

and “clinically modest.” Thus, the effect of pragmatic rehabilitation on fatigue seems to be simultaneously significant and small.

The effect cannot be both large and small, so which interpretation best matches the data? Fatigue was measured using the 11 item Chalder fatigue scale “scored dichotomously on a four point scale (0, 0, 1, or 1).” This scale has been criticised because it “has a low ceiling, so patients with maximal scores at baseline will not be able to record an exacerbation after treatment.”⁴ The maximum score is 11, and participants receiving pragmatic rehabilitation in this trial had a mean baseline score of 10.49. It can be calculated that 47-88 of the 95 participants started treatment with the maximum score. Thus, at least half of the participants could not report that treatment had worsened their fatigue, if it did. The inability of the Chalder fatigue scale to measure deterioration in this patient cohort therefore biases the trial in favour of finding “improvements” in fatigue and favours pragmatic rehabilitation because exercise induced relapses cannot be recorded.

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Competing interests: None declared.

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- Cite this as: *BMJ* 2010;340:c2988

Missing data

Wearden and colleagues published the protocol for this study in 2006,¹ so it is strange that they do not mention many of the measures in the current paper.² The most important omission is one of the outcome measures—the step test: time to take 20 steps (or number of steps taken if this is not achieved) and maximum heart rate reached on the test.¹

Another omission is the number of patients who satisfy the Centres for Disease Control chronic fatigue syndrome (CFS) criteria,³ the most widely used research criteria. Even though we were told how many people satisfied the London ME criteria, we were given no information on how they fared after the interventions. Just because the trial was largely unsuccessful does not mean such data are not of interest. For example, the patients in the pragmatic rehabilitation arm of the trial could have done worse at 20 weeks—there is certainly a large increase in the standard deviation of the Chalder fatigue scale scores at that stage.

It is unclear whether data from some of the

other measures—“CALPAS measure of therapeutic alliance” (measured at three time points), “Visual analogue scale: treatment expectation,” “Symptom interpretation questionnaire,” “Brief social support measure,”⁴ “Brief supportive listening process measure,” and “Brief belief measure”—will be published in the future. Given the cost of this trial to the taxpayer (an estimated £1.3m (€1.6m; \$1.9m)), it would be useful if all the data were made available.

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Competing interests: None declared.

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Cite this as: *BMJ* 2010;340:c2990

Authors' reply

We agree with Carter and other correspondents that the fatigue scale is limited by a ceiling effect, but this is more of a problem at baseline (before treatment starts) than at follow-up assessments.^{1,2} With the fatigue scale re-scored to 0, 1, 2, 3, we can demonstrate a clinically modest, but statistically significant, effect of pragmatic rehabilitation compared with general practitioner treatment as usual at both outcome points: Given the chronicity of chronic fatigue syndrome in our sample, we believe that this on average small improvement in fatigue is important to these individuals.

Kindlon points out that we have not analysed all the outcomes that we measured.³ We reported our primary outcomes and the related secondary clinical outcome data that we thought would be of interest in judging the clinical effectiveness of our intervention. We did not report the step test as an outcome because of a large amount of missing data. Further papers will examine exercise capacity and illness beliefs as potential mediators of the effects of pragmatic rehabilitation. We will also be reporting on predictors or moderators of treatment response; among the variables we will examine will be criteria fulfilled (Centres for Disease Control, London ME), ambulatory status, and comorbidities. Other papers will examine economic outcomes and barriers to delivering these treatments. All papers will use the acronym

FINE and have the same ISRCT number, so can be linked to the *BMJ* paper.

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Competing interests: None declared.

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DON'T TAKE ME TO YOUR LEADER

“Great men” need not apply

Delamothe's observations on leadership assume agreement on what we are talking about.¹ Leadership is not an option in any organisation: it has to happen. The question is, how should it operate?

Nearly 70 years ago army psychiatrists and psychologists, later associated with the Tavistock Clinic and Institute, introduced the “leaderless group” method of selecting officers for the next phase of the second world war. They found that the most effective leaders were those who could take initiatives attuned to the needs of the group rather than those who were necessarily the most educated or athletic.²

The chief innovator in the War Office Selection Boards (WOSB) was Dr Wilfred Bion (1897-1979), who had been awarded the DSO for his bravery as a tank commander in the first world war.^{3,4} He was supported by Dr John Rickman (1891-1951), a Quaker who as a conscientious objector had worked as a doctor and educator in pre-revolutionary Russia. Their colleague Dr John Bowlby (1907-1990), the originator of attachment theory, followed up No 2 WOSB and found that the rate of loss of officers fell from 45% to 15%.⁵

Group assessments of leadership were taken