Reviewer's report

Title: A systematic review of the efficacy of self-management programs for increasing physical activity in community-dwelling adults with acquired brain injury (ABI).

Version: 1 Date: 2 January 2015

Reviewer: Simon Driver

Reviewer's report:

Thanks for the opportunity to review the paper. I think the topic of physical activity self-management programs for individuals with ABI are an important area of research as there is a definite need for effective interventions. As such, the topic is relevant and would be of interest to readers of Systematic Reviews. I’d like to commend the author(s) on the manuscript which is well written and thoroughly completed. I have a few suggestions for the authors which I believe will strengthen the final manuscript.

Major Compulsory Revisions

Introduction

1. In my opinion there is too much overlap/similarity between the background section in the published protocol (Systematic Reviews 2014, 3:39 doi:10.1186/2046-4053-3-39) and this manuscript. Thus, I believe there is an opportunity to change the focus to avoid overlap and redundancy. For example, the authors could discuss how ABI is a public health issue and how it fits into evolving changes in healthcare with a focus on health promotion. The authors could also discuss the recognized benefits of physical activity for the population (e.g., physical, psychosocial, cognitive) and fact that there is a lack of physical activity promotion programs (see article as an example of manuscript demonstrating need for interventions - Pawlowski, J., Dixon-Ibarra, A., & Driver, S. (2013). Review of the status of physical activity research for individuals with traumatic brain injury. Archives of Physical Medicine and Rehabilitation, doi: 10.1016/j.apmr.2013.01.005).

2. While I recognize that the author’s protocol manuscript has been published with 3 objectives I am not sure about having a separate aim examining the effectiveness of programs that use remote delivery. Rather, I would consider that part of the discussion of the review and not a separate objective. It would also help to reduce the length of introduction (see #1).

Method

3. Why did the authors not use PubMed?

4. How did the authors identify the keywords and terms used in the search strategy? The Medline search strategy that was presented is not intuitive and cannot be replicated easily.
5. Why did the authors include both traumatic brain injury and stroke patient populations? It is difficult for a clinician to translate the implications of a study into practice when heterogeneous populations are being examined.

6. Why were RCT and QRCT the only studies included? Obtaining large samples/cohorts of individuals with ABI is extremely challenging – especially in a community setting – so pragmatic interventions using different study designs are important to consider. The inclusion of studies utilizing other methods would also help answer the author’s objectives that include identifying “acceptability” and “client satisfaction”. In my opinion, reporting information on a broader range of studies (non RCT) would add depth of information to the results that would be of use to readers.

7. Did the authors TMJ and CMD split the review of all 3096 articles? I see it says independently but that is a large number. Were articles placed in an “accept” “reject” “unsure” folder and then discussed accordingly? A little more detail could be provided to ensure the process could be replicated.

8. Why did the authors include the summary of risk across all studies figure (#3)? I’m not sure it adds anything as it is discussed in the text.

9. A key for figure 2 may be helpful for some readers.

Results

10. There is too much overlap throughout the text of the results sections and contents of Tables 2 and 3. For example, line 163-165: this information is already presented in Table 2 under the Type of ABI column.

Discussion

11. The authors discuss the important consideration of the facilitators delivering the program. Motivational interviewing is an approach to coaching and behavior change which is recommended to be included in interventions and warrants discussion. In addition, the authors state that physical therapists are well suited to program delivery. I don’t believe that statement is accurate as PTs do not undergo training as part of their degree program on behavior change and how it is integrated into therapy recommendations – rather therapy is prescribed. Based on my observations working in a hospital and outpatient clinic with therapists and teaching in a PT program for the past 10 years (in countries with and without socialized medicine) therapists do not receive sufficient training to make that statement. Therapists are expertly placed to discuss the mechanics of activity and prescribe activities but not the behavior change piece which is key to effective self-management programs.

12. The discussion on improving health literacy was confusing, although health literacy is an important consideration in developing appropriate interventions/education materials etc. Health literacy is a trait in the context of healthcare and is not something that is “improved” – rather it is critical that intervention/education materials are appropriately developed and take into consideration the health literacy of the patient (e.g., their knowledge, skills, and behavioral inclination to use their knowledge and skills).
Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
'I declare that I have no competing interests'