Effects of exercise on depression in old age

The study by Mather *et al* (2002) is a laudable work but has some important shortcomings. The control group had received health education, and the authors have justified this approach. Considering the fact that well-designed studies addressing the usefulness of exercise in depression in old age are lacking, we believe inclusion of a control group that did not receive another intervention other than continuing antidepressants could have made Mather *et al*'s findings more meaningful. Another issue is attendance rate: despite the fact that exercise facilitated recovery from depression and that no one dropped out, the reasons for a low mean attendance rate in the exercise group remain unclear. Also, whether this low attendance rate contributed to the lack of significant group differences in outcome measures at the 34th week (final assessment) needs clarification.

With regard to the statistical analysis, besides the analysis of outcome at certain points, the authors could have used the Wilcoxon test for paired samples or another comparable statistical test to detect differences in outcome from baseline scores. The authors state that both groups had scores of secondary outcome measures at the 10th and 34th weeks that were significantly different from baseline, but this statement is not supported by an appropriate statistical analysis.

Given that the authors had great difficulties while recruiting the study sample, rectification of the above limits could have made their conclusions more robust.

Mather, A. S., Rodriguez, C., Guthrie, M. F., *et al* (2002) Effects of exercise on depressive symptoms in older adults with poorly responsive depressive disorder. Randomised controlled trial. *British Journal of Psychiatry*, **180**, 411–415.

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Authors' reply: We are grateful to Dr Jagadheesan and colleagues for allowing us to reiterate the methodological strength of our trial design. If our control group had been as Dr Jagadheesan proposes (continued antidepressant treatment only), then we would wrongly have concluded that exercise is highly effective as an adjunct to drug treatment in old age depression. In fact when compared with the effects of a structured social intervention, group exercise offered only a modest additional benefit. Our non-exercise control crucially allowed us to disentangle the psychosocial effects of coming together as a group from the effects of exercise itself. This novel use of a non-exercise control intervention which matched the exercise intervention in duration, frequency and social contact represents an important methodological advance which future researchers will wish to consider (Lawlor & Hopker, 2001).

Perhaps Salmon may shed some light on the low mean attendance rate by his comment that advocacy of exercise as a treatment for depression 'must puzzle clinicians, who in treating depressed people, often have to contend with an absence of motivation to tackle much less strenuous features of life's routine' (Salmon, 1990).

The Results section of our paper is succinct, in part because of editorial constraints on article length. A typical finding (such as a group comparison of reduction in Hamilton Rating Scale for Depression score at 10 weeks) gives only a comparison of proportions and the associated *P* value, with the statement 'we used Fisher's exact test in our comparison on the two groups' implied, but unwritten. In fact, we prided ourselves on the explicit statement of numerator and denominator here and elsewhere (23/42 *v.* 14/43) when many authors might simply have said '55% *v.* 33%, $P=0.05$'.

We are surprised that Dr Jagadheesan *et al* advocate use of the Wilcoxon test for paired comparisons: this would have resulted in a highly undesirable plethora of comparisons (10 weeks *v.* baseline; 34 weeks *v.* baseline; 34 weeks *v.* 10 weeks). Statistical propriety necessitates the use of an approach which recognises the temporal ordering of the trio of results for each subject in each group; hence our appropriate use of repeated measures.

The analysis which resulted in the stated findings for secondary outcome measures was a one-sample *t*-test on the logarithms of the ratios of outcome:baseline scores.

There is a desperate need for better-quality research in the area of depression, and we believe that our trial design offers an important methodological advance.

Declaration of interest

M.E.T.M. is codirector of DD Developments, a University of Dundee company providing exercise classes for older people and whose profits support research into ageing.

Lawlor, D. A. & Hopker, S.W. (2001) The effectiveness of exercise as an intervention in the management of depression: systematic review and meta-regression analysis of randomised controlled trials. *BMJ*, **322**, 763–767.

Salmon, P. (1990) Psychiatric benefits of physical exercise. *British Journal of Hospital Medicine*, **43**, 107.

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Unethical use of placebo controls

Klysner *et al* (2002) evaluated the prophylactic efficacy of citalopram in comparison with placebo in elderly patients, and stated 'the highly recurrent nature of major depression in the young and the elderly warrants long-term antidepressant treatment'. In view of this, is it ethical to use a placebo arm? The answer to this question depends upon whether or not there is an already available treatment of proven or accepted value. In this context, Cochrane (1989) stated that 'placebo controlled trials are appropriate when there is no existing treatment for a disorder, otherwise comparison trials are indicated. No new treatments should be introduced into medicine unless they have been shown, in randomised controlled trials, to be superior to existing treatments, or equivalent to existing treatment but cheaper or safer'. Similarly, section 12.4 of the National Health and Medical Research Council (1999) statement on ethical conduct in research involving humans states: 'the use of a placebo alone or the incorporation of a non-treatment control group is ethically unacceptable in a controlled trial where: (a) other available treatment has already been clearly shown to be effective; and (b) there is a risk of significant harm in the absence of treatment. If there is genuine uncertainty about the net clinical benefit of a treatment, a placebo controlled trial or a trial with a no-treatment arm may be considered'.

The use of placebo in this clinical drug trial raises questions of deception, of patient information and of informed consent. The patients in the placebo group were left without any active treatment for 48 weeks – this raises doubt as to whether patients were fully informed, before giving their consent, that they might receive a placebo by random allocation. We are keen to

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