Reviewer’s report

Title: A systematic review protocol for measuring comorbidity in inpatient rehabilitation for non-traumatic brain injury

Version: 1
Date: 30 September 2014

Reviewer: Martin Rohling

Reviewer’s report:

Thank you for asking me to review this interesting proposed protocol for Systematic Reviews entitled A Systematic Review Protocol for Measuring Comorbidity in Inpatient Rehabilitation for non-traumatic brain injury, by Khuu, Chan, and Colantonio. I have attempted to answer the questions to which reviewers are directed by the Systematic Reviews editorial staff as follows:

1. Will the study design adequately test the hypothesis?

Despite my concerns expressed, specifically in reference to the authors’ exclusion of stroke from their review, I believe the study design is reasonable and will address some of the important issues in this field.

2. Are sufficient details provided to allow replication of the work or comparison with related analyses: if not what is missing?

For the most part, I believe that sufficient details have been provided by these researchers. There are some issues of circularity in the text that might lead future researchers to review a slightly different sample of manuscripts. However, the overlap between what these researchers described as their expected sample and that which is likely to be sampled by those trying to replicate their study design is likely to be substantial.

3. Is the planned statistical analysis appropriate?

The review proposed does not describe in great detail any statistical procedures planned for the data. Here I quote, “Extracted data will be grouped into clusters based on empirically important variables, such as the type of data used, population stratification, and type of co-morbidity measurement.” These are not inferential statistics per se. Rather, the proposed analyses might best be labeled descriptive statistics. As a meta-analyst, I am biased toward calculating effect sizes after so much time and energy has been expended on the data collection and extraction. I am worried that the current proposal may miss the opportunity to have an even bigger impact on the field than their current proposal is likely to have.

4. Is the writing acceptable?

The writing is reasonably acceptable, with the one recommendation of changing the label of nTBI to some other more reasonable and obvious acronym.
Major Compulsory Revisions – Issues the author(s) must respond to before a decision on publication can be reached)

a) Under the heading METHODS with the subheading of Study Selection - I do not see the logic of excluding “stroke” while simultaneously including “vascular conditions.” It would seem that the only reason stroke will be excluded is because the database would be too large and it would be too time consuming to collect the relevant data and complete the review. Maybe this is a legitimate position to take, but it is a practical argument and not a scientific one. I am concerned that such exclusion will result in the data collected being far less of interest to rehabilitation professionals.

Minor Essential Revisions – Problems that the author(s) can be trusted to correct

a) Under the heading METHODS with the subheading of Quality Assessment - The researchers refer to their expected use of the Critical Appraisal Skills Program (CASP), which will be used to guide their assessment of study quality. I believe a reference is needed here for the CASP and it is missing. I would also like to see some description of why the CASP is the best measure available for this purpose, as I am not sure that it is.

b) Under the heading DISCUSSION – The authors make an argument as to why their systematic review is needed. Yet, they provided no data or citations to support their position. The reader is left to assume that the authors’ have accurately assessed reality, despite the fact that we have no information upon which to base a judgment.

c) Under the heading DISCUSSION – The researchers try to persuade the reader of the need for such a review to be completed. However, the essence of their argument seems too circular to me and is essentially unsupported. What they claim as a justification for their project is that prior data may not be valid. Yet, again, they have provided the reader with no evidence to support such an assertion.

Discretionary Revisions - Recommendations for improvement, which the author can choose to ignore

d) Under the heading METHODS with the subheading of Study Selection - The researchers note that their review will not include studies that they have decide to exclude. This is circular reasoning and is potentially dangerous in the researchers’ attempt to complete an unbiased review. The researchers might want to cite a more concrete or definitive inclusion criteria; possibly one that has been proposed by others on this issue.

e) I have not liked the term non-TBI (nTBI) as it has been used in the literature. I understand that these researchers are simply using the term that has been more commonly used by others in the field. Nevertheless, I prefer the term “acquired non-traumatic brain injury” or AnTBI. I find it more descriptive of the population to
which these authors refer.

Additional Comments

1. Is the question posed original and well defined? I believe the question posed by the authors is identifiable, reasonable, and original in the field. I am not aware of other studies of a similar nature. I believe the protocol as the potential to bring new knowledge to the field.

2. Are the data sound and well controlled? I believe the data that are proposed to be collected are likely sound and reasonably accessible in the literature.

3. Is the interpretation (discussion and conclusion) well balanced and supported by the data? I do not think the authors did a very good at explaining how the data they propose to collect will be gathered in an unbiased manner and how the data extracted from the literature will be interpreted to benefit the field. This is an area in which improvements could be made.

4. Are the methods appropriate and well described, and are sufficient details provided to allow others to evaluate and/or replicate the work? I found the methods to be fairly well described, although this too is an area in which greater specificity and support from the literature as to why they have selected the inclusion and exclusion criteria they have would be helpful.

5. What are the strengths and weaknesses of the methods? I would like to see more explanation in the statistical section of the protocol as to what might be gleaned from the results and how these might improve services to the relevant population of patients.

6. Can the writing, organization, tables and figures be improved? I found the quality of writing to be acceptable, although at times the wording is filled with a bit too much jargon.

7. When revisions are requested. Some revisions would seem necessary, especially as it relates to the exclusion of stroke from the literature search and extraction proposed.

8. Are there any ethical or competing interests issues you would like to raise? No other issues come to mind at this time.

9. Reviewers are reminded of the importance of timely reviews. I do not foresee a problem with my identity being known or my name being associated with my review. I am sorry my review is slightly late. I hope that I have not caused too much trouble for the editorial staff or the authors of the protocol as a result.

10. Confidentiality. The details of this protocol has and will remain confidential.

11. Are the included additional files (supplementary materials) appropriate? The files included are considered appropriate.

Level of interest: An article of outstanding merit and interest 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 professional or financial conflicts of interest in my review of the proposed research.