

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Physical Activity Interventions for Adults who are Visually Impaired: A Systematic Review and Meta-Analysis
AUTHORS	Sweeting, Joanna; Merom, Dafna; Astuti, Putu; Antoun, Michael; Edwards, Kate; Ding, Ding

VERSION 1 – REVIEW

REVIEWER	Liv Berit Augestad NTNU, Norway
REVIEW RETURNED	23-Sep-2019

GENERAL COMMENTS	<p>BMJ Open</p> <p>Manuscript ID: bmjopen-2019-034036</p> <p>Title:</p> <p>Physical Activity Interventions for Adults who are Visually Impaired: A Systematic Review and Meta-Analysis</p> <p>The aims of the paper were: “to systematically review physical activity interventions for those with vision impairment and to assess the effectiveness of the interventions in improving health-related (physical and mental) outcomes and issues encountered”. Eighteen papers from 17 studies met the author’s inclusion criteria. They concluded that physical activity interventions in individuals with visual impairment could have positive results, particularly in physical measures such as mobility and balance. However, when performing a meta-analysis of randomized control trials, the evidence for effectiveness is less clear. More studies with larger sample size, stronger designs, broader age ranges and longer follow-up periods are needed.</p> <p>It is important to get scientific knowledge related to physical activity among people with Visual Impairment (VI), and we need more research to address the participation rates of physical activity to improve health outcomes for those with VI. Therefore, the topic of the paper is important, since there is a gap in current knowledge, and I find BMJ Open an appropriate place for the paper’s publication. I appreciate the effort and time the authors</p>
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have spent on writing this manuscript. It is a well-written paper. The authors have done a good job.

Comments:

In common with other adults in the society, people with VI need to be involved in physical activity. A sedentary lifestyle leads to increased health risks, mentally and physically. Different types of physical activity may promote better fitness, well-being and the feeling of self-control. Hence, it is important to develop suitable programs for physical activity that can promote better health. For these reasons, the reviewed study is of great interest.

However, the study has strengths and limitations. Some strengths are the study design and the use of different databases in the literature search, the use of the PRISMA, the AMSTAR 2 and the Cochrane Collaboration's tool for assessing risk of bias. Some limitations are the description of the search strategy and the study selection. The PRISMA Flow Chart at page 42 need more details or the authors' may add some more details in the method section. Only 17 unique studies was included in the qualitative synthesis, these may be due to the search strategy and use of keywords. (On the page 22, the authors demonstrate well that they are aware of strengths and limitations of the study). It is not clearly described in the Methods how the authors got from n=6.517 records screened (Prisma Flow Chart) to n=56 full-text articles assessed for eligibility, and then down to n=17 unique studies in qualitative synthesis.

People with VI, in the selected studies, might have differed regard to their eye-diagnosis and their physical activity. Even the onset of eye-diagnoses might have differed among the participants in each study. In this manuscript, the terminology and the definition of concepts related to ophthalmology and vision loss need to be strengthened. Many people with VI may have additional comorbidities that have an impact on their participation in physical activity. In addition, the wide age range of the samples in the 17 studies may also complicate the comparison. Example: How to compare a 20 years old person who is born blind with a 80 years old person with low vision, but normal vision until 60 years of age?

Table 1 present the summary of the included interventions. The report for each studies compliance is low. Please add the main results of each study in Table 1, not only if it is tested or not. I suggest add together Table 1 and Tabel 2. I found Table 3 (The risk of bias assessment for randomized trails (RCT's) and pre-post studies are) very suitable and useful.

Eleven out of 17 studies had intervention that focused balance training, risk of falls, Otago, tai chi, core stability and Alexander Technique. The definition of physical activity are wide. (Example: from the Alexander technique to aerobics). It would be nice if the

	<p>authors discussed the definition of physical activity and health benefits of the outcome variables more in details, since the outcome variables are closer to medical treatment and physiotherapy. Physical activity as pleasure and leisure time activity should also be in focus, not only possible health prevention and health promotion. "Fitness" should also have been an outcome measure.</p> <p>In full, I appreciated the conclusions of the paper. However, I disagree with the comments that more studies need a broader age ranges. In addition, I suggest rewriting the sentence: "This systematic review illustrates that physical activity interventions in individuals with visual impairment can have positive results, particularly in physical measures such as mobility and balance." It's possible to understand this sentence in different ways.</p>
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REVIEWER	Shahina Pardhan Anglia Ruskin University, Vision and Eye Research Unit (VERU), School of Medicine
REVIEW RETURNED	28-Oct-2019

GENERAL COMMENTS	Whilst the methodology and conclusions seem accurate, the paper does not actually inform the reader about much except that there is a dearth of published clinical trials and that more work is needed. The Reader actually does not get much insight into interventions that may work because of lack of literature.
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REVIEWER	Jonathan Shuster University of Florida, Department of Health Outcomes and Bioinformatics, College of Medicine, USA
REVIEW RETURNED	05-Nov-2019

GENERAL COMMENTS	<p>Review of Sweeting Meta-analysis</p> <p>This paper seems to address an important public health issue.</p> <p>Despite carefully completing the Prisma Table, it is completely inappropriate to conduct a meta-analysis with 2 or 3 studies, most especially a random-effects analysis. There are just too many unsupportable approximations, such as inaccurately estimated inverse variance weights, and normal approximations for the summary statistics. The warnings are conspicuously absent in the software, so I sympathize with the authors. Meta-analysis itself is in flux. For the authors, I am enclosing my invited American Statistical Association tutorial on meta-analysis of randomized clinical trials, which I gave in Philadelphia, March 2019. The audience included many meta-analysis researchers, and at the end of the day, all agreed with the content. Current mainstream methods are biased, weighted methods produce the wrong standard errors, and patient weighted methods as I recommend are valid when a moderate number of trials are combined. Nothing much works when the number of trials is as small as 2-3.</p>
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	<p>But there is a light at the end of the tunnel: Fishers method of combining independent test can be used to test the null hypothesis that the true mean effect for all studies is zero.</p> <p>Let P_j be the two-sided P-value for study j.</p> <p>Under this hypothesis, $Y(\text{obs}) = -2\sum \text{Log}(P_j)$ has a central chi-square with $2K$ degrees of freedom, where K is the number of studies being combined (2 or 3 in your case).</p> <p>The P-value is $\text{Prob}(Y > Y(\text{obs}))$ where Y has that chi-square distribution.</p> <p>Caveat: The American Statistician (3/19) had a 43 article sequence mostly declaring P-values should not be reported!</p> <p>Recommendation for authors: Remove all meta-analysis and call this a systematic review, saying there are not enough studies to do a formal meta-analysis. Optionally, report the Fisher combined study test that all true mean differences are zero.</p> <p>Authors should feel free to contact me at:</p> <p>Jonathan J Shuster, PhD shusterj@ufl.edu https://hobi.med.ufl.edu/about/faculty-directory-2/emeritus-faculty/shuster-jonathan/ Slides from my talk are enclosed for the editors.</p>
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VERSION 1 – AUTHOR RESPONSE

RESPONSE TO REVIEWERS

REVIEWER 1

The PRISMA Flow Chart at page 42 need more details or the authors' may add some more details in the method section. Only 17 unique studies was included in the qualitative synthesis, these may be due to the search strategy and use of keywords. (On the page 22, the authors demonstrate well that they are aware of strengths and limitations of the study). It is not clearly described in the Methods how the authors got from $n=6.517$ records screened (Prisma Flow Chart) to $n=56$ full-text articles assessed for eligibility, and then down to $n=17$ unique studies in qualitative synthesis.

The methods section explains that abstracts were screened, prior to a full text screening process to identify included studies.

Table 1 present the summary of the included interventions. The report for each studies compliance is low. Please add the main results of each study in Table 1, not only if it is tested or not. I suggest add together Table 1 and Table 2.

This information is available in the Supplementary Material 4. We respectfully decline the Reviewer's suggestion to combine Tables 1 and 2, as we believe this would complicate the tables and impact comprehensibility.

Eleven out of 17 studies had intervention that focused balance training, risk of falls, Otago, tai chi, core stability and Alexander Technique. The definition of physical activity are wide. (Example: from the Alexander technique to aerobics). It would be nice if the authors discussed the definition of physical activity and health benefits of the outcome variables more in details, since the outcome variables are closer to medical treatment and physiotherapy. Physical activity as pleasure and leisure time activity should also be in focus, not only possible health prevention and health promotion.

We discuss the range of physical activity types used in the interventions within the Discussion and highlight the focus on low intensity activities. We also discuss the need for higher intensity activity, where possible, to increase the health benefits.

It is difficult to determine whether the interventions had a 'health promotion or 'promotion of PA for pleasure' focus, especially given they are not mutually exclusive goals. Therefore we did not choose to focus particularly on either aspect.

"Fitness" should also have been an outcome measure.

Fitness is incorporated within the 'functional capacity' category (e.g. see Table 2).

In full, I appreciated the conclusions of the paper. However, I disagree with the comments that more studies need a broader age ranges.

Modifications have been made in the abstract and conclusions to clarify this.

In addition, I suggest rewriting the sentence: "This systematic review illustrates that physical activity interventions in individuals with visual impairment can have positive results, particularly in physical measures such as mobility and balance." It's possible to understand this sentence in different ways.

Modifications have been made to clarify this.

REVIEWER 2

Whilst the methodology and conclusions seem accurate, the paper does not actually inform the reader about much except that there is a dearth of published clinical trials and that more work is needed. The Reader actually does not get much insight into interventions that may work because of lack of literature.

We believe it is important to highlight the lack of literature in this important area and subsequent need for more, high-quality research studies to be conducted (as stated in the conclusion section).

REVIEWER 3

Recommendation for authors: Remove all meta-analysis and call this a systematic review, saying there are not enough studies to do a formal meta-analysis. Optionally, report the Fisher combined study test that all true mean differences are zero.

We recognise that this is a contested area and we do think there is a strong grounding to the reviewer's argument. However, this has not yet been accepted in the mainstream e.g. Cochrane. Furthermore, BMJ Open has previously published several systematic review and meta-analyses including three trials (e.g. Hemilä H. Vitamin C may alleviate exercise-induced bronchoconstriction: a meta-analysis. *BMJ Open* 2013;**3**:e002416. doi: 10.1136/bmjopen-2012-002416)

In our case, the conclusion of the paper would be the same even if we were to alter based on the advice of the reviewer (no meta-analysis but report Fisher test). As stated by the reviewer, use of a p-value is also a contested issue, leaving us with no good alternative. Moreover, both methods would lead to the same conclusion, given the homogeneity seen in our forest plot and the fact that we are not claiming significant effects of the interventions, but rather that we need further trials in this area to increase the statistical power of meta analysis.

VERSION 2 – REVIEW

REVIEWER	Liv Berit Augestad NTNU, Norway
REVIEW RETURNED	08-Jan-2020
GENERAL COMMENTS	Dear authors. The manuscript is important, interesting and well written. I have no further comments after reading the second submission. I recommend the editor to accept this paper.