Reviewer's report

Title: Economic evaluation of an experience sampling method intervention in depression compared with treatment as usual using data from a randomized controlled trial

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Reviewer: Christian Brettschneider

Reviewer's report:

In summary, the manuscript „Economic evaluation of an experience sampling method intervention in depression compared with treatment as usual using data from a randomized controlled trial“ addresses an innovative topic from the field of mental health care. Patients considered in this study were adults with at least mild depression, who were treated with antidepressants or mood stabilizers. The conduction of the study, a three-arm RCT, was thoroughly described. The economic evaluation took a societal perspective, considering intervention costs, health care costs, absenteeism and presenteeism. The time horizon was 32 weeks.

Unfortunately, I have some major concerns about the paper, which preclude a completely positive review. However, it is possible that some of these concerns are mere imprecisions in the description.

1. Generally, I would like to recommend that the authors restructure their statistical analysis section and perform a step-by-step description. Currently, it is really difficult to find all relevant information to reconstruct the analysis.

2. The authors write that costs and QALYs of week 1-8 were were estimated by individual mean imputation of the baseline and the week 20 assessment. How was this imputation performed exactly? As far as I comprehend the utilization questionnaire had a time horizon of 3 months (12 weeks). This is in accord with the week 20 assessment (20 - 12 = 8). The problem is that -according to the description- costs for an 8 weeks period were imputed by the mean of two cost measures assessing 12-week periods. Did I get this right?

3. It is not clear to me how the authors implemented the bootstrapping approach into the analysis. It is comprehensible to use bootstrapping in case on non-normal-distributed
costs. But, the way bootstrapping was applied is not clear. Did the authors reproduce their dataset x-times (the number of replications was not stated. That has to be done.), calculated mean and SD/SE of costs and QALYs (Table 2 presents costs, but it is not stated of these are mean (SD) or mean (SE). This has to be stated), and calculated the ICER from the bootstrapped values? Or, did the authors bootstrap the regression analyses? In this case, I do not understand why the authors used the delta method to “adjust” costs and said later that this approach is preferable to the regression method of adjustment. Under these circumstances it is appropriate and feasible to consider baseline costs as independent variable in the regression analysis.

4. To handle the problem of non-linearity the authors could consider constructing a generalized linear model assuming a gamma distribution and using a log link.

5. How was the CEAC calculated? I assume that the authors used the bootstrapping approach and reproduced the dataset (number of times?) and not the net-benefit approach. This would mean that the CEAC is not adjusted. The authors should give more information on their approach.

6. The authors write that they adjusted their linear regression models for baseline values. The authors should state which variables they considered.

7. The authors compared ESM-I to Pseudo-I and to Control separately in the regression analyses but not in the CEAC. I want to ask the authors to present CEAC for ESM-I vs Pseudo and ESM-I vs. Control.

8. Missing data were imputed by LOCF and NOCB. These approaches have their weaknesses and are prone to bias. For this reason, I want to ask the authors to state the proportion of missing data and to discuss the influence of this proportion on their chosen method of imputation.

9. There is no discussion on the point that the sample size was rather small. Sample sizes of 30 patients are less than optimal to perform an economic evaluation, among others because of the deviation of costs. Looking at the total costs in the three study arms, it is obvious that the Pseudo-I group has a rather small SD in comparison to the other groups.
This seems to be an effect by chance, but has an impact on the analysis of the differences between the groups. The authors should discuss the influence of the small sample size on the analysis of cost differences and on the generalizability of the results and observations.

10. The authors should report the results of their sensitivity analyses in the results section.

11. There is no information on acute suicidality or severe somatic diseases in the exclusion criteria. Were patients screened for suicidality and were severe somatic diseases assessed? If yes, I want to ask the authors to present this information in table 1.

12. I want to ask the authors to describe which cost categories were considered in the assessment of direct health care costs.

13. I want to ask the authors to create an appendix table presenting the unit costs per cost category.

14. The last paragraph of the discussion related to the correction for baseline costs is quite overstated. The correction for baseline cost is not relatively new, in the sense of "not common knowledge". Baseline correction is a standard in economic evaluations. This paragraph can be deleted.
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
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