Reviewer's report

Title: Rethinking the treatment of chronic fatigue syndrome—A reanalysis and evaluation of findings from a recent major trial of graded exercise and CBT

Version: 0 Date: 07 Jul 2017

Reviewer: James Coyne

Reviewer's report:

This is a well-written paper that sticks close to the results of the analyses. It is remarkably dispassionate, given all the intense emotions and controversies that the PACE trial inspires.

The paper carefully cites key original sources and accurately describes them. Readers have the choice of accepting the authors' narrative or going to the original sources. In a sense, the manuscript can stand on its own. However, there are a couple issues that might need to be addressed. The manuscript italicizes controlled rather than putting in scare quotes, but a reader not thoroughly familiar with the PACE trial will wonder why the authors are confused. After all, the original trial protocol title refers to a randomized trial. The issue, of course, that the authors must be familiar with, is that it is not clear what exactly is controlled by comparison condition. In fact, the paper in The Lancet drops the designation. This has lots of implications, but the authors could simply note the discrepancy between the protocol and the published paper with primary outcomes and leave it at that. The advantage is that they would not look like they were simply confused, rather than the PACE investigators being confusing in their contradictory titles of their papers. Similarly, the authors still rather delicately with the switching of outcomes. The PACE investigators have defended their switching by indicating that it occurred before all the data were collected. I'm sure the authors know otherwise. I think briefly noting the conditions under which the switching occurred and why it is inappropriate would be worthwhile. In general, the authors could be more explicit and cognizant of what the PACE investigators have said defending their trial so that there is no suggestion the authors are unaware. I understand their unwillingness to get embroiled in controversy, but I think some who are only familiar with the PACE investigators perspective might dismiss what is said in the current manuscript, just has the PACE investigators surely will. What the PACE investigators will say is quite predictable because they keep repeating themselves. However the recent response to the editorial commentary in Journal of Health Psychology is a rather succinct summary and perhaps there could be some siding of it and response to it. A little inoculation against their reaction might go a long way.
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

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If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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I do not believe I have a relevant conflict of interest to declare by the above standards, but I will mention that I have requested from the PACE investigators data that they had promised would be available as a condition for publishing in PLOS One.

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