Author’s response to reviews

Title: Progression of planovalgus deformity with age in patients with cerebral palsy

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Editor Comments:

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Thank you for your time and effort in reviewing our paper.

We have made a point-by-point response to the reviewers’ comments and answered their questions as thoroughly as possible.

Changes to our manuscript has been made according to the comments and elaborations were done on points lacking details while redundancies were deleted.

Reviewer reports:

Donald H Lein Jr, PT PhD (Reviewer 1): Please include all comments for the authors in this box rather than uploading your report as an attachment. Please only upload as attachments annotated versions of manuscripts, graphs, supporting materials or other aspects of your report which cannot be included in a text format.

Please overwrite this text when adding your comments to the authors.
The authors described the method so that it could be replicated by another group of investigators. I do have three suggestions.

1. First, I think that the authors should provide the reader the reason they had only one investigator perform testing for intrarater reliability.

Thank you for your valuable comment. We deemed interrater reliability to be more important than intrarater reliability and therefore based the study on the postulation that the sample size for reliability testing depends on how many patients three raters can most accurately examine. In many cases, intrarater reliability tends to show higher reliability than does interrater reliability, as in our study. Three raters separately measuring intrarater reliability would have been adequate, but we found the effort to be too cumbersome for this study.

2. In addition, they should provide some explanation of their decision to have only two investigators perform the measurements for the study when four of the investigators were involved in the interrater reliability testing and analysis. Did you select the two investigators that had the best interobserver reliability?

Thank you for raising this concern. We randomly selected two investigators to perform radiographic measurements in this study instead of selecting those with the best interobserver reliability.

The aim of reliability testing was to confirm whether the involved investigators could record each index with some degree of reliability or not. The study was not aimed at identifying the most reliable index but at determining if index scores showed consistency between the participating investigators.

3. Finally, in your method section you should include what type of validity and reliability that you used and found to determine the radiographic indices that you used in this study.

Thank you for bringing the missing information to our attention. Our selected indices were based on our previous study on reliability and validity of radiographic measurements in hindfoot valgus. Discriminant and convergent validities and intrarater and interrater reliabilities were investigated in our previous study1. In accordance with your suggestion, we have included this in page 4, line 4 of our Methods section.

“…and one of the authors(JJM) pooled seven indices relevant to the evaluation of PV and hallux valgus(HV) that have been established with convergent and discriminant validities and intra, inter-rater reliabilities1.”


I also have two addressable concerns about the interpretation of the results.
1. First, you did not address in that No Tal/Stayer was statistically significant in the GMFCS levels 1 and 2 groups for both the talo-second metatarsal angle and talo-first metatarsal angle your result section or discussion section. I would address these findings.

Thank you for your valuable comment. The result means that the talo-metatarsal angle was 4° higher in patients who did not undergo TAL or Strayer's operation than in those who underwent TAL or Strayer's operation. It does not mean that the angle increased by 4° in those who did not undergo TAL or Strayer's operation. Since we did not compare talo-metatarsal angles before and after TAL, it is difficult to attribute the change in the talo-metatarsal angle to TAL or Strayer's operation, contrary to patient age. In other words, patients in the TAL or Strayer's operation group underwent either procedure in the follow-up period. We attributed the result to the characteristics of our study population rather than deem it as a risk factor for progression of pes planovalgus.

We have mentioned this finding in our page 8, line 24 of Results section and have discussed it in page 9, line 33 limitation section as follows:

“The talo-2nd MT angle was 4° higher in patients with GMFCS level I (p = 0.0044) and 3.10° lower in patients with GMFCS level II (p = 0.05) among those who did not undergo TAL or Strayer's operation (Table 4). The talo-1st MT angle was 4.23° higher in patients with GMFCS level I (p = 0.0044) and 3.10° lower in patients with GMFCS level II (p = 0.05) among those who did not undergo TAL or Strayer's operation (Table 5).”

“Another limitation is that the angles were not compared before and after TAL or Strayer's operation; therefore, the history of TAL or Strayer's operation lacks causality to pes planovalgus. It is difficult to conclude that TAL or Strayer's operation is a risk factor for changes in the talo-metatarsal angle.”

2. Second, in the first statement of your discussion, you stated that the single most important factor influencing the progression of PV was GMFCS levels. You never provided this data to support this statement. You did provide us data that supported that within these groups age and No Tal/Stayer did influence some of your results. Please modify this statement in the discussion, conclusion and conclusions of the abstract.

Thank you for the valuable comment. We have toned down the intensity of our statement and changed the first paragraph (page 8, line 1-7) of the Discussion section as follows:

“In our longitudinal assessment of progression of PV in patients with CP, we found that the major risk factor for PV progression depending on age was the GMFCS level. The talo-1st and talo-2nd MT angles, which are surrogate indices of forefoot abduction, significantly increased with age in patients with GMFCS level II or III (p < 0.0001, p = 0.0007). HV aggravated in all three groups (p < 0.0001), and the lateral talo-1st MT angle, a surrogate of planus, showed significant improvement in patients with GMFCS level I or II (p < 0.0001, p < 0.0001) and no significant improvement in patients with GMFCS level III.”
DEVINA KUMAR, Ph.D. (Reviewer 2): Overall, the paper will be of interest to surgeons and clinicians. GMFCS is one the most commonly used mobility scale. However, minor changes which needs explanations:

1. Why was the inclusion criteria less than 18? Typically surgical corrections do not happen in children below 10 years. If 8 years gap is big to affect number of other variables. Why was this age selected as the cut off criteria

Thank you for your comment. Surgical interventions are usually performed after the age of 10 years and may be more delayed. We chose the age of 18 years as our inclusion criterion because children reach skeletal maturity at this age, and after skeletal maturity, changes in radiographical index scores would be minimal.

2. Are there any confounding variables to your study? What is the literature to support those factors?

The association between Achilles tightness and pes planovalgus is well known. We postulated that Achilles tightness may be a confounding variable in our study; however, because our study was retrospective in nature, it is hard to confirm whether Achilles tightness was an independent variable affecting the progression of pes planovalgus or not. In most cases of cerebral palsy, higher GMFCS level is associated with tighter achilles tendon.

We have included this in page 9, line 33 of the limitation section as follows:

“While discussing the clinical implications of the present study, it is crucial to address the limitations. First, the study was retrospective in nature. A uniform protocol was not implemented for all subjects: Patient age at the initial assessment, duration of follow-up, and number of radiographs acquired during follow-up differed. However, a linear mixed model consisting of fixed and random effects was selected to adjust the repeated measures or longitudinal data. This is an inherent limitation of the study design. Second, confounding variables, such as Achilles tightness and patients’ activity level, could not be identified with our data. This may be explored in future studies with the aim of verifying the effects of such variables.”


3. What factors (play, work) effects the outcome? Is GMFCS

As pointed out by the reviewer, factors other than the GMFCS level, such as patients' activity level, including work and play, may affect the outcome. However, because our study had a retrospective design, it was not feasible to describe the effects of the patients' activity level with
our data. In future studies, however, a prognostic assessment could explore the effects of the patients' activity level on pes planovalgus.

We have included this in our text as follows:

“While discussing the clinical implications of the present study, it is crucial to address the limitations. First, the study was retrospective in nature. A uniform protocol was not implemented for all subjects: Patient age at the initial assessment, duration of follow-up, and number of radiographs acquired during follow-up differed. However, a linear mixed model consisting of fixed and random effects was selected to adjust the repeated measures or longitudinal data. This is an inherent limitation of the study design. Second, confounding variables, such as Achilles tightness and patients' activity level, could not be identified with our data. This may be explored in future studies with the aim of verifying the effects of such variables.”

4. The study should be made more to the point: lot of redundancy in the text

Thank you for your valuable comment. We have reviewed and deleted the redundant points from our manuscript.