PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Nature versus urban hiking for Veterans with posttraumatic stress disorder: a pilot randomized trial conducted in the Pacific Northwest United States
AUTHORS	Littman, Alyson; Bratman, Gregory N; Lehavot, Keren; Engel, Charles C; Fortney, John; Peterson, Alexander; Jones, Alex; Klassen, Carolyn; Brandon, Josh; Frumkin, Howard

VERSION 1 – REVIEW

REVIEWER	Vin-Raviv, Neomi Colorado State University College of Health and Human Sciences
REVIEW RETURNED	03-May-2021

GENERAL COMMENTS	Thank you for inviting me to review this manuscript. The aim of this study was to design and conduct a pilot study to test the feasibility and acceptability of a nature-based PA intervention for PTSD symptoms in military Veterans, regardless of PTSD etiology. The intervention (nature hiking) and the active control (urban hiking) were group based and involved similar amounts of PA, to ensure control of the potential benefits of the group-based social support and of PA I believe that the topic is very interesting with scientific interest and that the authors put effort in drafting this work. However, the current version of the manuscript is let down by its written presentation, the results are not well presented and important details in the methods are lacking or not clear. Nevertheless, I offer comments and suggestions for the author's consideration. Introduction Definitions: The authors stated in the objectives [page 7 line 35]: "Our goal was to design and conduct a pilot study to test the feasibility and acceptability of a nature-based PA intervention for PTSD symptoms in military Veterans, regardless of PTSD etiology. The intervention (nature hiking) and the active control (urban hiking) were group based and involved similar amounts of PA, to ensure control of the potential benefits of the group-based social support and of PA." There is differences between physical activity and recreation or leisure activity. It look that the intervention in the current manuscript was actually walking [measured by physical activity assessment] either in nature or urban. The correct terminology is actually green exercise (i.e., exercise in nature-based green environments) compared to exercise in urban environments. While there is gap, regarding

	veteran population there is research on mental and physical health outcomes of green exercise. The authors should revise the introduction to fit better their current study. Below they can find some papers to help: 1. Beil K, Hanes D, Beil K, Hanes D. The influence of urban natural and built environments on physiological and psychological measures of stress— A pilot study. Int J Environ Res Public Health. 2013;10(4):1250–1267. doi:10.3390/ijerph10041250 [PMC free article] [PubMed] [CrossRef] [Google Scholar] 2. Pretty J, Peacock J, Sellens M, Griffin M. The mental and physical health outcomes of green exercise. Int J Env Heal Res 2005;15(5):319–337. doi:10.1080/09603120500155963 [PubMed] [CrossRef] [Google Scholar] 3. Marselle MR, Irvine KN, Warber SL. Walking for well-being: Are group walks in certain types of natural environments better for well-being than group walks in urban environments? Int J Environ Res Public Health. 2013;10(11):5603–5628. doi:10.3390/ijerph10115603 [PMC free article] [PubMed] [CrossRef] [Google Scholar] 4. Rogerson, M., Colbeck, I., Bragg, R., Dosumu, A., & Griffin, M. (2020). Affective Outcomes of Group versus Lone Green Exercise Participation. International journal of environmental research and public health, 17(2), 624. https://doi.org/10.3390/ijerph17020624 Methods: The study methods is confusing and hard to follow, it would be strongly recommended to re-organize the section. Suggestions for reorganization: 1. Identification and Recruitment of Participants 2. Study Design- it is not clear what was the study design. The authors should create subheading and clearly explain the two harms, durations etc. 3. Experimental Sites [Hiking Sites]- The authors should expends the information about the sites 4. Activities- It seem it include only hiking activities. Where there any additional group/ therapeutic activates? Regarding the two non-clinicians who led the hike, what where there training? Where they were veterans? Who trained and supervised the process? 5. Outcomes- The authors only report only regarding PTSD-Checklist-5 (PCL-5), however in the tables they report also Patient Health Questionnaire-8 6. Baseline and follow-up assessments 7. Data Analysis For clarity separate to two subheading and explain how data was analyzed a. Quantitative Analysis b. Qualitative Analysis Regarding-Patient involvement: The authors should consider moving this section to Author contributions at the end of the manuscript, or add to acknowledgment to Veterans who took part in the study. Results should start with the CONSORT participant flow diagram. Followed by table describing the demographic characteristic of the sample. Was there differences in PHQ-8 or self-reported health

	physical activity level? The authors should also present the qualitative results [what was the common themes]. And address them accordantly in their discussion.

REVIEWER	Trick, Leanne Durham University, Psychology
REVIEW RETURNED	07-May-2021

GENERAL COMMENTS	This paper describes a pilot feasibility study of a nature hiking intervention for military veterans with PTSD. Given the prevalence of PTSD among veterans and limitations of existing treatments, the exploration of alternative interventions is an important topic and emerging preliminary work suggests that nature-based interventions are an interesting direction for future work in this area. The manuscript is generally well written and presented clearly. I would suggest that the following minor comments are addressed before this manuscript is accepted for publication. INTRODUCTION: 1. The majority of the introduction is not specific to military veterans with PTSD, and a more detailed justification for choosing this particular sample should be provided. 2. The introduction omits previous studies investigating the efficacy of other nature-based therapies among veterans with PTSD (e.g. Greenleaf & Roessger, 2017; Poulsen et al. 2016). This literature is of relevance and should be included. METHOD: 3. Please provide a justification for the number of hikes and the duration of the intervention (i.e. 6 hikes over 12 weeks). 4. The purpose, and other details (e.g. timing and duration of recording), of wrist activity monitors is not described. RESULTS: 5. A small proportion of invited/eligible potential participants were eventually randomized. Is it possible to explore whether the included participants were comparable to those who were invited but chose not to participate in terms of basic demographic information (such as age/sex/geographical location) to indicate if those who participated were broadly representative of the target population? And will this be taken into consideration in relation to the sampling strategies employed in the full trial to ensure generalisability of findings? 6. Since the main objectives of the study were to evaluate feasibility and acceptability, the description of findings related to acceptability in the text is rather light. This section would benefit from being expanded (or to include better signposting to Table 2 – which currently seems to be referenced in regard to social connectedness).

	7. In the section regarding efficacy the authors state that: “Median PCL-5 scores decreased from baseline to week 12 and 24 for those in the nature but not among those in the urban hiking group”. In fact, median scores did also appear to decrease from baseline to week 12 in the urban hiking group (but not from baseline to week 24). This should be clarified. DISCUSSION: 8. The first paragraph of the discussion would benefit from revision to more accurately reflect the study findings. For example, the authors state that “Most participants completed most hikes”. However, given that only just over half of the hikes were attended (56% and 58% for the nature and urban groups respectively) I believe this overstates the case. Similarly, the authors also state that “Patients reported high acceptability, enjoyment and value”. However, as per my previous comment, the reporting of these outcomes lacked detail (the results section simply states that participants felt positively about their experiences) so it is unclear to what extent this comment is justified. 9. On average just over half of the hikes were attended – meaning the 12 week intervention consisted of around only 4 hikes. Can the authors provide any comment on the anticipated effective ‘dose’ of the intervention?

VERSION 1 – AUTHOR RESPONSE

Reviewer #1

**** Response: We appreciate the reviewer’s enthusiasm for our work.**

However, the current version of the manuscript is let down by its written presentation, the results are not well presented and important details in the methods are lacking or not clear.

Definitions: The authors stated in the objectives [page 7 line 35]: “Our goal was to design and conduct a pilot study to test the feasibility and acceptability of a nature-based PA intervention for PTSD symptoms in military Veterans, regardless of PTSD etiology. The intervention (nature hiking) and the active control (urban hiking) were group based and involved similar amounts of PA, to ensure control of the potential benefits of the group-based social support and of PA.” There is differences between physical activity and recreation or leisure activity. It look that the intervention in the current manuscript was actually walking [measured by physical activity assessment] either in nature or urban. The correct terminology is actually green exercise (i.e., exercise in nature-based green environments) compared to exercise in urban environments. While there is gap, regarding veteran population there is research on mental and physical health outcomes of green exercise. The authors should revise the introduction to fit better their current study.
Below they can find some papers to help:
1. Beil K, Hanes D, Beil K, Hanes D. The influence of urban natural and built environments on physiological and psychological measures of stress— A pilot study. Int J Environ Res Public Health. 2013;10(4):1250–1267. doi:10.3390/ijerph10041250 [PMC free article] [PubMed] [CrossRef] [Google Scholar]
2. Pretty J, Peacock J, Sellens M, Griffin M. The mental and physical health outcomes of green exercise. Int J Env Heal Res 2005;15(5):319–337. doi:10.1080/09603120500155963 [PubMed] [CrossRef] [Google Scholar]

3. Marselle MR, Irvine KN, Warber SL. Walking for well-being: Are group walks in certain types of natural environments better for well-being than group walks in urban environments? Int J Environ Res Public Health. 2013;10(11):5603–5628. doi:10.3390/ijerph10115603 [PMC free article] [PubMed] [CrossRef] [Google Scholar]
4. Rogerson, M., Colbeck, I., Bragg, R., Dosumu, A., & Griffin, M. (2020). Affective Outcomes of Group versus Lone Green Exercise Participation. International journal of environmental research and public health, 17(2), 624.
**** Response: We appreciate the reviewer's suggestion and have reviewed the literature that the reviewer provided (Beil et al., 2013; Pretty et al, 2005; Marselle et al, 2013; Rogerson et al, 2020). These are now cited in the manuscript.**

The study methods is confusing and hard to follow, it would be strongly recommended to re-organize the section.
Suggestions for reorganization:
1. Identification and Recruitment of Participants
2. Study Design- it is not clear what was the study design. The authors should create subheading and clearly explain the two harms, durations etc.
3. Experimental Sites [Hiking Sites]- The authors should expends the information about the sites
4. Activities- It seem it include only hiking activities. Where there any additional group/ therapeutic activates? Regarding the two non-clinicians who led the hike, what where there training? Where they were veterans? Who trained and supervised the process?
5. Outcomes- The authors only report only regarding PTSD-Checklist-5 (PCL-5), however in the tables they report also Patient Health Questionnaire-8
6. Baseline and follow-up assessments
7. Data Analysis
For clarity separate to two subheading and explain how data was analyzed
a. Quantitative Analysis
b. Qualitative Analysis
**** Response: We apologize that our methods were confusing and hard to follow and appreciate these suggestions. We have reorganized the methods as suggested by the reviewer. Because the changes are so extensive, we are not detailing them here.**

Please note that since there was no mention of where to include "Ethics approval", we moved some of this (clinicaltrials.gov registration) to the study design section and the rest to its own section at the end of the methods. We would appreciate input from the editor and/or the reviewer if these sections should be placed elsewhere. Additionally,

Regarding-Patient involvement: The authors should consider moving this section to Author contributions at the end of the manuscript, or add to acknowledgment to Veterans who took part in the study.
**** Response: We have moved some of the section entitled "Patient involvement" to the Acknowledgement section. However, we retained this section because it is a journal requirement and we were explicitly instructed by the editor to include it. This section is the second to last section in the methods. If the editor and reviewer feels it should be elsewhere, we would be happy to move it.**

Results should start with the CONSORT participant flow diagram. Followed by table describing the demographic characteristic of the sample.
**** Response: We moved the section entitled "Feasibility – recruitment statistics" to the start of the results section, followed by information about participants. The CONSORT diagram is Figure 3.**

Figure 1 is the conceptual model (included in the introduction) and Figure 2 is the study overview (in the methods section). Thus, the Figure 3 CONSORT diagram is the first figure presented in the Results.

Was there differences in PHQ-8 or self-reported health physical activity level?
**** Response: In the revised manuscript, we present baseline levels (in categories) of PHQ-8. While we did not perform statistical tests, distributions between the two groups were similar; 33% and 31% of those in the nature and urban hiking groups, respectively, had a score <10, indicating no depression; 58% and 54% had a score 10-19, indicating major depression, and 8% and 15% had a score ≥20, indicating severe major depression. While we had participants complete the PHQ-8 during follow-up, given the feasibility goals of this pilot study, we have not presented these results for this measure nor those for other secondary outcomes.**

In terms of self-reported physical activity levels, a slightly greater proportion of individuals in the nature vs. urban hiking arm had low physical activity (38% vs. 23%), while a smaller proportion of those in the nature vs. urban hiking group had moderate (8% vs. 15%) and high (54% vs. 62%) physical activity at baseline. Depression and self-reported physical activity were NOT secondary outcomes for this pilot study. Instead, we collected information on these measures to describe the sample at baseline. We now explain in lines 262-254 that physical activity was a potential mechanism of benefit, which we would want to measure precisely in a future study.

The authors should also present the qualitative results [what was the common themes]. And address them accordantly in their discussion.
**** Response: We now present the qualitative results in lines 344-354 in the results and in Table 2, and they are integrated into the discussion e.g., lines 367-371, 378-380, 424-428, and 432-444.**

Reviewer #2

**** Response: We are pleased that the reviewer found our paper to be generally well-written and presented clearly.**

INTRODUCTION
1. The majority of the introduction is not specific to military veterans with PTSD, and a more detailed justification for choosing this particular sample should be provided.
**** Response: We have now provided a more detailed justification for choosing military veterans with PTSD. Specifically, in lines 97-99, we state that "**Group-based PA interventions may be particularly well-suited for military Veterans, due to 1) proportionally higher rates of PTSD among Veterans (40), 2) consistency of PA interventions with values cultivated during military service, and 3) benefits of social interaction with Veteran peers (41)..**" We also note in lines 128-130, "**Social support forged through group activity could be particularly relevant for Veterans, as camaraderie and solidarity are critical components of military culture, and ones that are frequently lost in the return to civilian life (61)."

2. The introduction omits previous studies investigating the efficacy of other nature-based therapies among veterans with PTSD (e.g. Greenleaf & Roessger, 2017; Poulsen et al. 2016). This literature is of relevance and should be included.
**** Response: We appreciate the reviewer's suggestion and have added the papers noted by the reviewer to the introduction (lines 123-124).**

METHOD:
3. Please provide a justification for the number of hikes and the duration of the intervention (i.e., 6 hikes over 12 weeks).
**** Response: In the methods (lines 200-202), we now explain, "We chose to offer 6 hikes (vs. more or fewer) because this number was thought to be feasible and would be sufficient to assess feasibility and acceptability."**

4. The purpose, and other details (e.g. timing and duration of recording), of wrist activity monitors is not described.
**** Response: We have added the requested information on the wrist activity monitors. In the "baseline and follow-up assessments" section (lines 262-265), we now state, "In addition to questionnaires, to obtain objective information about PA (a potential mechanism of benefit, which we would want to measure precisely in a future study), we asked participants to wear a wrist worn-activity monitor (Garmin vivosmart 4) every day, for at least 10 hours per day, for the first 12 weeks of the study. No incentives were provided for wearing or synching the watch."**

RESULTS:
5. A small proportion of invited/eligible potential participants were eventually randomized. Is it possible to explore whether the included participants were comparable to those who were invited but chose not to participate in terms of basic demographic information (such as age/sex/geographical location) to indicate if those who participated were broadly representative of the target population? And will this be taken into consideration in relation to the sampling strategies employed in the full trial to ensure generalisability of findings?
**** Response: We appreciated the reviewer's question. We have added the following sentences to the results (lines 302-305): "Compared to those contacted and not randomized, a greater proportion of those randomized were women (27% randomized vs. 15% of those contacted), white (73% vs. 63%), and Hispanic (8% vs. 6%). Additionally, those who were randomized were younger (mean age = 47, range 25-65) than those not randomized (mean age = 52, range: 21-95)."**

6. Since the main objectives of the study were to evaluate feasibility and acceptability, the description of findings related to acceptability in the text is rather light. This section would benefit from being expanded (or to include better signposting to Table 2 – which currently seems to be referenced in regard to social connectedness).
**** Response: We apologize for this omission in the first submission. We now provide more information on how acceptability was assessed in the methods (lines 236-246). In the results, on lines 337-354, we now more fully describe the acceptability findings. We have moved the reference to Table 2 in the results to line 350 to make it clearer that this table includes all qualitative findings, not just those related to social connectedness.**

7. In the section regarding efficacy the authors state that: "Median PCL-5 scores decreased from baseline to week 12 and 24 for those in the nature but not among those in the urban hiking group". In fact, median scores did also appear to decrease from baseline to week 12 in the urban hiking group (but not from baseline to week 24). This should be clarified.
**** Response: We apologize for not being more clear in our description of the findings. The revised sentences (lines 357-360) now acknowledge the decrease in PCL-5 scores from baseline to 12-weeks follow-up among those in the urban hiking group. We now state, "Median PCL-5 scores decreased from baseline to week 12 and remained at the 12-week level at week 24 for those in the nature hiking group (baseline=41, 12-weeks = 32, 24 weeks=31). Among those in the urban hiking**

group, PCL-5 scores decreased from baseline to 12 weeks but increased nearly back to baseline levels at 24 weeks (baseline=48, 12-weeks = 43, 24 weeks=47) (Supplemental Figure 1)."

DISCUSSION:
8. The first paragraph of the discussion would benefit from revision to more accurately reflect the study findings. For example, the authors state that "Most participants completed most hikes". However, given that only just over half of the hikes were attended (56% and 58% for the nature and urban groups respectively) I believe this overstates the case.
**** Response: We have revised this sentence (lines 368-369) to read, "In both arms, more than half of participants completed most hikes."**

Similarly, the authors also state that "Patients reported high acceptability, enjoyment and value". However, as per my previous comment, the reporting of these outcomes lacked detail (the results section simply states that participants felt positively about their experiences) so it is unclear to what extent this comment is justified.
**** Response: In responding to comment #6 by Reviewer #2, we hope that we have better addressed this concern.**

9. On average just over half of the hikes were attended – meaning the 12 week intervention consisted of around only 4 hikes. Can the authors provide any comment on the anticipated effective 'dose' of the intervention?
**** Response: Unfortunately, this pilot study does not provide information on the anticipated effective dose of the intervention. We suspect that more than 4 hikes are needed and suggest that 8-12 hikes (similar to a standard psychotherapy course) may be optimal for achieving clinically meaningful results. Additional research would be necessary to examine this important question. We address this question in the revised manuscript in lines 435-438.**

VERSION 2 – REVIEW

REVIEWER	Vin-Raviv, Neomi Colorado State University College of Health and Human Sciences
REVIEW RETURNED	15-Aug-2021

GENERAL COMMENTS	Dear Editor, Thank you for inviting me to review the revised manuscript. I read carefully both the revised manuscript and the author's response to reviewers. The authors have address the comments raised in the previous round of review. I believe the revised manuscript resubmitted is now vastly improved over their previous version. The revised manuscript represents some excellent work that should be published in BMJ Open. Thank you again for the opportunity to review this excellent paper!

REVIEWER	Trick, Leanne Durham University, Psychology
REVIEW RETURNED	09-Aug-2021

GENERAL COMMENTS	This revised manuscript, describing a pilot study of nature vs. urban hiking for military veterans with PTSD, has been substantially re-worked and improved. In general, I found the revised version of the manuscript to be well written and presented clearly. The introduction is more focused which is welcomed and I am pleased to note that my comments related to this section (and the discussion) have been dealt with carefully and adequately. My comments regarding the methods and results sections have also been incorporated satisfactorily. However, since these sections have been substantially altered there are a few additional (very minor) points remaining that require amendment/clarification, as follows: METHOD Identification and recruitment of participants 1. The revised methods section draws attention to the strict eligibility criteria. The exclusions are understandable and the authors comment on this (in the discussion) in the context of inefficiency in recruitment. However, another consequence is the impact on generalisability (e.g. the findings may not apply to veterans with severe mental health comorbidities for instance). This is a common issue, but will be especially important in the full trial, and I think brief mention of this limitation could be made (potentially somewhere around lines 400 to 405). 2. Line 156 - 'whichever happened first' – I suspect this is a hangover from the study protocol, and as the study is now complete you can specify which it was. Study design 3. Line 178 – for consistency (with the manuscript title and other sections) the study should be described as a pilot study rather than a 'feasibilility trial'. Outcomes 4. Line 234 – how was the 70% target for retention and attendance chosen? If based on previous studies could a reference be provided? Baseline and follow-up assessments 5. Line 256 –'each week after the first hike for 12 weeks (after the 6th hike)' doesn't make sense, please clarify. Data analysis 6. The new data analysis section re. quantitative analysis is not complete. The quantitative questionnaire data related to acceptability that is summarised in the results section (lines 336 to 342) should appear here. RESULTS 7. Lines ~339 to 340 – the description of the scales is a bit unclear and would benefit from clarification. I think it would help to specify in the methods (line 238) how many points each scale had and how the scores relate to the value labels (since descriptors and values are variously reported in the results). Giving both the

	descriptors and the values in the results section may also help (for those that varied between groups and those that didn't). 8. Line 340 – 'Scores (minimum=1, maximum=5)' belongs in the methods section.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1
Dr. Neomi Vin-Raviv, Colorado State University College of Health and Human Sciences

Comments to the Author:
Dear Editor,
Thank you for inviting me to review the revised manuscript.
I read carefully both the revised manuscript and the author's response to reviewers. The authors have address the comments raised in the previous round of review.
I believe the revised manuscript resubmitted is now vastly improved over their previous version.
The revised manuscript represents some excellent work that should be published in BMJ Open.
Thank you again for the opportunity to review this excellent paper!

**** Response: We thank the reviewer for re-reviewing the manuscript and are pleased to hear that the reviewer feels that we have addressed the comments previously raised.**

Reviewer: 2
Dr. Leanne Trick, Durham University

Comments to the Author:
This revised manuscript, describing a pilot study of nature vs. urban hiking for military veterans with PTSD, has been substantially re-worked and improved. In general, I found the revised version of the manuscript to be well written and presented clearly. The introduction is more focused which is welcomed and I am pleased to note that my comments related to this section (and the discussion) have been dealt with carefully and adequately.

**** Response: We thank the reviewer for their kind comments.**

METHOD
Identification and recruitment of participants
1. The revised methods section draws attention to the strict eligibility criteria. The exclusions are understandable and the authors comment on this (in the discussion) in the context of inefficiency in recruitment. However, another consequence is the impact on generalisability (e.g. the findings may not apply to veterans with severe mental health comorbidities for instance). This is a common issue, but will be especially important in the full trial, and I think brief mention of this limitation could be made (potentially somewhere around lines 400 to 405).
**** Response: Thank you for the reminder to consider generalizability. We have revised the final sentence of the paragraph (lines 411-412) to raise this important point. We now state, "Changing inclusion criteria (e.g., eliminating restrictions related to suicidality) might improve recruitment and generalizability, but would require tradeoffs related to safety and retention that must be considered carefully."**

2. Line 156 - ‘whichever happened first’ – I suspect this is a hangover from the study protocol, anas the study is now complete you can specify which it was.
**** Response: The reviewer makes an excellent point. The sentence now reads, “We followed the mailing with up to three phone calls until the recruitment period ended.”**

Study design
3. Line 178 – for consistency (with the manuscript title and other sections) the study should be described as a pilot study rather than a ‘feasibilility trial’.
**** Response: We have revised the sentence to read, “We conducted a two-arm randomized controlled pilot trial.”**

Outcomes
4. Line 234 – how was the 70% target for retention and attendance chosen? If based on previous studies could a reference be provided?
**** Response: We aimed for 70% target for retention and attendance as that is a commonly cited standard for trials** (1,2)**.**

Baseline and follow-up assessments
5. Line 256 –‘each week after the first hike for 12 weeks (after the 6th hike)’ doesn’t make sense, please clarify.
**** Response: We can now see that this sentence did not make sense. We have rewritten it to now read**, **“We conducted assessments online using commercial software (QuestionPro) at baseline (before hikes began), and then weekly for 12 weeks, starting with the week of the first hike and ending the week after the 6th hike, and finally at week 24 …”**

Data analysis
. The new data analysis section re. quantitative analysis is not complete. The quantitative questionnaire data related to acceptability that is summarised in the results section (lines 336 to 342) should appear here.
**** Response: The reviewer makes an excellent point. We have added a few sentences to describe data analysis for acceptability measures. We now state (lines 273-280): “For acceptability measures related to communication, we categorized responses as positive if respondents chose one of the two most favorable response options (e.g., satisfied/very satisfied; good/excellent) and not positive if they chose one of the other response options (extremely unsatisfied/unsatisfied/ neither satisfied or unsatisfied; inadequate/very poor/adequate). We then calculated the proportion of urban and nature participants with favorable responses for each question. In addition to proportions, we also calculated the mean scores for hike locations, distance, pace, pre-hike information, pre-hike communication, and trailhead communication by group.”**

RESULTS
7. Lines ~339 to 340 – the description of the scales is a bit unclear and would benefit from clarification. I think it would help to specify in the methods (line 238) how many points each scale had and how the scores relate to the value labels (since descriptors and values are variously reported in the results). Giving both the descriptors and the values in the results section may also help (for those that varied between groups and those that didn’t).
**** Response: We apologize for the confusion. Note also that our previous manuscript included a minor error, which we have corrected. All measures were on a 5-point Likert scale. We have revised the text to reflect this and hope that it is now clear.**

8. Line 340 – ‘Scores (minimum=1, maximum=5)’ belongs in the methods section.

**** Response: We removed mention of the range in the results, and state in the methods (lines 236-241), “To assess acceptability, in the 6- and 12-week questionnaires, we included questions created for the study about the difficulty of the hikes’ distance, pace, and the terrain (rated on a 5-point scale from extremely difficult to effortless), and satisfaction with the locations of hikes (rated on a 5-poit scale from extremely unsatisfied to very satisfied). Lastly, pre-hike and trailhead information and communication were assessed on a 5-point scale (e.g., from very poor (1) to excellent (5)). We also included open-ended questions for participants to report what they thought went well and what could have been better.**

VERSION 3 – REVIEW

REVIEWER	Trick, Leanne Durham University, Psychology
REVIEW RETURNED	06-Sep-2021

GENERAL COMMENTS	This latest revision of the manuscript has fully addressed my remaining comments. It was a pleasure to have the opportunity to review this interesting work which I would now recommend is accepted for publication. I look forward to seeing the outcome of the planned full trial in the future!