Reviewer’s report

Title: Individualized support for informal caregivers of people with dementia - Effectiveness of the German Adaptation of REACH II

Version: 0 Date: 10 Jul 2017

Reviewer: Johanna de Almeida Mello

Reviewer's report:

Dear authors, thank you for this promising research paper on the effect of a multi-component intervention (DE-REACH) on informal caregivers' burden. I have read it with great interest. The paper could confirm the effect of this type of intervention in stabilizing burden of informal carers, when compared to the control group, and this can be considered a positive result of the intervention. Below you can find my remarks:

A few language issues: Please, replace 'which' by 'who' in: "We excluded carers who were involved in another carer intervention study, which had an actual psychiatric diagnosis of mental illness or another illness that would prevent 6 months of study participation, or forthcoming institutionalization of the person being cared for." Please correct the word procedures. It is separated in the manuscript: "Written informed consent was obtained from informal carers and their relatives with dementia before conducting study-related procedures." Clarifications: Please explain what you mean by 'active teaching techniques'. Is this about the training of the interventionists or the training of the informal carers? Please, give an actual location (in which section) in the manuscript instead of mentioning 'see above': Process quality was documented by structured interventionist protocols of each problem-solving process (see above). Methods: As you have used covariate analysis, it is normal that you have small changes in Zarit scores since the range of scores goes from 0 to 88. The analysis you used is acceptable and valid and the inclusion of the Cohen's d score was an added-value to the analysis. I just wonder if your results would show more significance if you would use the cut-off of the Zarit scale instead of using the change in the scores. For example, if people move from burden to no-burden, they could show even a small difference of 2 points in the scale, but they would be coming from a state of burden to no-burden. Other option with be: high burden to low burden. This would have to be done by dichotomization and by means of logistic regression. However, I still think your analysis is valid as it is now. Please make an apart section for the limitations.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

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?
If not, please explain in your comments to the authors.

Yes

**Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?**
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

**Quality of written English**
Please indicate the quality of language in the manuscript:

Acceptable

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