Reviewer's report

Title: Effect of standing posture during whole body vibration training on muscle morphology and function in older adults: a randomised controlled trial

Version: 1 Date: 11 March 2010

Reviewer: Tom J Hazell

Reviewer's report:

Major Compulsory Revisions

1. The idea of the study is of interest but why not wait till the end of the 6 month training protocol to present both the mid and end time points? It is confusing to refer to pre-tests that were not measured at 3 months and to 6 months of training when only 3 month measures were completed. If the data is stand alone at the 3 month time point as presented then the mention of it being a 6 month training study is unnecessary. Why not complete the 6 month study and report all the data?

2. The limitation of the lack of familiarization trials is a large one. The lack of familiarization trials means that a learning effect could explain much of the response over time rather than a training effect. It is questionable that the control group would see such increases from just standing with no exposure to WBV and this may suggest the increases are due to performing the measures for the second time.

3. The placing of subjects into groups poses another problem as it was not based on any of the pre-measures made. Why not match subjects based on key measures, i.e., muscle strength, power, performance, or even muscle mass. The small number of subjects is also problematic as is the unequal number of subjects per group. While this is addressed as a limitation it is a significant methodological problem that undermines the results collected.

4. In Figure 1 it appears that the control group performed a similar position to the LK group then the FK group? Is this true? This is not clear in the methods.

5. Why report percent changes instead of actual changes over time? Should Figures 3-5 not present the baseline and 3 month post-training data to demonstrate the results more clearly? While the control group was not significantly increased over the two WBV groups statistically, the strength/power measures are consistently higher in the control group which confounds the use of simple percent changes to report the data.

6. If the increases in leg muscle CSA are clinically meaningful but not statistically significant then shouldn’t the functional performance tests show an improvement? With the control group simply standing for their training resulting in an increase in 2.2 cm² and the FK and LK groups increasing 2.4 and 3.7 cm²
respectively may suggest that the standing position alone caused most of the increase in CSA if these increases are in fact important.

7. The results do not cover all measures made and the tables include pre-data that did not get measured at 3 months which is the end point for this study.

8. With the training being overseen by two trained research assistants couldn’t they watch for any subjects performing an isometric contraction? Have you done any EMG work to assess this? Has any data been collected on the potential for performing isometric contractions while being exposed to WBV and its effect on muscle strength and power?

9. The co-morbidities differ substantially between groups (control group [18] when compared to the FK [8] and LK [9])? Could this have any effect on the results?

Discretionary Revisions

Abstract
- Pg 3 – The purpose of the study should be clarified (ie. why examine the differences in standing posture?).
- Pg 3 - Methods are inaccurate as the current study was on 16 people who completed 3 months of training and not 19 men and women participating in 6 months of training.
- Pg 3 - The results are unclear. Why not use a specific format (ie. lower body strength and power followed by upper body strength and power).

Background
- Pg 5 - The explanation of the proposed mechanism by which WBV acts is unclear. Do alpha-motorneurons cause motor units to contract or do the muscle fibres contract? The referencing and explanation of the proposed mechanism of action should be more clear.
- Pg 6 - There are a large number of WBV training studies on older adults demonstrating positive adaptations that should be used as the background literature to explain the rationale for this study (see Bogaerts et al. 2007, 2007, 2009; Furness & Maschette 2009; Machado et al. 2009; Raimundo et al. 2009; Rees et al. 2008, 2008; Runge et al. 2000).
- Pg 7 – the specific hypothesis should not include bone strength and density as no post-bone measures were completed or presented.
- Pg 7 – The rationale for flexed knees increasing muscle activity should be supported with relevant literature examining WBV and muscle activity (see Cardinale & Lim 2003; Roelants et al. 2006; Abercromby et al. 2007; Hazell et al. 2007; Marin et al. 2009).

Methods
- Pg 7 - The current study is 3 months not 6 months. The continual reference to 6
months of training is misleading and inaccurate.

- Pg 8 & 12 - Should body composition be reported when post-measures of body comp were not performed?

- Pg 9 - Exclusion criteria includes acute or lower back pain but one of the co-morbidities is chronic lower back pain?

- Pg 9 - Both the frequency and amplitude should be in the methods. Why use 12 Hz and 1 mm? The rationale that it is “beneficial to bone and not destructive to osteoblasts” needs to be further substantiated. There are several WBV studies on older adults showing WBV stimuli of higher frequencies and amplitudes are beneficial to bones (ie. Verscheuren et al. 2004).

- Pg 11 - Did the subjects use their arms to balance while on the platform? The subjects holding on to the bars could potentially have been the reason for the increases seen in upper body strength and velocity. It doesn’t appear so from Figure 1 but this should be clarified in the methods.

- Pg 11 - Can you comment on the potential dangers of subjects standing erect on the platform with locked knees? Most WBV platform manufacturers suggest not standing upright on the platform to prevent transmission of the accelerations to the head.

- Pg 12 - Why was power tested at 9 different percentages of 1RM? If doing this was required then why are the data not presented at each percentage? Also, it is not clear which power data are presented in the results/tables.

- Pg 12 – Suggest that there is a separation of the pQCT and body composition DEXA so it’s clear that the DEXA was for body composition and the pQCT was for muscle CSA.

- Pg 13 - Are Height and Weight really covariates? Weight should be mass because weight is the force exerted by a given mass and this should be changed throughout the entire manuscript.

- Pg 14 – Which variables were not normally distributed and which were measured with non-parametric stats?

- Pg 15 – The use of the month when each subject’s baseline assessment for muscle performance and body composition as a covariate is rationalized by a reference using post menopausal women, is this necessary for men too? This is relevant when you have more men than women in your WBV training groups.

Results

- Table 1 is not necessary where 8 of the 11 columns are identical across all three groups. At the very least it could be significantly reduced to avoid the repetition.

- Pg 16 – Compliance was over 13-17 weeks? Wasn’t the study 3 times per week for 3 months/12 weeks? Where does 13-17 come from? This is unclear.

- Pg 16 – Is it necessary to report the pre-DEXA data when a post-test was not completed? Should the pre-data for muscle performance not be in a table with the post-data for performance to show the reader the actual pre-values and
post-values instead of a percent change? Maybe Tables 3 and 4 could be modified and combined.

- Pg 17 – The primary outcomes section could be more clearly and thoroughly presented. This particular section omits lower body power results as well as the muscle performance data. Perhaps using a simple format with sub-headings that cover the 4 measures you made: lower body strength and power then upper body strength and power followed by muscle performance tests.

Discussion

- Pg 18 – Would the data collected at 6 months help with the potential Type II errors?

- In general, this section does not adequately discuss the results in terms of how they compare to other literature and what the potential mechanisms by which WBV causes the reported adaptations.

- The idea that contraction velocity and muscle power is extremely important in older adults is a great point but it needs to be supported better with the available literature that also demonstrates WBV may increase power. See the references listed above that have not been referenced.

- While the result of an increase in upper body strength/power is intriguing and surprising there has been some muscle activity work in static postures examining lower and upper body muscles (see Hazell et al. 2007).

- A paper by Machado et al. (2009) looked at the ability of WBV training to increase muscle mass, they used CT scans but its still relevant as it is one of the few WBV papers looking at body composition.

- Do you believe the simple standing protocol to be intense enough to cause significant adaptations? Maybe the discussion should comment on the fact that dynamic exercises may need to be performed with WBV exposure to significantly increase power, performance, and muscle mass in older adults.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Not suitable for publication unless extensively edited

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare I have no competing interests.