Reviewer’s report

Title: Investigating the effectiveness and cost-effectiveness of FITNET-NHS (Fatigue In Teenagers on the interNET in the NHS) compared to Activity Management to treat paediatric Chronic Fatigue Syndrome (CFS)/Myalgic Encephalomyelitis (ME): Protocol for a randomized controlled trial

Version: 0 Date: 26 Jul 2017

Reviewer: Jennifer Hislop

Reviewer’s report:

This study compares ways of providing virtual/online alternatives to face-to-face treatments for CFS/ME. The protocol is well written and so I do not have many comments but it would be good if the authors could elaborate in places to aid clarification of specific details, namely:

The rationale for not including a usual care group. The pathway for such a group is provided in Figure 1, and the Dutch study upon which this intervention was developed included a usual care group that allowed "individual or group-based rehabilitation programmes, cognitive behavioural therapy face-to-face, or graded exercise treatment, or both, by a physical therapist." Now, the inclusion of a usual care group may complicate the study instead of a straightforward comparison between online CBT and activity management using video calls, but it needs to be acknowledged somewhere that not including a usual care group could also limit the value of the study as well. By not having a usual care group, the implicit assumption if one treatment is found to be clinically effective will be that online CBT or videocalls in activity management will be at least as effective as current care for the study population. This may well turn out to be the case but it needs stated that this is the assumption at this stage.

Further to this it was not really clear upon reading, what usual care involves, until Figure 1. It would be good if the main text could allude to Figure 1 more frequently when describing the intervention, e.g. p8/24 (or 9/30 in the pdf).

Also I wondered what the rationale was for comparing a 6 month intervention with a 3 month one? This may be down to how these interventions were designed, but it would be useful to understand whether or not the difference in potential intensity can be considered? Also, given there is no video-call CBT or online activity management arm (again may be down to how the interventions were designed originally) it may also be useful to consider how differences in mode of delivery between the arms may influence results?

The authors have done well to note the difficulties associated with the EQ-5D-Y in the absence of an existing value set for it. But I did wonder, given the use of the SF-36 physical score as the primary outcome, why the full SF-36 instrument could not just be used as these values can be converted to utilities. Cost? Proxy considerations? It may be useful to state somewhere.
I also wondered about intellectual property for the intervention(s) and whether this needs mentioned?

The authors are undertaking secondary analysis on those with co-morbid mood disorders as CBT is said to be less effective among adults with co-morbid depression (p17 -or 18/30 of the pdf -lines 33-36) But care should also be taken in case this is not the case among the study population of interest. So those without co-morbid mood disorders should also be considered, given that they will account for 70% of the study population (as opposite is cited on p5-24 - or 6/30 of the pdf -lines 11-12).

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