PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Determining the optimal dose of reactive balance training after stroke – study protocol for a pilot randomized controlled trial
AUTHORS	Mansfield, Avril; Inness, Elizabeth; Danells, Cynthia; Jagroop, David; Bhatt, Tanvi; Huntley, Andrew

VERSION 1 - REVIEW

REVIEWER	Itshak Melzer Ben-Gurion University of the Negev, Israel
REVIEW RETURNED	05-Mar-2020

GENERAL COMMENTS	dear editor, this is an important study (protocol) on a very important topic, fall among stroke survivors. Perturbation based training for stroke survivors are a novel approach and seem to be a very effective way to reduce falls, much more then the traditional physical therapy exercises. The traditional physical therapy exercises were not effective and the number of falls in this population is huge. in this study protocol the investigators will monitor falls in the real world and train their patients in a different dosage (3 groups) this is novel. there are several suggestions (minor): 1. please write how are you going to treat (statistically) stroke survivors who dropped out. I maybe missed it are you planning to perform intention to treat analysis? or maybe due to the pilot nature of the study you will perform statistics only on these patients who competed the study (should be added to limitations) 2. exclusion criteria - why Severe spasticity in the legs? so why no severe flaccidity of the legs? and how do you define severe spasticity? 3. exclusion criteria - Cognitive impairment ? using MMSE? or MOCA? 4. Page 10, line 205 - "Training strategies will be individualized to each participant", how exactly? his/her comments? his/her ability not to fall? or his/her ability to recover with a single step only? "based on their balance impairments and rehabilitation goal", please write how balance impairments can influence the training strategy? what can be these rehab goals? I assume reduce falls? 5. page 10 line 209, how did you make sure that the push and pull perturbations are unpredictable ? or maybe this is not so important ? 6. page 10 line 211-212, how you increase the push and pull perturbation magnitude in summary, this is an excellent study protocol and should be accepted to BMJ

REVIEWER	Sarah Dean University of Exeter Medical School; UK.
REVIEW RETURNED	20-Mar-2020

GENERAL COMMENTS	This is an excellent pilot trial protocol, thorough and easy to read - thank you. I have only minor suggestions to make: 1) Please consider changing the term 'compliance' to 'fidelity' for your secondary outcomes as listed on page 4 line 76. 2) It is a shame that no Patient and Public Involvement took place to help design this study. There is clearly nothing you can do about this now but it may be worth considering this when you write up the results of this study (it's limitations) and whether having this input prior to any definitive trial would be useful. 3) It would be good to clarify on Page 13 line 277 if your analyst is also going to be blinded - this also helps reduce potential bias and is worth reporting (but is often omitted in reporting even though it is done). 4) Page 14 line 290. Correct the spelling error ' severs' should be 'servers'? Whilst I have indicated that these are suggested minor revisions I would be happy for the editor to sign these off, I do not think this protocol needs re-reviewing. A useful study, good luck with carrying it out.

REVIEWER	Vicki Gray University of Maryland Baltimore
REVIEW RETURNED	20-Mar-2020

GENERAL COMMENTS	BMJ Lines 130-131, the authors state the training may promote sustained training effects beyond three weeks. The citation for this study is a 6-week training program, and the results did not show any benefits to fall reduction, the results were inconclusive, as stated by the authors. Based on these results published previously by the authors (ref.19), it is unclear why they are proposing a lower dosage for the current study. The previously published study was an exercise intervention that was one hour/two times per week over six weeks, with additional booster sessions at three months and six months. If the authors found inconclusive benefits of reducing the number of falls or observing increases in the clinical balance outcome measures, then I doubt they would find benefits to falls in a reduced exercise dosage. The authors need to justify the rationale behind the reduced number of exercise sessions in the proposed study. Page 8, Lines 145, the authors need to clearly state how the study will determine the two intervention groups that will be used in a clinical trial. There is only one intervention type, RBT, presented in this study with different dosages. How will this data be used to determine the comparator? Page 9, line 178, “Severe spasticity” is an exclusion criterion. What defines severe, and how will spasticity be assessed? How will you ensure there is no bias in eliminating someone from the study without an objective measurement to determine “severe spasticity”?

	Also, will people be included that have baclofen treatment? How will cognitive impairment be defined or assessed? Page 10, lines 196-197, what is the justification for doubling the perturbations per session from the previous study? Is the number of perturbation trials a convenience factor, or is there evidence to suggest that the authors need to increase the number of perturbations to have benefits? What about fatigue? Will this be too many trials to tolerate? If the participants need rest breaks, will the intervention still fit into these 45 minutes? It is unclear why the authors are establishing feasibility when they have already carried out an RCT assessing this same intervention over six weeks. It seems like the authors should have already determined the feasibility. For the outcome measures, the authors should provide a delineated timeline of the data collection. For example, if there is only one training session, will the pre and post-testing be completed all within the same week. Or will post tests be completed after 6 PTs regardless of whether there is RBT in all the sessions. It does create a challenge as to when to post-test because some individuals will have more PT sessions than others, which could have an impact on the results. Also, there will be a longer course of recovery, how will the authors ensure that natural recovery is not playing a role in those that tested after waiting a more extended period to have a post-test. Clarify the number of weeks that the training will occur over. How long it will take for the 6 RBT training sessions to be completed. And will the post-test occur immediately after the RBT and before additional PT sessions? Page 11, lines 216-218 – One of the research questions is to track the training visits. The groups only have 1, 3, or 6 visits. Are you planning to follow the visits for outpatient therapy? If this is the case, how will the authors account for differences in when PTs will discharge the person from outpatient treatment. The differences in treatment given during the PT sessions. How do you account for a therapist that may do perturbation training as part of their clinical practice? Page 11-12 lines 230-234, the authors state that there will be 8-10 walking trials on a movable platform. Two trials the platform will move forward and two trials it will move backward. This only accounts for 4 of the 8-10 trials, what is the task for the remaining trials? Page 12, 251-253, please clarify the new data collection time post-discharge at 2- 4- and 6-months post-discharge for the PASIPD and SIPSO. What does the post-discharge mean, inpatient rehabilitation, outpatient PT, or from the study? These time points are very different from the data collection of the rest of the outcome measures. Why were different time points selected? Page 14, 285-286, Since it is necessary to train the research assistant in data collection techniques from the PI, there is a potential that bias will be introduced since the training is from the PI who is unblinded. How can the PI ensure quality data collected by the research assistant that they will oversee if they are unblinded? It is recommended that the person that is overseeing the testing and quality of this data should also be blinded. The aims of the study and the assessment tools used to answer these questions are not clear. The authors state that they want to determine the optimal dose of reactive balance training in people with subacute stroke to design a larger study to answer this question. Below is a list of research questions and outcome measures the authors want to answer. It is hard to decipher the connection of the outcome measures to the aims of the study.

	1. Page 8 lines 14-145, optimal sample size, how long to achieve sample, feasibility to prescribe a specific dose of RBT, secondary outcomes to use, and what two interventions to use. 2. Page 11 lines 215-218, Outcome Measures: feasibility: accrual rate, missed training sessions, missing data for outcomes. 3. Page 11, lines 226-228, CMSA, mini-BEST, ABC, reactive balance control over unpredictable and novel perturbations 4. Page 11 starting at line 229 - slips and trips walking on the treadmill (vague description of the outcome measure and model used to assess balance stability) 5. Page 8, lines 150-152, reactive balance control, functional balance, balance confidence, falls, physical activity, and participation. Will there be secondary outcome measures? The feasibility of dose – it is unclear if the authors can answer this question with only 6 sessions. How will the authors know if the participants continue to improve and could benefit from more sessions? It is not until I reach the data analysis section that I realized the question related to the optimal sample size was based on the fall rate. The data analysis section was not specific on the fall rate over a time period or if the comparison would be done comparing each group of the groups would be collapsed. The authors state that they want to determine the best measure to use for assessing change but if you are also testing the dose over three groups you have introduced two variables that could potentially have an impact on your results. Is the measure good to capture change? Or is the dosage not enough to evoke change?

REVIEWER	Stephen Lord NeuRA, UNSW, Australia
REVIEW RETURNED	31-Mar-2020

GENERAL COMMENTS	This study protocol describes a study that aims to provide pilot data for a larger trial to determine the optimal dose of reactive balance training for people in sub-acute rehabilitation after stroke. The paper describes the design of the pilot trial providing useful information to the scientific community regarding current research in this field. Please find my comments below. 1. P 3, L50. The actual date of first enrolment should be able to be stated now. Please clarify. 2. P6, L55. 'This is a crucial period for fall prevention due to the high risk…'. It may eventuate reactive balance training may not be suitable for this period. In this study, reactive balance training sessions were embedded into the existing rehabilitation program. As the authors state 'there is a risk that many patients will decline participation in the study as they will not want their rehabilitation care to be disrupted' (P6, L8). It may be worth considering providing this reactive balance training program after completion of the existing rehabilitation. Further comment on this issue could be included in the discussion. 3. There are many balance exercises without postural perturbations that can induce a 'loss of balance' (P7, L18). Further information is required regarding how the current intervention differs from

	‘conventional balance training’ (P7, L15)? This should be clearly stated as the authors are claiming that ‘Unlike other forms of exercise,’ (P7, L34) reactive balance training may result in rapid improvement in reactive balance control. 4. P25, L20. Repeating the same set of perturbations 3 times at pre-training, post-training and 6-month follow-up, will reduce the novelty / unpredictability of the perturbation. Some comment is warranted. 5. P10, L203. The RBT method is not written in a way that other researchers or clinicians could replicate. (a requirement in the SPIRIT checklist). If the references cited in this section present the detailed methods of the intervention, please indicate this. 6. P13, L22. ‘as they were occur’ , needs correcting. 7. P14, L23. ‘secure institutional severs’, check the spelling. 8. P14, L34. What is the comparator to ‘the one-session group’ regarding fall rate? 9. Some sections are not well ordered. E.g. ‘6.2 Participants’ and ‘6.6 Recruitment’ should be next to each other. ‘6.4 Outcome measures’ and ‘8.1 Data collection methods’ should be close to each other. Consider reordering the sections by study timeline.

VERSION 1 – AUTHOR RESPONSE

REVIEWER 1

Comment
dear editor, this is an important study (protocol) on a very important topic, fall among stroke survivors. Perturbation based training for stroke survivors are a novel approach and seem to be a very effective way to reduce falls, much more then the traditional physical therapy exercises. The traditional physical therapy exercises were not effective and the number of falls in this population is huge. in this study protocol the investigators will monitor falls in the real world and train their patients in a different dosage (3 groups) this is novel.
there are several suggestions (minor):
1. please write how are you going to treat (statistically) stroke survivors who dropped out. I maybe missed it are you planning to perform intention to treat analysis? or maybe due to the pilot nature of the study you will perform statistics only on these patients who competed the study (should be added to limitations)

Response
We have clarified that we will document rate of withdrawal from the study, in addition to rates of accrual, and missing data etc (Page 12, Line 242) and that this information will be used to estimate how long it will take to achieve the sample size in the larger study (Page 16, Line 334).

Comment
2. exclusion criteria - why Severe spasticity in the legs? so why no severe flaccidity of the legs? and how do you define severe spasticity?

Response
We have clarified that ‘severe spasticity’ is defined as being unable to safely accept weight on the limb (Page 10, Lines 188-189). Individuals with severe flaccidity would likely not meet the inclusion criteria (i.e., able to stand independently for > 30 seconds and able to walk without assistance for > 10 m).

Comment
3. exclusion criteria - Cognitive impairment ? using MMSE? or MOCA?

Response
Inability to understand the purpose of training or provide informed consent will be determined by the healthcare team (Page 10, Lines 190-191). Capacity to provide consent is complex, and specific cut-off scores on tools such as the MMSE or MOCA are not predictive of capacity to provide consent.

Comment
4. Page 10, line 205 - "Training strategies will be individualized to each participant", how exactly? his/her comments? his/her ability not to fall? or his/her ability to recover with a single step only? "based on their balance impairments and rehabilitation goal", please write how balance impairments can influence the training strategy? what can be these rehab goals? I assume reduce falls?

Response
We have added some examples of how individual impairments and rehabilitation goals will be incorporated into the training sessions (Page 11, Lines 219-224). Additionally, we refer the reader to our previous paper, which includes more specific details of the training approaches (Page 11, Lines 224-226).

Comment
5. page 10 line 209, how did you make sure that the push and pull perturbations are unpredictable ? or maybe this is not so important ?

Response
If the physiotherapist is positioned behind the participant then the push or pull can be unpredictable in timing and direction; we have clarified this in the paper (Pages 11-12, Lines 231-232).

Comment
6. page 10 line 211-212, how you increase the push and pull perturbation magnitude

Response
The physiotherapist can increase the perturbation magnitude by increasing the force of the push or pull. We have clarified this in the paper (Page 12, Lines 235-236).

Comment
in summary, this is an excellent study protocol and should be accepted to BMJ

Response
Thank you for the positive comments and evaluation.

REVIEWER 2
Comment
This is an excellent pilot trial protocol, thorough and easy to read - thank you.
I have only minor suggestions to make:

1) Please consider changing the term 'compliance' to 'fidelity' for your secondary outcomes as listed on page 4 line 76.

Response
We have made the requested change (Page 4, Line 77).

Comment
2) It is a shame that no Patient and Public Involvement took place to help design this study. There is clearly nothing you can do about this now but it may be worth considering this when you write up the

results of this study (it's limitations) and whether having this input prior to any definitive trial would be useful.

Response
Thank you for this suggestion. We will consider adding patient/public involvement for the definitive trial.

Comment
3) It would be good to clarify on Page 13 line 277 if your analyst is also going to be blinded - this also helps reduce potential bias and is worth reporting (but is often omitted in reporting even though it is done).

Response
We have clarified that the analyst will not be blinded to group allocation (Page 15, Lines 312-313). Unfortunately, we do not have funds to support a separate data analyst.

Comment
4) Page 14 line 290. Correct the spelling error ' severs' should be 'servers'?

Response
We have corrected this error (Page 15, Line 323).

Comment
Whilst I have indicated that these are suggested minor revisions I would be happy for the editor to sign these off, I do not think this protocol needs re-reviewing.

A useful study, good luck with carrying it out.

Response
Thank you for your positive comments and evaluation.

REVIEWER 3
Comment
Lines 130-131, the authors state the training may promote sustained training effects beyond three weeks. The citation for this study is a 6-week training program, and the results did not show any benefits to fall reduction, the results were inconclusive, as stated by the authors. Based on these results published previously by the authors (ref.19), it is unclear why they are proposing a lower dosage for the current study. The previously published study was an exercise intervention that was one hour/two times per week over six weeks, with additional booster sessions at three months and six months. If the authors found inconclusive benefits of reducing the number of falls or observing increases in the clinical balance outcome measures, then I doubt they would find benefits to falls in a reduced exercise dosage. The authors need to justify the rationale behind the reduced number of exercise sessions in the proposed study.

Response
We have clarified that the previous paper reported that improvements in reactive balance control were sustained up to one year post-training. We also cite two additional studies in sub-acute stroke reporting retained improvements in reactive balance control 5 weeks and 6 months post-training. In these two sub-acute studies, participants completed 1-12 30-minute sessions of reactive balance training, so the dose of training was less than half that of our previous study in chronic stroke. In one of the sub-acute studies, 29% of participants only completed a single 30-minute reactive balance

training session, and the median number of sessions completed was 6. We have clarified these details in the paper (Pages 7-8, Lines 135-139).

Comment
Page 8, Lines 145, the authors need to clearly state how the study will determine the two intervention groups that will be used in a clinical trial. There is only one intervention type, RBT, presented in this study with different dosages. How will this data be used to determine the comparator?

Response
The long-term goal is to determine the optimal dose of RBT in people with sub-acute stroke (Page 8, Lines 144-145). Therefore, it will be appropriate to compare two different doses of RBT in the larger clinical trial. We have clarified that the one-session group will be used as the 'control' group in this larger trial (Page 16, Lines 351-352).

Comment
Page 9, line 178, "Severe spasticity" is an exclusion criterion. What defines severe, and how will spasticity be assessed? How will you ensure there is no bias in eliminating someone from the study without an objective measurement to determine "severe spasticity"? Also, will people be included that have baclofen treatment? How will cognitive impairment be defined or assessed?

Response
We have clarified that 'severe spasticity' is defined as being unable to safely accept weight on the limb (Page 10, Lines 188-189). Those whose spasticity is treated by Baclofen, such that they meet this criterion, can be included in the study.
Regarding cognitive impairment, inability to understand the purpose of training or provide informed consent will be determined by the healthcare team (Page 9, Lines 179-180). Capacity to provide consent is complex, and specific cut-off scores on tools such as the MMSE or MOCA are not predictive of capacity to provide consent.

Comment
Page 10, lines 196-197, what is the justification for doubling the perturbations per session from the previous study? Is the number of perturbation trials a convenience factor, or is there evidence to suggest that the authors need to increase the number of perturbations to have benefits? What about fatigue? Will this be too many trials to tolerate? If the participants need rest breaks, will the intervention still fit into these 45 minutes?

Response
This was done to more feasibly fit the training into participants' therapy schedules for out-patient rehabilitation (the previous study was done in in-patient rehabilitation; see Pages 11, Lines 210-213). We have clarified that we expect participants will be able to tolerate 60 perturbations in the 45-minute sessions, and that rest breaks will be included (Page 11, Lines 213-216).

Comment
It is unclear why the authors are establishing feasibility when they have already carried out an RCT assessing this same intervention over six weeks. It seems like the authors should have already determined the feasibility.

Response
The goal of the current study is to inform the feasibility of the larger trial, not the feasibility of the intervention. Given that this trial is embedded in clinical care, it may be particularly challenging to prescribe a specific dose of RBT within participants' routine physiotherapy. We have clarified this in the paper (Page 8, Lines 151-152).

Comment
For the outcome measures, the authors should provide a delineated timeline of the data collection. For example, if there is only one training session, will the pre and post-testing be completed all within the same week. Or will post tests be completed after 6 PTs regardless of whether there is RBT in all the sessions. It does create a challenge as to when to post-test because some individuals will have more PT sessions than others, which could have an impact on the results. Also, there will be a longer course of recovery, how will the authors ensure that natural recovery is not playing a role in those that tested after waiting a more extended period to have a post-test. Clarify the number of weeks that the training will occur over. How long it will take for the 6 RBT training sessions to be completed. And will the post-test occur immediately after the RBT and before additional PT sessions?

Response
We have clarified that the outcome measure will be obtained at study enrolment (as soon as possible after admission to out-patient rehabilitation), discharge from rehabilitation, and 6-months post-discharge (see changes throughout the paper). At our institution, out-patient rehabilitation is typically 4-8 weeks long. Randomization should ensure that there is no between-group difference in length of stay in rehabilitation and, therefore, total dose of post-stroke physical rehabilitation.

Comment
Page 11, lines 216-218 – One of the research questions is to track the training visits. The groups only have 1, 3, or 6 visits. Are you planning to follow the visits for outpatient therapy? If this is the case, how will the authors account for differences in when PTs will discharge the person from outpatient treatment. The differences in treatment given during the PT sessions. How do you account for a therapist that may do perturbation training as part of their clinical practice?

Response
While we will prescribe 1, 3, or 6 sessions for research participants, participants or their physiotherapists may decline to complete any sessions that they feel are not helpful. Alternatively, participants or their physiotherapists may choose to complete additional sessions, outside of the study, if they feel this is beneficial. Therefore, we will track how many of the prescribed sessions are completed and any additional sessions completed outside of the study. We have clarified this in the paper (Page 16, Lines 345-348). For enrolled participants, physiotherapists will be asked not to complete RBT in their clinical practices in addition to that prescribed by the study, although we are unable to prevent physiotherapists from doing this.

Comment
Page 11-12 lines 230-234, the authors state that there will be 8-10 walking trials on a movable platform. Two trials the platform will move forward and two trials it will move backward. This only accounts for 4 of the 8-10 trials, what is the task for the remaining trials?

Response
To ensure unpredictability of the perturbations, only 4 of the 8-10 walking trials will include perturbations. The remaining trials will be unperturbed walking. This is clarified in the paper (Page 13, Lines 262-263).

Comment
Page 12, 251-253, please clarify the new data collection time post-discharge at 2- 4- and 6-months post-discharge for the PASIPD and SIPSO. What does the post-discharge mean, inpatient rehabilitation, outpatient PT, or from the study? These time points are very different from the data collection of the rest of the outcome measures. Why were different time points selected?

Response

We trust that these timepoints are clearer now with clarification of the other data collection timepoints. The PASIPS and SIPSO measure daily physical activity and participation in daily life. These are assessed post-discharge from rehabilitation, after participants have returned to their 'normal' lives.

Comment
Page 14, 285-286, Since it is necessary to train the research assistant in data collection techniques from the PI, there is a potential that bias will be introduced since the training is from the PI who is unblinded. How can the PI ensure quality data collected by the research assistant that they will oversee if they are unblinded? It is recommended that the person that is overseeing the testing and quality of this data should also be blinded.

Response
The research assistant has already received this training; this is clarified in the paper (Page 15, Line 318). The principal investigator will not be involved in scoring the assessments.

Comment
The aims of the study and the assessment tools used to answer these questions are not clear. The authors state that they want to determine the optimal dose of reactive balance training in people with subacute stroke to design a larger study to answer this question. Below is a list of research questions and outcome measures the authors want to answer. It is hard to decipher the connection of the outcome measures to the aims of the study.

1. Page 8 lines 14-145, optimal sample size, how long to achieve sample, feasibility to prescribe a specific dose of RBT, secondary outcomes to use, and what two interventions to use.
2. Page 11 lines 215-218, Outcome Measures: feasibility: accrual rate, missed training sessions, missing data for outcomes.
3. Page 11, lines 226-228, CMSA, mini-BEST, ABC, reactive balance control over unpredictable and novel perturbations
4. Page 11 starting at line 229 - slips and trips walking on the treadmill (vague description of the outcome measure and model used to assess balance stability)
5. Page 8, lines 150-152, reactive balance control, functional balance, balance confidence, falls, physical activity, and participation. Will there be secondary outcome measures?

Response
We have clarified how the balance and mobility related outcome measures will be used in the pilot study (Page 16, Lines 339-342).

Comment
The feasibility of dose – it is unclear if the authors can answer this question with only 6 sessions. How will the authors know if the participants continue to improve and could benefit from more sessions?

Response
If the 6-session group is not feasible then any additional sessions will not be feasible. Our physiotherapists did not think it would be feasible to include any more than 6 sessions within the out-patient rehabilitation schedule. For example, some patients are only prescribed 2 sessions per week of physiotherapy for 8 weeks. For participants assigned to the 6-session group, this only leaves 10 physiotherapy sessions to work on other rehabilitation goals besides improving reactive balance control.

Comment
It is not until I reach the data analysis section that I realized the question related to the optimal sample size was based on the fall rate. The data analysis section was not specific on the fall rate over a time period or if the comparison would be done comparing each group of the groups would be collapsed.

Response
We have included information on the time period for collection of falls data in the data analysis section (Page 16, Line 331). This information is also included in the Outcome measures section (Page 13, Line 276).

Comment
The authors state that they want to determine the best measure to use for assessing change but if you are also testing the dose over three groups you have introduced two variables that could potentially have an impact on your results. Is the measure good to capture change? Or is the dosage not enough to evoke change?

Response
Our intention is not to determine which outcome measures are useful for assessing change. We have modified Research Question 3 to clarify that we wished to determine which of the proposed secondary outcomes are feasible (Page 8, Line 150 and Page 16, Line 336).

REVIEWER 4

Comment
This study protocol describes a study that aims to provide pilot data for a larger trial to determine the optimal dose of reactive balance training for people in sub-acute rehabilitation after stroke. The paper describes the design of the pilot trial providing useful information to the scientific community regarding current research in this field. Please find my comments below.

1. P 3, L50. The actual date of first enrolment should be able to be stated now. Please clarify.

Response
Unfortunately, the study start has been delayed due to changes in care related to the COVID-19 pandemic. We have updated the anticipated start date.

Comment
2. P6, L55. 'This is a crucial period for fall prevention due to the high risk…'. It may eventuate reactive balance training may not be suitable for this period. In this study, reactive balance training sessions were embedded into the existing rehabilitation program. As the authors state 'there is a risk that many patients will decline participation in the study as they will not want their rehabilitation care to be disrupted' (P6, L8). It may be worth considering providing this reactive balance training program after completion of the existing rehabilitation. Further comment on this issue could be included in the discussion.

Response
While we suspect that some patients may decline participating in the study, which requires that they agree to being randomized to one of the three doses of RBT, we have no reason to believe that patients will object to RBT being included in their rehabilitation care if this is recommended by their physiotherapists. Indeed, in our previous experiences implementing RBT as part of routine care, patients for the most part seem to enjoy the training and acknowledge that it is beneficial to their care. We have clarified that we believe it is the protocol-mandated training schedule, specifically, that patients may object to (Page 6, Lines 105-106). However, we believe we do not have enough data at this point to speculate further on this in the Discussion section.

Comment
3. There are many balance exercises without postural perturbations that can induce a 'loss of balance' (P7, L18). Further information is required regarding how the current intervention differs from

‘conventional balance training’ (P7, L15)? This should be clearly stated as the authors are claiming that ‘Unlike other forms of exercise,’ (P7, L34) reactive balance training may result in rapid improvement in reactive balance control.

Response
We have clarified that the goal of ‘conventional’ balance training is to maintain balance during the exercises (Page 7, Lines 117-118). Conversely, with reactive balance training, clients repeatedly lose balance to intentionally evoke balance reactions (Page 7, Line 119). Additionally, we have clarified that ‘other forms of exercise’ refers to exercises to improve other components of physical fitness (e.g., strength or cardiorespiratory fitness), which takes weeks or months of regular training to show improvements (Page 7, Lines 126-127).

Comment
4. P25, L20. Repeating the same set of perturbations 3 times at pre-training, post-training and 6-month follow-up, will reduce the novelty / unpredictability of the perturbation. Some comment is warranted.

Response
We have added further discussion on this point (Page 13, Lines 265-268).

Comment
5. P10, L203. The RBT method is not written in a way that other researchers or clinicians could replicate. (a requirement in the SPIRIT checklist). If the references cited in this section present the detailed methods of the intervention, please indicate this.

Response
We have provided some specific examples of training approaches in this section, and refer the reader to our previous paper where we have provided more specific details of the intervention approaches (Page 11, Lines 219-226).

Comment
6. P13, L22. ‘as they were occur’ , needs correcting.

Response
This typographic error has been corrected (Page 14, Line 297).

Comment
7. P14, L23. ‘secure institutional severs’, check the spelling.

Response
We have corrected this error (Page 15, Line 323).

Comment
8. P14, L34. What is the comparator to ‘the one-session group’ regarding fall rate?

Response
There is no comparison being made for the fall rate. As this is a pilot study and not an efficacy/effectiveness study, we will not compare the fall rate (or other outcomes) between groups. We will use the fall rate from the one-session group to estimate the sample size for the larger trial.

Comment

9. Some sections are not well ordered. E.g. ‘6.2 Participants’ and ‘6.6 Recruitment’ should be next to each other. ‘6.4 Outcome measures’ and ‘8.1 Data collection methods’ should be close to each other. Consider reordering the sections by study timeline.

Response
The paper follows the order of items in the SPIRIT checklist. We have clarified this in the paper (Page 8, Lines 156-157).

VERSION 2 – REVIEW

REVIEWER	Itshak Melzer Ben-Gurion University of the Negev, Beer-Sheva, Israel
REVIEW RETURNED	06-May-2020

GENERAL COMMENTS	I think the authors provided the answer to my comments in review 1.

REVIEWER	Vicki Gray University of Maryland School of Medicine, USA
REVIEW RETURNED	15-May-2020

GENERAL COMMENTS	The authors have done a good job addressing all of my concerns.

REVIEWER	Stephen Lord NeuRA, UNSW, Australia
REVIEW RETURNED	04-May-2020

GENERAL COMMENTS	The authors have addressed all my issues. Thank you. I have no further issues to raise.