Author’s response to reviews

Title: Coordination among the rearfoot, midfoot, and forefoot during walking

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Author’s response to reviews:

Response file

We would like to thank the editor and the reviewers for carefully reading our manuscript and for giving useful comments and fruitful suggestions. I have included the reviewer’s comments along with our responses. Revisions and additions to the manuscript are highlighted. Our responses to reviewer’s comments are as follows:

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Response for the Reviewer #1

☐ Regarding the Methodology:

Comment 1: Where were the participants recruited from?

Response:

I added the sentence on the page 7, line 128 to 129 as follows: “Participants were recruited from the student population of Niigata University of Health and Welfare.”

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Comment 2: Why is foot deformity assessed using the concept of the "too many toes" sign? What was the rationale behind this? Why did the study not use a quantification technique such as the foot posture index (FPI-6)?

Response:

If the purpose of the study was to compare normal and pes planus conditions, we believe that it would be necessary to strictly evaluate the foot. However, this study is not a kind of research such as classifies normal foot and pes planus, and compare between normal foot and pes planus. In other words, since there was only data on the normal group in this study, we decided that visual examination could be easily performed and was sufficient. In addition, other studies (Leardini et al, 2007, Gait Posture; Dubbeldam et al, 2013, Gait Posture; Caravaggi et al, 2010, J Biomech) that have investigated intra-foot kinematics in healthy subjects also did not use a quantitative method for foot assessment.

The “too many toes” sign is positive if the forefoot appears to be in the abducted position while the subject is standing. We decided that performing this visual method of evaluation would clearly show the abduction position of the forefoot. However, as you say, we considered that foot assessment is desirable to quantitatively carry out.

In the consideration of comment 13 and reviewer 2’s comment, I revised the sentence on the page 7, line 121 to 127 as follows: "Ten healthy male subjects (height 171.7 ± 5.0 cm; body mass 64.8 ± 8.0 kg; age 22.6 ± 3.1 years), without pes planus or other pathologies volunteered to participate in this study. The pes planus presents as an abduction of the forefoot during standing. Thus, the foot posture was evaluated using the “too many toes” sign [20], and participants who had a positive evaluation (the forefoot in the abduction position when viewed from the back in the standing position) were excluded. By using this method, obvious pes planus was excluded.”

Additionally, I added the sentence on the page 19, line 371 to 377 as follows: “Thirdly, the participants’ foot posture during standing was only visually inspected to eliminate obvious pes planus. In this study, all foot of participants is not symptomatic and obvious pes planus. However, because the result of the visual assessment is qualitative, it may not strictly distinguish a normal foot from one with pes planus. Therefore, foot posture may need to be assessed using a quantitative method such as the foot posture index [33].”

Reference 33 was added: Redmond AC, Crane YZ, Menz HB. Normative values for the Foot Posture Index. J Foot Ankle Res. 2008; 1:6.
I revised the sentence on page 7, line 130 to 131 as follows: “The present study was reviewed and approved by the ethical committee (No. 17575-150422) at our institution.”

Comment 4: When listing the sites for marker placement according to the Leardini protocol it would be useful to be more specific regarding their exact position at sites such as the metatarsal heads.

Response:

In reference to Leardini’s study, I revised the sentence on page 7, line 134 to 146 as follows: “…fixed to the right shank and foot at the most anterior prominence of the tibial tuberosity, most proximal apex of the fibula head, distal apex of the medial malleolus, distal apex of the lateral malleolus, Achilles tendon attachment, most medial apex of the sustentaculum tali, lateral apex of the peroneal tubercle, most medial apex of the tuberosity of the navicular, most lateral apex of the tuberosity of cuboid, dorso-medial aspect of the first metatar-so-cuneiform joint (first metatarsal base), dorso-medial aspect of the first metatar-so-pha langeal joint (first metatarsal head), dorso-medial aspect of the second metatarso-cuneiform joint (second metatarsal base), dorso-medial aspect of the second metatarso-pha langeal joint (second metatarsal head), dorso-medial aspect of the fifth metatarso-pha langeal joint (fifth metatarsal head), and most distal and dorsal point of the head of the proximal phalanx head of the hallux (Figure 1a, b).”

Comment 5: The manuscript states that the stance phase was analysed "according to previous studies" - what does this mean exactly?

Response:

This sentence means that the analysis interval in this study was only during the stance phase, same as the previous study.

I removed this sentence and revised the sentence on the page 10, line 188 to 189 as follows: “The analysis interval in this study was only during the stance phase, same as the previous study [13].”

Comment 6: It would be helpful to clarify why a treadmill was used when analysing gait. Why was each participant required to walk at 1.3 m/s specifically? Where does this methodology derive from and why did you choose this technique? Why did the study not analyse gait using level over ground walking at self-selected walking speed? Did this approach have any bearing on the results of the study i.e. could standardising spatial-temporal parameters have affected the underlying kinetics of the foot and hence affected coupling relationships?
Response:

A previous study (Dubbeldam et al, 2013, Gait Posture) reported that the coupling angle is sensitive to small angle changes. Thus, if subjects who walk fast presented, it is likely that the coupling angle between subjects would increase in variability. Thus, in this study, subjects with similar physical characteristics were gathered and the speed (1.3 m s⁻¹) was set by referring to the speed (1.2 to 1.35 m s⁻¹) reported in previous studies (Pohl et al, 2007, Gait Posture; Chang et al, 2014, J Biomech; Dixon et al, 2012; J Biomech). In fact, although it was confirmation of oral, speed was preliminarily confirmed to each participant that it is roughly equivalent to the usual walking speed. However, because gait analysis in previous studies (e.g. Leardini et al, 2007, Gait Posture) have performed walking at comfortable speeds, methods of this study were not common, and standardized spatial-temporal parameters may have affected results in this study.

I added the sentence to the methods section on the page 8, line 153 to 159 as follows: “…set to the walking speed of 1.3 m s⁻¹ because the coupling angle is very sensitive to small angle changes [3], and the difference in walking speeds among participants may increase variability of the coupling angle among subjects. Walking speed in this study was set in reference to the speed used in previous studies [5, 23, 24], and this speed was verbally confirmed to each participant that it is roughly the usual speed preliminarily.”

I also added the sentence to the limitation section on the page 20, line 377 to 382 is as follows: “Finally, the walking speed in this study was set to 1.3 m s⁻¹. While the speed of 1.3 m s⁻¹ was verbally confirmed with each participant that it is roughly equivalent to the usual walking speed, it may be different from the real usual walking speed. Also, because such standardized spatial-temporal parameters may have affected results in this study, walking speed may need to be set as a comfortable speed to each participant.”

Regarding the Background to the Study:

Comment 7: The document refers to previous research that has investigated coupling relationships between both adjacent and non-adjacent segments. Previous research has also explored the coupling relationships of segments rotating in opposing planes. In addition, although there is reference to more extreme abnormalities of foot posture i.e. pes cavus and pes planus, to strengthen the clinical applicability of this research it would be helpful to clarify explicitly what the rationale was for studying the adjacent segments and the rotation patterns chosen for this study?

Response:
I revised the sentences on the page 6, and line 100 to 114 as follows: “Dubbeldam et al. [3] investigated kinematic coupling between the hallux and rearfoot during walking. While the midfoot and forefoot are present between the rearfoot and hallux, they did not include midfoot and forefoot in kinematic coupling [3]. Hence, because midfoot and forefoot motion have not been understood, the mechanism with which the kinematic coupling between hallux and rearfoot occurs has not been understood. Also, a previous study [16] investigated the coordination between the adjacent rearfoot and midfoot and between the adjacent midfoot and forefoot. Their rotation patterns were analyzed in the same plane, and other studies [13, 17] have performed the same analysis. Furthermore, we have shown that kinematic coupling between the adjacent rearfoot and midfoot and between the adjacent midfoot and forefoot in the frontal plane during walking have stronger coupling (r = 0.84, r = 0.75, respectively) than that of the opposing plane [19]. Considering these previous studies, the present study investigated coordination between the rearfoot and midfoot and between the midfoot and forefoot in the same planes. The results of…”


According to journal’s editor, this research has been scheduled for publication in January/February 2018.

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Comment 8: The manuscript highlights the limitations of the Pearson's correlation technique and cross correlation. Although the limitations of these approaches are highlighted later in the document it might be useful to clarify more explicitly at an earlier stage within the document what the limitations of these techniques are.

Response:

I briefly summarized the original first paragraph in the introduction, and the limitation of correlation techniques was added to the first paragraph.

In consideration of your comment 9, I revised the sentences on the page 3, line 45 to 57 as follows: “Pathologies of foot structures, such as pes planus, pes cavus, and first metatarsaophalangeal joint stiffness, affect foot kinematics at the site of the impaired structure during gait [1,2]. Additionally, such localized abnormal motion of one segment may also influence kinematics in the other segment. Such information on kinematic coupling between segments is often quantified using the Pearson correlation-coefficient [3] and cross-correlation coefficients [4]. However, limitations of these techniques are that these only provide an indication of the temporal similarity between two angular waveforms throughout the stance phase [5]. For instance, while two angular waveforms undergoing similar directional movement patterns will produce a high correlation coefficient, they might have very different angular
amplitude. Hence, correlation methods cannot well understand angular amplitude parameters, such as peak values and range of motion.”

Comment 9: To the wider readership unfamiliar with this topic it would be helpful to explain why angular amplitude parameters are "not understandable"?

Response:

I added sentence the sentences on page 3, line 53 to 57 as follows: “For instance, while two angular waveforms undergoing similar directional movement patterns will produce a high correlation coefficient, they might have very different angular amplitude. Hence, correlation methods cannot well understand angular amplitude parameters, such as peak values and range of motion.”

Comment 10: The manuscript refers to reference 15 (Arnold et al., 2016) as also having investigated coordination patterns but did not explain the relevance of this work.

Response:

I removed the sentence “By using this classification of coordination patterns, a recent study [15] also investigated coordination among the rearfoot, midfoot, and forefoot during walking.”

I revised the sentence on the page 5, line 90 to 94 as follows: “While Arnold et al. [16] have categorized the coordination among the rearfoot, midfoot, and forefoot during walking between younger and older adults by using Chang’s classification method [13], this study provides no information on which segment is dominant.”

Comment 11: The manuscript refers to Cheng et al (2008) by comparing their results with those of Bosjen Moller (1979) but did not offer an explanation as to why these two studies may have provided differing results.

Response:

I revised the sentence on the page 5, line 78 to 83 as follows: “In contrast, other studies [14,15] have reported features of the stable foot during the push off as having an anti-phase pattern in rearfoot-forefoot coordination (rearfoot inversion and forefoot eversion). However, since this research does not use a modified vector coding technique, Chang et al. [13] pointed out that it oversimplifies the complexity of rearfoot and forefoot interactions.”
Regarding the Results and Discussion:

Comment 12: The manuscript presents a description of the coupling relationships between adjacent segments during the stance phase which show a predominance of midfoot motion. How do these results concur or differ with what is currently understood regarding foot motion during gait? This is an area which could be expanded further within the manuscript.

Response:

I added previous studies that are consistent or inconsistent with the results of this study.

In consideration of reviewer 2’s comment, I revised the sentences on the page 17, line 316 to 338 as follows: “Furthermore, the present study is the first to quantify the coordination among the rearfoot, midfoot, and forefoot during walking using a coordination pattern classification [17]. First, the present study showed results similar to in vivo measurement of the rearfoot, midfoot and forefoot during walking [31]. Buldt et al. [2] showed the range of motion of the rearfoot (9.2 degree) in the frontal plane during walking in a normal foot is greater than that of the midfoot (6.0 degree). However, because this range of motion assigns a single value for the whole stance same as the method of ratio, angular amplitude in time series cannot be understood. While previous studies [13, 17] have investigated intra-foot coordination during walking, it is different from the present study because midfoot segment was not included. To our knowledge, only Arnold et al. [16] have quantified the coordination among the rearfoot, midfoot, and forefoot during walking, which has shown that coordination between the rearfoot and midfoot was partly in phase during the early phase with young adults. This result was partially consistent with the findings of this study. However, this information being in phase does not show which of the angular amplitude of the rearfoot or the midfoot substantially moves. Conversely, the results in the present study demonstrated the rearfoot-midfoot coordination during the early stance was mainly in phase with distal dominancy. Further, midfoot-forefoot coordination were mainly in phase with proximal dominancy. Hence, this study provided both in phase and dominancy of the angular amplitude; the results showed that the midfoot dominantly everts, rather than the rearfoot and forefoot.”

Comment 13: Whilst the manuscript is careful to describe this research as a preliminary study, can we necessarily describe these results as being representative of normal foot function? These results are specifically representative of these data in these participants. Although presumably
asymptomatic, it is difficult to contextualise these results as the underlying foot posture characteristics of these participants are not quantified.

Response:

In consideration of comment 2, I added the sentence on the page 19, line 371 to 377 as follows: “Thirdly, the participants’ foot posture during standing was only visually inspected to eliminate obvious pes planus. In this study, all foot of participants is not symptomatic and obvious pes planus. However, because the result of the visual assessment is qualitative, it may not strictly distinguish a normal foot from one with pes planus. Therefore, foot posture may need to be assessed using a quantitative method such as the foot posture index [33].”

Comment 14: How can this research be used to inform the assessment of lower limb biomechanical function, clinical decision making and treatment interventions?

Response:

In the consideration of reviewer 2’s comment, I revised sentences to discussion as on page 18, line 339 to 359 as follows: “The results obtained in the present study may be considered as basic research knowledge with which future data can be compared. A previous study [28] suggested that the coordination patterns can be adversely influenced by pathology. Rodrigues et al. [10] showed that runners with anterior knee pain have different coupling angles between the internal rotation of the shank and eversion of the rearfoot compared to those without anterior knee pain, which suggests that this alternation is useful for the detection of injuries. A recent study [2] reported that the range of motion of the midfoot in the transverse plane during walking in patients with pes planus was significantly narrower than that in patients with pes cavus. Thus, in patients with pes planus, the coupling angles and coordination patterns in the transverse plane may differ from the results obtained from healthy participants in the present study. Hence, the results of this study could be used as data to distinguish the presence of injuries or abnormal inter-segmental foot motions such as pes planus. Additionally, Arnold et al [16] have shown that the coordination between the rearfoot and midfoot in the frontal plane during the early stance was more frequently in phase in older adults compared to younger adults; they have implicated that older adults are adaptive in their stability by reducing intrinsic foot mobility. Thus, this study will be comparable data to evaluate the stability of the foot. The results in this study may also be helpful in considering clinical assessment and treatment interventions.

Response for the Reviewer #2

☐ General:
Comment 1: The manuscript presents a description of rear, mid and forefoot motion during walking using standard experimental methods based upon reflective skin markers and a previously described model of the foot. The modified vector coding technique is a new means of describing coordination between these three segments. The study was well performed and the manuscript is clearly written. This reviewer has two concerns.

Comment 2: Firstly, it is unclear why only 10 subjects were included. As this is a non-invasive experiment with little or no risk to the subjects, it would appear advisable to include more subjects to increase the statistical power of the results. If a power analysis had been performed and 10 subjects could be shown to provide sufficient power then this should be presented. If not a decisive argument why only 10 subjects were included is requested.

Response:

The purpose in the present study was to quantify coordination among the rearfoot, midfoot, and forefoot in healthy subjects. It was not a study that performed statistical tests between two groups such as the comparison of a normal foot and pes planus. Therefore, this study did not perform statistical power. In fact, without using statistical power, previous studies (Leardini et al, 2007, Gait Posture; Caravaggi et al, 2010, J Biomech; Dixon et al, 2012, J Biomech) quantifying intra-foot kinematics with healthy subjects targeted only 10 subjects (non-invasive experiment). Moreover, research of coordination between lumbar and pelvis (Needham et al, 2014, J Biomech) and between the rearfoot and forefoot (Needham et al, 2015, Footwear Sci) also targeted healthy 10 subjects. In reference to these previous studies of quantifying coordination, this study decided on 10 subjects. Also, because obvious pes planus was expected by performing evaluation of the “too many toes” sign, we thought that the coupling angle variability (CAV) between subjects with a normal foot was within acceptable ranges. However, the CAV of the transverse plane is greater than that of the sagittal and frontal plane. Thus, we reconsidered that this study would include more subjects.

I revised sentences on the page 19, line 366 to 371 as follow: “Second, while subject numbers in this study determined in reference to previous studies [17, 28] quantifying intersegmental coordination in healthy subjects, the sample size is likely to be small. Actually, the CAV in the transverse plane obtained in the present study especially showed high values throughout the stance phase, and this result may be due to the influence of the sample size.”

Comment 3: Secondly, the use of skin markers severely restricts the possibilities of describing relative motion of intrinsic foot segments. Differentiating between midfoot and forefoot motion is difficult as it is not certain the skin is representing what the underlying bones are doing. See
specific point in methods below. These issues need to be addressed for the manuscript to be considered for publication.

Response:

As you say, since the use of skin markers is a disadvantage of the skin motion error, skin markers may be not track actual bone movements.

In consideration of your comments 10 and 18, we addressed this issue, and added sentences on page 19, line 360 to 366 as follow: “The present study has certain limitations. First, the major limitation of the present study pertains to the use of skin markers to track the underlying skeletal structure. While Leardini’s foot model [21] has been confirmed by validating the in vitro study, the validation of this model has not been investigated for in vivo study [32]. Thus, skin markers mounted on externally identifiable bony landmarks in the foot may not follow the underlying individual skeletal segments during running.”


Specific: Abstract:

Comment 4: L34-38: This is very vague and does not indicate that the authors have any concrete conclusions to draw from the study. What is a "more significant role"? It is obvious that the study contributed to the understanding of this coordination - but is that a conclusion? What is the purpose of comparing to patients with foot deformities? The conclusion needs to be rewritten with concrete, concise facts.

Response:

We reconsidered that this study cannot determine whether the midfoot plays a more significant role. Thus, I removed the following sentence: “This result may suggest that the midfoot plays more significant role than the rearfoot and forefoot during early stance.”

I concretely and concisely rewrote the purpose of comparing patients with foot deformities, such as pes planus.

I revised the sentence on the page 2 line 37 to 40 as follows: “The results of the present study can help in understanding the interaction of the intersegmental foot kinematic time series during walking. Additionally, the results could be used as data to distinguish the presence of injuries or abnormal inter-segmental foot motions such as pes planus. It may be considered as basic research knowledge with which future data can be compared.”
I also revised the conclusion in the same manner (refer to the response for comment 20).

Background:

Comment 5: L53: Here a study with bone-mounted markers is referenced, but there is no mention of what the study reported. Also, the studies with bone mounted markers should be described in a more detailed description of the limitation of skin markers (see General points).

Response:

In consideration of reviewer 1’s comment, I revised the first paragraph in the background and removed the reference about bone-mounted markers (reference 4). The limitation of skin markers in this study was added in the discussion (refer to the response for comment 3).

Comment 6: L87: Please clarify which study "the previous study" refers to. It is not clear whether this is 13 or 15.

Response:

I revised the sentence on page 5, line 84 as follows: “However, in the classification method of the study by Chang et al. [13]…”

Methods:

Comment 7: L105: See comment under General points concerning the low subject number.

Response:

I revised the low subject number. Please refer to the response for comment 2.

Comment 8: L106: Please rewrite this sentence (in parentheses).

Response:

I revised the sentence on page 7, line 121 to 127 as follows: “Ten healthy male subjects (height 171.7 ± 5.0 cm; body mass 64.8 ± 8.0 kg; age 22.6 ± 3.1 years), without pes planus or other pathologies volunteered to participate in this study. The pes planus presents as an abduction of the forefoot during standing. Thus, the foot posture was evaluated using the “too many toes” sign [20], and participants who had a positive evaluation (the forefoot in the abduction position when
viewed from the back in the standing position) were excluded. By using this method, obvious pes planus was excluded.”

Comment 9: L109: Please state which review board this was.
Response:
I revised the sentence on page 7, line 130 to 131 as follows: “The present study was reviewed and approved by the ethical committee (No. 17575-150422) at our institution.”

Comment 10: L118: See General comments. It is positive that the validity and reliability are addressed, however the study by Leardini et al did not perform a validation. They did compare their results to those of an in vitro (cadaver) simulation of walking, which however has its own methodological problems. The model used in this study has therefore not been validated against the gold standard bone-pin method. This is a major limitation of this study and must be clearly addressed.
Response:
As you say, Leardini and colleagues have not investigated validation compared to the in vivo study. I removed “The validity” in the sentence, then revised on the page 8, line 147 to 148 as follows: “The repeatability [22] of this model has been confirmed in previous studies...”

Additionally, in consideration of your general comment and comment 18, I added a sentence about validation in this model to the limitations (refer to response to comment 3).

Comment 11: L130: Change running to walking.
Response:
I revised from running to walking (page 9, line 163).

Results:
Comment 12: The text describing the coordination patterns is very lengthy and repetitive, apart from different numbers. A suggestion would be to include the numbers in the graphs of figure 5 and remove all this text. Please consider this.
Response:
I revised the results section on the page 13 to 15 (line 244 to 280). Because the sentence is long, please refer to the results section in the manuscript. Additionally, proportion of coordination in text included Figure 5.

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Discussion:

Comment 13: L309: Repetition; please correct.
Response:
I removed the repetitive sentence.

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Comment 14: L310: The study by Lundgren et al could be used as a comparison of many of the rotations presented here with a bone-pin study. This would not be a real validation, but at least an indication of similar results.
Response:
I revised the sentence on the page 17, line 318 to 319 as follows: “First, the present study showed results similar to in vivo measurement of the rearfoot, midfoot and forefoot during walking [31].”

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Comment 15: L315: Remove text in parentheses - this reference has already been made.
Response:
I removed the text.

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Comment 16: L320: Why should these data be preliminary? If it is due to the low subject number then I suggest the data are not presented until more subjects are tested (see General point).
Response:
We reconsidered that our data is not preliminary data but rather basic research knowledge with which future data can be compared.

In the consideration of reviewer 1’s comment, I revised the sentence on page 18, line 339 to 359 as follows: “The results obtained in the present study may be considered as basic research knowledge with which future data can be compared. A previous study [28] suggested that the
coordination patterns can be adversely influenced by pathology. Rodrigues et al. [10] showed that runners with anterior knee pain have different coupling angles between the internal rotation of the shank and eversion of the rearfoot compared to those without anterior knee pain, which suggests that this alternation is useful for the detection of injuries. A recent study [2] reported that the range of motion of the midfoot in the transverse plane during walking in patients with pes planus was significantly narrower than that in patients with pes cavus. Thus, in patients with pes planus, the coupling angles and coordination patterns in the transverse plane may differ from the results obtained from healthy participants in the present study. Hence, the results of this study could be used as data to distinguish the presence of injuries or abnormal inter-segmental foot motions such as pes planus. Additionally, Arnold et al [16] have shown that the coordination between the rearfoot and midfoot in the frontal plane during the early stance was more frequently in phase in older adults compared to younger adults; they have implicated that older adults are adaptive in their stability by reducing intrinsic foot mobility. Thus, this study will be comparable data to evaluate the stability of the foot. The results in this study may also be helpful in considering clinical assessment and treatment interventions.”

Comment 17: L340: Including only healthy subjects is not a limitation. These data are required for basic research knowledge with which future data can be compared.

Response:

I removed the limitation about healthy subjects.

Comment 18: L340: The major limitation that skin markers were used needs to be clearly presented and the potential effect on the results described. At present it is not even mentioned.

Response:

In consideration of your general comment and comment 10, I added the sentence about validation in this model to limitation (refer to response to comment 3).

Comment 19: L344: Exactly. If the authors can not explain why only 10 subjects were included, they should test more before presenting this study.

Response:

In the consideration of your general comment, I added sentence about subject numbers to limitation (refer to response to comment 3).
Comment 20: L360: Better conclusion here than in the abstract, but once again, what is a "more significant role"? Also the last sentence does not state anything useful.

Response:

We reconsidered that this study cannot determine whether the midfoot plays a more significant role. Thus, I removed the following sentence: “This result may suggest that the midfoot plays a more significant role than the rearfoot and forefoot.”

I rewrote the purpose of comparing to patients with foot deformities such as pes planus. I revised the sentence on the page 20, line 389 to 398 as follows: “The present study elucidated that the midfoot dominantly everts, rather than the rearfoot and forefoot, in the early stance. The results of the present study can help clarify the interaction of intersegmental foot kinematic time series during walking. The coordination patterns can also be adversely influenced by pathology [3,28], and the change of intersegmental coordination is related to injuries [10]. Thus, the results of this study could be used as data to distinguish the presence of injuries or abnormal inter-segmental foot motions such as pes planus. It, therefore, may be considered as basic research knowledge with which future data can be compared.