Reviewer's report

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

Version: 4 Date: 18 December 2009

Reviewer: Yves Chaput

Reviewer's report:

Thank you once again asking me to review this manuscript. Having re-read my review (which was submitted august last) and having now read those of the two other peer reviewers, it is clear that the authors needed to make substantial changes to the manuscript, which they have done. This is a much better manuscript than the one submitted last summer. With regards my original queries to the authors they have largely been taken into consideration (see below). As such, I no longer have any major compulsory revisions to suggest.

My first major comment about the introduction has now been addressed. The review of the literature is clearer and adequately sets up the goals and objectives of the study. Newer, pertinent references have been added (also at the suggestion of reviewer # 3). My fourth comment, about the choice between individual versus group CBT, has been sufficiently detailed in the text. My fifth comment concerning an IIT group and the statistical procedures, have been fully addressed. Other reviewers also had similar concerns and these were also addressed. My sixth comment concerning a control TAU group (the absence thereof) was not addressed by the authors, who seem to have misunderstood the content. That being said however this issue was addressed by the authors as this topic was also raised by reviewer number 2 (the fourth comment). My seventh comment concerning the confusion with the HRSD reporting, which indeed was a typo in the manuscript, has been corrected.

In addition, the authors have addressed all of my minor points also, including a timeline in the flowchart and a list of abbreviations at the end of the manuscript.

I have, however, a number of discretionary revisions that the authors might want to consider. Most pertain to my second and third major comments (in fact, a series of comments regarding the tendency of the authors to give sparse additional data or not to fully explain their data and, the very low HRSD scores at admission) were inter related and, have also been addressed, although the author’s corrections also bring about a further few comments on my part. The abstract now much better represents the content of the manuscript, with one caveat (see below). The rating scales ranges are given. End points of the rating scales are provided in the results section and the constantly decreasing N values are better explained. Treatment refractoriness is better described, although still not fully defined in my opinion (see below).
The caveat is related to my second point about fully explaining TR. Clearly, the authors have chosen a strategy of selecting patients with 2 separate pharmacological trials using antidepressants at above (although slightly) minimal effective dosages. I believe that this should be more clearly stated in the methods section of the manuscript.

There is no universal consensus as to what constitutes TR. Some suggest that only those treated with “maximal recommended non-toxic doses” (for instance, paroxetine # 50 mg/day) for both pharmacological trials can be considered. Others that only those with HRSD scores of at least 16 can be considered, see a review on the topic by Gustavo Turecki in the Canadian Journal of Psychiatry (2007;52:46–54) available online. The authors cite that the Thase and Rush criteria were used to define TR, although in later publications Thase and Rush suggested that non response or the persistence of clinically significant symptoms, rather than remission, should be considered in order to define TR. Clearly, the authors have chosen less stringent clinical and pharmacological definitions.

This strategy does have repercussions. A case in point is that 16% of their ITT patients had what I (and most other clinicians) would consider very mild depressive symptoms (HRSD of 8 to 10). A further 30% had mild depressive symptoms (HRSD 11 to 14). The ITT GAF average pre treatment is 59.5 and 60.2 for the completers, either way, mild impairment.

My first discretionary revision would thus be to suggest to the authors to consider slightly modifying the abstract to reflect this. For instance, the mention should be made that these are ‘mild’ TRD patients (or at least, some are partially remitted?, whatever the terminology the authors might be comfortable with) in the fifth line of the first paragraph of the Background sub section of the abstract. I would also suggest that this be done on page 6 of the Background section (second paragraph), in the phrase beginning with “Therefore, we examined the short-term effectiveness…. as well as in the METHODS section.

My second discretionary revision concerns the average for the HRSD prior to treatment is not consistent. It is given as 14.4 on page 12 and 14.7 on page 13.

My third discretionary revision concerns page 7 of the METHODS section (second paragraph). I would imagine that a rapid deterioration in a patient requiring substantial modification in his/her medication would have resulted in exclusion or drop out? Was this the case? If so, it should be mentioned in the text.

My fourth discretionary revision concerns a phrase on page 17, where the authors state that it seems reasonable to conclude that « combining cognitive behavior therapy with medication improved social functioning more that medication alone. ». First, it is a bit difficult to assess if this statement refers to their own or, to the Dunner data®or, to a combination of both. If they are referring to the results of their own study then their data do not really support this statement (a control group would have been needed to prove that point).
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