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## Cost-effectiveness of cognitive behaviour therapy for patients with chronic fatigue syndrome

Sir,

I read Severens *et al.*'s article on the cost-effectiveness of cognitive behaviour therapy for patients with unexplained chronic fatigue<sup>1</sup> with interest, although as several subjects met the CDC criteria for 'idiopathic chronic fatigue' rather than 'chronic fatigue syndrome',<sup>2,3</sup> I prefer to use the term 'unexplained chronic fatigue' as defined by Fukuda *et al.*<sup>3</sup> to describe the patient sample under consideration.

To be able to regard the presented cost estimates as a valid reflection of the medical costs of patients with unexplained chronic fatigue, it is imperative to demonstrate that there are no differences between participants who are included in the analysis and participants who are excluded from the analysis.

According to the authors: 'An extensive comparison between participants in the cost-effectiveness analyse ( $n=171$ ) and the remaining clinical study participants ( $n=99$ ) did not reveal any statistically significant differences regarding age, duration of

CFS complaints, and scores for Sickness Impact Profile, Karnofsky score, physical activity, a self-efficacy scale, a causal attribution list, and functional impairment.' (pp. 158–9).

Although details are lacking in the article, baseline data of the included and excluded participants are available from a publication of the Health Care Insurance Board of the Netherlands (College voor zorgverzekeringen).<sup>4</sup> Comparing baseline variables of the two groups using two-tailed independent sample *t*-tests yields the results that are presented in Table 1. The table shows that physical activity (measured by a motion-sensing device called the actometer), self-efficacy, and psychological well-being (measured by the symptom checklist 90) are significantly different at the 0.05 level. The *p* values for physical activity ( $p=0.0081$ ) and self-efficacy ( $p=0.0046$ ) are particularly small. As the authors did not correct for multiple comparisons when analysing the primary outcome measures in the clinical part of the study,<sup>5</sup> I did not apply such a correction here either.

Because the authors' statement that there are no statistically significant differences is in contradiction with the two-tailed independent sample *t*-test results discussed above, it would greatly enhance the reproducibility and external validity of the study if the authors could provide the reader with information on the methods used to compare the included and excluded participants.

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**Table 1** Comparison of included and excluded participants in the cost-effectiveness analysis: baseline data from Van Essen *et al.*<sup>4</sup>

	Included ( $n=171$ )	Excluded ( $n=99$ )	Estimated difference	<i>p</i>
Age in years	37.5 (10.1)	35.3 (9.99)	2.2 (–0.30 to 4.70)	0.0845
Duration in years	6.0 (5.9)	4.9 (5.1)	1.1 (–0.30 to 2.50)	0.1224
CIS fatigue	51.8 (4.3)	52.7 (3.3)	–0.9 (–1.89 to 0.09)	0.0733
SIP total	1766 (589)	1909 (653)	–143.0 (–295.5 to 9.5)	0.0659
Karnofsky	71.3 (7.5)	70.8 (8.9)	0.5 (–1.50 to 2.50)	0.6228
Physical activity (actometer)	63.5 (20.4)	70.8 (23.7)	–7.3 (–12.69 to –1.91)	0.0081
Self-efficacy	15.1 (3.4)	13.9 (3.2)	1.2 (0.37 to 2.03)	0.0046
Somatic attributions	13.9 (2.6)	13.7 (2.5)	0.2 (–0.44 to 0.84)	0.5373
Focusing on bodily symptoms	29.6 (7.4)	31.1 (6.7)	–1.5 (–3.28 to 0.28)	0.0979
Symptom checklist 90	165 (37)	175 (40)	–10.0 (–19.48 to –0.52)	0.0388
EuroQol	42 (15)	44 (17)	–2.0 (–5.92 to 1.92)	0.3159

Data are means (SD), or means (95%CI) for differences.

## References

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