Reviewer's report

Title: The necessity of moderating overestimated effectiveness of computerised CBT for adult depression - A meta-analytic re-evaluation of the effectiveness of CCBT on adult depression from the perspectives of long-term effectiveness, post-treatment function, dropout and publication bias

Version: 1 Date: 7 November 2012

Reviewer: Joanna Leaviss

Reviewer's report:

The main findings of this paper are original and interesting, however they are lost in the paper in its present form, which would benefit from restructuring, and by clarification of a number of issues listed below.

Major compulsory revisions:
1) Do the 2 included Proudfoot papers (2003 and 2004) report the same data? The second paper (Proudfoot 2004) states that the data analysed is an ‘expanded dataset’ of their first paper. If the data from 2004 includes the data from 2003, then it needs to be made clear that the authors have not included it twice in their analyses. As these papers constitute 2 of the 3 papers used to argue that CCBT + TAU is effective in the long-term for both depressive symptoms and functional improvement, it needs to be made explicit in the paper that the authors were aware that the 2 papers include the same data and that they have taken this into account when conducting their analyses. If this is the case, the authors should justify why they have included the 2 different papers in the meta-analyses rather than just using the data from the latter paper, with a reference to the previous paper. If it is the case that the papers contain shared data but the authors were not aware of this, then their results and conclusions will be affected and the analyses will need to be run again.

Minor essential revisions:
1) The description of CBT in the introduction, paragraph 1 is a little brief, and makes assertions such as CBT has been receiving ‘increasing attention because of several advantages’. CBT is a well-established intervention. More and better examples of its advantages are needed here.
2) More recent citations are needed throughout the introduction and discussion to support some of the authors’ statements, e.g. that depression is not a highly recoverable disorder; that the effect of standard face-to-face CBT on depression does not attenuate sharply.
3) The numbers in the PRISMA flow chart do not equal the number of papers reported, with 20 included papers in the flow chart, but only 17 reported on throughout the paper. But in paragraph 1 – characteristics of included studies, 45/65 studies were excluded, leaving 20 studies. If the total number is indeed 17,
this number needs to be amended, along with reasons for the additional excluded papers added in to the appropriate box. If the number is 20, this needs to be made clearer in the text.

4) Acceptable attrition rate and Imputation techniques – paragraph 1: It is not clear what all of these citations refer to, with Titov 2009 mentioned twice in the same group of trials, Spek 2007 and Clarke 2009 mentioned in each set of trials, and Clarke 2005 mentioned twice in the same group of trials. Is it Titov a and b? Clarke a and b? Only 1 Titov paper and 1 Clarke paper appear in the characteristics summary table, but then the b papers appear in the first meta-analysis. Does each paper contain 2 sets of data? More explanation is needed here. There is no clear pathway that lets the reader easily see how many papers are included for each analysis out of the total papers, and which these are. A flow chart with information about which analyses the data from each paper contributes to would make this less confusing.

5) Characteristics of included studies: McCrone 2004 is discussed in this section, but is not on the list of included studies, nor is it mentioned in the summary table of characteristics.

6) It’s not clear how the search terms used reflected the inclusion criteria. For example, the inclusion criteria states that proper allocation, concealment and single or greater blinding was used. Was this reflected in the search terms, or were all papers considered in the search then subsequently excluded on this basis once they were retrieved? The authors state in the methods that the Cochrane Risk of Bias Tool was used, but there is no section on the quality assessment anywhere in the paper other than a 1 or 0 in the summary characteristics table. There are no explanatory footnotes on this table to assist understanding.

7) All abbreviations in table 2 should be explained with footnotes.

Discretionary revisions:
1) The title is long-winded and would have more impact if simplified.
2) A copy of the search would be useful in the appendix.

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: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
No competing interests.