Reviewer's report

Title: No effects of a 12-week supervised exercise therapy program on gait in patients with mild to moderate osteoarthritis. A secondary analysis of a randomized trial.

Version: 2
Date: 9 October 2014
Reviewer: Celena Scheede-Bergdahl

Reviewer's report:

Major compulsory revisions:

Thank you for your corrections. Although, they do add to the quality of the manuscript, some outstanding issues remain.

Overall comment: Text still requires revision for English language.

In abstract: brief reference to exercise protocol should be given (ie: X weeks of program, consisting of XXX), especially since the authors conclude that they did not find evidence to support that their exercise program was not an efficient intervention to induce gait alterations in this population. In this case, the reader should be provided with some insight into the exercise program employed. Do the authors still believe that exercise interventions may be useful? Any suggestions, based on this work?

Introduction: I appreciate the reasons why the authors did not want to include a hypothesis but the justification in the text begs the question whether the authors were of the mind that the program would work or not? Did the authors think that the program would work? Reference 13 could have provided justification, even for a feasibility standpoint.

Methodologies: I am still concerned about the exercise program used versus the analysis conducted. Firstly, more detailed information regarding the exercise program should be provided in the text and not referred to in another article (which is a case study- ref. 13). As this is a negative result, the reader must be as informed as possible to the protocol in order to be certain that the lack of results are not due to study design issues. Looking at reference 13 (Fernandes L et al., 2010), it is still not certain whether measuring gait parameters best reflect the type of exercise prescribed. What control was given as to whether the exercises were performed or not? For example, in the Fernandes et al., 2010 article, there is indication that some exercises were not regularly performed (or performed at all). How was this dealt with in the present study? Would the lack of performance of these exercises affect the gait? Why the lack of execution of these exercises? What difference may have existed if the patients were required to come in the 2 x per week?

It must also be noted that the poor compliance for the exercise program may
have resulted in the study group not being adequately trained, therefore no detectable differences in chosen measurements parameters. This puts into question the conclusions reached in this paper, as is postulated by the authors themselves on page 12 of manuscript (The lack of treatment effects on gait could be reflecting lack of adequate participation rather than the lack of efficacy of the program itself). The results may not have been significant because the sample size was so low, compared to what was suggested by the power calculations.

Discussion: Again, could the results acquired from previous work (ie: Fernandes et al., 2010) provide some insight into whether the exercise program was expected to be feasible? Perhaps a set of pilot data could have served this purpose?

As a reader of this paper, I am not convinced that the lack of significant results stems purely from the inability of this exercise program (or any exercise program) to provoke ameliorations in gait parameter measurements.

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