Reviewer's report

Title: A rehabilitation intervention to promote physical recovery following intensive care: A detailed description of construct development, rationale and content together with proposed taxonomy to capture processes in a randomised controlled trial.

Version: 1 Date: 22 August 2013

Reviewer: Sharon McKinley

Reviewer's report:

Thank you for the opportunity to review this well written manuscript describing the rationale and process of the development of an RCT to evaluate an intervention to promote physical rehabilitation following treatment in intensive care. The subject of the manuscript is one of increasing importance and interest to the intensive care community, and likely to be of interest to readers of the journal seeking information on this topic. The authors report on a comprehensively well designed study which will examine process as well as outcome measures, making a unique contribution to this field of study. I have mainly only minor comments for the authors to consider, presented according to the template requested by the journal editors.

Major compulsory revisions:

1. My only major concern about this manuscript if that of overlapping publication with the Walsh et al (2012) publication of the RECOVER study protocol. This article is appropriately cited (#17). The ICMJE recommendations do not specify an approximate amount of allowable duplicate publication. However there seems to me to be considerable overlap. Perhaps I have this impression because of looking at the supplements that were added to this submission, some of which were in the Walsh article. This may not require revision, but rather an explanation by the authors to the editors and a judgement by the editors as to whether the amount of overlap is acceptable to the journal. If it is considered excessive, major revisions would be required.

Minor essential revisions:

2. Add ‘former’ prior to patients on p. 5 (my page numbering from 1st page of body of text), line 11.

3. Please clarify the statement p. 6, para 2 (This was . . . identified as an excess treatment cost for the purposes of the economic evaluation, and specific appointments (were) made . . .). It is not clear why these two points are linked.

4. Please clarify the numbers given for expected ICU mortality rates of 20-25% (p. 6, para 3). This is exceptionally high for Western countries and higher than the rate cited in the Walsh article in which the main study protocol is published. Do you mean in-ICU mortality or that plus mortality within the 3-month followup
period? Even then the rate would be high, and no reference is given to support the numbers given.

5. In the discussion (p. 9, line 4) it is stated that this is a new area of research. While this may have been somewhat correct at the time of development of the study protocol, there has been more research in the area published in recent years. This further research would obviously be cited and discussed in relation to the results of the RECOVER study, but the authors may wish to consider whether more recent findings should be included in the Discussion in the present manuscript. In particular, the lack of assessment of sleep and its relationship to recovery, a finding that is reported in one of the current references, Orwelius et al (2010) (ref #24) and others, might be mentioned as a limitation of the study.

6. On p. 10, line 2, it would assist the reader if the authors expanded on the main points stated as the ‘. . . information we have provided’.

Discretionary revisions

7. The 90-day followup is relatively short but probably appropriate for the primary outcome of physical recovery, with other studies showing minimal change after this time. Whether it is long enough for study of mental health outcomes is not clear at present, but it may be a limitation that should be acknowledged, at least in the publication of the results.

8. The use of the HADS, a widely used instrument, has recently been questioned. It was the subject of an editorial on ‘time to abandon the HADS’ (Coyne JC, van Sonderen E. No further research needed: abandoning the Hospital Anxiety Depression Scale (HADS) J Psychosom Res. 2012 Mar;72(3):173-4), notwithstanding its widespread use. This is possibly another limitation that should be acknowledged, at least in the publication of the results.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I have no competing interests