Author’s response to reviews

Title: Supporting at-risk older adults transitioning from hospital to home: who benefits from an evidence-based patient-centered discharge planning intervention? Post-hoc analysis from a randomized trial

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Reviewer reports:

Paolo Fabbietti (Reviewer 1)

Comment 1: At page 11, line 17-19, you consider Chi-Square test as a no-parametric test, but instead is a parametric test for categorical variables

Answer: We have now edited our analyses to more clearly state that we applied statistics which did not require the assumption of normal distribution and have provided a citation to ensure readers may follow up on the reasons for our statistician’s choice: McHugh ML. The chi-square test of independence. Biochem Med (Zagreb). 2013;23(2):143–149. doi:10.11613/bm.2013.018
We have also identified that we refer to the Pearson’s chi square.

Comment 2: why don't you show overall sample in this paper and you prefer a reference? It's could be easier for a good lecture.

Answer: The overall sample is summarised in the current submission and is published in a table in the main clinical trial publication which we have referenced.. We therefore kindly request editorial advice to resolve this reviewer comment- while our paper presents a summary of the overall sample, if we were to duplicate this in a table, we will need to request JAGS approval to reproduce the table.

Daniel Liebzeit (Reviewer 2):
Introduction, para 3-4. While your introduction sets up the question well. I think there are many opportunities to cut in these two paragraphs. There is quite a bit of information provided about the primary RCT that could be found elsewhere, and you could hit the key points here to cut down the introduction, improve flow, and keep focus on the question at hand. Examples include page 6, lines 37-44, page 7, lines 56-58 (redundant with beginning of introduction), page 6, lines 29-34 (could be merged and cut down).

Answer: We agree with the reviewer that the description of the HOME intervention can be shorten in the introduction (and some sentences could be moved in the methods) to enhance the logical flow. As suggested, page 6 lines 36-43 and 7 lines 56-58 have been removed. We also merged lines 29-34 (now 2 sentences).

Introduction: I think there are also some opportunities to reorganization here. The logic jumps around between rationale for the primary study and the secondary study. Consider increasing focus on the argument for the present study (focusing on the subgroups), rather than the primary study focusing on why the trial was developed and what the trial was/resulted in (this is in the primary publication).

Answer: Thank you for these suggestions. We have now removed sentences on page 6 (lines 36-43) and page 7 (lines 56-58). Moreover, we merged page 6 lines 29-34. However, we think it might be helpful to keep a short description of the results of the trial so that the readers can understand the context of the study (no significant difference found on the main outcomes for the overall sample).

Page 8, lines 73-74: you say here that those with significant cognitive impairment were excluded, but the present analysis focuses on those with cognitive impairment. This creates a concern that your sample could be biased in that you excluded many individuals who showed signs of CI, leading to their exclusion. So think about what type of individuals with MCI you may have in your sample, given the exclusion criteria. You will want to provide more detail about how your exclusion criteria may not have biased or altered your sample.

Answer: The paper currently reports the participant criteria: “Study participants were 70 years and older, without significant cognitive impairment (less than 5 errors on the Short Portable Mental Status Questionnaire (SPMSQ)) [39] and English speaking”. Please note, not all individuals with cognitive impairment were excluded.

In the methods, we further state: Cognitive status was determined by the score of SSPMSQ [39] (…). The score interpretation is based on the number of errors: 0-2 errors: normal mental functioning; 3-4 errors: mild cognitive impairment; 5-7 errors: moderate cognitive impairment; 8 or more errors: severe cognitive impairment. Using the cut-off point of three errors, the SSPMSQ proved to be a sensitive (86.2%) and specific (99.0%) screening cognitive test among medical inpatients [43]. In the present study, the sample was split to create two subgroups: the at-risk subgroup refers to participants showing mild cognitive impairment (3 or 4 errors), while the other subgroup includes those having no cognitive impairment (0 to 2 errors).

In addition, we have now added into our limitations section: “Second, the overall sample does not include older adults with severe cognitive impairment, since these potential participants were excluded from the main trial.” In line with the reviewer’s comment, we thus added: “does not include older adults with moderate or severe cognitive impairment…”

Page 8, lines 75-79. Same concern as above with mobility, FIM, and comorbidities exclusions. So
you're looking at "high risk" individuals, but it sounds like many of the higher risk individuals were excluded from the study. Again, please provide more justification and information about procedures related to exclusion.

Answer: We have removed reference to “high risk”, instead state it as risk. We added in the limitation section: “The sample does not include older adults who required assistance for mobility”. However, for the comorbidity, because we mentioned that “An age-adjusted score of CCI > 5 has been found to predict in-hospital complications [44]”, the participants in our subsample might be expected to show high morbidity.

Page 9, lines 98-104: do you have any data to support this cut point between high and low comorbidity score?


Page 9, lines 108-110: the scores here for significant cognitive impairment do not line up with the scores you identified for significant CI in the exclusion criteria above. Please clarify

Answer: We presented on page lines 105-110 the score interpretation for the SSPMSQ, based on the number of errors. To avoid confusion, we removed the score interpretation for moderate and severe cognitive impairment.

Page 10, lines 135-138: you mention test-retest reliability of this instrument, is there any other evidence of testing of this instrument available?

We thus added: “Current data supported the construct validity and sensitivity to change of this tool among various clinical populations of community dwelling older adults”.

Page 12, lines 166-77: this all sounds like data from the original study. What is the new data here?

Answer: The lack of significant difference in the proportion of “at-risk” participants (e.g., walking difficulty, higher comorbidity) between the HOME and the in-hospital groups is not a finding from the original study. We thus edit this sentence for clarity: “Proportion of “at-risk” participants in the HOME (n=198) and in-hospital groups (n=202) were also comparable…”

Page 12, lines 183-185: I don't know that you can say that home intervention also reduced ED presentation. Findings were not significant so no evidence to determine this or if differences were random.

Answer: The sentence has been revised: “However, the effect of the HOME intervention on ED
presentation did not reach significance within the at-risk subgroup of cognitively impaired participants (p=0.074).”

Page 12, lines 188-190. Same as last, I don't think you can say intervention improved participation if results were not significant.

Answer: The sentence has been revised in the text and in the abstract: “However, within the at-risk subgroup with no support from family, the effect of the HOME intervention on participation did not reach significance compared to in-hospital consultation (p=0.058).”

Page 13, lines 193-195. This statement is not supported by the results.

Answer: Changes were made in line with the reviewer’s comment.

Page 13, lines 207-209. This sentence is complex and difficult to follow. I don't know what point author is trying to make.

Answer: The sentence has been revised.

Page 14, lines 219-221. Please revise, not sure what this sentence means

Answer: The sentence has been revised.

Page 14-15, lines 227-241. Intervention did not contribute to increased participation in results. It did not reach significance so cannot make this claim. This could have been random differences, which is the reason for setting an alpha level to determine what could be randomly observed differences versus significant differences that would indicate a positive finding. Also see Liebzeit et al. 2018 and 2019, which can provide information on issues with measuring only ADLs and focusing more on participation in OA transitioning from hospital to home.

Answer: The sentence has been removed. We also added a reference from Liebzeit (2018).

Page 15, lines 242-243, wondering why you did not look at combinations of indicators for at-risk older adults. Typically, we consider multiple factors that would put an individual at risk. Perhaps you can speculate on the importance of considering risk factors occurring together, versus individually. Because need to consider that perhaps many of the individuals that were determined to be high risk because of one factor CI, could also be high risk due to another comorbidity or walking. This is important to consider because you are looking at a multitude of factors that have been related in prior research.

Answer: We agree that this issue can be added as a limitation or to explore in future research. We thus added: Finally, the “at-risk” characteristics were analysed individually. Even if results suggested that older adults with walking difficulty or high comorbidity do not benefit from HOME, future research may further explore whether this intervention may be more beneficial to older adults with mild cognitive impairment showing a combination of “at-risk” characteristics.”

Page 15, lines 250-252, I don't know what this statement means because we do have a lot of research on individuals hospitalized with these risks.

Answer: The sentence has been removed.
Page 15-16, lines 259-261: it is still important to look at effects at 90 days, because effects are less important if they are present at 30 days but then fall off after that anyways. Ideally, we would look at and see effects over 30-90 days and beyond.

Answer: Thank you – we added this information “even though it is clinically important to assess whether its effects maintain over time”.

Page 16, lines 262-264, I don't know what this final sentence in limitations is trying to say, please revise.

Answer: The sentence has been removed.

Page 16, lines 266-267, I would suggest softening the conclusions here given that this was a post hoc analysis and trial was not designed for this group, in addition to the limitations you outline above. While this may suggest importance of follow up after hosp. in MCI individuals, I believe more research would be necessary to determine if this particular intervention is most appropriate to apply to this group or if certain parts of the intervention are most important. Especially given the results on primary outcomes for the RCT. For example, why would you use an intervention designed to improve functional outcomes (ADL) for this group when it did not improve functional outcomes at any level.

Answer: We thank the reviewer for this relevant comment. We have added the following sentence in the conclusion: “Even if the intervention has not been found effective to improve functional outcomes (ADL), our findings…” “…and whether some components of the intervention are more “essential” to implement than others”.