Reviewer’s report

Title: Group Cognitive Behavior Therapy for Japanese Patients with Social Anxiety Disorder: Outcomes and Their Predictors

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Reviewer: Ewa Mörtberg

Reviewer’s report:

General
This article examined if a Western CBT programme for social anxiety disorder (SAD) is equally effective in a non-Western country (Japan). Fifty-seven outpatients participated in a group CBT programme. Assessment with self-report social phobia scales and clinician-administered questionnaires were conducted pre- and post-treatment. It was concluded that the effect of group CBT in the Japanese sample was acceptable showing a similar degree of symptom reduction as in Western samples. No significant predictors of outcome were indicated.

The study is clearly written, well structured and the subject matter is interesting. The basis for conducting the study is, however, unclear and needs expansion. The methods used are overall appropriate, the measures and statistical analyses are well described. However, details concerning treatments (e.g. treatment length) and assessments (time-points) do remain unclear.

The study lacks a control group, which makes it difficult to determine whether the treatment had an effect. The method of making comparisons with other studies on CBT is a valuable context, however, it is not ideal and do not substitute for well-controlled trials. The relevance of comparing this treatment (2 therapists, 3-4 patients) seems important to discuss, as it is very different from most CBT programmes (2 therapists, 6-8 patients). The authors enrolled patients to the treatment over an excessive timeframe (July 2003- January 2007), which raises concerns about treatment consistency. The therapists do not seem to have used a treatment manual and did not obtain supervision, which would have increased treatment consistency and adherence to treatment. The fact that therapists met monthly to discuss treatment is valuable, but not sufficient.

It is unclear when treatment effects were assessed. It seems that patients were assessed at different time-points. Finally, a serious confounder is that patients received antidepressant medication and benzodiazepines during the study.

Overall, it seems that the study could be described as a pilot-study of CBT in a Japanese sample conducted within routine psychiatric service? This is partly expressed in the discussion. I think, however, that it is important to make this point clear from the start and address it in the background, abstract and possibly by modifying the title. Moreover, it is important to state the preliminary status of
results, as the study is uncontrolled.

In order to further improve the ms I would suggest the authors to address the following points:

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Background

1) p. 3, 2nd paragraph: Please develop the background describing the reasons for examining a CBT treatment programme developed for the Western countries in Japan. Why would the results of CBT conducted in a Japanese sample differ in results from other countries? Hypotheses?

Please indicate the setting of the study.

Methods

Treatments

2) p. 4, 1st paragraph: The description of treatment length is unclear and it could be questioned how the treatment was conducted. Please clarify the length of the treatment carried out in this study. Did patients receive different treatment, different amount of treatment (different treatment-length?) depending on when they were included (during July 2003 to January 2007)? I.e. some got 12 two-hour sessions, some got 12 sessions “or more depending on the groups need” plus booster sessions, and some got 16 weekly sessions “or more”.

Was there a treatment manual used?

Why is the period of inclusion so long?

3) p.5, 2nd paragraph: Please describe the reasons for treating only 3-4 patients (by 2 therapists), a format that is very different from group CBT programmes in general.

4) p.5, 3rd paragraph: If patients are stabilized on the medication prior to beginning a CBT programme, the possible confounding factor of medication may be regarded as less important. However, in this study it seems that patients received antidepressant medication and benzodiazepines during the treatment, which is a confounder of results. These medications are shown to be highly effective for the treatment of SAD (see e.g. Gould et al (1997), Fedoroff & Taylor (2001), Van der Linden (2000), Blanco (2003). Davidson et al (2004) found that combined treatment did not yield any further advantage compared to fluoxetine and CBT, respectively. However, Davidson et al., examined fluoxetine and CBT separately and found that each of them were effective.

In your study it could be possible that the treatment effects are a consequence of medication.

Assessment

5) p. 6 1st paragraph: Please clarify “end of the treatment”. How many weeks of
treatment? In some cases the authors have used the end of booster session as a post-treatment score. I would suggest that the authors use the “real” post-treatment assessment, as scores obtained at booster-sessions usually differ from post-treatment scores. In addition, it is important to use similar time-points for each patient.

Discussion
6) p. 13, 1st paragraph: As mentioned above, it is unclear why the outcome of a Western CBT would differ in a Japanese sample. Please discuss.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
Assessment
7) p. 5, 1st paragraph: The principle therapist conducted the diagnostic assessments before and after treatment. The possible bias using this procedure could be addressed in the discussion.
8): p. 6, 2nd paragraph (SPS/SIAS): Please note that SPS and SIAS are two separate self-report scales each consisting of 20 items.

Results
9) p. 10, 1st paragraph: Please clarify whether the axis 1 disorders are present or lifetime diagnoses.
10) p.18, Table 1: The table would be easier to capture by reducing it to 2 columns showing the m (SD) and n (%) in the first column, and the data in the second column.

What is “Offensive” social phobia? Please change to e.g., “non-generalised” social phobia.
Please insert a note indicating the meaning of “Any medication use”.
11) p. 19, Table 2: Please comment the reported results of NEO e.g., do patients differ from healthy controls?

Table 2 and 3: As the FNE was administered in a smaller group this should be indicated in the tables.
12) p. 19, Table 2: Please show p-values in the table

Discretionary Revisions (which the author can choose to ignore)
Statistical analyses
13) Repeated t-tests increase the risk of Type I error, which could be addressed in the discussion.

Discussion
Dropout-rates: Dropout rates are usually higher in randomized controlled trials when patients are allocated to a group condition (of 6-8 patients). This is probably because patients do not meet with the therapists before the treatment begin (in contrast to naturalistic settings).

I hope the authors find these comments helpful.

Best regards,
Ewa Mörtberg

**What next?:** Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

**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:** No, the manuscript does not need to be seen by a statistician.