Author's response to reviews

Title: Relationship between Depressive Symptom Severity and Emergency Department Use Among Low-Income, Depressed Homebound Older Adults Aged 50 Years and Older

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Author's response to reviews: see over
Cover Letter and Summary of Revision

**MS (revised) Title:** Relationship between Depressive Symptom Severity and Emergency Department Use Low-income, Depressed Homebound Older Adults Aged 50 Years and Older

Dear Dr. Menchetti and Reviewers:

Thank you so much for your very thorough review and constructive critique of our manuscript. We really appreciate your comments and suggestions for improving the manuscript. We have incorporated all your comments and suggestions in the revision. Most importantly, in accordance with Reviewer 3’s invaluable comments about the statistical analysis, our project’s primary statistician, Dr. C. Nathan Marti, conducted all the statistical analyses for this revision. He is now listed as the second author of the manuscript. All the text and table revisions are highlighted in yellow for the ease of your review. The summary of revision follows:

**Reviewer 1 (ARA):**

Two minor concerns deserve a revision.

1) Studies based on self-report are likely to encounter a recall or a response bias. Please provide a comment on this issue in the discussion section.

**Response:** Done

2) Please reconsider your definition of the study population in terms of age. For example, change the current title as follows: “Relationship between depressive symptom severity and emergency department use among low-income, depressed older adults aged 50+”.

**Response:** Done

**Reviewer 2 (MS)**

**Title and Introduction:**

1. Major: It does become not clear that the participants are homebound adults. The term is neither used in the title nor described in the introduction…. In the introduction the description “chronic medical illness” and “high levels of comorbidities and disabilities” are used, but they do not describe the special situation of homebound patients, which are even more limited than adults with comorbid conditions, that in most cases are still able to leave the house. Please go more into detail in terms of homebound adults. Please specify in the title that the study refers to homebound adults.

**Response:** Thanks for pointing these out. We have now a title that includes “homebound in it” and have extensively revised the introduction section to include the special situations of depressed, low-income homebound older adults. We have also clarified the unique contributions of this study as compared to previous studies of older adults’ ED visits.

2. Minor: Further the introduction could more focused and shorter.

**Response:** Please see our response above.

**Results:**

3. Major: The section that repeats the results of another study (Reference 29) should be omitted…

**Response:** Done
4. Discretionary: Optionally these results could be mentioned in the discussion section.

Response: Please see our response above.

5. Minor: The text should be shortened since large parts are only repeating the information from the tables and are therefore redundant.

Response: Because we have revised our analyses, we no longer repeat the same information.

Discussion:
6. Minor: It would be more explicit if you said that ED visit frequency was significantly associated with higher depressive symptoms or with depressive symptom severity.

Response: done


Response: We have read and cited the above article in the manuscript. Thank you for bringing attention to it.

Reviewer 3 (AS)
- Major Compulsory Revisions

Abstract
1. The abstract adequately describes the objectives of the study and the results. However, the methodology and the conclusions need to be modified to properly reflect the content of the article. The methodology seems more describe the RCT than study itself, and conclusions must be stated in the implications explored the association between depressive symptoms and use of emergency services.

Response: Revised as recommended

Background
1. The authors clearly articulate the possible relation between depression in older adults and use of Emergency Services. They provide a clear rationale for the study. Additionally, the literature review is adequate to support the introduction and links past research to the authors proposed research showing how the current study is an extension of previous work. However, its weakness lies right there, because the relationship between depressive symptoms and use of emergency services is more than demonstrated, and it is unclear that in this study population the results have to be different.

Response: We have now clarified the unique contribution of our study that examined the longitudinal relationship between depressive symptoms and ED visits among low-income homebound older adults who participated in a depression treatment study. Although a few previous studies examined return visits to ED among older adults, little research examined the question of whether or not changes in depressive symptom severity would be associated with the changes in ED visit frequency over time among low-income homebound older adults who are largely underexposed in any research study.

2. The authors would have to argue why this relationship in this population (low-income homebound older adults) would be different, and to establish the magnitude of the problem in terms of the proportion of the population represented by these homebound older adults.
Response: We have extensively revised the introduction section to include statistics about the proportion and healthcare cost burden of homebound older adults and to bring home the reasons why these older adults are especially likely to use ED services.

3. Additionally, since the relationship and depressive symptoms - use of emergency services is demonstrated, the study hypotheses seem to pose an almost obvious relationship, with an anticipated result: greater severity of depressive symptoms involves greater use of emergency services.

Response: Our focus was on H3 that tested the longitudinal relationship stated above.

4. But that's not the biggest problem, as the authors do not seem to take into account that the main outcome variable (use of emergency services) is not necessarily a negative event. This means that a reduction in the number of visits should not be taken as a positive, since for some older adults such visits could have a beneficial effect in reducing depressive symptoms. And additionally, visits to emergency services could, as the authors say, be indicated by a general practitioner.

Response: In general, these older adults did not think use of ED services to be desirable. They went there because they did not think there was any alternative care. In the urban area where the subjects reside, EDs are often crowded with other more urgent and life-threatening cases (such as victims of crime/violence and auto/other accidents) and the patients whose conditions are not life-threatening have to invariably wait long hours (average wait time is 3 hours in the area; the first author’s personal communication with an ED director at Seton Hospital in Austin in October, 2011) to be seen by a physician. The older adults’ PCPs/on-call nurses told them to go to an ED during off hours and weekends, and many older adults told us that they “dreaded” going to an ED as the visits take long waiting hours and exhausting. Moreover, ED visits are hugely expensive ways of managing chronic conditions. As the findings of our study show, the subjects frequently visited ED for body pain and blood sugar levels. These symptoms can better be managed using more effective and far less costly means (such as more effective routine outpatient care and chronic disease self-management protocols) than frequenting EDs. We have now included a short description of these negative impacts of ED use in our text.

5. Finally, in the concluding paragraph of the introduction, the authors state that also analyze visits to the emergency services “before and after their participation in an RCT of a short-term psychotherapy for depression.” This part is confusing because it is not clear whether they are analyzing the relationship depressive symptoms - use of emergency services only, or they are also interested in estimating the effect of participation in the RTC on the use of services. If this were the case, both the objective and the hypothesis would have to be modified, and the background too.

Response: We have now clarified that the primary aims were to examine the longitudinal relationship between depressive symptoms and ED visits.

Methods
1. The empirical definition of the outcome variable has one major limitation. According to the authors the final variable is the sum of the number of visits in the three measurements: baseline, and 12- and 24-week follow-ups (italics are ours). But the authors seem to ignore the fact that these visits are not independent. That is, the first follow-up visits depend on the status of the baseline visits, and the second follow-up visits depend on the status at the first follow-up. This means that the definition of the outcome variable authors should take into account the probability that the elderly have a visit, in the immediately preceding measurement. I think the simple sum of the number of visits does not adequately reflect the phenomenon they are interested in analyzing.

Response: We really appreciate this comment. To clarify, however, the outcome variable for our H1 was the number of ED visits during 6 months prior to baseline and that for H2 and H3 was the number of ED
visits during 24 weeks after baseline (= sum of the number of ED visits between baseline and 12-week follow-up and the number of ED visits between 12-week follow-up and 24-week follow-up). While the reviewer brings up an important point regarding the visit frequencies that they are certainly correlated across time, the hypotheses 1 and 2 are simply testing the association of HAMD and ED visit frequency during the same time period. Thus, hypothesis 1 and 2 are essentially parallel hypotheses; the former investigates the relationship prior to intervention and the latter after intervention. Hypothesis 3 adds the additional parameter of examining HAMD change scores to examine whether or not the longitudinal changes in HAMD scores since baseline may be associated with the follow-up ED visit frequency, controlling for the baseline ED visit frequency.

2. HAMD description is not clear enough. The authors have not specified why they used the cutoff of 15 or higher on the scale score.

**Response:** We clarified that the score of 15 or higher is used to select the subjects with moderately severe and severe depressive symptoms.

3. In bivariate analysis the authors used ANOVA to determine whether there were differences between different study groups regarding HAMD scale score. However, one of the assumptions of this procedure (ANOVA) is that the probability distribution of the outcome variable must be normal. The authors do not specify whether this assumption is fulfilled, and if not, if they used, for example, robust standard errors, or even a nonparametric procedure.

**Response:** Following the reviewer’s suggestions, we investigated the probability distribution of the outcome variable and determined that the outcome was not normally distributed. We followed his suggestion of using robust standard errors and reanalyzed the data using robust regression models.

4. The authors write, in multivariate analysis paragraph "Negative binomial regression was used because the number of ED visits was a count outcome, with a large proportion of the participants having had no visits (= 0). However this is not necessarily true. On one hand, they have a count outcome variable, and therefore the use of the negative binomial distribution is suitable. But if they also have an excessive number of zeros, then the authors should have used a zero-inflated distribution (poisson or negative binomial). In fact, what determines the use of the negative binomial is not an excessive number of zeros, but the presence of overdispersion, issue that the authors did not mention anything.

**Response:** We followed the reviewer’s suggestion to use zero-inflated models. We first examined the dispersion of the count portion of the model and determined that the count data were not overdispersed and therefore used a Poisson distribution.

6. Another alternative analysis, which has proven to be more efficient when analyzing data on use of health services, is the two-part hurdle model; which, in the first instance, models the probability of use, and secondly, the frequency or intensity of use.

**Response:** We appreciate the reviewer’s suggestion to implement two-part hurdle models, however, because this suggestion was an alternative to the suggestions in the previous point, we did not investigate this option extensively, as the zero-inflated Poisson models addressed the concern of separately modeling the probability of an ED visit and the count of the ED visits.

7. The authors do not mention what kind of factor depressive symptoms are: Predisposing, Enabling, or need factors.

**Response:** We have now clarified depressive symptoms as a need factor.
Results
1. Paragraph 4. Delete this, is irrelevant and does not help in testing the hypotheses, and in achieving the stated objective.

Response: Done

Discussion
1. Since it is likely that the authors must modify the analysis, conclusions about the validity of their hypotheses will have to be reconsidered.

Response: The Discussion section has been modified in accordance with the results of new analyses.

2. As mentioned above, it is not clear why the authors attempt to reduce ED visits. Since these visits are not in themselves neither positive nor negative, then the conclusion is also unclear. & 3. Even so, more direct implications based on your findings needed.

Response: Please see our responses to your comment #4 regarding the Background section. We have added more implications from our findings in the conclusion section.