Reviewer’s report

Title: Psychosocial functioning in patients with treatment-resistant depression after group cognitive behavioral therapy

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Reviewer: elisabeth H eurelings

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Review of the manuscript: Psychosocial functioning in patients with treatment resistant depression after group cognitive behavioral therapy.

Dr. E.H.M. Eurelings-Bontekoe

This is a clearly written manuscript describing a naturalistic follow up study on the effect of adding a short term cognitive behavioural group therapy to medication on psychosocial functioning among patients with treatment resistant depression. Authors conclude that addition of group CBT improved psychosocial functioning and that the positive effects have maintained after one year.

This is a very interesting and important topic and results are positive. Strengths of the study are that it is a longitudinal one and that several outcome variables have been assessed, including cognitions, which is very relevant here. However, I have some major concerns. Some, but not all, of these concerns are mentioned as limitations of the study by the authors.

I will divide my comments in categories of revision needed.

Major compulsory revisions

1. The small number of patients included in the study (N=38 for T0 and T1; N= 20 for the FU).

The small number of patients is especially problematic in the use of multiple hierarchical regression analysis (MHRA). In general the ratio variables:subjects needs to be at least 1:15. The number of predictors in the analyses was 10, whereas the number of subjects was 38, a very unfavourable ratio, precluding the use of MHRA.

Besides, since it is well known that the level of distress/impairment at the start of treatment is often the best predictor of level of distress/impairment after treatment, it would have been better to include GAF and SF-36 at T1 as the dependent variables (in stead of difference scores) and to enter GAF and SF-36 as measured at T0 in the first step of the HRMA, before entering the other predictors.

Also in the repeated measures ANOVA, one should control for baseline levels of impairment.

-Advise: leave regression analyses out, and use baseline levels of dependent
variables as covariates in repeated measures ANOVA.

2. Authors excluded a.o. patients with comorbid personality disorder. Severe depression however often goes together with a personality disorder. So how many patients were excluded on the basis of the exclusion criteria, especially comorbid PD? Next, and more importantly, how were comorbid personality disorders assessed?

3. Authors need to provide reliability coefficients (Cronbach’s alpha) for the DAS, the ATQ-R and the SF-36.

4. The most important limitation concerns the lack of a control group. In fact, the study shows that treatment resistant depressed patients show an improvement in social functioning after adding short term group CBT to medication, and that those who were willing to participate in the FU, maintain improvement. This holds for a specific group of depressed patients without comorbidity with axis II disorders and without high suicidal risk. However, it remains unknown whether the change in scores is related to natural course, to having had more attention, to other, unknown, factors or to CBT in particular. Without a control group that received only medication, and a control group that received another type of treatment in addition to medication, the main questions of the study remain unanswered, unfortunately.

Although this limitation of the study is mentioned in the discussion, the discussion says that adding CBT to medication enhanced social functioning, speaks of a study on the long term effectiveness of CBT, mentions as reason for improvement of patients that CBT provided patients with appropriate cognitive and behavioral coping strategies. In other words, the conclusions described in the discussion and the abstract are to my opinion overstated because the present study design does not allow for such conclusions.

I would suggest to be somewhat more modest in discussing the results of this study and start with the limitations. So I would not say: “despite the findings, the study has several limitations..”, but rather: “despite several limitations, the present study suggests that adding CBT to medication might have a positive effect, a suggestion that needs to be confirmed in larger samples using randomized controlled trials”.

Discretionary Revisions

1. Effect sizes were expressed as partial eta squared ($\eta^2_p$). According to conventional criteria an $\eta^2_p$ of 0.01 is small; 0.06 moderate; 0.14 large. As such all effects found, except for one, are (very) large. However, a more appropriate ES measure is Cohen’s d. According to conventional criteria, d # .20 is considered a small ES; d # 0.50 a medium ES; and d # .80 a large ES.

   Expressed in terms of Cohen’s d, four of the ESs found in this study are medium (SF-36, SF-36 physical functioning, SF-36 PCS general health perception and SF-36 MCS role functioning-emotional) and not large, as is suggested by the $\eta^2_p$.

2. Treatment response was defined as a 50% or greater reduction on the HRSD,
compared to pretreatment score. However this way of calculating response may not imply a reliable change. Why did not the authors use Jacobson and Truax formula to calculate reliable change and clinical significant change?

5. The fact that only 20 patients participated in the follow up measurement, may constitute a serious bias. In addition, it is unknown whether these 20 patients continued to use medication or took other forms of treatment. It is a pity that an intention-to-treatment analysis could not be done, due to the small number of patients included in the study.

Minor points: a typo in abstract under Methods (number of patients included) and unclear wording on page 14, third line from below: "higher than these scores). Which scores?

To summarize: an interesting and relevant study, with results that might be of interest to CBT, but with a design that does not allow to draw any firm conclusion about effectiveness of adding group CBT to medication.

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

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

I declare that I have no competing interests