Author’s response to reviews

Title: Decreased risk of falls in patients attending music sessions on an acute geriatric ward: results from a retrospective cohort study

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Author’s response to reviews:

Manuscript:

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-Title: “Decreased risk of falls in patients attending music sessions on an acute geriatric ward: results from a retrospective cohort study”

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We thank the Reviewers for their thoughtful review. As suggested, we performed a revision of our manuscript. We hope that the revised version has significantly improved the manuscript.

Please, find below our point-by-point responses to the comments and queries raised by the Reviewers and the Editor.

Comments from the Editors:

Comment 1: Please provide more details about the study design – it is not clear from the present manuscript whether you “recruited” the patients or retrieved their records. If they were not recruited then this cannot be considered a clinical trial and it more likely that this is a
Our study was a retrospective cohort analysis. These clarifications have been made in the revised manuscript. Please refer to pages 3, 6 for corrections. Also, further modifications were made as per the comment #9 of Reviewer 1. Please see corrections on pages 6.

Comment 2: If this is a retrospective cohort study as, at present, it appears then the title and the results need to be adjusted to reflect this. For instance, the results need to report the relative risk of the dancing group to the control group.

We followed the editor’s suggestions. The title was changed from “Decreased risk of falls in patients attending music sessions on an acute geriatric ward: results from a non-randomized open-label trial” to: “Decreased risk of falls in patients attending music sessions on an acute geriatric ward: results from a retrospective cohort study”. The results have been changed as requested and as per the comments #13 and 14 of Reviewer 1. Please see corrections on pages 9 and 10.

Comment 3: If the patients were recruited as part of a clinical trial, then you need to provide more details such as selection criteria for both groups and the Inclusion/Exclusion criteria for a patient to take part in the trial.

As stated above, this was not a clinical trial. This was a retrospective cohort analysis. Also, please refer to the modification which was made according to the comment # 9 of Reviewer 1. Please see corrections on page 6.

Comment 4: As per BMC Complementary and Alternative Medicine policy the trial should be reported as advised by the CONSORT statement (http://www.consort-statement.org/).
This is corrected in order to make sure that the study is appropriately reported. Please see corrections throughout the revised manuscript.

Comment 5: Also, if it was a clinical trial, which recruited patients, then full informed consent would be required for those patients. This, at present, is not mentioned in the Ethical Approval and Consent for Participation section.

Full consent form was not required because it was a retrospective analysis. Because there were musical sessions which had been ongoing on our Geriatric Assessment Unit, we took the opportunity to perform a retrospective cohort study comparing individuals who participated in musical sessions and those who did not. The study was approved by the Research Ethics Committee of St. Mary’s Hospital Center (SMHC #14-31). As this was a retrospective study, written consent from the participants was not obtained. This study was registered as Clinical trial registry: ClinicalTrials.gov. Registered number: NCT03348657 (November 17th, 2017). Please find all the details on page 8 of the manuscript.

Comment 6: Regardless of the study more details are required for the precise nature of the intervention (which, if this is a retrospective study is not intervention but instead ‘exposure’). The precise length of time dancing took place for, how often, how were they instructed, was this a routine procedure, did all patients participate in all sessions etc.

We apologize for the lack of information about the intervention. Clarifications were made to pages 3, 6, 8, 9 and 11 in order to change the word “intervention” to “exposure”. Also, on pages 6 and 7, more details were added to better explain the musical intervention: “the exposed group participated in average to 1.5 music listening sessions (minimum 1, maximum 4) which lasted 1 hour each”.

Comments from Reviewer 1:

Comment 7: This manuscript presents research in an area of high (and increasing) relevance. Music therapy has frequently shown to have positive effects and can be part of an easily
available but cheap intervention. Despite this fact, there is very little systematic research in this area. Most studies suffer from a lack of statistical power and/or potentially confounding factors that limit robust conclusions. This study's main strength is their large sample size, a posthoc design that still allows for the control of many potential confounding factors (e.g., medication, time since admission etc) to make groups comparable. I consider the manuscript relevant and well written in it's very concise way.

We thank the reviewer for this positive comment.

Comment 8: I recommend to provide effect sizes rather than p-values in the abstract. Statements about an "effect" already imply that it is beyond chance level. Effect sizes give the reader a better picture about the potential relevance for his/her own research and is not dependent on sample size.

We followed the recommendation of the Reviewer. The effect size for the change in Morse Fall Score was revised in the result section of the abstract. Please refer to page 3 of the manuscript.

Comment 9: The authors might have a look at some of the wordings and adjust it to the posthoc analysis design. The term "inclusion criteria", e.g., might be better replaced with "selection criteria".

The term "inclusion criteria" was changed to “selection criteria” on page 6 of the manuscript. Also, please refer to the methods section for further clarifications about the retrospective nature of this study.

Comment 10: A potential limitation of this study is the lack of indicators of emotional well-being or mood. Where music intervention improves mood or reduces agitation and anxiety (as previously shown), this might have an effect on mobility (the urge of it, and the behaviour when attempting to be mobile). The authors discuss potential mechanisms in their discussion section accurately, but the limitation of not having collected data on the subjective level (by observation, self-report, professional assessment) limits the explanatory power. This should be stated as a limitation and/or recommendation for future research.
We agree and followed her/his suggestion. A sentence was added to the manuscript in order to also include this limitation. See the modifications on page 10.

Comment 11: More generally, a recommendation for future research should be included in the manuscript.

We agree with this comment and changes have been made. Please refer to the modifications made on page 10 as per the previous comment: “Our study, however, did not include any indicators of emotional well-being or mood. In the future, questionnaires should be added to this effect”. Also, in the conclusion section (page 12), a recommendation for future research was added.

Comment 12: The authors might want to explain the relevance of controlling for season of admission for those readers not familiar with it.

This is an important comment. Season of admission was controlled for in our study because Montreal is a Nordic city with significant weather changes and patients admitted over the winter tend to require more medical care (higher rates of falls due to ice, higher rates of upper respiratory tract infections…). This has been clarified on page 7.

Comment 13: The authors might have a look at for example APA guidelines (or similar) regarding the presentation of multiple linear regressions and accordingly provide both beta and standardized beta weights (instead of only one) and explained power (adjusted R-square). This helps to compare this regression’s results with regressions from other articles.

We now report that: The overall regression model was not significant F (8,143) = 1.703, p = 0.102, R2 = 0.087. However, after adjusting for multiple participant’s characteristics, a significant decrease in MFS score after the music listening sessions was found (B = -17.1 and \( \beta \) = -0.186 with p = 0.043). Please see the modifications on pages 4 and 10.
Comment 14: The authors may also add effect sizes to the results section/tables (Cohen's d), as this would allow for a comparison with other music intervention effect sizes previously reported in e.g. meta-analyses (see e.g. Pedersen et al., 2017).

We followed the Reviewer’s suggestion. Please see the Cohen’s d which were added in Table 1 and Table 2 for the t-tests. Please see corrections on pages 18, 19 and 20.

Comment 15: The literature discusses various ways to apply music intervention for elderly, for example as passive listening or active participation, with self-selected music or "prescribed" music, in individual or group settings. To make the inclusion of this paper into later overview articles easier, I suggest to be very careful with providing the exact information/grouping of this study by describing the characteristics more detailed (along these mentioned criteria: did the participants have an influence on the music played, did they participate actively or just listened, how large were the groups, perhaps also describe the type of music) in the method section. In the discussion section, it should be made clear that other ways of conducting music intervention may have yielded different results and that the generalization of these results might be limited along these categories.

More details regarding the music listening sessions were added to pages 6 and 7. Also, the limitation mentioned is now added in the discussion section. Please refer to page 12.

Comment 16: The effect sizes could be compared to the context of other studies investigating music intervention, may it be with similar or different populations, with similar or different presentation modes.

We agree and added more details regarding the music listening sessions and in the discussion section. However, considering the heterogeneity of different studies (different music intervention and populations, we did not compare the effect sizes. Please refer to the modification made for comment #14.
Comments from Reviewer 2:

Comment 17: Why this equation: change in the MFS score was calculated as follows: \[((\text{discharge} - \text{upon admission})/ ((\text{discharge} + \text{upon admission})/2)) \times 100\] is used? Do you have any references for this?

This formula is simply the percentage difference. The nominator of the formula is the difference between the 2 values, while the denominator is the average of these 2 values. This is afterwards multiplied by 100 in order to present the results in a percentage. We used this approach as first reported by Beauchet O et al. Dual-task-related gait changes in the elderly: does the type of cognitive task matter? J Mot Behav. 2005;37:259-264.. We added this information in the revised Methods Section. Please see correction on page 9.

Comment 18: In the results section, don't just provide P value. You can provide some important numbers from your table.

We followed the Reviewer’s suggestion and modifications were made accordingly. Please refer to page 9 which is now providing the percentage of participants who were classified as high risk for falls upon admission.

Comment 19: I would suggest the authors to move Morse Fall Scale in table 1 to table 2. In this way, all information in table 2 are Morse Fall Scale related. You can use one paragraph to introduce the characteristics of your sample (Table 1) and one paragraph to describe MFS (Table 2).

This is an excellent suggestion which we followed up. Please see correction on page 9. Also, please refer to Table 2 on page 20 to find all the results pertaining to the MFS.

Comment 19: Explain this: eg. listening to music can decrease induce a ... change in....
We apologize for this lack of information. The following sentence: “In other words, music listening sessions can decrease the risk of falls as measured by the MFS” was added to page 10.

Comment 20: These two variables (reason and season for admission) have 3 and 4 categories respectively. They are categorical variables based on their traits. It’s actually inappropriate to treat them as continuous variable in the regression model.

We agree and computed the multiple linear regression differently (we excluded the 2 categorical variables with more than 2 categories) (refer to page 21 to see the new Table 3). Also, please refer to the changes made in the manuscript mainly in the abstract and results sections on pages 4 and 10.