Reviewer's report

Title: Tai Chi and Vestibular Rehabilitation Improve Vestibulopathic Gait via Different Neuromuscular Mechanisms: Preliminary Report

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Reviewer: Tim C Hain

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General

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
McGibbon and others. Tai Chi and Vestibular Rehabilitation improve vestibulopathic gait via different neuromuscular mechanisms: preliminary report. Revision 1

Overview:

A large number of gait laboratory parameters are presented this uncontrolled, preliminary study of 36 subjects, randomized to vestibular rehabilitation or Tai Chi exercises. The two groups were found to differ in several subtle outcomes involving mechanical energy expenditure at the ankle, leg and hip. Because a very large number of comparisons were used, without correction on what appear to be a-posteriori questions the significance of the findings is questionable.

Comments are presented as page-line

2-3 As 19 subjects were omitted, data is presented concerning 36 subjects, not 53 subjects. Please revise the abstract so that it is not misleading.

2-9. It would be clearer to say that measures of gait time-distance were improved in both groups following treatment. As it stands, it sounds as if measures did not improve till after treatment.

2-9. Please indicate here that the statistical criteria for significance were not conventional in this exploratory study. This must go in the abstract so that it is not misleading. An alternative, and much clearer approach, would be for you to say that you noted some trends that did not attain statistical significance due to the exploratory nature of this study.

2-13. Conclusions. These conclusions do not follow from your data. Please defend or revise.

In particular, in 2-14 the term “better organized lower extremity neuromuscular patterns” embodies a qualitative value judgment (better), which seems out of place in the conclusion of a basic science paper. The results support the adjective “differently”. Also, in 2-15 the vague term “vigorous gait” seems out of place in the conclusion of a basic science article. Could you replace this term with something quantifiable? In 2-18, we do not see that these data suggest that TC confers benefits not attainable with more conventional interventions, because we don’t see that there is any proven benefit of TC vs VC.

In the reviewers view, both groups of subjects walked more quickly after an exercise program.
Kinematic measures suggested that they might have walked with a different organization, according to the training program. The VC subjects exhibited a pattern that may reflect optimization for head stability. The TC subjects may have optimized for something else, or have been less constrained, which resulted in the difference.

3-2. Suggestion – don’t use the “VSP” abbreviation – spell vestibulopathy out.
3-4. This sentence, the first one in the body of your manuscript, is awkward. What exactly do you mean by “typical role activities”?
3-7. This sentence is a little murky. Isn’t VR designed to compensate for VSR loss as well as VOR loss?

3-14. This sentence directly states that TC “balance the flow of … life force”. This is an inappropriate statement for a basic scientific paper. Please reword – you could say that TC is intended to balance flow of life force.

3-16. This sentence does not logically follow from the preceding sentence (about the “soft unfocussed gaze”). Please revise. You might wish to say that in VC, the explicit object of many of the exercises is to improve the stability of the eye in space, while in TC, emphasis is placed upon smooth and circular movement.

4-1. How do locomotor exercises enhance somatosensory input? Do you mean that they increased central weighting for somatosensory input? (this is a repeat of the same question in the first review).

4: 4-7. Here you simply seem to be saying that both VR and TC should have positive carryover into gait because they both involve exercises done while standing. Could you shorten this paragraph?

4-16. Do you mean “outcome variables?” instead of “outcomes variables”? Is reference 19 (to yourselves) really needed in this sentence that says that you don’t know of anyone who has done this work before?

5-7. Your specific hypotheses do not follow from your rationale because you have not indicated how the locomotor activities of TC differ from VR. You are making a prediction about output without providing any information about input. Thus, they seem more likely to be a-posteriori than a-priori in nature. Please defend or revise.

5:9-10. Please use a more precise term than “directly related”. Do you mean “correlated” in the statistical sense?

6-1 – do you mean “had”?
6-5. What does the abbreviation “SVAR” mean? Assuming that it means something related to rotational testing, as patients with bilateral loss often have normal gain at 0.5 hz, the implication is that these bilateral patients were very severe. You might want to make this observation in the text.

6-6. As 25% unilateral paresis is roughly the 5th percentile of for normal subjects, using a 30% unilateral reduction as a criterion for a unilateral loss means that your UL subjects may be nearly normal. Because your UL’s may have been nearly normal, and your bilaterals were very severe, the proportion of bilaterals between your two subject groups may have been critical.

6-11. Should use past tense.

7-13. During the 20 minutes of warm-up exercises, how many minutes were spent standing?

7-19 through 8-19. Please take out the irrelevant material about the intent of the exercises. This was also requested in the first review of this paper, and the material was not removed.
You describe a conventional vestibular rehabilitation program, and should shorten this section by referring to a textbook (such as Herdman's). We do need to know what was done and how long was spent on it. How long did the subjects exercise? Did the stand for 70 minutes or did they spend some time sitting?

11-4 to 11. Here you appear to be testing you’re a-priori “general” hypothesis given on 4-20 through 21 that both groups would improve their time-distance measures. No correction is needed for the multiple t-tests.

11-12. You could leave out “when correcting for baseline differences”, as this is what “change” means. This construction is used in several other places, and could also be shortened there.

11-15 and all later. Here you appear to be testing questions that I felt were a-posteriori (from 5:7-9), because no rationale was provided. Certainly very many t-tests were performed in which the investigators appear to have no expectations as to their results. In my view, you should have reduced level of “p” required for significance at this point, by dividing 0.05 by the number of a-posteriori comparisons (the Bonferroni correction) or something similar. On the web, there is a nice discussion of this problem at http://www2.sjsu.edu/faculty/gerstman/StatPrimer/anova-b.pdf As there are an immense number of comparisons, and the ‘p’ values are not terribly low, it seems unlikely that any of them would be significant using this conservative approach. The rewording in the manuscript would just be leaving out the word “significant” throughout.

11-25. Table 2 is neither interesting nor very informative. A graphical comparison would be more useful as one could both see the change as well as the variability. Another option would be to leave this table out. Same comments for table 3. These suggestions were made in the first review. These tables are not useful enough to keep. If you cannot put the data into a figure, please just delete them. Perhaps this journal has a way for you to put supplementary data on the web.

13-16. Discussion: In general, the discussion is overly long.

13-26. Your hypothesis is about between-groups. Your results did not support a between groups difference. Therefore, your hypothesis was not “indirectly supported”, rather it was simply not supported. This comment was also made in the first review, and the authors did not adequately defend. Please revise.

13:17-18. The statistics said that there was no significant difference between groups. By “clinically important” difference do you mean a trend?

14-24. This sentence strikes me as inappropriate as the goal for TC or VR is not to obtain a “more normal healthy coordinated pattern”, but rather is to optimize a number of interrelated variables – minimize energy expenditure, maximize speed, maximize safety, given their individual constraints in their sensorium, central processor, and motor plant. A “normal healthy coordinated pattern” of movement may be unsafe in a person with bilateral vestibular loss. In other words, the pattern observed in TC may not necessarily be good for them.

This idea is related to the topic brought up in the original review relevant to the original 9-11. The authors responded that reduced walking speed … are classical characteristics of impaired gait in people with … balance difficulties, and went on to argue that increases in these measures are signs of improved motor function. This argument misses the point that slowed gait (for example) in persons with imbalance may be an optimization weighting safety as noted before, and changes are not necessarily improvements. There is at least one study in the literature where increased activity from an exercise program was associated with increased falls.

17-19. I disagree that the statistical approach used was reasonable for these multiple unplanned
comparisons.

18-9. I disagree with this statement. Your BVH patients were extremely impaired, but your UVH patients could have been very nearly normal. It would help if the mean paresis and range was provided.

18-16. In the last sentence of this paper the idea of “spinal reflex compensations” is introduced. This idea was not mentioned in the discussion. This is not a good place to introduce a new idea.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

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Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

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

Quality of written English: Needs some language corrections before being published

Statistical review: Yes

Declaration of competing interests:

None