Author's response to reviews

Title: Exploring the feasibility of a community-based strength training program for older people with depressive symptoms and its impact on depressive symptoms

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The Editor
BioMedCentral Geriatrics
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Dear Editor

Re: MS: 1273694564104895 Exploring the feasibility of a community-based strength training program for older people with depressive symptoms and its impact on depressive symptoms

Thank you for the opportunity to revise our manuscript for publication. We are grateful that you have obtained further advice on our paper. We have sought to address the referee's comments in the attached, revised manuscript. Please find below point-by-point responses to the comments the referees have raised.

Referee 2

The control intervention was not adequate as an attention control. Any hint that the experimental group might have benefited more than the control group may be due to the PRT itself, to attention from exercise instructors and others directly associated with the PRT, or to the weekly telephone calls received by the members of the experimental group. This should be appropriately acknowledged in several places, including:
1. Abstract
2. Para 5 of Discussion
3. Para 1 of Conclusions

The text has been modified at the suggested places, namely: Abstract (pages 1-2); Pages 12-13; Page 14.

The authors' claims extrapolate too far beyond their data. Claims which should be toned down include:
1. In the Conclusion section of the Abstract, the words "for the widespread benefit of older people" should be deleted.
2. In the penultimate sentence of para 1 of the Discussion, the claim that the study "provided preliminary support for PRT for older people" should be removed.

The suggested deletions have been made (refer to pages 2 and 13)
From the first two paragraphs of the section on Objectives and Outcomes, it is evident that the main study objective was not matched by the primary outcome. This dissonance should be corrected.

The text on page 7 has been modified to indicate that the primary objective was to assess the intervention’s impact on depressive symptoms within the context of an implementation, rather than an effectiveness study.

Semantic corrections required include:-
1. In 4th sentence of Background - regimen (pg 3)
2. In 6th sentence of Background - suggest (pg 3)
3. In 1st sentence of Randomisation Procedure and Allocation Concealment - “and/or” appears to have been used incorrectly. (pg 8)
4. In para 1 of Recruitment Results - [greater than or equal] 11 (pg 10)
5. In 2nd sentence of Participant Characteristics - was (pg 10)
6. In 1st sentence of Follow Up - were (pg 11)
7. In 4th sentence of Adherence to the Exercise Program - criterion (pg 12)
8. Most of the contents of table 2 have been struck out.

We are most grateful for the referee’s attention to detail. The changes have been made at the places indicated in parentheses above. A revised version of Table 2 has been provided.

Referee 3

The abstract should state that the study was negative on the primary outcome, % with lower GDS score, although there was a trend for those in the intervention group to have greater improvement (p=.08). It should also state that the finding that those who adhered better had a better mood response was a post-hoc analysis and could be confounded.

The abstract (pages 1-2) has been extended to include this information.

There is a control group, but there is a question as to whether such a study could be ‘blinded.’ If the answer is that those administering the rating instruments could not be blinded, then this should be stated.

Matters concerning the control group and blinding have been discussed in more detail on page 8. There is still a glitch with reference 37 (an electronic source) and we seek advice on how to address this.

We look forward to hearing from you.
Yours sincerely

Jane Sims
For the co-authors