However, in the body of the text, they qualify their use of the term 'recovery'. Citing Nisenbaum *et al*. (2003) they write, 'recovery may be taken to imply that the patient has made a transition from ill health to remission and also is at little risk of recurrence' but then acknowledge that, in the absence of longitudinal data, it is not possible to discriminate between remission and recovery in CFS.

Thus, in the current paper, 'recovery' does not mean recovery as understood by Nisenbaum but 'recovery from the current episode of the illness', a state described by Nisenbaum as 'remission'.

This difference is important because CFS is known to pursue 'a fluctuating course with periods of relative remission and relapse' (CFS/ME Working Group, 2002) and Cochrane reviews of CBT (Price *et al*. 2008) and GET (Edmonds *et al*. 2004) have reported inconsistent findings at long-term follow-up, with some studies showing that initial gains can diminish with time. Writing about the PACE trial, Edmonds *et al*. concluded 'Even when the results of that study are available, it is possible that uncertainty will remain. Further randomized studies are needed, with longer follow-up, to determine whether patients who respond to exercise stay well or relapse.'

Declaration of Interest

None.

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Letter to the Editor

'Recovery from chronic fatigue syndrome after treatments given in the PACE trial': an appropriate threshold for a recovery?

The main trial recovery criteria, described by White *et al*. (2013), allow participants with SF-36 physical function scores of $\geqslant 60$ to be classed as recovered if, for example, their 'main symptom' is no longer fatigue.

In terms of clinical interpretation, such a threshold is problematic because it is in conflict with how the condition itself is defined. For example, it indicates worse impairment than the PACE Trial entry criteria threshold of $\leqslant 65$ (White *et al*. 2011) and the diagnostic threshold of $\leqslant 70$ used by Reeves *et al*. (2005) to indicate 'substantial' physical impairment.

Further, a score of $\leqslant 65$ has been used to indicate severely impaired physical function in similar patient groups (Stulemeijer *et al*. 2004; van't Leven *et al*. 2009).

Declaration of Interest

None.

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Letter to the Editor

'Recovery from chronic fatigue syndrome after treatments given in the PACE trial': data on the recovery groups as a whole would be useful

White *et al*. (2013) report various recovery rates from chronic fatigue syndrome (CFS) following the PACE Trial. However, additional information would have been useful.

White *et al*. use a selection of broad criteria to define recovery, none of which allow one to be confident recovery has been achieved. Firstly, Chalder Fatigue Questionnaire (CFQ) and SF-36 Physical functioning (PF) scores within the normal range are in fact possible at baseline. This means it is possible to have fatigue that is classed as 'severe, disabling and affected physical and mental function' and yet satisfy this particular recovery criterion.

Secondly, not satisfying the Oxford criteria only requires a change on just one measure, and the change may be minimal, across a threshold, e.g. going from an SF-36 PF score of 65 to 70 or a CFQ (bimodal) score of 6 to 5. A sign that this criterion is not that stringent can be seen with the fact that 41% of the specialist medical care (SMC) group, which received no active treatment, no longer met the Oxford criteria at 12 months, much higher than recovery rates seen in previous studies (Cairns & Hotopf, 2005).

Finally, a CGI score of 2, which means a participant rated as 'much better' but not 'very much better' also gives no assurance that somebody had recovered. It seems quite possible that many with CGI scores of 2 have simply improved but not recovered.

Declaration of Interest

None.

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Letter to the Editor

Comments on 'Recovery from chronic fatigue syndrome after treatments given in the PACE trial'

It is debated whether cognitive behaviour therapy (CBT) or graded exercise therapy (GET) reliably facilitate recovery in chronic fatigue syndrome (CFS). As such, any data on this issue, such as those presented by White *et al*. (2013), are always of interest.

The trial was not blinded, however, with participants, therapists and research assessors aware of the treatment group for each individual (White *et al*. 2007). Consequently, there is the possibility of significant response bias. Indeed, while the CBT group performed better than the adaptive pacing therapy (APT) and the specialist medical care only (SMC) groups on the self-rated SF-36 physical functioning (SF-36 PF) scale, there were no significant differences and minimal numerical differences on the more objective six-minute walk distance test (6MWD) (White *et al*. 2011).

This discrepancy between subjective and objective outcome measures is not a novel finding in the CFS literature. Wiborg *et al*. (2010) analysed three randomized control trials (RCTs) of three CBT interventions, finding that while fatigue was improved in the CBT groups compared to waiting-list controls, there was no difference in actometer readings between the two groups. Moreover, a mediation analysis showed changes in physical activity were not related to changes in fatigue. Similarly, in a GET RCT, Moss-Morris *et al*. (2005) found that an increase in physical fitness did not mediate the treatment effect of reduced fatigue. In an uncontrolled trial of a graded activity programme, Friedberg & Sohl (2009) reported improvements in SF-36 PF and fatigue while actometers showed overall reduction in total activity levels.

The 6MWD is one objective outcome measure White *et al*. (2013) could have incorporated into their recovery criteria (White *et al*. 2007). Reference ranges for 6MWDs, which adjust for gender and age *inter alia*, exist for healthy adults (e.g. Chetta *et al*. 2006;