Reviewer's report

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

Version: 1 Date: 16 March 2004

Reviewer: Gordon Taylor

Reviewer's report:

General

This is a preliminary report of a study comparing Tai Chi and vestibular rehabilitation for the improvement of vestibulopathic gait improvement. The results presented show significant within group differences but minimal between group changes. The authors go on to suggest that there may be a different mechanism involved in the improvements seen in both groups.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

I am concerned about the sample size of 53 people. There is no justification for the sample size. I suspect that 53 people is really too small to provide any meaningful results.

From reading the paper my understanding is that the analysis of the underlying mechanisms is really a post-hoc analysis. I am concerned that undertaking a post-hoc analysis on such a small number of patients where the analysis is not prespecified is highly likely to lead to false positive results.

For a small study there are a large number of dropouts. These dropouts may have a significant impact on the analysis. The influence of the dropouts on the analysis needs to be considered. The usual method would be to undertake an intention to treat analysis (with possibly a last value carried forward approach). However, I accept that this may not be possible under these circumstances. An approach considering differences in baseline factors between the completers and non-completers could be considered.

The authors present a large number of correlations in the results section and a couple of these are further explored using scatter charts. The first scatter chart shos a significant correlation of -0.536. However, visual inspection of the chart shows that this level of correlation is being strongly influenced by one point and that this point is strikingly different from all the others. If this point was removed then the correlation would be very different and I suspect non-significant. I am therefore rather dubious of the worth of the correlations in the paper. Please can the authors provide some indications of the robustness of these calculations. ie Calculating confidence intervals for the correlations possibly using Jack-knifing.

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

The authors are inconsistent in the way that they use P values. On some occasions they give the exact values where on others they only specify that it is larger than a certain amount (usually 0.05).
The authors should alter the paper such that the P actual values for the P values are given. This will provide the reader with information about the degree that individual tests are significant or non-significant and not rely on the 0.05 dichotomy.

Some of the information represented as means and SDs are clearly highly skewed. eg. page 5 In 11: 2.94 years (+/- 2.73 yrs). This information needs to be presented in a statistically valid manner.

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:** Acceptable

**Statistical review:** Yes

**Declaration of competing interests:**

None