation (SD) in these inter-subjects kinematics patterns, therefore
corresponding mean standard deviation data are now reported in the Results.

Minor Essential Revisions
1) The order of the sub-figures in figure one appears to be counterintuitive. Please work left to right.

Ok, a new layout for Figure 1 has now been provided.

Reviewer: Ryan Mahaffey
Reviewer’s report:
Reviewer’s Report
Title: MULTI-SEGMENTAL FOOT MOTION DURING GAIT IN CHILDREN
Version: 2
Date: 26/06/2014
Reviewer: Ryan Mahaffey

Major Compulsory Revisions

1. The title does not inform the reader of the scope of the manuscript. Acknowledgment of the modification of the IOR foot model is warranted.

The authors fully agree with the reviewer on that the title might have been misleading and is not summing up the content of the paper. The correct scopes and focus of this study has been discussed also with the other Reviewer. In this revised version therefore we hope the reviewer will appreciate that the title has been changed to: “Tweaking” the IOR foot protocol to improve the diagnosis of pes-planus: application to the kinematics of feet in a 13 years old population. The modified title is highlighting both the current modifications to the foot protocol and its original application to this special young population.

2. The authors state that the new anatomical reference frame for the hallux improves robustness of the segment. However, no definition or measure of robustness is given. A comparison with previous work by Leardini et al (2007) may not be relevant due to the difference in participant’s age (adolescence compared to adults). Both F2Pt and F2Ps should be presented using the modified and the original foot model so robustness and consistency may be compared.

The authors understand the sensible reviewer’s comment on this point.

The most common definition of robustness applied to an algorithm relates to its capacity to withstand errors in the input and during the calculations without interrupting.

According to this definition, the newly defined 3-marker based hallux segment has proved to be mathematically robust in that no singularities with abnormal angular measurement and no interruption of the calculations was detected in any of the trials analysed. Errors which were in fact experienced by the authors when
the old planar definition of the MTP joint was used.

The authors did not compare the new with the previous definition, this being possible only as new ad-hoc tests for MTP joint rotations, to be in fact conducted on the same participants that were enrolled in the study.

Nevertheless, while the authors are not aware of any reference data-set for the kinematics of the MTP joint in children, we can only observe and comment on the good inter-participant consistency of the measurement (which has now been explicitly reported in the Results).

In addition, in order to address further this important reviewer’s comment, definition of “robustness” and its relevance to the modified numerical approach in calculating MTP joint rotations has now been made more explicit in the revised manuscript.

3. The modification of the calcaneus in the frontal plane was compared between the modified and original foot models. However, considering the modification “valgised” the calcaneus, it could be assumed that most if not all calcaneal frontal plane orientation would be more valgus and yet 9 feet showed more valgus and 9 feet are in more varus orientation. This result needs explaining, can it be described as “valgisation” if the offset is 50% varus and 50% valgus?

By showing that 50% of the feet had a calcaneus orientation more valgus and 50% more varus compared to the calculations from the previous protocol is in fact highlighting that 100% of the feet had a calcaneus varus/valgus orientation different from what determined with the previous calculation (which, by any means, was not the varus/valgus of the calcaneus but merely the inclination of the ST-PT axis in the frontal plane of the shank; this has been made clearer as well). Needless to say, the previous varus/valgus angle can not even be used as reference for the frontal-plane angle of the calcaneus. The authors do agree with the reviewer on that the term “valgisation” is probably not correct to describe the extent of the correction in modeling the calcaneus frontal-plane inclination and therefore “valgisation” has now been removed from the revised version.

The authors would like to point out that the clinical assessment performed by the clinicians during the subjects’ physical examination was consistent with the kinematic measurements, as explicitly reported – “..all subjects had normal feet presenting a hindfoot frontal-plane inclination lower than 7 deg in valgus..”.

4. The authors state that the MLA angle is based on Moreau and Costa-Bertani angle. As I understand, the Moreau and Costa-Bertani angle has an internal and external parameter. However, the manuscript does not mention which is being represented; one would assume the internal angle? If it is the internal Moreau and Costa-Bertani angle does the sustentaculum tali marker represent the appropriate landmark to represent this angle? I believe the internal angle is taken from the lateral view of the foot from the vector between the medial sesamoid and the talonavicular joint and the vector between the talonavicular joint and the posterior calcaneal. Clarification on the representation of this clinical measure should be considered together with why this clinical MLA angle was used rather
than other clinical definitions of MLA including the use of the medial malleolus and navicular markers (see Razeghi & Batt, 2002, Gait and Posture; 15(3):282-91).

The authors are aware that this addressed by the reviewer is certainly an important topic within currents international debates, and that it needs attention and requires further clarification. First of all, the Moreau and Costa-Bertani (MCB) angle that is discussed in the text as that being similar to what defined in the IORfoot protocol is the internal parameter (the latter implicitly from the term ‘medial’ as in MLA, as opposite to ‘lateral’=external). The MCB angle, estimated via geometrical measurements from lateral X-rays of the foot, appears to be still much used in the clinical community.

However, the reviewer is correct in stating that other clinical indicators or measures of pes-planus have also been proposed. The authors are aware of several other clinical measures of MLA (normally obtained from X-ray examination) such as the Meary line, or the talo-calcaneal angle. Lately, the “calcaneal pitch” has become a widely used clinical measure of MLA which is highly popular with podiatrists. The latter is defined as follows: in the lateral projection of the foot, it is the angle formed by the line passing through the most plantar aspect of the calcaneus (PC) and the most antero/inferior distal aspect of the calcaneus, with a line passing through PC and the medial sesamoid. The newly defined IORfoot MLA angle is a compromise between the clinical MCB angle and the calcaneal pitch. Similarly to the calcaneal pitch, the new IORfoot MLA angle is defined according to two anatomical landmarks on the calcaneus and one landmark on the head of the first metatarsal bone. And, similarly to the MCB angle, flattening of the MLA results in larger IOR MLA angles (and smaller calcaneal pitch).

While other markers could certainly be used to provide an estimate of the MLA angle, we do not believe our currently proposed definition to be much inferior or less valuable in estimating MLA angle deformations during gait. In other words, our MLA angle is defined as close as possible to the internal MCB angle given the marker set of the IORfoot. It is also though important to show normative/reference values, thus these were explicitly shown together with the rotation time-histories.

All the relevant sections of the manuscript have been revised in accordance with what replied in this answer to the reviewer.

5. The references are out of sync with the text particularly in the methods section (lines 142 and 144). Reference should be reviewed and reorganised throughout the manuscript.

We are grateful to the reviewer for spotting the error. The references have been fixed in this revised version.

Minor Compulsory Revisions
6. The abstract states (line 66) “high intra- and inter-participant repeatability”, but this was not mentioned or measured in the main text results. The abstract should summarise the main text.

We agree with the reviewer. The “intra- …. repeatability” has been deleted, whereas “good inter-participant repeatability” is now supported by standard deviation data which have now been added to the Results (also to address a similar request by the other Reviewer).

7. Line 98 – “joint had be sought as to avoid” needs rephrasing

Thanks for pointing this out, this sentence has now been rephrased as from reviewer’s suggestion.

8. Line 99 - “singularities arising for large flexion angles” change to “singularities arising from large flexion angles”

ok, this has been fixed in the revised version.

9. Methods (line 110) - “participants….. [had] possible pes planus” How would this affect the results regarding frontal plane orientation of the calcaneus? You would expect more valgus orientated feet from the participant regardless of the modification to the foot model. This may negate the comparison with “typical clinical observations of valgus or neutral calcaneus” reported in the discussion (line 196-197) and comparison with previous literature (e.g. Leardini et al 2007). Clarification on this as a potential limitation is required. Perhaps a clinical measure of calcaneal frontal plane orientation should be considered in order to compare with the results foot model rather than just stating (line 112-113) “all participants had normal feet presenting a hindfoot frontal plane inclination lower than 7 deg valgus”.

The authors, in accordance with the clinical measurements, were expecting calcaneus orientation to be either neutral or valgus, whereas in fact 5 out of 20 feet (25%) resulted being in varus. The authors believe that the varus orientation measured in these feet should be related to the physiological malicious alignment present in the lower limb of these subjects (and clinically observed) – femur in neutral position and tibia adducted and internally rotated (genu valgum). In fact, the calcaneus varus/valgus alignment is calculated relatively to the tibial anatomical axis. A calcaneus with neutral frontal-plane orientation may appear as being slightly in varus if the tibia is slightly adducted. This point has now been made clear in the Discussion as possible limitation of the current definition.

10. Line 111 – “medial longitudinal arch has” change to “medial longitudinal arch as”

Thanks, this error has now been fixed.

11. Line 163 – “patterns the rotation” rephrase
Thanks again for spotting the error, the sentence has now been fixed.

12. Discussion (line 183-184) – “…less expected…MLA angle in static up-right posture was larger than 180 deg”. This would infer that the sustentaculum tali was very near to the floor. If the representative Moreau-Costa-Bertani angle is considerably more than what would be expected can normative values be presented? Furthermore (line 186-187), “…accounted for by the participants’ young age… not fully developed foot arches are considered physiological”. One would not expect such a large MLA angle from age alone and the authors should consider the definition of the MLA angle used in the IOR foot model.

Several definitions of medial longitudinal arch (MLA) have been proposed in the literature. One of the most commonly used and popular definition is the one we have taken from traditional radiographical analysis, i.e. the Moreau-Costa-Bertani, and applied as much as possible to the marker-set of the IORfoot protocol. This is expected to be smaller than 180° in normal feet in up-right posture, but also to be possibly larger during the stance phase of walking and in morphological flat feet.

The parents of the children who showed up at the authors’ clinics for physical examination were in fact concerned about possible pes-planus diagnosis. While clinical analysis revealed that the children indeed presented low-arched feet, no indication for pes-planus treatment has been found according to the Institution current clinical guidelines. Measuring large MLA angle in a young population with visibly low-arched feet is not surprising. In light also of what already answered to comment#4, normative data on the Moreau-Costa-Bertani (pes planus larger than 130°) angle do not apply to the angle measured using the IOR foot protocol. The novel MLA definition implemented, which employs the same marker set of the IOR foot protocol, aimed at providing a better anatomical description of the calcaneus inclination in the sagittal-plane. By doing this, the new definition is more consistent with the clinical measure of calcaneal pitch. Nevertheless, this point has now been addressed more carefully.

13. Figure 1B and 1C are labeled incorrectly. I believe the labels should be switched around.

Thanks for pointing this out, the figure caption has now been fixed.

Minor Discretionary Revisions

14. The reference (line 309) for Mahaffey et al 2012 [17] is from a conference proceeding, there is a full article here: Mahaffey et al. Journal of Foot and Ankle Research 2013, 6:43

The conference proceeding reference has been replaced with that of the full article, thanks.