PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Perturbation-based balance training of older adults and effects on physiological, cognitive, and sociopsychological factors: A secondary analysis from a randomised controlled trial with 12- month follow-up
AUTHORS	Nørgaard, Jens; Andersen, Stig; Ryg, Jesper; Andreasen, Jane; Oliveira, Anderson; Stevenson, Andrew; Danielsen, Mathias Brix; Jorgensen, Martin Gronbech

VERSION 1 – REVIEW

REVIEWER	Werner, Christian
	Heidelberg University, Centre for Geriatric Medicine
REVIEW RETURNED	29-Oct-2023
GENERAL COMMENTS	General comment This study presents a secondary analysis of randomized controlled trial that evaluates short- and long-term effects of a 4-session perturbation-based balance training (PBT) on physical, cognitive, and psychological outcomes in community-dwelling older adults. Research on such effects of PBT is still sparse, but relevant to gain a better understanding of more general, non-specific effects of this training approach. Overall, the manuscript is well written, presents the methodology and results very clearly and thoroughly. Only for the introduction and discussion do I have major comments. All others refer to minor suggestions for improvement.
	 Abstract It would be beneficial to briefly mention the objective of this secondary analysis in the background. In the methods, the authors alternate between general physical domains (ST/DT gait and static balance, step reaction time) and specific tests (SPPB). I would suggest using a consistent approach, e.g., strictly naming domains (instead of SPPB lower extremity performance), as is the case with cognitive function and psychological measures. Please mention already in the methods that community-dwelling older adults were included.
	 Introduction Overall well written and describes nicely the state of research on the effects of PBT on falls. However, the authors should be more precise in delineating their secondary analysis from studies that have already been conducted. See my comments below: The authors mentioned that "yet, no previous PBT studies have evaluated such outcomes [physical, cognitive, sociopsychological] after a detraining period". Recently the study by Rieger et al. 2023 (https://doi.org/10.1093/ptj/pzad136) has been

 published with some similar secondary outcomes (e.g., balance, gait, concerns of falling). Thus, the authors should revise this sentence and mention this study in the introduction as well as consider it in the later discussion of their results. I also assume that the authors relate this statement to community-dwelling older adults, as there are some studies in other patient groups available, e.g. Parkinson's disease/spincal cord injury patients, that evaluated such outcomes after a detraining period. (e.g., https://doi.org/10.1177/1545968310376057; https://doi.org/10.1177/1545968317721976; https://doi.org/10.3389%2Ffneur.2021.620367)
 Methods Very well and thoroughly written methodology, which contains sufficient details that would allow reproducibility. Only a few very minor comments: Authors should write the established assessments such as SPPB, TMT, and FES-I with capital letters at the beginning. Please use "Short Falls Efficacy Scale" instead of "short fall efficacy scale"
 Results Results are clearly presented. Few suggestions: "which was the limit for being included in the per-protocol analyses." This sentence should be moved to the statistical analysis section in the methods. Table 1: How was the daily PA level assessed? Please provide also the information on this assessment in the table legend. It might be interesting to also analyze the development of dual task costs for gait speed and static balance ([dual task – single task]/single task*100), which are established outcomes in dual-task research. Did the authors think about that? Discussion Please introduce the abbreviation CNS. Please discuss the results of the studies with some of the
studies mentioned above.

REVIEWER	Taylor, Lynne The University of Auckland, General Practice
REVIEW RETURNED	02-Nov-2023

GENERAL COMMENTS	Thank you for the opportunity to review this manuscript. It presents the results of secondary outcomes measures of perturbation- based balance training for fall reduction, an intervention which will be of interest to many. The authors found immediate but minimal post training improvements in physical performance measures, but no sustained changes in these measures, and no changes in cognitive, or sociopsychological measures. The ceiling effect of cognitive, and physical (SPPB) and sociopsychological tests/ conversely the high functioning group and lack of assessor blinding to group allocation are limitations. RECOMMENDATIONS
	1. In the abstract, align the results and conclusions. Include actual results (CIs) in the result section and summarize their implications in the conclusion. Currently, the results mention short-term

REVIEWER	Sveinsson, Thórarinn University of Iceland
REVIEW RETURNED	08-Dec-2023

GENERAL COMMENTS The study presented in the paper is a secondary analysis of	
secondary outcomes of a RCT study. An analysis of the prim	ary
outcome has already been published in a separate paper.	
Statistical power analysis was conducted for the primary outc	ome
as pointed out in the present paper.	
In the present manuscript, a mixed model analysis is conduct	ed
for the secondary outcomes which seems to be an appropriate	e
choice (see though comments below on assumptions check.	etc).
No correction is applied to correct for cumulation of type II er	or
risks, as pointed out as one of the limitations of the study. Th	s is
appropriated for the purpose of the study	
The results from the statistical analysis are presented in Tabl	<u> </u>
Average values and SD are presented for all variables for all	lovole
for both main effects. The three right most columns in Table ') are
titled. Between group differences." Differences and confidence	
littled "Detween-Group differences". Differences and confidence	
inflits are presented. However, no details on now these value	sale
obtained, or estimated, are given. Actually, this looks like	
estimations from interaction terms in the mixed model fitting	
procedure. If so, they present an estimate of the between gro	up
differences in the within-group changes of the marginal mean	S
from the pretraining values in the fitted mixed models (betwee	en-
group difference in within-group differences). This needs to b	е
clarified in the manuscript. The method used to estimate thes	е
differences should be described and explained. And their	
interpretation should be better detailed. Furthermore, more	
information should be given on the fitting procedure. What	

assumption checks were used (residual plots or tests)? Fitting
indices used, DF calculation method, etc?

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Dr. Christian Werner, Heidelberg University

General comment

This study presents a secondary analysis of randomized controlled trial that evaluates shortand long-term effects of a 4-session perturbation-based balance training (PBT) on physical, cognitive, and psychological outcomes in community-dwelling older adults. Research on such effects of PBT is still sparse, but relevant to gain a better understanding of more general, non-specific effects of this training approach. Overall, the manuscript is well written, presents the methodology and results very clearly and thoroughly. Only for the introduction and discussion do I have major comments. All others refer to minor suggestions for improvement.

Thank you for your time; we value your in-depth assessment of our paper and your input on improving the manuscript.

Abstract

• It would be beneficial to briefly mention the objective of this secondary analysis in the background.

We agree and have added a sentence mentioning the study's aim in the background section of the abstract.

• In the methods, the authors alternate between general physical domains (ST/DT gait and static balance, step reaction time) and specific tests (SPPB). I would suggest using a consistent approach, e.g., strictly naming domains (instead of SPPB lower extremity performance), as is the case with cognitive function and psychological measures.

Thank you for highlighting this, which improves the consistency throughout the section. We have changed 'SPPB' to 'Lower extremity performance' as appropriate.

• Please mention already in the methods that community-dwelling older adults were included.

The word "community-dwelling" has been added to the method section of the abstract

Introduction

Overall well written and describes nicely the state of research on the effects of PBT on falls. However, the authors should be more precise in delineating their secondary analysis from studies that have already been conducted. See my comments below:

• The authors mentioned that "yet, no previous PBT studies have evaluated such outcomes [physical, cognitive, socio-psychological] after a detraining period". Recently the study by Rieger et al. 2023 (https://doi.org/10.1093/ptj/pzad136) has been published with some similar secondary outcomes (e.g., balance, gait, concerns of falling). Thus, the authors should revise this sentence and mention this study in the introduction as well as consider it in the later discussion of their results.

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Thank you for directing us towards this study. We submitted the current study on the 4 of October 2023 and the study by Rieger et al. 2023 was published on the 9 of October 2023, so we were unaware of the study at submission time. However, upon reviewing the study, we found that Rieger et al 2023 only evaluated physical (balance, gait) and self-efficacy outcomes Pre- and post-training and therefore did look at outcomes long-term (more than 6 months). Regardless, we think it is worth mentioning in the introduction, so we have added the reference to our introduction.

• I also assume that the authors relate this statement to community-dwelling older adults, as there are some studies in other patient groups available, e.g. Parkinson's disease/spincal cord injury patients, that evaluated such outcomes after a detraining period. (e.g, https://doi.org/10.1177/1545968314537559; https://doi.org/10.1177/1545968310376057; https://doi.org/10.1177/1545968317721976; https://doi.org/10.3389%2Ffneur.2021.620367)

Thank you for suggesting that we add these references. We have added a sentence about other groups (e.g., Parkinson's disease/spinal cord injury patients) where long-term (more than 6 months) effects have been explored.

Methods

Very well and thoroughly written methodology, which contains sufficient details that would allow reproducibility. Only a few very minor comments: • Authors should write the established assessments such as SPPB, TMT, and FES-I

with capital letters at the beginning.

Good observation - the abrivation "short FES-I" has been added to the manuscript.

• Please use "Short Falls Efficacy Scale" instead of "short fall efficacy scale"

We have now added "Short Falls Efficacy Scale-International" to the manuscript.

Results

Results are clearly presented. Few suggestions:

• "which was the limit for being included in the per-protocol analyses." This sentence should be moved to the statistical analysis section in the methods.

Good observation. We have added a sentence in the statistical methods section explaining the limit for the per-protocol analysis.

• Table 1: How was the daily PA level assessed? Please provide also the information on this assessment in the table legend.

Great observation. We have now updated table 1 so it states that we used the International Physical Activity Questionnaire. We have reported the time participants on average sat down during the day.

• It might be interesting to also analyze the development of dual task costs for gait speed and static balance ([dual task – single task]/single task*100), which are established outcomes in dual-task research. Did the authors think about that?

This is a good idea and yes we thought about it, but we think this might be included in another article

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Discussion

• Please introduce the abbreviation CNS.

Done

• Please discuss the results of the studies with some of the studies mentioned above. We have tried to incorporate the above-mentioned studies in the discussion section of the paper. However, we don't believe the studies should be included as main sources as the population group and trial design differ greatly from ours. Especially the duration and number of training sessions are vastly higher in the studies you have linked to, which may explain the better transfer effect described in some of these studies.

Reviewer: 2 Dr. Lynne Taylor, The University of Auckland

Comments to the Author:

Thank you for the opportunity to review this manuscript. It presents the results of secondary outcomes measures of perturbation-based balance training for fall reduction, an intervention which will be of interest to many. The authors found immediate but minimal post training improvements in physical performance measures, but no sustained changes in these measures, and no changes in cognitive, or sociopsychological measures.

The ceiling effect of cognitive, and physical (SPPB) and sociopsychological tests/ conversely the high functioning group and lack of assessor blinding to group allocation are limitations.

Thank you for your time; we value your in-depth assessment of our paper and your input on improving the manuscript.

RECOMMENDATIONS

1. In the abstract, align the results and conclusions. Include actual results (CIs) in the result section and summarize their implications in the conclusion. Currently, the results mention short-term differences in CSRT, dual task walking speed, and the SPPB without providing the actual results, while the conclusions mention differences in CSRT only. The results should also acknowledge null effects on cognitive and sociopsychological measures.

Thank you for your observation. We have now updated the abstract according to your suggestions.

2. Revise the abstract's conclusion on Page 3: "PBT may have limited effects on physical, cognitive, and sociopsychological factors," appears misleading due to the ceiling effects in these tests. I think the main manuscript's summarises your findings well.

We agree – good observation. The abstract's conclusion has now been changed and is now more in balance with the conclusion in the main manuscript.

3. I would not have considered the PBT to change the Short Orientation-Memory-Concentration Test. Was this used as dementia screening tool (for your exclusion criteria) or a measure you would expect to change with PBT training?

You are right – the Short Orientation-Memory-Concentration Test was used for our exclusion criteria, and in our table 1 shows the baseline characteristics of participants. We did not report it in table 2, which shows the changes over time.

4. Table 1: Previous fallers

Include the time frame for the falls history. (e.g. falls in the past year) to better characterise the group. If 40% had fallen over the past year, I would not consider that a 'low risk' falls group.

Great – the text in table 1 has been changes for a better understanding of the timeframe of previous falls.

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5. Table 2. (Line 38, Page 13) Please address the apparent discrepancy in the Fear of Falling (FES) scores in Table 2 (7/28, with 28 being the best score) and the discussion, where they are described as 'close to perfect.'

Good observation. The text below table 2 was incorrect and has now been changed. 7 is, of course the best score, and 28 is the worst score. MINOR EDITORIAL ITEMS

Change "TRAIL" to "TRIAL" in the subheading "TRAIL design and ethics."

Corrected

Page 2, Line 54 Change to past tense i.e. "this study showed that PBT, in the short term, IMPROVED Corrected

Page 14, Line 12, Change wording from "this study" to clarify that you are referring to previously published results of the primary outcome paper. I initially thought these were new results in the discussion section.

Corrected

Reviewer: 3 Dr. Thórarinn Sveinsson, University of Iceland

Comments to the Author:

The study presented in the paper is a secondary analysis of secondary outcomes of a RCT study. An analysis of the primary outcome has already been published in a separate paper. Statistical power analysis was conducted for the primary outcome as pointed out in the present paper. In the present manuscript, a mixed model analysis is conducted for the secondary outcomes which seems to be an appropriate choice (see though comments below on assumptions check, etc). No correction is applied to correct for cumulation of type II error risks, as pointed out as one of the

limitations of the study. This is appropriated for the purpose of the study.

The results from the statistical analysis are presented in Table 2. Average values and SD are presented for all variables for all levels for both main effects. The three right most columns in Table 2 are titled "Between-group differences". Differences and confidence limits are presented. However, no details on how these values are obtained, or estimated, are given. Actually, this looks like estimations from interaction terms in the mixed model fitting procedure. If so, they present an estimate of the between group differences in the within-group changes of the marginal means from the pretraining values in the fitted mixed models (between-group difference in within-group differences). This needs to be clarified in the manuscript. The method used to estimate these differences should be described and explained. And their interpretation should be better detailed.

Thank you for your time; we value your in-depth assessment of our paper and your input on improving the manuscript.

You are perfectly right that Table 2 and the statistical section needed to be clarified or updated. We therefore consulted with a statistician Regitze Gyldenholm Skaks who works at Aalborg University Hospital. In collaboration with her, we have now analysed data with a more simple mixed model (mixed `var' i.time##i.intervention|| record_id: , reml) which has led to slightly different values. However, we believe that these slight changes to outcomes do not change the overall conclusion of the article. Thus, you will see that we have made substantial changes to both Table 2 and the statistical analysis section.

In addition, when reviewing Table 2, we also saw that some variables were given as median and IQR, which we, in retrospective sight, believe is incorrect. We have, therefore, changed all estimated variables to mean and CI 95% as these are estimates which are outputted from the mixed model.

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Furthermore, more information should be given on the fitting procedure. What assumption checks were used (residual plots or tests)? Fitting indices used, DF calculation method, etc? We have added more details about the mixed effects model in the statistical section. Furthermore, we have added a sentence about how we checked the model assumptions. However, we believe no alternative method exists for such a complex analysis if the assumptions are not fulfilled. The mixed model is very robust (please see the attached article titled: "Robustness of linear mixed-effects models to violations of distributional assumptions"). Therefore, we have chosen to do the analysis anyway. But we would like the reader to know that some measures did not fulfil the assumptions, which are also added in Table 2 now.

Reviewer: 1

Competing interests of Reviewer: None.

Reviewer: 2 Competing interests of Reviewer: I have no competing interests

Reviewer: 3 Competing interests of Reviewer: No competing interests.

VERSION 2 – REVIEW

REVIEWER	Werner, Christian
	Heidelberg University, Centre for Geriatric Medicine
REVIEW RETURNED	05-Jun-2024
GENERAL COMMENTS	The authors appropriately addressed all my comments and
	revised the manuscript accordingly. The manuscript has
	significantly improved. I congratulate the authors on a very well-
	written manuscript. I do not have any further comments and
	recommend the manuscript be accepted in its current form.
	· · ·
REVIEWER	Taylor, Lynne
	The University of Auckland, General Practice
REVIEW RETURNED	25-May-2024
GENERAL COMMENTS	Thankyou for addressing all previous recommendations. I have no
	further questions or concerns.
REVIEWER	Sveinsson, Thórarinn
	University of Iceland
REVIEW RETURNED	28-May-2024
GENERAL COMMENTS	The revisions of the manuscript have satisfactorily addressed my
	previous concerns.