Reviewer’s report

Title: Evaluation of multi-segmental kinematic modelling in the paediatric foot using three concurrent foot models

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Reviewer: Matthias Hösl

Reviewer’s report:

Overall Comment:

The authors assessed the repeatability and error of three different foot models in gait of healthy children in order to inform about the clinical utility of these models. Generally, this information is of high value when evaluating foot function in clinics as foot kinematics are frequently used to assist decision making by comparing data of foot pathologies with normative, typically developing feet. Yet, the presentation of the results (text and graphs) remains somewhat unclear and erroneous and should be therefore improved. In particular, there seems to be an error in the OFM concerning the definitions of angular rotations in frontal and transversal plane which has to be changed. Results and Discussion have to be revised. I am also looking forward to a stronger conclusion.

Major Compulsory Revisions

1) Background /Paragraph 2: Next to the work of Stebbins et al. [3], the authors are encouraged to also consider the work of Curtis et al. [1] about the repeatability of the OFM. Their results pointed out that “poorest repeatability was in transverse plane (hindfoot rotation and ab-/adduction of the forefoot), especially for hindfoot rotation”. This contrasts their results and was also not considered in the discussion. The plots of Curtis et al. also include the data of the study by Stebbins et al. [3]. The kinematic patterns in transversal plane resemble the current frontal plane data (e.g. for the hindfoot). See also the normative traces of Hösl et al. [2].

Discussion /Paragraph 5 (p. 10): The authors compare the amount of error from the 3D Foot and Kinfoot with previous studies. No such comparisons were made with previous studies using the OFM (Stebbins [1], Curtis [3] etc.). This would be very informative.

2) Results /Figure 1: For the sake of comparability, I suggest to stick to the nomenclature of Stebbins et al (2006) when describing the rotations of the OFM, e.g. Plantar-, Dorsiflexion etc. as done in Figure 2. It seems that transverse and frontal plane are interchanged! The middle plot in the first row likely shows hindfoot to tibia rotation, hence transverse plane motion and not frontal plane Inversion/Eversion. Inversion/Eversion is missing. The last two plots in the first and second row depict that in session one (back line) an outlier around 30% of stance may have been included.
In the plot titled ‘Hindfoot tibia frontal plane motion’ the SD of the 2nd session is about twice the SD of the first session. This may also imply that repeatability was rather low, however, the associated ICCs shown in Table 3 are rather high, ranging from 0.68 to 0.83.

3) Results/ Figure 1: the traces of the first and second session of the OFM for hindfoot and forefoot motion in sagittal plane seemed to be nearly identical. In Figure 2, concerning the 3D Foot Model, there is a shift of ~5° in hindfoot sagittal plane motion (shank calcaneus) between sessions (first plot, first row), as well as a shift of ~5° concerning calcaneus to midfoot motion. One may therefore reason that the 3D Foot model is less repeatable. Yet, comparing Table 3 and 4, the calculated mean ICCs and SEMS of the 3D Foot Model for these rotations (ICC 0.68, SEM 3.8° and ICC 0.63, SEM 3.8°) would outperform the OFM (ICC 0.51, SEM 3.9° and ICC 0.61, SEM 4.2°). Is there an error involved?

4) Discussion / Paragraph 2: It is very much appreciated that the paper addresses the issue involved when using the PIG Marker placement in associations with the OFM. However, major problems likely occur also in transverse plane. Misplacement of the Thigh-Marker also shifts transverse plane rotations, internal and external hindfoot rotation with relation to the tibia segment. Assuming that the authors confused transverse and frontal plane also in Table 3 (consistent with the plots), the smallest mean SEM (~2.85°) of the OFM would be found in the frontal plane, so the argument of this paragraph has to be revised.

Minor Essential Revisions

6) Protocol/Marker Application: The marker application has to be clarified: The authors refer to a table for presenting the markers. In total, 34 markers were applied. The anterior aspect of the shin (OFM) and the anterior tibia marker (Kinfoot) are counted as separate markers (#7 and #8), while Femoral condyle and the medial tibial condyle are counted as the same marker (#1). Marker #23 is named differently and counted as one marker. Is that correct? For Marker #22, the notation base of first metatarsal (OFM and 3D Foot) and base of first metatarsal medial (Kinfoot) was used. Is that the same marker? An additional photograph may be helpful to get an impression of the marker placement, see Dixon et al. [3]. There is no comment in this section about the handling of the ‘proximity issue’ mentioned in paragraph 5 of the discussion.

7) Marker Application OFM+PIG-Model: In the discussion the authors briefly touch the topic that the PIG Marker placement is used for shank definition in the OFM and the problems of varying the Thigh-Marker placement. Yet, in the method section, the usage of the PIG Model is not mentioned. Did the authors use wand-markers for the thigh or a knee-alignment device etc.? If the standard lower body was used, Davis and Deluca [5] may be cited, as well. Again, a picture may be helpful.

8) Data-analysis/ Paragraph 2: There should be no need to average anthropometric data within subjects. Table 2 suggests that non-parametric test statistics were also used which is not mentioned.
Paragraph 3: Interpretation of ICCs: r # 0.8 represents very high…

9) Results /Paragraph 2 (p. 7, line 4): ‘lowest ICCs for 3D Foot were at the Hallux’. Which plane? Sagittal or transverse? Or were both averaged? The summary of the results for the Kinfoot are not clear. Values (ICCs and SEMs) for subtalar, midfoot, hallux and medial toes and lateral toes are presented. Which angular rotations (plane) are the authors referring to?

10) General Comments on Figures: The names of the segments (Table 3) are not the same as in Figure 1. The authors may add that the group averages (of 14 children) are plotted, as I assume, and that, for completeness, the unit on the y-axis is [°]. I would also suggest using different scales on the y-axis of the different plots since too wide axis limits make it hard to follow the kinematic traces and locate the discrete parameters.

11) General Comments on Tables/ Style: The Layout has to be improved. ICCs and SEMs should be more easily detectable. The unit of SEM is missing. In the captions, to off should be toe-off.

It seems advisable to also include the mean ICCs and mean SEMs for each angular rotation in the tables, since they are mentioned in the result section.

12) Figure 2: Same issue than in Fig.1: Appropriate adjustments of y-axis limits are required. Nomenclature in Figure 2 and Table 4 is not consistent. Forefoot-Phalanx (Table 4) ~ First Metatarsal-Hallux in Figure 2.

13) Discussion /Paragraph 5 (p. 10): The authors suggest future work should test the repeatability of the models in isolation. Was the proximity issue only present in the forefoot or were there additional issue related to the amount of markers and the size of the children’s feet?

14) Conclusion: Since three models have been compared, the reader may expect a final statement from the investigators which foot model performed best. The authors generally pointed out the choice of kinematic model may depend on the question or clinical problem which I do agree on. To my opinion, the ‘abundance of information’ from the Kinfoot limits its use in paediatric orthopaedics. Next, it also performed worst in the current study. Did the authors draw any conclusion from this, any recommendations? Eventually, the final statement about the 3DFoot Model: ‘a balance between moderate repeatability and reasonable test-retest errors’ applies also to the OFM.

Discretionary Revisions

5) Methods / General comments: 1) It may be informative to include ROM values, in addition to the discrete parameters, since ROM values are less prone to error. If the authors think that discrete parameters are superior to ROM values for clinical interpretation they may also comment on that.

References:


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 have no conflict of interest to declare.