Reviewer's report

Title: Improvements in Gait Characteristics after Intensive Resistance and Functional Training in People with Dementia: A Randomised Controlled Trial

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Reviewer: Sarah Lamb

Reviewer's report:

Thank you very much for asking me to review this paper. This is an important topic as so many health professionals believe that people with dementia have little or no rehabilitation potential. These data, if robust, demonstrate that in the short term at least it is possible to improve gait parameters.

I have some concerns with the data presented.

Major issues

The main issue with the paper is selection bias and whether the data can be considered a random sample or not.

The authors describe a parent study of 122 people, and the sample for the submitted manuscript as being 61 of those original participants. They provide no explanation as to why there are only 61 of the original participants, and any selection criteria that might have been made with the gait assessments. It is unclear whether in essence the 61 people represent those who were retained for follow up or had particular characteristics that made them eligible for the gait assessments, or were from one particular centre where gait assessments could be performed. This needs to be clarified.

With only 50% of the original randomised sample remaining in the study, the case that this remains a random sample is questionable. To use significance tests to establish that there is no statistically significant difference between the groups at baseline is not sufficient to confirm a random sample, and is methodologically incorrect (Altman D.G., (1999) Practical Statistics for Medical Research London Chapman & Hall/CRC). The important things to establish are whether the data are missing at random, or whether there are patterns to the missing data. If the data were missing at random, then as a very basic indication, you would expect the proportions of people in the intervention and control arms to be the same, and from the data presented this does not look to be the case. It is difficult to be exactly sure as the information on the original sample is missing.

I would be particularly interested to know whether retention in the intervention and/or control arms are related to adherence/attendance or experience of the intervention at the intervention. There is substantially less variability in the measures in the baseline measures in control group than the intervention group.

Having said that, the groups appear reasonably well matched. There is a difference in age (2 years) and a small difference in the number of fallers. Testing that the results are not sensitive to these differences at baseline by using an
analysis of co-variance (or equivalent) would be helpful. Some of the data in Table 1 are not correctly reported and need checking for example in Line 2 under women the proportions appear as a SD.

The co-variate adjustment needs greater description, and should include the baseline value of the variable being reported.

Some of the data in percentage change column are incorrectly computed. Report the absolute difference between the groups with a 95% confidence interval for the difference (not the change from baseline) as the main estimate of effect. The effect size calculations are new to me, quite different from a standardised difference, and I fail to see how a difference of 16 seconds over a walk which is taking on average 138 seconds, equates to very large effect size. It seems to equate to a difference of 0.33 in the baseline standard deviation, which would be considered small to moderate by most other reckoning.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

'I declare that I have no competing interests'