Author's response to reviews

Title: Is Computerised CBT really useful for adult depression? - A meta-analytic re-evaluation of CCBT for adult depression in terms of clinical implementation and methodological validity

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Author's response to reviews: see over
Dear Luigi Rocco Rocco Chiri and Joanna Leaviss,

Thank you for suggesting the revision points for our paper. We corrected it based on what you and another reviewer pointed out. In particular, as a major change, we completely eliminated the parts related to the analysis on CCBT + TAU versus CCBT, while we left the parts relevant to the analysis of CCBT versus control, because we concluded that, at least in the former, the number of data items is insufficient to be meaningful in a meta-analysis. Also, we considered the structure to be too complicated to logically follow. Therefore, we changed it by adding one sensitivity analysis of the result without the BDI scale that had been removed in the first version in order to further the methodological discussion. As a result, we believe that the logical organisation has been made simpler and clearer, resulting in a more understandable structure by dividing the discussion into two parts, discussing clinical implementation in the one part and the perspective of methodological validity in another. We would greatly appreciate if you could read our revised paper and the following response to the reviewer’s comment.

Yours sincerely,

Mirai So MD, PhD
Responses to the Comments from Reviewer Luigi Rocco Rocco Chiri

We greatly thank the Reviewer for the helpful comments concerning improvements of our manuscript.

Comments:

1) MAJOR COMPULSORY REVISION:

a) The methods is well described and formally appropriate but the analysis seem fragmented and the presentation of results do not contribute to clarify the main findings of meta-analysis. To overcome this imprecision it is possible to focus the statistical design on the comparison supported by adequate number of CCBT study reported in literature. This measure could contribute to ameliorate the powerful of analysis and above all it could prevent to “jump to conclusion” in the absence of sufficient amount of data (e.g. table 5-8).

Author’s response:
We completely agree with the Reviewer. Following the reviewer’s suggestion, we just focused on the analysis that can be supported by sufficient data. In fact, we eliminated the part of analysis of CCBT + TAU versus CCBT, consequently leaving the analysis of CCBT versus control.

b) Also, the discussion need to be summarized emphasizing the main conclusion that should find different support in the several facets of the analysis.

Author’s response:
As a result of removing the part with insufficient analysis, we necessarily came to a simpler and clearer structure in which we only summarise and emphasise the main conclusion and the relevant issues without logical fragmentation.

c) Moreover, the list of inclusion criteria could be refined through more specific and scientific terms while the list of exclusion criteria must be provided.

Author’s response:
We modified the inclusion criteria list with more scientifically sophisticated expressions, and also added exclusion criteria.

d) A brief description of the inclusion studies should be provided.

Author’s response:
We added a brief description such as what type of intervention was done and what kind of measurements or evaluations were conducted.

e) From conceptual perspective, despite the subgroups analysis seems statistically adequate, the aggregation of TAU-individuals and waitlist-individuals in the same control groups appears inconsistent. The authors tried to elucidate the rationale of this aggregation but it appears partially sufficient.
Author’s response:
In addition to the existing explanation, we provided the fact that all significant previous meta-analyses did likewise as an additional rationale.

2) MINOR ESSENTIAL REVISION:

a) The authors delineate the topic of their research but it seems necessary to focus the limitations of previous data (e.g., the list on page 2-3 appears uneven) in order to obtain a concise description of the premises and potential of such meta-analysis.

Author’s response:
We elaborated on the limitations of previous meta-analyses.

b) Moreover, in the background section it appears reasonable to update the references “AHCPR, 1999” which is reported as recent finding.

Author’s response:
We changed the reference of this part from AHCPR, 1999 to more recent citations of Busch et al., 2012 and Pampallona et al., 2004.

c) Lastly, table 2 should be the first placed in the presentation of results.

Author’s response:
We changed the order of tables, putting Table 2 at the top of presentation of results.

d) Quality of written English: Needs some language corrections before being published

Author’s response:
We utilised the professional proofreading company Cambridge Professional Services Co., Ltd. (http://www.cambridgepro.co.jp/)

We hope that the revised manuscript can now again be considered for publication.
Responses to the Comments from Reviewer Joanna Leaviss

We greatly thank the Reviewer for the helpful comments concerning improvements of our manuscript.

Comments:

1) About major compulsory revisions:
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.

Author’s response:
As a result of carefully reading both of Proudfoot’s papers (published in 2003 and 2004), we came to the conclusion that the point you suggested that the second paper (Proudfoot 2004) is an expanded dataset of their first paper (Proudfoot 2003) is completely accurate. Even though we firstly considered re-analysing the result of CCBT + TAU versus TAU by using only the two studies (Proudfoot 2004 and de Graaf 2009), we eventually decided that these analyses should be omitted mainly because another reviewer required us to eliminate all related analyses as a major compulsory revision. His first reason is that the results of combination CCBT and TAU make the main findings of single CCBT logically fragmented and thus incomprehensible. Also, the second reason is that the analyses of combination therapies are unreliable due to the insufficient number of studies to support them. Therefore, we just focused on only the main findings of single CCBT effectiveness.

2) Minor essential revisions:
a) 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.

Author’s response:
The description of CBT was substantially added to in the introduction part, and we expanded the part on increasing attention on CBT and specified several more advantages. Also, we added more and better examples of the advantages of CBT, including the enhancement of quality of life (IsHak et al., 2011), increase in adherence to pharmacotherapy (Pampallona et al., 2004), and comparative advantage for pregnant women (Yonkers, 2009).
b) More recent citations are needed throughout the introduction and discussion to support some of the authors’ statements,

Author’s response:
Throughout the introduction and discussion, a number of more recent citations were added, including the statements that “depression is not a highly recoverable disorder” and “the effect of standard face-to-face CBT on depression does not attenuate sharply”, by citing papers by Busch et al. (2012) and Pampallona et al. (2004) for the former and Hollon et al. (2006) for the latter, respectively.

c) 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 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.

Author’s response:
We incorrectly included three economic-evaluation studies on CCBT for adult depression, therefore we removed those three from the 36 eligibility studies. Also, we decided to remove two studies by Proudfoot (2003 and 2004) from the 36 eligibility studies, because both are on CCBT plus TAU versus TAU, and we decided to focus on only single CCBT research. Even though de Graaf 2009 is also included the combination of CCBT plus TAU versus TAU, it also has, in another branch, single CCBT versus TAU. Therefore, we included this study. So, the final number of included studies was 15 rather than 17. Further, the number has been clearly presented in the paper, including in the PRISMA flow.

d) 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.

Author’s response:
Each of the two Titov 2009 and the two Clark 2005 papers are three-branch studies with two sets of data. Therefore, the two studies (Clark 2005 and Titov 2009) were separately divided into two comparisons, thus being identified as Clark 2005a and Clark 2005b (according to the difference in reminders with CCBT) and Titov 2009a and Titov 2009b (technician-assisted and clinician-assisted CCBT), and this has been clearly stated at the end of the Results section. The PRISMA flow diagram was changed in order to reflect a clear pathway with which readers can easily see not only how many papers, but also how many comparisons were included, in the
same way as with a flow chart, as you suggested. Also, we showed how many papers and how many comparisons were included for the analysis and used in the Results and Discussion sections.

e) 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.

Author’s response:
This study was an economic evaluation, thus it was omitted because of inappropriateness. Originally, we had intended to analyse the economic evaluation of CCBT for adult depression. However, we did not search for economic evaluation-specific research terms, so we believe that we can apply the same research elimination process as with a PRASMA flow.

f) 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?

Author’s response:
We considered all papers at first, then eliminated the inappropriate ones. We put the search terms in the appendix.

g) 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.

Author’s response:
Though we first tried to put the Cochrane Risk of Bias Tool in the paper, we removed it from the analysis but incorrectly left the expression in the text, so we have now deleted the reference to it.

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

Author’s response:
We added the footnotes to explain all abbreviations.

3) Discretionary revisions:
a) The title is long-winded and would have more impact if simplified.

Author’s response:
We changed the title from a long-winded one to more impactful and simplified one.

b) A copy of the search would be useful in the appendix.
Author’s response:

We attached a copy of the search in the appendix.

4) Quality of written English: Needs some language corrections before being published

Author’s response:
We utilised the professional proofreading company Cambridge Professional Services Co., Ltd. (http://www.cambridgepro.co.jp/)

5) Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Author’s response:
Since another reviewer commented that the manuscript does not need to be seen by a statistician, we judged that we did not need further statistical qualification.

We hope that the revised manuscript can now again be considered for publication.