Reviewer's report

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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Reviewer: Archie YOUNG

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General

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™

Authors: Sims, Hill, Davidson, Gunn, Huang

Reviewed by Archie Young, University of Edinburgh

This paper reports the feasibility study for a randomised controlled trial of progressive resistance training (PRT) plus weekly telephone support, compared with the provision of local exercise information, for the alleviation of depression in old age. Despite contacting 170 GPs in 39 practices and sending out 984 invitations, only 12 and 18 people started the PRT and control interventions, respectively. The authors calculated that a total of 130 subjects would have been required to give their study adequate statistical power for their chosen primary outcome measure. Accordingly, it is important that the paper should be judged, not as a randomised controlled trial (despite its title), but as a feasibility study.

Potentially useful feasibility findings already included in the paper

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.

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.

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

Potentially useful feasibility findings not yet included in the paper

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.

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

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

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

Comments on other findings or conclusions offered by the authors
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 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 group’s mean score on the physical activity scale for the elderly (PASE) was lower after the intervention than before.

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

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.

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.

If Biomed Central should decide to publish this paper, it should be aware that the version of the text which I was given to read did not include tables 1 and 2. Tables 1 and 2 were extracted from what I have been informed was an earlier version of the paper.

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

What next?: Reject because too small an advance to publish

Level of interest: An article of limited interest

Quality of written English: Acceptable

Statistical review: Yes

Declaration of competing interests:
'I declare that I have no competing interests'