Reviewer’s report

Title: The course of Depression in Late Life as measured by the Montgomery and Aasberg Depression Rating Scale in an observational study of hospitalized patients.

Version: 2
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Reviewer: Klaas J Wardenaar

Reviewer’s report:

The authors describe a study on the short-term course of specific depressive symptoms (measured on the MADRS) in 145 older inpatients (>60 years of age). The results showed that change over time varied across symptoms. In addition, dementia, poor health and previous episodes of depression were all found to be associated with unfavorable course of MDD. The paper is clearly written and the presented analyses and results seem sound. However, I feel that the paper could benefit from thorough clarifications, especially the statistics section. Moreover, the role of the follow-up duration should be much more explicitly addressed in the analyses before hard conclusions can be made based on the study results. On a side note, I also noted some minor mistakes/typos in the written English so extra proof-reading is recommended.

I listed my comments below.

Major Compulsory Revisions

1. The mean/median length of follow-up and S.D.’s or IQRs should be mentioned much earlier in the paper and the Abstract. The authors state that one of the features that makes their study is innovative is that it looks at short term course. In order to enable readers to judge for themselves, the authors should make explicit from the outset what they mean by that.

2. Line 271-272: What are the mentioned proportions of intra-center variance based on?

3. Line 273: Why were only random intercepts estimated? Did the authors check if random slopes improved statistical fit?

4. Line 275: Which adjusted SD was used? The SD of the change scores? How was this SD adjusted?

5. Line 288: What is meant by ‘reduced’: reduction of the number of fixed/random parameters or the number of predictors in the model? Why was the AIC used for this?

6. Line 289-290: ‘Finally the multivariate models were adjusted for confounders’. Why were confounders not included in the model-reduction process? If strong confounding is present in the model, including the confounders could help to help to keep redundant predictors out of the multivariate model. Using the current method, a model is optimized in terms of statistical fit in the first step and
confounding is tackled in a second step. In this way, part of the variance that was assumed in step 1 to be explained by the included predictors, is eventually explained by something else (i.e. age, gender).

7. There is considerable heterogeneity in the follow-up durations (Lines 306-307). This affects the longitudinal structure of the data and the change scores that are calculated on the basis of this data: i.e. change scores will on average be larger over longer follow-up periods than over short follow-up periods. This need not be a problem for the current results if we assume that these differences are similar for all studied outcomes. However, we cannot simply assume this and need to thoroughly check it. Also, the role of time needs to be considered much more explicitly in the analyses and results.

8. If the follow-up duration turns out to bias the change scores, a statistical correction could be tried: e.g. by (1) investigating the univariate predictive effect of follow-up duration on outcomes and (2) using follow-up duration as a covariate in the reported regression models. Adding an additional Time-level to the LMM model could also be considered.

9. An additional note on the role of time in the current study: why were ‘time to remission’ or ‘time to discharge’ not used as outcomes. This would be advantageous in two ways: (1) the differences in follow-up duration would become an informative feature of the outcome and (2) the outcomes would be clinically more appealing: it seems clinically and economically very interesting to find out how long someone takes to remit/get discharged from hospital rather than just checking if someone is remitted at discharge, without considering the time to reach this status.


11. Line 468-477: the manuscript fails to mention one of the study’s largest limitations (see comments 7 and 8). Even when addressed in the analyses, the large variability in follow-up duration should be mentioned as a limitation.

Minor Essential Revisions

12. The word ‘Aasberg’ in the title and the manuscript is usually spelled Asberg or Åsberg in the English language literature, including the papers by the authors of the MADRS instrument.

13. The paragraph about the Norwegian mental Healthcare facilities for DLL felt a little out of place in the introduction as it does not lead up to specific research questions of the study, but merely paints a picture of the study’s context. I would remove this part and try to establish a structure that more clearly leads from background to research questions.

14. Line 124-125: what is meant by short-term? Across which time intervals has the course of DLL been studied (weeks, months, years?). Please clarify.

15. Lines 124 and 232: Why do the authors refer to European studies only? Are studies from other parts of the world (e.g. USA, Australia) not considered?


17. Line 481: What is meant by ‘…the scale is very much in use in Norway’?
18. Lines 491-493: why is this an issue. I assume the first assessment date marks the entry into the study?

Discretionary Revisions
19. Consider removing lines 486-489 to the methods section.
20. Consider merging the paragraphs in lines 479-484 and lines 495-498 into a single strengths paragraph. Currently, the two paragraphs provide overlapping information.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**
I declare that I have no competing interests