Author's response to reviews

Title: Interpersonal psychotherapy (IPT) for late-life depression in general practice: uptake and satisfaction by patients, therapists and physicians.

Authors:

Digna JF van Schaik (a.van.schaik@ggzba.nl)
Harm WJ van Marwijk (hwj.vanmarwijk@vumc.nl)
Aartjan TF Beekman (aartjanb@ggzba.nl)
Marten de Haan (m.dehaan@vumc.nl)
Richard van Dyck (R.van.Dyck@ggzba.nl)

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Author's response to reviews:

Dear editor

Hereby I resubmit our paper ¿Interpersonal psychotherapy (IPT) for late-life depression in general practice: uptake and satisfaction by patients, therapists and physicians. ¿

Our reaction to the comments of the fourth reviewer, M.A. Serfaty

General comments:

1. The reviewer has doubts whether this part of the research project was a priori planned. This comment was also made by other reviewers, see comments J Markowitz and our reaction to it.

2. The reviewer states that he needed the effectiveness paper to review the current paper. In the current paper we concentrate on uptake and satisfaction by patients, therapists and physicians. The methodology we used to do that is described in this paper, and is relevant to those who consider to organize IPT in primary care. Indeed we refer to our paper on effectiveness, but as this paper was already thoroughly reviewed, we assumed that summarizing the findings was enough. We do not think that it would have been possible to integrate these papers into one.

3. The reviewer doubts whether the data are sound and well controlled, as in this paper only the data about the IPT group are presented. To answer this question one has indeed to go back to the effectiveness study. However, in the discussion section of the current paper, in the limitations paragraph, we pay attention to the extent to which our findings are representative for the general population.

4. The reviewer comments on whether relevant standards for reporting and data deposition were used. The first comments here focus mainly on the effectiveness trial not on the subject of the current paper. Yet, as they indeed are intertwined, we have made some adaptations in the current paper.
According to the statement of the reviewer we added more details in the current paper about how patients were recruited and about their characteristics: Those who signed informed consent were randomly allocated to IPT or CAU. An independent research assistant performed randomization per practice at the patient level by using a table of random numbers. As mentioned before, the possible limitation regarding the generalizability of our findings is described in the discussion section. At page 12 we added: There were no difference between the intervention and control group regarding baseline sociodemographic and clinical characteristics. As we focus on exploring uptake and satisfaction of IPT, we make no further references to the control group in this paper except for the part where we discuss the limitations regarding the way we measured patients satisfaction (Page 16).

The reviewer suggests that a cut-off score for the MMSE of 18 is too low. There are no strict criteria to select patients for psychotherapy. In our sample, there was no difference in MMSE score between the groups. In our analysis of predictors of outcome (not yet published) MMSE score had no predictive value.

The reviewer suggests that non-specific factors as warmth and empathy are more important than the specific protocol of IPT. The discussion whether protocollized, specific psychotherapeutic interventions are more effective than non-specific ones, is beyond the scope of this paper. We only wanted to explore an intervention that is evidence based and advised in all depression treatment guidelines.

The reviewer mentions that the prevalence of major depression we found in our sample is lower than that found in community studies. This is a misunderstanding, because in earlier stages of the screening procedure patients who were being treated for depression were excluded. The prevalences we found in our study and in a parallel study were studied in detail and were comparable to those in other studies. Licht-Strunk E, Van der Kooij KG, van Schaik DJ, van Marwijk HW, van Hout HP, de Haan M et al.: Prevalence of depression in older patients consulting their general practitioner in The Netherlands. Int J Geriatr Psychiatry 2005, 20: 1013-9.

The issue about the questionnaires we used to assess engagement was already addressed by another reviewer, we answered that question before.

5. The reviewer mentions the puzzling finding that all patients had a diagnosis of major depressive disorder, yet minor depression on the HADS. We did not use the HADS, but the MADRS. Not all patients had a minor depression according to the MADRS, but indeed the mean score was relatively low (19, mild depression). We discussed the consequences of these findings in the current paper where we summarize our effectiveness data (page 14). It is beyond the scope of the current paper to go into detail about the psychometric properties of the different scales.

The reviewer states that there is no measure on whether therapists had actually learnt IPT. Indeed we have not used a measure to assess this. It is possible that the delivery of IPT was not optimal, and that more experienced IPT therapists,
would have yielded better results. We have mentioned this in the discussion of our effectiveness paper. Again, this would not have changed our conclusions.

6 en 7. These comments overlap with former comments.

We look forward to see our paper being published in your journal!

Kind regards

Anneke van Schaik