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On "Comparison of a laboratory grade force platform with a Nintendo Wii Balance Board on measurement of postural control in single-leg stance balance tasks"

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accurate for quantifying CoP trajectory, and overall amplitude and velocity during single-leg stance balance tasks."

Conflict of interest statement

The authors have no affiliation with any of the products and companies mentioned in this document. However, Guido Pagnacco and Elena Oggero are affiliated with Vestibular Technologies, LLC., a registered medical device manufacturer of force platforms and devices for balance testing.

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Response to Letter to the Editor: On "Comparison of a laboratory grade force platform with a Nintendo Wii Balance Board on measurement of postural control in single-leg stance balance tasks" by Huurnink, A., et al. [J. Biomech. 46 (2013) 1392–1395]: Are the conclusions stated by the authors justified?

We thank Pagnacco and colleagues for their interest in our work, but we have to disagree with their conclusions. Below we will argue that the rhetorical question in the title of their letter can be answered in the affirmative. Pagnacco and colleagues raise concerns about the methodology of our study, and even claim that the conclusions offered are misleading and unsupported. We agree that the two devices (laboratory grade force plate (FP) and Wii Balance Board (WBB)) cannot be considered interchangeable, but this is not what we state in our paper. In fact, the limitations of the WBB are extensively presented and discussed in the paper. We did, however, conclude that the WBB is sufficiently accurate to be used for sway measures of single-leg stance, as used in the field of sports medicine and sports rehabilitation. Although not exactly equal, the center of pressure (CoP) measurements with the WBB were shown to provide very good proxies for parameters derived from more accurately measured CoP data. For more information about the specifications and durability of the WBB, we would like to advise the recent work of Bartlett et al. (2014).

To support our conclusion, we have presented comprehensive results on the agreement between the WBB and FP in 420 trials. In our work, we have performed two analyses to evaluate the agreement between WBB and FP: (1) a comparison of CoP trajectories (time series), and (2) a comparison of commonly used parameters calculated from these trajectories (CoP path velocity, mean CoP sway).

(1) To evaluate the similarity of CoP trajectories, both time series data sets (WBB and FP) were compared using the Pearson's correlation coefficient (r), and the root-mean-square (RMS) technique. In contrast to Lee et al. (1989) (cited in the letter by Pagnacco et al.), the study by Derrick et al. (1994) (cited in our paper) comprehensively investigated the pros and cons of the use of r to assess similarity between two time series. They showed that r is sensitive to differences in both amplitude and timing. Advantages are that it is easy to use and can be used to evaluate the entire curve. Derrick et al. (1994) concluded that a very high correlation is always indicative of temporal similarity, however, a low correlation does not guarantee a lack of temporal similarity. Obviously, a high r does not guarantee absence of amplitude differences. This is why we also calculated the root-mean-squared differences between CoP trajectories of FP and WBB. Since our Pearson's correlation coefficients were consistently very high (mean $r > 0.996$), and our root-mean-square differences were consistently very low (mean RMS < 0.74 mm), we are confident in concluding that the CoP trajectory of FP and WBB posses similar characteristics.

(2) The CoP parameters derived from the CoP trajectories were compared between FP and WBB by means of Pearson's correlation coefficient (linearity), the mean difference (systematic bias), and the standard deviation of the difference (consistency). We dispute the insinuation of Pagnacco et al. that we exclusively used r to verify the consistency between the measures obtained from the FP and the WBB, or used r as a single indicator of agreement. In our work, we state in the discussion section:

"Despite the limitations of the WBB, we found that the present balance measures showed very high Pearson's correlations and small differences in error between FP and WBB. This indicates

linearity and consistency of measurement outcomes. In addition, Figure 2 reveals that the WBB measurement error is consistent over the present ranges of 'CoP path velocity' and 'mean CoP sway'. Therefore, it is unlikely that errors of WBB estimates in single-leg balance tests will lead to false conclusions."

The *r* reflects linearity, and the small differences in error (as illustrated by the low standard deviation of the error between FP and WBB) reflect consistency. As Lee et al. (1989) correctly pointed out, high linearity alone is not sufficient to conclude on agreement between two data sets, although calibration would be an option to achieve agreement in case of strong linearity. The scatter plots presented in the paper reflect strong linearity and strong consistency. As the presented results in our work are clear on the observed systematic bias, in our opinion, a statistical comparison is redundant. With very high correlation (1.000) between the paired data sets, with the error always in the same direction, a statistical comparison obviously indicates a significant bias. However, this surely does not signify that the WBB is useless as a surrogate system of CoP measurements to evaluate single-leg stance balance. Nevertheless, the presented systematic bias of the WBB is an important finding of our study and it does indicate that the FP and WBB devices cannot be considered interchangeable. For single-leg stance with the eyes open, the 95% confidence interval (CI) of the mean difference between FP and WBB was 5.0–5.6% (percentage related to FP outcome) for 'CoP path velocity' and 3.3–3.6% for 'mean CoP sway'. We would like to point out that our scatter plots illustrate this error is small compared to the variance between trials, whereas the mean SD within subjects was 13% and 19% (% of mean FP outcome), and the SD between subjects was 16% and 16% for 'CoP path velocity' and 'mean CoP sway', respectively. Additionally, besides to the instrumentation, the resulting 'CoP path velocity' is dependent on numerous environmental conditions and data processing settings (e.g., compared to 12 Hz, a cutoff filter frequency of 20 Hz or 40 Hz would lead to 2% or 5% higher values, respectively). Therefore, instrumentation, environmental conditions, and data processing should always be the same when comparing groups or experimental conditions within a study. Thus, interchangeability of instrumentation is not a necessary requirement within a study. Rather, when comparing exact values of an outcome measure between studies these issues become important.

As we have shown that the CoP trajectories of FP and WBB are very similar, the consistency of the error of derived parameters is far more important than systematic errors to assess the usability of the WBB for evaluations of single-leg balance. This is best quantified by the SD of difference. As mentioned in our paper, the SD's of differences (%) were small (eyes open: 1.9% for CoP path velocity; 0.7% for mean CoP sway) and considerably smaller than the aforementioned within and between subjects variance. Additionally, the SD of difference can be used to estimate the possible bias due to error inconsistency of WBB measurements, i.e. the difference between the upper and lower limit of the 95% CI of the error. For instance, the possible bias for one trial of single-leg stance with the eyes open due to error inconsistency is 7.4% (9−1.6%=7.4%) for 'CoP path velocity' and 2.8% (4.9−2.1%= 2.8%) for 'mean CoP sway', while the mean outcome of one individual (averaged across trials) is subject to a possible bias of 5.4% (8−2.6%=5.4%) for 'CoP path velocity' and 1.2% (4.1−2.9%= 1.2%) for 'mean CoP sway'. Although these values are small compared to within subject variability, we acknowledge this is not explicitly stated in our paper and these ranges should be taken into account when outcomes are compared on the individual level. Most research employ comparisons on group level, hence the possible bias due to error inconsistency is even much smaller.

Pagnacco et al. propose the use of intra-class correlation (ICC) as a single indicator of agreement. We did not calculate the ICC in view of its limitations. The ICC is strongly dependent on the variance between subjects. Consequently, the practical implications of the ICC value are not always clear. For this reason, Lee et al. (1989) postulated that an extremely useful insight pertaining to the concordance of two data sets could be gained by visually examining the pattern of discord. As mentioned before, we performed multiple analyses and used the scatter plots to justify our conclusions as well. Therefore, additional ICC analyses can be considered redundant. Nevertheless, such ICC values should be in line with the above observations and conclusions. Therefore, and considering the serious imputation against our scientific approach, we felt obligated to provide the ICC values in this letter. The ICC two-way mixed single measures 'consistency' values of our CoP parameter data was 1.00 (95% CI 1.00–1.00) for both 'CoP path velocity' and 'mean CoP sway'. This is in line with our observation that the error of the WBB parameters outcome is highly consistent (note that this is related to the variance of all trial outcomes). Additionally, the ICC two-way mixed single measures 'absolute agreement' values (this includes systematic bias) were 0.99 (95% CI 0.62–1.00) for 'CoP path velocity' and 1.00 (95% CI 0.63–1.00) for 'mean CoP sway'. This is in line with the observation that the systematic bias is small compared to the variance between trials. However, it also indicates that the two devices cannot be considered interchangeable within 95% certainty, due to systematic bias, as the lower limit of the 95% CI of the ICC absolute agreement is < 0.75 (Lee et al., 1989). Additionally, the ICC absolute agreement values of 0.99–1.00 also put into perspective the claim by Pagnacco et al. that testing the same subjects on the two types of devices would lead to significant differences. To which we would add, as argued above, that it is never advisable to use different instrumentation, environmental conditions or data processing settings within a single study.

A second point of criticism refers to the reference system used. Pagnacco et al. point to the existence of force plates specifically designed for balance measurements, which they claim to show better performance compared to gait/motion force plates. Although this certainly may be true, to our knowledge, no direct comparisons of CoP trajectories of those force plates to the type used in our study have been performed. It is important to realize that in general specifications of force plate accuracy and precision are obtained during static measurements and without the application of horizontal forces, which may not be representative for measuring single-leg stance CoP trajectories (Faber et al., 2012). For instance, Faber et al. (2012) showed precision values (SD) of 0.7–1.0 mm and accuracy values (RMS) of 1.4–1.8 mm when different directions of forces were applied to specific locations on a laboratory grade force plate, that is, as proposed by Pagnacco et al., portable and specifically designed for balance measurements (in this case Kistler type 9286 AA). Nevertheless, the accuracy (RMS) and precision (SD) of the FP reported in our paper was high for static calibration (< 1 mm), which is sufficient for the type of outcome measures considered (Bizzo et al., 1985; Browne & O'Hare, 2000). An improvement in accuracy, precision or resolution of the instantaneous CoP would have little effect on measures like sway path and the RMS of the CoP (see also the equation in our paper used to estimate the effect of precision). It can even be argued that with a better reference system the differences between the WBB and the reference system would be smaller. Making the safe assumption that errors of the reference system and WBB are independent, the variance of the differences in CoP equals the sum of the errors in both systems. Additionally, we would like to comment on the referral of Pagnacco et al. to Scoppa et al. (2013) as a guideline for

minimal technical specification concerning the use of force plates in balance research. First, the article specifically provides recommendations for the Rhomberg test (bipedal stance). As we pointed out in our paper, bipedal stance measurements are more likely to be influenced by accuracy and precision than single-leg stance measurements. To illustrate, we made a calculation in our Discussion section showing that, in bipedal stance on a WBB with a very low 'CoP path velocity' value, a participant with low body weight may reach an error of 15% in 'CoP path velocity' due to noise (which Pagnacco et al. erroneously cited in their letter as being the WBB error). Secondly, the paper by Scoppa et al. (2013) does not justify the arguments used to reach their recommendations, and the references cited therein do recommend far more liberal limitations (accuracy < 1 mm, precision < 1 mm) (Bizzo et al., 1985; Browne and O'Hare, 2000). Therefore, it seems rather premature to characterize a laboratory force plate that does not meet the strict technical standards postulated by Scoppa et al. (2013), as an inferior device to measure single-leg stance balance.

Finally, we are grateful that Pagnacco et al. have pointed out two minor errors in our text. Indeed, the Nintendo Wii Balance Board (WBB) has a manufacturer advised maximum load of 1471 N, in contrast to the 1962 N reported in our paper. Additionally, the ranges for the static noise values as described in the Discussion section should be exactly similar to the ones presented in the Results section.

Conflict of interest statement

All authors declare there is no conflict of interest.

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