Author’s response to reviews

Title: Etiological Factors in Hallux Valgus, a Three-dimensional Analysis of the First Metatarsal

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Version: 1 Date: 17 Aug 2017

Author’s response to reviews:

Dear Dr. Bonanno,

Associate Editor of Journal of Foot and Ankle Research

We thank the Reviewers for careful reading our manuscript and for providing us with useful comments and suggestions. We have revised the manuscript JFAR-D-17-00060 on the basis of the Reviewer's reports. We look forward to a publication of our manuscript in Journal of Foot and Ankle Research.

Please address all correspondence to:

Takeo Nagura, MD, PhD,
We look forward to hearing from you at your earliest convenience.

Yours sincerely,

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Our responses to Reviewers’ reports are as follows:

Response for Reviewer #1:

Thank you very much for your thorough review of our manuscript. In accordance with your comments and suggestions, we revised the manuscript as follows.
Major comments:

1) The HV patients were diagnosed by one of the authors (T.K.) who is a foot and ankle orthopaedic surgeon having more than 21 years’ experience. We revised the manuscript accordingly (L119).

2) The inter- and intra-observer reliability for the torsional angle measurement was evaluated by analyzing all the CT data independently by two of the authors who are foot and ankle orthopaedic surgeons (T.O. and M.K.) and by re-analyzing the data after an interval of more than one year by the same author (T.O.). The correlation coefficients were calculated to assess the inter- and intra-observer reliability using the same statistical software. We revised the manuscript accordingly (L155 and 183).

3) We added information about the participants such as body weight and body mass index (BMI) in the methods. The HV angles for the control group were not available, since we did not perform dorsoplantar weight-bearing radiographs since there was no clinical necessity for those control patients. It was confirmed by T.K. that the selected 12 healthy (unaffected) feet in the control group had no obvious foot and ankle pathologies such as HV deformity, osteoarthritis, and sesamoid subluxation.

4) We carefully reviewed pathological conditions of the feet used in the present study and decided to exclude some of the feet used in the original manuscript. Furthermore, as suggested by the reviewer, we now used only the healthy unaffected side of the foot as the control group. Because of this, the number of the feet used in the present study decreased to 27 and 12 for the HV and control groups, respectively. However, the result of the present study basically did not change.

Minor comments:

We replaced “women” to “elderly people” (L91).

Response for Reviewer #2:

Thank you very much for your thorough review of our manuscript. In accordance with your comments and suggestions, we revised the manuscript as follows.

Firstly, we are sorry for the confusion about the description of the torsion of the 1MT. We rephrased the description as follows (L146): The torsional angle of the 1MT is positive for
eversion, i.e., the plantar portion of the articular surface of the 1MT head faces externally with respect to the base of the 1MT.

EG1: We changed the title to “Etiological factors in Hallux Valgus, a Three-dimensional Analysis of the First Metatarsal”.

EG2: We revised the sentence.

EG3: “Planar” was replaced by “plain”.

EG4: We deleted the words “It was confirmed that”.

EG5: “Planto-dorsal” was replaced by “dorsoplantar”.

EG6: The word “the” was added.

The paper was language-edited by a commercial language-editing service prior to resubmission.

Response for Reviewer #3:

Thank you very much for your thorough review of our manuscript. In accordance with your comments and suggestions, we revised the manuscript as follows.

- Introduction, line 78.: We revised the sentence and cited a more appropriate reference here.

- Line 84.: We mentioned in line 215 that Kilmartin et al. previously reported that there is no clear association between first metatarsophalangeal joint pathology including HV and the 1MT head shape in juvenile (10-year-old children). This finding, combined with the present finding, possibly indicates that the change in the morphology of the 1MT might have occurred developmentally. This discussed this in the revised manuscript.

- Nix et al. (2012) was cited in the revised manuscript.

- Methods, line 100.: The inclusion/exclusion criteria were better explained in the revised manuscript.

- Lines 106-111.: We included the evaluation of the tibial sesamoid grades [18] in the revised manuscript to show that the sesamoid position was completely congruent for the control group. We also reported the average HV angle of the HV and control groups based on the CT scan data.
• Results, line 144.: The present study successfully quantified the torsion of the 1MT head with respect to the base of the 1MT, but did not clarify where in the metatarsal the torsion occurs. Although we visually observed that the torsion of the 1MT occurred in the diaphyseal region, but not in the head region, we must quantitatively confirm this in future studies. This was added in the last paragraph of the Discussion section.

• Available demographic details of the subjects such as the country of origin (all Japanese), body weight and body mass index (BMI) were now provided.

• Discussion, lines 159-160.: In the present study we only analyzed the morphology of 1MT bone, but not the change in the relative position of the metatarsal with weight-bearing. Therefore it is now difficult to assess if the torsion of the 1MT head is adaptive or not. We would like to investigate this in future studies.

The original comments of the Referees are as follows.

Reviewer #1: This is a potentially interesting study which used three-dimensional CT to assess morphological changes associated with hallux valgus. Most existing studies have undertaken two-dimensional assessment using radiographs. There are some important issues with the manuscript however. As discussed below, some key methodological detail is missing, the statistical analysis needs to be revisited, and the volume of data presented is small.

Major comments:

1) Methods, patient selection (pg 6): no detail is provided of how hallux valgus cases were recruited from and how they were identified.

2) Methods, radiographic evaluation: who and how many observers read the CT scans? What was their professional background and experience/training? What is known about the reliability of the CT assessments undertaken in the study? Were inter- or intra-observer reliability assessed in this study, or are data available from previous studies?

3) Results: no descriptive characteristics are provided about cases and controls, apart from the mean age which is provided in the methods. This would be better placed in the results. The hallux valgus angle is provided for cases but not for controls - again this would be better placed in the results section. Are any other descriptive characteristics available such as musculoskeletal or other comorbidities? Did any participants have inflammatory arthritis,
gout or osteoarthritis (OA)? The discussion (pg 11) states that care was taken that controls had no first metatarsophalangeal joint osteoarthritis - yet this is not mentioned in the patient selection section of the methods.

4) The data presented were obtained for 31 feet from 30 cases and 32 feet from 16 controls. This is problematical because the statistical analysis employed treats these as independent observations and does not take into account the fact that the cases mostly provided data on one foot each whereas the control feet consisted of 16 paired observations. The analysis needs to be revisited using statistical methods which take account of this.

Minor comments:

1) The first sentence of the introduction (pg 5) states that hallux valgus is one of the most common and significant forefoot problems in women. Whilst this is true and hallux valgus is more common in women than men, it is also a common and significant problem in men also so I feel it would be more accurate rather to refer to older people, not specifically women.

Reviewer #2: I found this article very stimulating. The investigation and its results are of significance both clinically and academically to the undertraining of a complex condition. The limitations are well identified and these should lead to further investigations.

I have one area of clarification that I would like the authors to assist with.

The section on "radiographic evaluation" as currently written I found somewhat confusing.

The text "The torsional angle of the 1MT 130 is positive for eversion, i.e., the 1MT articular surface of the head faces externally with in respect to the base of the 1MT (Fig. 1-c)" seems to describe a rotation of the cartilage in the transverse plane such as would be seen with an increase in PASA, yet the study results and conclusions focus on torsional rotation in the metatarsal shaft.

This confusion appears again in line 154 "However, the present study clarified that the 1MT is not only rotated, but is also twisted, in the HV group" again suggesting two rotations have been measured.

Can the authors please clarify this.

In addition I would suggest that a review of the language used would be worthwhile.

EG
1. Title might be worded more simply such as - Etiological factors in Hallux Valgus, a Three dimensional Analysis of the First Metatarsal.

2. The following line should be reworded - the medial arch is not the cause of ground reaction force. Line 77 - The first ray bears a large magnitude of ground reaction force during locomotion due to the presence of the medial longitudinal arch.

3. Line 87 Planar should be replaced by plain?

4. Line 109 The words "It was confirmed that" are not necessary from a grammatical perspective.

5. Line 123 "planto dorsal;" - is this correct?

6. Line 199 the word "the" should be added as second word in sentence.

There are further examples of such grammatical areas, as such and indicated above a through review of grammar is recommended.

Reviewer #3: This is an interesting paper which introduces an analysis of the torsion within the first metatarsal in patients with hallux valgus. It is worthy of publication subject to the following comments.

Introduction, line 78.

The author's quote a paper from 1928 stating the first ray bears a large magnitude of ground reaction force due to the presence of the medial longitudinal arch. I understand the author's wish to indicate how first ray pathology can affect the foot, but I feel a clearer description/more up to date reference would be appropriate.

Line 84.

The author's quote a number of references relating to the shape of the metatarsal head and hallux valgus. However, the Kilmartin reference regarding juvenile hallux valgus concludes that assessing shape has little place in the scientific assessment of the deformity. The author's may be
better reviewing the evidence and indicating that there is still some controversy and this would help to justify their investigation.

The reference below also referred to metatarsal shape but indicates that conclusions regarding the causality cannot be made from cross-sectional studies and may be worth including.


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Methods, line 100

Were the presence of hallux valgus on x-ray and the absence of features on CT scan the only inclusion/exclusion criteria?

Lines 106-111.

The author's report that the control group were taken from patient's who had CT scans for rearfoot pathology but no evidence of hallux valgus deformity or sesamoid subluxation on CT. The authors should at least consider that these are non-weightbearing assessments and could potentially underestimate the presence of hallux valgus. Can they confirm that the sesamoid position was completely congruent?

The author's could report the average hallux valgus angle and sesamoid deviation observed on the CT scan of the hallux valgus group. If all of these patients showed an angulation consistent with hallux valgus and sesamoid deviation greater than the control group, this would provide some support for their process.

Results, line 144.

The author's report that the torsion seem to occur in the diaphyseal region. As this is not a definitive result, how can make this conclusion?
I feel the author's should publish demographic details of the subjects (i.e. age range) and the hallux valgus angle on x-ray (mean and standard deviation). It may also be of interest to compare the degree of deformity on x-ray compared to CT (this relates to the comments above) and whether or not the degree of torsion relates to the degree of deformity.

Discussion, lines 159-160.

The author's have not discussed how the relative position of the metatarsal may change with weight-bearing. They have measured a rotation within the metatarsal in the subject group but the relative position of the first metatarsal in relation to the lesser metatarsals in weight-bearing or the ground might alter. It is possible that the rotation they see is adaptive which is something they go on to discuss but they should at least consider the effect of weight-bearing on the overall position.