**Reviewer's report**

**Title:** Effects of resistance and functional training on wellbeing of older adults living in long-term care facilities: a randomized controlled trial

**Version:** 1  **Date:** 2 October 2003

**Reviewer:** Elaine E Cress

**Reviewer's report:**

Mandatory revisions: do not use the term 'Well being' as it cannot be adequately defined. Use the actual names of the psychological outcomes.

Discretionary: Consider reanalyzing combining the exercise groups.

1. **Is the question posed by the authors new and well defined?**

The question of whether or not strength, functional training or some combination changes psychological well-being is novel. However, this reviewer is not convinced that ‘psychological well-being’ is a valid construct that can be accurately and reliably be measured. The term is not well defined particularly with respect to a quantifiable measure. As such I believe the primary outcomes of this research are Depressive Quality of Life and Vitality. It is recommended that the title be changed to reflect the actual measures used unless the authors can build a case for ‘well-being’ and that these are measures of ‘well-being’. In the introduction the authors state that there is no standardized way to measure psychological wellbeing as such, this research as written adds to the semantic and actual confusion over the use of the term ‘well-being’. While the authors do a good job of reviewing the literature in endurance training and ‘psychological well-being’ they do not build a case for why they have a ‘functional training’ group or a combined functional resistance training group. Again it seems the researchers have ventured into an undefined area. They do not show justification that ‘functional training’ is a defined and efficacious form of physical exercise training.

2. **Are the methods appropriate and well described, and are sufficient details provided to replicate the work?**

The randomized controlled design is appropriate, however the authors broke the randomization by randomizing some individuals as couples. They may want to only use one of the members of the couple data to adhere to a randomized protocol. This will, however lower the n in some groups.

The population of interest would be expected to have a wide range of functional abilities since they lived in six different personal care or nursing homes. Exercise has been shown to be efficacious in improving several measures, aerobic capacity, strength, power, and physical function, gait and balance (depending on the initial measure). Unfortunately the authors do not document the efficacy of the exercise programs in some parameter that would be expected to change. Further, measuring initial strength/function and strength/function at the end of the study would provide insight into the effectiveness of the strength/function/combined training protocols. Therefore they cannot say with certainty that the exercise program was effective and as such how can they say that it didn’t improve well-being?

Training protocol: The authors need to justify the use of a two days per week of training program when in the introduction they state that previous research shows that frequency may be more important than the intensity of exercise. A 3 day per week training schedule is customary and there
is evidence supporting a 3 day per week approach, which leads to increases in strength, power, and function (Hruda, et al., 2003, Miszko, et al., 2003, Vincent, et al., 2002, Frontera, et al., 1990, 1988). In the intervention section, the authors state that there were two different warm-ups for the strength group and the functional group. The strength training group performed one set of the specific exercise as a warm-up, which is not a very thorough warm-up and could be a reason that the participants had a hard time maintaining the intensity of the strength training protocol. Why not use the same warm-up for both groups? Also, there was no indication of how the combined group warmed up prior to exercising. The exercises incorporated into the resistance training covered five of the eight major muscle groups that are customarily trained. The protocols omitted exercise for the chest, shoulders, or hamstrings muscles. A justification for inclusion and exclusion of muscle groups needs to be given. There was lack of consistency across protocols. Within the description of the training protocols, it is noted that there was an instructional video demonstrating the functional training. However, there is no indication of an instructional video demonstrating strength exercises. This could be a confounding factor because the assistants may not have been properly prepared to instruct the participants in the proper execution of strength exercises.

The control group is a relaxation intervention rather than a placebo intervention. It does control for social interaction, however. The relaxation may have lead to improvement in the psychological outcome measures as shown by this improvement in the control group over the exercise training groups.

Comments concerning the primary outcomes are given above.

Concerning study power, which of the primary outcomes were used in the calculations to power the study. In the methods section, the authors state that power was estimated based on starting each group with 60 subjects, a 25% drop-out rate, alpha of 5% and power of 80%. Based on the table values, only one group started with 60 participants (functional) and only one group was within the percentage drop-out range (combined). With fewer subjects than those estimated to show significant results this study is in danger of a Type II Statistical error, claiming now significant difference when three actually was a significant difference. It would be nice to see power values and ?2 (Eta squared) values in the data tables.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
This reviewer recommends that this paper undergo a statistical review. I don’t believe appropriate statistical procedures have been applied. For example are some of the scales ordinal, shouldn’t the researches be using nonparametric statistics or Rasch analysis?

In the results section provide the mean and range of the participants attendance for each group. In participation rates, there is some inconsistency in the text and the tables.

5. Are the discussion and conclusions well balanced and adequately supported by the data?

The discussion and conclusions section is well written and appear to provide justifications concerning the lack of improvement from the training groups. A section should be added listing the limitations of the study.

Adjust the title to accurately reflect the paper, e.g. randomization at this time is not fully a complete randomization, ‘well-being’ is a nebulous term.

6. Do the title and abstract accurately convey what has been found?

Yes

7. Is the writing acceptable?

Yes

Suggestions: Given the lack of power to show changes the authors may want to combine their exercise programs and analyze the effect on their psychological outcomes. They will need to deal
with the problem of unequal group numbers.

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

**Level of interest:** An article of importance in its specialized field or of broad interest

**Quality of written English:** Acceptable

**Statistical review:** Yes

**Declaration of competing interests:**

none