Author's response to reviews

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

Authors:

Taryn M Jones (taryn.jones@mq.edu.au)
Catherine M Dean (catherine.dean@mq.edu.au)
Julia M Hush (julia.hush@mq.edu.au)
Blake F Dear (blake.dear@mq.edu.au)
Nickolai Titov (nick.titov@mq.edu.au)

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Author's response to reviews: see over
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Dear Editors-in-Chief Systematic Reviews,

RE: MS: 1821356962153594

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

Thank you to the Systematic Reviews Editorial Team, A/Prof Driver and A/Prof States for their reviews of this manuscript. We were particularly excited to have such positive comments in regards to our manuscript from A/Prof Driver as a leader in this field of research. Please find below the requested clarifications for this manuscript.

Response to Reviewer: A/Prof Driver

Major Compulsory Revisions

Revision #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).

We thank A/Prof Driver for his comment and we agree with his observations regarding the overlap between the published protocol and this manuscript. We also appreciate the reference example offered, which has been included in our manuscript revisions. Based on A/Prof Drivers comments the introduction section has been revised substantially to ensure it is more succinct and less redundant than the original version. These revisions can be seen in Lines 41 to 89 of the revised manuscript.

Revision #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).
We appreciate this suggestion, however we feel that as the protocol manuscript has been published with these three objectives it is important to maintain consistency and include all three objectives in the full review. Remote delivery of self-management program is an emerging area of research and examining the efficacy of remote delivery was an important aspect of this review. However, as per the revisions made for comment #1 above we have been able to shorten the introduction substantially.

Revision #3

Why did the authors not use PubMed?

We used Medline in our search of electronic databases. Medline is the largest subset of PubMed (http://www.nlm.nih.gov/pubs/factsheets/dif_med_pub.html) and, therefore we felt that the databases we used in this review provided a thorough search of the literature on this topic.

Revision #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.

The search strategy was developed using standard systematic review methodology. The steps included (1) development of the keywords by examining relevant key terms used in existing systematic reviews (i.e. for variables such as self-management, physical activity, acquired brain injury), (2) a thorough examination of the MeSH Database, and (3) expert guidance and review by a specialist librarian. Finally, the search strategy was trialled and refined in order to ensure it was the most effective strategy for this review. The search strategy has also been published to allow for replication.

Revision #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.

We agree that there are differences between traumatic brain injury and stroke populations. However, there are also substantial similarities: both populations are generally physically inactive and the barriers to physical activity reported in the literature are similar. We felt that the heterogeneity was not so great that it would prevent the readers of this review translating the implications into practice.

However, we have added a paragraph to the discussion section (Lines 380 to 389) noting this as a limitation to the review: An additional limitation of this review may come from the common diversity seen in an ABI population. Studies examining both individuals with stroke and those with TBI were included in this review. There are obvious differences between these populations, for example aetiology and average age, as well as limited information regarding the specific mobility or physical activity status of the included participants. This may impact on both the examination of overall efficacy and the ability to translate these results into practice. However, all the participants were community dwelling adults with the cognitive and communicative ability to participate in a self-management program. There are also similarities in many aspects of these populations, such as the levels of physical inactivity and the barriers to physical activity reported in these populations in the literature [4-9].
Revision #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.

While the inclusion of studies utilising other methodology would have expanded the literature available for review within this manuscript, our primary objective was to examine treatment efficacy. Therefore, in accordance with current systematic review guidelines, we only included trials that were RCTs or QRCTs to avoid high risk of bias often seen in trials that do not use this methodological approach. It is noted that this is likely to have limited the information available to answer the objective regarding acceptability and client satisfaction. Certainly a review examining acceptability and client satisfaction specifically would need to include a broader range of studies, including those non-RCT studies, and the following was added to Lines 406 to 408 in order to make this point: “...and it is important to note that the acceptability of remote interventions may be examined in more detail in these earlier stage research studies”. However, given the focus was predominantly on efficacy we felt it was important to only draw conclusions from the papers actually included in this review.

Revision #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.

Both authors reviewed all titles and abstracts of 3096 articles. In order to clarify the review process further, extra detail has been added to section 2.4 (Lines 128 to 131) in the form of the following:

“At each stage of the process records were marked “accept”, “reject” or “unsure”. Those records marked “unsure”, or where disagreements between reviewers arose, discussion between the reviewers was undertaken in order to reach consensus.”

Revision #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.

We included risk of bias across all studies in Figure 3 as it provides a visual overview of the main elements of bias across the papers covered in this review. This is a process recommended by the Cochrane Collaboration (Higgins JPT, Green S (editors). Cochrane Handbook for Systematic Reviews of Interventions Version 5.1.0 [updated March 2011]. The Cochrane Collaboration, 2011. Available from www.cochrane-handbook.org). Although bias is discussed in the text, this is only a brief summary. Figure 3 gives greater detail in regards to each element of bias than is provided in the text.
Revision #9
A key for figure 2 may be helpful for some readers.

Review Manager 5.3 software was used to generate this figure and figure 3. The software does not generate a key automatically for this figure in the same way it does for figure 3. A key has been added to figure 2 (see attached), but this does have a slightly different appearance to the key generated automatically in Review Manager for figure 3.

Revision #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.

We agree with this observation. The text of the results section has been modified to minimise the overlap.

Revision #11
The authors discuss the important consideration of the facilitators delivering the program. Motivational interviewing is an approach to coaching and behaviour 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.

We thank A/Prof Driver for his comment and, upon reflection, we agree with this observation. The statement regarding physical therapists being well suited to program delivery has been removed and replaced with the following: “The experience of the facilitators in regards to changing physical activity behavior is an important factor to consider in any study that aims to increase physical activity levels of individuals with ABI.” (Lines 355 to 357).

In addition, we have made the following addition to the discussion section (Lines 445 to 448): “A review of more than 550 pieces of high quality research by de Silva (28) suggests that it is worthwhile to support self-management of individuals with chronic health conditions, particularly when there is a focus on behavior change and increasing self-efficacy, through approaches such as motivational interviewing and coaching with active goal setting.”

Revision #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).

We certainly agree that health literacy needs to be taken into consideration when developing programs and materials aimed at changing physical activity behaviour. However, we are inclined to disagree that health literacy levels cannot be improved. For example, Prof Nutbeam remarks on the importance of health education in improving health literacy (Nutbeam, D. (2000). "Health literacy as a public health goal: a
challenge for contemporary health education and communication strategies into the 21st century." Health Promotion International 15(3): 259-267.), a paper cited by the WHO in their statement on Health literacy and health behaviour (http://www.who.int/healthpromotion/conferences/7gchp/track2/en/). A review by Jacobs et al examining indicated that it is feasible to deliver eHealth interventions specifically designed to improve health literacy skills for people with different health conditions, risk factors, and socioeconomic backgrounds (Jacobs, R. J., et al. (2014). “A systematic review of eHealth interventions to improve health literacy.” Health Informatics J.) We do feel that, through health education, such as that provided within a self-management program, health literacy can be improved. In order to make this clearer in the manuscript, the term “through health education” has been added to the text discussing health literacy as per the following: “This is important because improving health literacy through health education programs helps build the capacity of individuals to seek, access, comprehend and effectively utilise health information and services.” The addition of the above references (Nutbeam & Jacobs et al) has also been made.

Response to Reviewer: A/Prof States
Minor Essential Revisions

Revision #1
Line 1 and following: The abstract should better highlight the range of intervention approaches that falls within the overall category or give some indication of the ways in which the authors group the interventions considered within the reviewed studies.

We thank A/Prof States for her comment. The abstract has been amended with additional information in the Results section (Lines 15 to 24) to reflect this comment. The abstract is now 347 words (350 maximum stipulated).

Revision #2
Line 36 and following: Given that effective interventions are likely to vary depending on the long-term effects of an individual’s brain injuries, the authors should provide additional justification regarding why they should focus on the population of ABI as unified whole rather than on more specific subset(s) within it.

We thank A/Prof States for her comment. As was outlined earlier, we agree that are differences between traumatic brain injury and stroke populations. However, there are also substantial similarities: both populations are generally physically inactive and the barriers to physical activity reported in the literature are similar. We felt that the heterogeneity was not so great that it would prevent the readers of this review translating the implications into practice.

However, we have added a paragraph to the discussion section (Lines 378 to 387) noting this as a limitation to the review: An additional limitation of this review may come from the common diversity seen in an ABI population. Studies examining both individuals with stroke and those with TBI were included in this review. There are obvious differences between these populations, for example aetiology and average age, as well as limited information regarding the specific mobility or physical activity status of the included participants. This may impact on both the examination of overall efficacy and the ability to translate these results into practice. However, all the participants were community dwelling adults with the cognitive and communicative ability to participate in a self-management program. There are also similarities in many aspects of these populations, such as the levels of physical inactivity and the barriers to physical activity reported in these populations in the literature [4-9].
Revision #3
Line 112: When describing the inclusion criteria, give further definition for ABI. For example, are brain injuries sustained during childhood such as those leading to cerebral palsy and development delay included? If not, why not? Is a particular severity of brain injury or duration of symptoms assumed? For example, are people who have had concussions included within this population?

We thank A/Prof States for her comment. In order to provide further clarification the following text has been added to the manuscript in Lines 114 to 118: “ABI was defined as damage to brain occurring after birth. However, for the purpose of this review studies examining individuals with degenerative ABI (for example Parkinson’s Disease or multiple sclerosis), cerebral palsy, developmental delay, foetal alcohol spectrum disorder (FASD), concussion or transient ischaemic attacks (TIA) were not included. There was no limit based on time since injury.”

Revision #4
Line 312: “were quasi-randomized controlled trials” or “were NOT quasi-randomized controlled trials”?

We thank A/Prof States for picking up this error. The line (now Line 305) has been amended to now read: “not randomized or quasi-randomized controlled trials”

Revision #5
Line 323 and following: Another reason it’s difficult to summarize these studies is that the disabilities of the individuals involved must vary widely depending on their presentation and type of ABI. The difficulty of generalizing across types of disability presentations should be better addressed.

This point is similar to Revision #2 and the revision made to the discussion section in response to this request has been made which also addresses this point (Lines 378 to 387)

Revision #6
Line 363: Same issue as above. In discussing the limitations of this review, the authors should discuss the limitations created by grouping all types of ABI together.

As per Revision #5 above, the discussion section has been amended (Lines 378-387) to address this issue.

Revision #7
Line 466: Add something in the conclusions to reflect the diversity of the ABI population.

A revision has been made to Line 463 to reflect the diversity of the ABI population in the conclusions.

Minor Discretionary Revisions

Revision #8
Line 228: Should this read “Insert table 3 about here” as that’s where the outcome measures are shown?

We thank A/Prof States for picking up this error. This statement was incorrectly positioned and has been removed.

Revision #9
Line 249: In the sentence beginning, “Likewise, in stroke survivors,” alter to say “Likewise, in one study of stroke survivors”?

The statement has been amended and now reads: “However, in one study of stroke survivors,...” (Line 246)
Revision #9
Line 272: “was” should be “were”

The statement has been amended as requested (Line 269)

Revision #10
Line 438: typo – “in as a component”

The statement has been amended with the removal of the word “in” (Line 453).

Editorial requests

In addition the following changes were made as requested by the Editorial team:

Request #1: The email addresses for all co-authors have been added to the title page as requested.

Request #2: Keywords have been moved to just below the abstract as requested.

Request #3: A list of all abbreviations used in the manuscript has been included at the end of the revised document.

Request #4: A figure title and legend section has been included after the reference list.

Request #5: PROSPERO registration number has been added to the end of the abstract.

We hope you will find the above revisions to our manuscript meets your standards and that you will consider our manuscript for publication in your journal.

Yours sincerely,

TARYN JONES
BAppSc(Phty), APAM.