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: a randomised controlled trial

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Author's response to reviews: see over
The Editor
BioMedCentral Geriatrics

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Dear Sir/Madam

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 providing us with your reviewer’s valuable feedback on our manuscript. They have raised some important points. We have sought to address these in the revised document. We have outlined below our responses to the specific comments made.

Reviewer 1’s comments

1) For other investigators in this field, the problems in the recruitment and adherence should be discussed even in more detail. E.g. in this age group PRT three times a week is quite much, especially when the subjects are depressed. Twice or even once a week might be enough (see. e.g. Timonen et al Int J Geriatr Psychiatry 2002;17:1106-11). A dose-response pattern not necessarily mean that more exercise is more effective in depression. The reason might be also a group of patients who responded quickly and were therefore more active in attending PRT later.

Several researchers have discussed the barriers to recruitment in healthcare research, such as Hewison and Haynes, BMJ 2006; 333: 300-302. Further details of the UPLIFT study’s recruitment and adherence challenges have been added on page 9.

There was no distinct response pattern. We used the protocol successfully used by Singh et al (1997) – 3 sessions /week for 10 weeks. We agree that a less frequent program may also be helpful and may be more conducive to attracting participants. We are currently testing a two session/week program in depressed stroke clients. We acknowledge that a dose-response effect has yet to be established (Spirduso and Cronin 2001, reference 32, page 13). There is also the possibility of reverse causality, as raised by reviewer 2 (point 9 below).

2) There is only one figure (flow chart). The article could be more readable if there were two tables: one with basic characteristics of the subjects and another with main results.

Table 1is inserted at the end of the manuscript (page 18) and Table 2 and Figure 1 have been sent as separate files.
Reviewer 2’s comments

1) Accordingly, it is important that the paper should be judged, not as a randomised controlled trial (despite its title), but as a feasibility study.

We were originally advised to cite the study as an RCT and duly obtained registration on that basis. We are happy to refer to the study as a feasibility study in the title and omit ‘RCT’ (page 1).

2) There is an important lesson to be learned from the authors’ account of the difficulty encountered in identifying potential participants. Of itself, however, this is not a new finding.

This has been addressed on page 9 (see point 1, reviewer 1).

3) 7 of the 12 subjects in the PRT group achieved the adherence criterion of participating in at least 60% of scheduled exercise sessions. However, this was achieved only with the aid of supplementary transport for an unspecified number of subjects. This is potentially useful information for anyone designing such a study.

A range of providers routinely provide community transport. We arranged the use of community transport to aid one participant’s attendance of the program. One person attended with the support of a taxi voucher system that is available to pension card holders. This additional information has been inserted on page 12.

4) There is a useful reminder that distribution of questionnaires by post means that several will be returned incomplete.

Given the small numbers in this study, it was possible to liaise with participants to optimize completion.

5) Although this was a controlled study, little attempt has been made to compare the feasibility of the control group intervention with the feasibility of the PRT intervention. For example, there is no report of the take up rate of community exercise opportunities by the members of the control group.

The control group largely served as an (albeit limited) attention control group. We have evidence that brief advice giving can produce short term effects on exercise intent and reported behaviour (Eakin et al 2000). There was no change in PASE scores in the control group (Tables 1 and 2). One person in the control group reported taking up a strength training program. Other researchers have reported that where brief advice giving is followed by facilitated support, a longer term impact is attained (Kerse, N., C. R. Elley, E. Robinson and B. Arroll (2005). ”Is physical activity counseling effective for older people? A cluster randomized, controlled trial in primary care.” Journal of the American Geriatrics Society 53(11): 1951-6).

6) There is no mention of adverse events during the course of the study.

No adverse events were reported. This is now noted on page 11.

7) There are no details of whether any particular outcome measures or particular questions in questionnaires was the cause of incomplete returns.

With regard to guidance for a larger study, there was no distinct pattern to the items left blank. Commonly the problem occurred when more than a page was turned over.

8) There are no details of whether the very act of completing the questionnaires had adverse effects on any individual subjects.

A distress protocol guided the completion of the questionnaires. No adverse effects were reported.

9) The authors’ post hoc analysis showed a strong association between an improvement in depressive status and the number of exercise sessions completed. They describe this as an intervention dose response, implying, as I understand it, causality. On the other hand they ignore the equally likely possibility of reverse causality.
The reviewer makes a very valid point. The text (page 13) has been amended to reflect that the association may reflect either possibility. The dataset is too small to distinguish between the alternative explanations of the association.

10) The authors also make the point that their post hoc analyses identified the finding that, after the intervention, the members of the intervention group were more likely to be sufficiently active to achieve health benefits than were the members of the control group. They do not address the question of whether this numerical finding might have any meaning despite the fact that the intervention groups’ mean score on the physical activity scale for the elderly (PASE) was lower after the intervention than before.

We have decided to omit this post-hoc analyses as it is not helpful to the reader.

11) The Abstract would be better if the methods were reported before the results.

We have amended the abstract (page 1) accordingly.

12) If Biomed Central chooses to publish this paper, it should first be subjected to expert statistical review. I am unable to make sense of the paragraph on statistical analyses (in the Results section) and I am not familiar with the conventions adopted in the layout of table 2.

We sought further advice and feedback from our biostatistician colleague and have modified the text (page 8) and table 2 to clarify the analyses.

13) In the third and fourth sentences of the subsection on participant characteristics (in the Results section) the comment that “depressive status was of mild severity” seems inconsistent with the comment that “mood was relatively low”.

Mood was measured on the Philadelphia Geriatric Morale Scale, which measures aspects of morale and general life satisfaction. We have modified the wording on page 10 slightly to reflect this.

14) Reference 23 is incorrect. (This should not be taken to mean that the others are necessarily correct.)

Reference 23 has been revised and the remainder of the references checked for accuracy. Two further references have been added to the conclusions section to reflect current evidence and programs. The manuscript contains references produced in Endnote 9 using the BMC Geriatrics style setting.

We hope that our revised manuscript meets with your approval. Please do not hesitate to contact us if you have any queries.

We look forward to hearing from you in due course.

Yours faithfully

Jane Sims
For the authors.