Author's response to reviews

Title: Different ankle muscle coordination patterns and co-activation during quiet stance between young adults and seniors did not change after a bout of high intensity training

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Author's response to reviews: see over
Title: “Different ankle muscle coordination patterns and co-activation during quiet stance between young adults and seniors did not change after a bout of high intensity training”

BMC Geriatrics

Dear Dr. Lamoth, Dear Reviewers

We are thankful to have had the opportunity to re-submit a carefully revised version of our paper entitled “Different ankle muscle coordination patterns and co-activation during quiet stance between young adults and seniors did not change after a bout of high intensity training”. Dr. Lamoth offers this possibility since an extensive revision is required. According to your suggestion we were able to respond to the three reviewers concerns in detail. Please find below a very carefully prepared point to point reply following each raised point. Would you please send the article to the former reviewers so that they can judge the answers given to each points. We would like to thank all Reviewers for their efforts put in the present manuscript. Their detailed comments remarkably improved the quality of the manuscript. We now feel that the manuscript has a considerable impact to the journal. Please find a point-by-point response to the Reviewers’ comments below.

Lars Donath in the name of all coauthors

Former reviewer contact details including their reports (the comments of the reviewers are also included in our point to point reply)

Referee #1: Stephane Baudry, University of Brussels: Email: sbaudry@ulb.ac.be
http://www.biomedcentral.com/imedia/6486274914012550_comment.pdf

Referee #2: Peter Wolf, ETH Zürich, Email: peter.wolf@hest.ethz.ch
http://www.biomedcentral.com/imedia/1630677928140378_comment.pdf

Referee 3: Stanislaw Solnik, Pennsylvania State University, Email: solnik@psu.edu
http://www.biomedcentral.com/imedia/5447083111411201_comment.pdf

Point-to-point-reply

Reviewer #1: One of the main issues of the manuscript is the lack of normalization for the EMG. Although the different ratios changed significantly, the correlation between TA ratio and postural sway cannot per se be interpreted as a true association between the amount of coactivation and postural control. As raw EMG is influenced by numerous factors independent of muscle activity beneath the electrodes (position relative to the tendon, subcutaneous tissue, etc... ), comparing two raw EMG signals through a ratio does not provide relevant information. This can be even more relevant
when comparing different sample of the population (young vs. elderly) that do not have the same amount of subcutaneous tissue. This is also an issue when considering EMG activity after a fatiguing exercise. Indeed, in this latter case, in addition to the nonlinear summation of motor unit action potentials in an interference EMG signal (Keenan et al. 2005) and the progressive decrease in muscle activation (Dideriksen et al., 2011), the slowing of the action potential conduction velocity of individual motor units during fatiguing contractions increases the amount of amplitude cancellation (Keenan et al. 2005). As a consequence, the use of non-normalized EMG activity to compute coactivation ratio may be misleading (Day and Hulliger 2001; Keenan et al. 2005). Finally, the lack of EMG normalization for single EMG and coactivation ratio impedes the ability to extract a clinical relevance as it is not possible to quantify the amount of EMG activity (expressed relative to maximal EMG, for example) and the difference between young and elderly adults in the level of coactivation. The authors should provide normalized EMG, and coactivation ratio computed from the normalized EMG to improve the relevance of their data.

Reply 1: We absolutely agree, normalization of absolute EMG amplitudes is a very important issue. Numerous methods are presented and their pros as well as cons are well known (Burden 2010, *J Electromyogr Kinesiol*). Regarding the comparison of two or more raw EMG signals through a relative measure serving as a computed ratio and the relevance of such information we would like to respectfully attenuate the reviewers’ comment on this. Presenting (normalized) EMG amplitudes as percentage activation (relative contribution) in order to represent muscular coordination patterns is accepted in the scientific community (Edgerton et al. 1996, *Med Sci Sports Exerc*; Hug 2011, *J Electromyogr Kinesiol*). More applied studies employing these analyses are available meanwhile (Kurz et al. 2012, *Haemophilia*). Indeed, amplitude cancellation is an important topic. To the best of our knowledge above mentioned drawbacks are based on data collected in cats (Day & Hulliger 2001, *J Neurophysiol*) or within experimentally simulating approaches (Keenan et al. 2005, *J Appl Physiol*).

We feel that the transferability on human data seem to be restricted and should be discussed very carefully.

Non-normalized co-activation indices (based on raw EMG data) are also commonly used and reported in the literature (e.g. van Dieën et al. 2003, *Spine*). We feel the point that the absence of a maximal or reference EMG exertion hinders the quantification of the magnitude of the EMG activity is fairly correct. But we would like to take distance to the statement that a normalized EMG per se expressed relative to maximal EMG is the only way to reveal clinically relevant information. The presented activation ratio (percentage activation) represents a relative intra-individual contribution to a summed activation. Of course this result does not replace the magnitude of EMG activity (neither as an absolute nor as a relative quantity). We solely aimed at addressing a relative contribution, not at the magnitude of activity. We hope that you can go along with our points and your concerns have been appropriately put into context.

Specific Points:

Reviewer #1: Abstract, Line 39. SLEO should be defined.

Reply 2: changed accordingly

Reviewer #1: Methods, Line 104. A 10-s epoch to characterize CoP displacement is rather low and should have affected the appropriateness of the CoP measures (Carpenter et al. 2001).
Reply 3: We appreciate the comment of Reviewer #1 on the rather short investigation window to depict the CoP path length parameter. We are totally aware of transient signal shift when applying shorter data acquisition recordings. However, the study of Carpenter and colleagues (2001) was measuring young adults (< 35 years) during a bipedal standing task. With respect to ecological validity and comparability to other balance studies in seniors, we refrained from data collection duration of more than 10 seconds (e.g. MacRae et al. 1992, J Orthop Sports Phys Ther). Many seniors are not able to stand longer than 10 seconds during single limb stance. To achieve more consistency within the results, we decided to use the shorter time period. Although we were aware of this limitation, it was not considered within the original study design.

Discussion, Study limitations: Although shorter data collection periods (<20 seconds) can yield a transient time signal shift that may affect COP assessment [Carpenter et al. 2001], we refrained from collecting longer time frames since a majority of seniors are unable to stand longer than 10 seconds [MacRae et al. 1992].

Reviewer #1: Results, The authors should indicate whether CoP parameters differed between groups.

Reply 4: In accordance to the third Reviewer, we appreciate this attentive annotation. We now conducted the respective analyses. No group differences were found (see figure below). We added this information to the result section. See also Reply 25.

Results: We did not find differences of COP path length displacements between groups from pre to post after conducting the acute HIIT intervention for DLEC (0.30<p<0.80, $\eta_p^2<0.04$) and SLEO (0.38<p<0.98, $\eta_p^2<0.03$).

Reviewer #1: Discussion, Line 187. When considering age differences in EMG activity of leg muscle during upright standing, the authors should also consider the force capacity of these muscles (Billot et al. 2010).

Reply 5: You are completely right. Since our study did not carry out force measurements it is fairly hard to consider. We agree with Billot et al. (2010) that significantly lower muscle volumes of tibialis anterior compared with triceps surae group (Fukunaga et al. 1996, J Appl Physiol) might induce mechanical imbalance. However, Billot and colleagues (2010) estimated co-activation in a completely different way. Although a direct comparison of the two different methods is hard to conduct, we appreciate the transferability. Thus, we amended our discussion section according to your valuable comment.

Discussion: Although ageing is accompanied with muscle atrophy, force generation capacity of plantar flexors seems to be unaffected (Kubo et al. 2007, J Gerontol A Biol Sci Med Sci).
However, due to basically lower muscle volume of TA compared with triceps surae muscle group (Fukunaga et al. 1996, J Appl Physiol) even a high CAI does not necessarily reflect a balanced mechanical state (Billot et al. 2010, Muscle Nerve).

Reviewer #1: Discussion, Line 226. The meaning of these two sentences are not clear.

Reply 6: This section is indeed confusing and does not contribute to a better understanding of the co-activity interplay. We deleted this section carefully and rephrased the parts before in order to deliver a better understanding.

Reviewer #2: In general, a well written paper with a comprehensive discussion. However, the reviewer would like to suggest some changes and requests some additional information so that almost all readers can easily read the paper. Details are listed below:
Major Compulsory Revisions

**Reviewer #2:** Abstract / Introduction: It is argued that young adults and seniors use different strategies to adjust for increasing body sway during quite standing which might be caused by an altered ankle muscular pattern. Therewith, one would expect that the different strategies such as hip/ankle were also investigated in the present study, but they were not. Consequently, (in that way) the justification of the study is questionable.

**Reply 7:** We absolutely agree with you in that point. We now attenuated this speculative conclusion. We merely focused on relative ankle muscle coordination and indirectly concluded some aspects concerning the hip. We now stated that we are not able to address the hip contribution with certainty in the limitation section.

**Discussion:** Moreover, kinematic measures were not included. Also EMG applications of the trunk muscles were not employed. Thus, we can only speculate on potential underlying postural strategies. Further cross-sectional and longitudinal studies need to be conducted in order to address changes in trunk muscle activity and kinematics in more frail subjects before and after balance training interventions.

**Reviewer #2:** Methods – Testing and analyses: The EMG outcome measure is not clearly described (lines 115 onwards). After rectification and smoothing by a moving RMS, it is not clear whether the integral, mean activity or something else was considered for further analysis. Authors may consider to indicate that in line 116 as well when presenting the equation.

**Reply 8:** For more transparency we provided the equation for you. We refrained from putting the formula into the manuscript due to the main readership of BMC Geriatrics.

The signal was smoothed with a root mean square (RMS) value applied with a sliding rectangular (box car) window, with each window of data calculated according the equation below:

$$\text{RMS}(t) = \sqrt{\frac{1}{T} \sum_{i=1}^{T} w_i(t)x_i^2(t)}$$

where $x_i(t)$ is the $i^{th}$ sample of the signal centered around $t$ as seen through the window $w_i(t)$, $t$ is the number of samples the analysis window moves (100 samples overlap), and $T$ is the window length (200 samples or 0.2 seconds). Applied to our signals (10 seconds) a total of 99 RMS values represented the envelope and were averaged to obtain single value activation magnitude, separately for each muscle and condition.

**Methods, Testings and analyses:** Electrical activity was calculated with a moving root mean square (RMS) window of 0.2 s resulting in a total of 99 RMS values representing the envelope. Mean activity of the envelope was considered for further analysis, separately for each muscle, condition and trial.

**Reviewer #2:** Methods – Testing and analyses: Please also clarify your co-activation index – it is not clear on which metric this one is based and why it was calculated in that way. The term might also be
misleading as a few people may think of a simultaneous activity in different muscles (thus, on-off / timing is important) rather than integral/mean/max of different muscles.

Reply 9: We added the respective information. Due to the nature of the performed tasks we assumed a nearly constant activation level of the antagonists. Thus the mean activation over the 10 seconds acquisition time was used. The factor “2” has been introduced for plausibility reasons. Neglecting the factor two would lead to maximal co-activation indices around 0.5. In turn, considering the factor “2” provoke plausible co-activation indices around 1.0.

Methods, Testings and analyses: This equation assumes that TA is acting as an antagonist. Through this CAI a relative measure (arbitrary units, 0 indicating no co-activation) of TA contribution to total activation of both ankle muscles during the standing is provided.

Minor Essential Revisions

Reviewer #2: Introduction, second paragraph: Please reorder/rephrase paragraph so that transition from paragraph is clearer – e.g. authors may skip the first sentence, as the second one is better in line with the last sentence of the first paragraph. Please also think of a reduction of probably equivalent terms (neuromuscular adaptations of ankle muscles, ankle muscle co-activation, agonist muscle co-activation, ankle muscle coordination pattern). In addition, after this paragraph it is not clear why it is important to look at ankle muscle activity after intense exercise training. What is expected? Later, in the discussion, expectations are much better described and underpinned by literature – this was expected to be read here by the reviewer.

Reply 10: Good recommendation. Thanks. The Introduction section was changed. Please refer to Reply 26 of the third Reviewer, who requested to justify the motivation behind the design of the study. According the rationale to look at ankle muscle contribution after intense exercise training we refer to Reply 24 of the third Reviewer, who also was in line with your claim.

Introduction: Although muscle activity, postural control strategies (e.g. hip vs. ankle) and neuromuscular adaptations to balance training were previously examined in seniors (Laughton et al. 2003, Gait Posture), age-related differences of ankle muscle coordination and ankle-co-activation patterns have not yet been cross-sectionally examined during resting states and following an intense bout of aerobic exercise training. Several studies indicated that aerobic exercise training lead to transient impairments of postural control (Donath et al. 2013, Eur J Appl Physiol; Donath et al. 2014, Gerontology, Stemplewski et al. 2012, Arch Gerontol Geriatr). It is not clear to date whether underlying changes of muscle coordination account for these increases of postural sway.

Reviewer #2: Methods, first paragraph:
- Please try to avoid the plus minus sign or explain (and use it consistently either with or without spaces.
- Please introduce HRmax (although pretty clear :-))
- Please add the information in brackets after local ethics committee

Reply 11: We changed the presentation of the standard deviations and added the respective information.

Reviewer #2: Methods – Testing and analyses:
- Please replace KIS with Kistler
- Sampling rate is usually reported in Hz, and I guess, signals were low pass filtered afterwards (sounds a bit uncommon to mention that in the same sentence).
Please introduce sEMG (it’s clear to the reviewer, however, from a scientific writing point of view – you may also skip this abbreviation as not needed anymore in the paper).
- It is suggested to use the singular when reporting on further signal processing as mentioning „filters“ suggest the use of a few filters – however, I guess, just one bandpass filter was applied.
- line 116: one bracket is missing
- line 119: SO => SOL.

Reply 12: KIS was replaced by Kistler. We reworded the sentence accordingly. Actually we applied two filters on SEMG data: a high-pass filter (20 Hz) and a low-pass (400 Hz) filter. Thus, according to your comment and for more clarity we reworded this sentence. SEMG was deleted, one bracket was added, SO was replaced by SOL.

Methods: Data were collected for 10 s at 40 Hz and analysed off-line using a low pass cut-off frequency of 10 Hz (Donath et al. 2012, Gait Posture). [...] After correcting possible offsets, removing 50 Hz and odd-numbered harmonics of the signals, a 20 Hz high-pass and a 400 Hz low-pass filter were applied, respectively. (Kurz et al. 2012, Haemophilia)

Reviewer #2: Methods – Acute Intervention:
- Please clarify testing procedure – does this refer to pre-post testing (which, actually, was not introduced before)?

Reply 13: This is indeed confusing. We rephrased the first two sentences and now feel that this section can be better understood.

Methods: Four consecutive high-intensity intervals of 4 min duration at a target exercise intensity of 90 to 95% \( HR_{\text{max}} \) were completed on the treadmill. Outcome variables were computed before and after the acute exercise intervention.

Reviewer #2: Results:
- Third sentence: p-values are related to pre/post or both? In addition, link to Figure 1 is not clear, in particular when interactions are mentioned.
- A positive correlation was found for AR – due to the type of calculation of AR, I would also expect a negative one... nothing observed?

Reply 14: Pre vs. post (as introduced in the statistics section). As indicated in the Results section: “For the remaining muscles [for SLEO] and for DLEC no such significant associations were found (-0.13 < r < 0.23; 0.34 < p < 0.78).” Due to the type of AR calculation one might expect also a negative one. The variation based on the intra-individual contributions could explain the one-directional observation.

Reviewer #2: Discussion, second paragraph:
- Where is the „summed mean difference“ shown? In addition, the first sentence should be rephrased (a passive verb might be more appropriate – xy decreased at rest => xy was decreased by x% at rest) to make it clear.
- line 233/234: The reviewer did not understand the message of that sentence.
- line 235: ... and are not affected by ... => even though baseline variability was high?

Reply 15: Thanks for these recommendations. We adapted this part accordingly. For more clarity, we deleted this part. The summed mean difference has been calculated roughly by head in order to put it in perspective for the readership. However, this point does not deliver further detailed information that need to be discussed. In contrast, we inserted a relevant sentence here that points to the fact that higher tibial AR were mainly caused by decreases of planter flexion muscle activity.
Discussion: Merely considering percentage contribution does not clarify the origin of activity changes. Thus, tibialis AR increases might be a result of increased TA activity or, in turn, decreases of plantar flexor activity.

Reviewer #2: Conclusion:
- line 244f: body sway is neither linked to modulated ankle muscle coordination pattern nor to co-activation.
- Merge sentences in line 245 and 246.

Reply 16: Changed accordingly.

Conclusion: Although several studies revealed increases of postural sway after submaximal cycling (Stemplewski et al. 2012, Arch Gerontol Geriatr) or exhaustive walking (Donath et al. 2013, Eur J Appl Physiol) in seniors, an increased exercise-induced body sway is neither linked to modulated ankle muscle coordination pattern nor co-activation. Although this finding might be of relevance for balance training programs in seniors, training recommendations from young adults are not necessarily applicable to seniors and vice versa.

Reviewer #2: Figure captions:
- Figure 1: left/right panel => top/bottom?

Reply 17: thanks, changed accordingly

Reviewer #2: Figure 1
- Please indicate more clearly which axis belongs to which muscle
- What is represented by the provided delta? Values seems not to represent the mean difference between adults and seniors (e.g. when looking at SLEO pre, TA, 20%)

Reply 18: thanks, changed accordingly for more clarity (see also Reviewer #3).

Reviewer #2: Figure 2
- dashed line in legend should also be visible in plot

Reply 19: thanks it is now visible.

Reviewer #2: Figure 3
- A & B and C & D might be merged to one figure (or to A, and B, as pre/post are provided).

Reply 20: We removed A, B, C, D for less confusion

Reviewer #2: Discretionary Revisions
- line 115: electrical activity => muscle activity
- line 116: one bracket is missing
- line 218: Please replace This in „This seems to be...“ to clearly indicate to what you are referring to.

Reply 21: Changed accordingly.

Reviewer #2: The term „pattern“ can be misinterpreted. Here, the authors refer to the activation of a single muscle in relation to the overall activation of all muscles investigated. However, a muscle activation pattern might also be interpreted as the development of the muscle activity over time (in
particular, when used in combination with „coordination“ and when „occurrence“ is mentioned as well).

**Reply 22:** We left “pattern” since many groups use this term in this circumstance.
Reviewer #3: Authors investigated possible effect of one bout of high intensity interval training (HIIT) on the activity of major ankle muscles of young and elderly subjects during quiet standing. The study was motivated by the fact that age-related reduction in postural stability is manifested by altered muscle activation patterns and increased muscle co-activation. Authors hypothesized that one bout of exercises (i.e., HIIT) will amplify age-related alteration of muscular activity in elderly subjects while standing. Authors anticipated that results of this study would have a clinical significance in helping to develop future training regimes to improve balance in the elderly population.

This paper addresses an issue that could definitely be of interest to the readers of the journal. The topic is important in the field geriatrics and potentially has a practical application; however, several experimental design and methodological shortcomings limit potential significance of the results.

Reply 23: We appreciate this initial evaluation of the study outline. We tried to work on the points you addressed adequately below.

Major comments:

Reviewer #3: My first concern is that it is not clear what scientific question/questions motivated this study. In the Introduction, authors introduce the idea of balance training in seniors, but its role on improving postural stability is not well explained. Also, it is not clear how an acute response to the single bout of HIIT relates to the balance training. The Introduction feels very general and needs a better justification on why was the HIIT used in this study?

Reply 24: We did not intend to draw attention to balance training and its consequences for postural stability. We aimed at investigating whether aging lead to different coordination patterns in adults and seniors. We further wanted to address if these changes persist after an intense bout of aerobic exercise. However, we totally agree with you on the lacking study outline. We now improved the introduction accordingly and have the impression that the study and propose are better outlined.

Introduction: Although muscle activity, postural control strategies (e.g. hip vs. ankle) and neuromuscular adaptations to balance training were previously examined in seniors (Laughton et al. 2003, Gait Posture), age-related differences of ankle muscle coordination and ankle-co-activation patterns have not yet been cross-sectionally examined during resting states and following an intense bout of aerobic exercise training. Several studies indicated that aerobic exercise training lead to transient impairments of postural control (Donath et al. 2013, Eur J Appl Physiol; Donath et al. 2014, Gerontology, Stemplewski et al. 2012, Arch Gerontol Geriatr). It is not clear to date whether underlying changes of muscle coordination account for these increases of postural sway. Although it seems well known that increased antagonist muscle co-activation in the elderly provides mechanical stability via stiffening joints and reducing degrees of freedom during balance tasks during several standing and walking tasks (Laughton et al. 2003, Gait Posture; Tucker et al. 2008, Hum Mov Sci), these indices were not yet addressed after intense exercise.

Reviewer #3: My second concern is that authors do not provide any outcome measures that would describe the performance of the quiet standing in both age groups. Authors computed the path length of the center of pressure (COP) but they do not report these results. Therefore, it remains unknown if elderly subjects had increased postural sway when compared to young subjects, or more important, did HIIT exercise affected the postural sway in both age groups?

Authors do not report EMG results of the individual muscles. Was there any change in muscle activity after a bout of HIIT? Did HIIT altered activity of some muscles more than other muscles? Authors use activation ratios to study ankle-muscle coordination patterns. These ratios represent percentage activity of one muscle of the overall activity of the recorded muscles. Providing only results of activation rations is not sufficient to understand the effects of HIIT on the muscle activity. For
example, is the increased activation ratio of the TA in elderly group a result of increased activity of this muscle alone or decreased activity of other muscles (i.e., SOL or GM)?

Lastly, authors did not measure any kinematic variables during the tests. Authors indeed acknowledged the lack of the kinematic measurements as the limitation of the study; however, the kinematic data are necessary to assure that subjects from both age groups maintained the same standing posture during data collection. Results of this study could be affected by the different standing postures between young and elderly subjects. For example, if young subject were leaning forward more than elderly subject then it would partly explain an increased soleus activity in young subjects to generate plantar flexion moment to compensate for the shift of the COM in the forward direction.

**Reply 25:** Thank you for this attentive annotation on the necessity of reporting COP results. Please refer to Reply 4 (first Reviewer). The respective analyses were conducted and are reported in the Results section. No group differences were found (see figure attachment to Reply 4).

You are completely right that without the respective magnitude of activity the amplitude ratios are hardly interpretable. In our case it is evident that the activation ratio of the TA in the elderly is a result of mainly decreased activity of the plantar flexor muscles (see table). For you we provide the absolute activation of all investigated muscles. However, we refrain to insert the Table in the main manuscript in order to focus on the relative contribution and in order not to confuse the reader.

**Table 1.** Absolute EMG values of ankle muscles during single limb stance with eyes open (SLEO) before (pre) and after (post) HIIT by age-group. Values are median (inter-quartile range) and minimum-maximum [unit: mV]

<table>
<thead>
<tr>
<th>Muscle</th>
<th>Adults (n=20)</th>
<th>Seniors (n=20)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>pre</td>
<td>post</td>
</tr>
<tr>
<td>tibialis anterior</td>
<td>66 (102)</td>
<td>8-377</td>
</tr>
<tr>
<td>peroneus longus</td>
<td>145 (199)</td>
<td>14-869</td>
</tr>
<tr>
<td>gastrocnemius medialis</td>
<td>97 (181)</td>
<td>20-768</td>
</tr>
<tr>
<td>soleus</td>
<td>153 (130)</td>
<td>57-369</td>
</tr>
</tbody>
</table>

According to your comment on the lack of kinematic data: The standing posture of all study participants was assured by the same experienced investigator. Further, the requested posture (slightly bent knees) together with the loss of spinal flexibility with aging at least partly suggests that subjects prevent from an active forward lean of the body. This is a very common procedure to obtain standing balance data. Moreover, they were standardized instructed. We now included the instruction scheme more detailed. We refrained from collecting kinematic data since they are known to be very unreliably with huge inter-individual changes, even during upright stance.

**Discussion:** Merely considering percentage contribution does not clarify the origin of activity changes. Thus, tibialis AR increases might be a result of increased TA activity or, in turn, decreases of plantar flexor activity.

**Reviewer #3:** The motivation behind the design of the study is not well justified. Please explain why one- and two-leg standing postures where used in this study. Moreover, why subjects had eyes closed for double leg condition and eyes opened for the single leg condition?

**Reply 26:** Previous investigations in our lab proposed a progressively applied sequence of balance exercises (Muehlbauer et al. 2011b, *J Strength Cond Res*). Double limb stance with closed eyes and single limb stance with opened eyes are commonly applied during static balance testing. We have chosen these tasks since a bulk of comparable data already exist on these task. Moreover, these tasks
are feasibly applicable during laboratory and field conditions with providing higher ecological validity. Corroboratively, both tasks deliver different modifications of sensory afferents (e.g. visual deprivation) and altered mechanical constraints (base of support) that affect postural control system. Further, the employed tasks should be adequately challenging on the one side and feasible for older adults on the other hand. Concerning our motivation behind the design of the study we added the following paragraph:

**Introduction:** Postural sway serves as an appropriate measure to examine postural control under static balance conditions. Thereby, single and double limb standing have been frequently applied (Donath et al. 2012, Gait Posture). The base of support and different sensory conditions can be modified in order to provide adequate progression (Muehlbauer et al. 2011b, J Strength Cond Res). Many studies indicated that aging lead to declines in static postural control under various conditions (Muehlbauer et al. 2012, Gerontology).

Minor comments:

**Reviewer #3:** L35: Please introduce muscle abbreviations before they are used.

**Reply 27:** changed accordingly

**Reviewer #3:** L38: Similarly, please introduce the SLEO abbreviation.

**Reply 28:** We defined SLEO as recommended. See also Reply 2 to Reviewer #1.

**Reviewer #3:** L49: According to the journal standards this section should be named 'Background'.

**Reply 29:** Thank you for this important advice. We renamed the mentioned section.

**Reviewer #3:** L96: Is 'XXX' a typo?

**Reply 30:** We changed this since institutional blinding seems not necessary.

**Reviewer #3:** L99: Please explain which leg subjects used during single-leg test? Was it dominant or non-dominant limb?

**Reply 31:** The subjects used their dominant limb as identified by means of the lateral preference inventory (Coren 1993, Bull Psych Soc). The respective information is already given.

**Reviewer #3:** L103: Why were subjects asked to bend their knees during quiet standing?

**Reply 32:** The used setup is based on standardized postures performed long lasting in our laboratory. We would like to emphasize that the leg was only slightly bended. The executed tasks are based on the figure/ picture exemplified in Muehlbauer and colleagues (2011b, J Strength Cond Res).

**Reviewer #3:** L114: Two best trials were used for further analysis. What constitutes a good trial?

**Reply 33:** This was wrong. We would like to refer to the 3rd and 4th line of the “Testings and analyses” section. We conducted the same analyses for the EMG signals. This part has now been changed accordingly.
Reviewer #3: L146: Authors report results of the correlation analysis, but it is not introduced in the Method section.

Reply 34: Thank you for this important remark. We amended the Methods section with the respective content.

Methods, Testing and analyses: Correlations between total COP path lengths and activation ratios were examined using Pearson’s product-moment correlation.

Reviewer #3: Figure 1. Please explain the meaning of the 'delta' symbol?

Reply 35: The ‘delta’ symbol is now explained in the figure caption.