Author's response to reviews

Title: Counselling versus low-intensity cognitive behavioural therapy for persistent sub-threshold and mild depression (CLICD): A pilot/feasibility randomised controlled trial

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Author's response to reviews: see over
Dear Prof. Patrizia Zeppegno,

We are very grateful for the referees’ comments and suggestions. Please, see below our responses (in blue) to the referees’ comments. We hope you will be satisfied with our responses and will consider the revised version of our manuscript suitable for publication.

Yours sincerely,

Jill Morrison

Responses to referee 1

1. On page 4 line 54, the authors should identify a comparison group when describing an effect size.
   We have clarified that the comparators were no treatment controls.

2. On page 4, the authors should identify a study method when persisting an equivalent efficacy.
   We have clarified that these are comparative effects in randomised controlled trials.

3. On page 7, the overall rate of screening pack might be 28.1%.
   We corrected this – thank you for spotting it.

Discretionary Revisions

In the future trial, I think that the authors should plan to conduct a non-inferiority or equivalent trial rather a superiority trial. There is a limited-rationale to conduct a superiority trial.

Thank you for this. We have had many discussions about this issue and are still taking statistical advice about it. Within the text we have not advocated one type of trial over another and, in this paper, we would prefer to leave the text as it stands. We have said “a comparative study would need to be powered to detect much smaller between-group differences, whether designed to show equivalence/non-inferiority or superiority”.

Responses to referee 2

a. In the last sentence of the first paragraph there are a number of central concepts named, perhaps taken as given, it might be useful to consider these for the audience and reader; especially persistent – how might that be defined and referenced; the
question of amount of time again how might that be defined and referenced; similarly active monitoring and low-intensity interventions; although the latter is defined and referenced in paragraph 2.

We replaced ‘considerable time’ by ‘several months’ and we added the appropriate reference. The definition of ‘persistent’ is given in the sentence: “Depressive symptoms are considered ‘persistent’ if they have been present for several months or continue despite active monitoring by a clinician or low-intensity intervention”

b. The reference used (8) is 14 years old, is there any more recent supportive data?
   We could not find a more recent supportive reference, so we changed the text from ‘is’ to ‘has been’ to reflect that the finding is from the past.

c. In the fourth paragraph the authors mention that the two approaches are recommended, are you referring to National Institute for Health and Clinical Excellence (NICE) recommendations? Can you clarify this in the text?
   We have clarified this in the text. Thank you for raising it.

METHOD:
a. It might be useful to explain more fully the importance of timing, as it is relative to mention short-term. It seems to me in the context of the study that a. 3 months = outcome from intervention – depressive symptoms (+ or -) b. 6 months = depression diagnosis or not (moved to remission/ recovery or not)
   Although participants were assessed at 3 and 6 months after randomization, all outcome measures of the study were defined in relation to the 6 months follow-up. We considered 6 months to be a ‘short-term’ outcome, as we would define a long-term outcome as being at least at one-year follow-up.

b. Reference [19] under the heading recruitment, line 2, referring to the recruitment methods developed for the study, is missing
   Apologies, that was a typo. The number of the reference should have been ‘11’ in the previous version. In the revised version it is number 12. Many thanks for noticing that.

c. The inclusion/ exclusion criteria could be integrated into the text opposed to having it as a figure/ table.
   We have changed this as recommended to address this.

d. How was the eligibility of ‘capable of taking part in the research procedures’ assessed?
   During the baseline assessments, the researcher would informally assess the participant’s capacity to understand and complete research questionnaires.
e. Small point: in the list of inclusion/exclusion only use punctuation at end of list only

We have changed the list to text.

f. Screening took place with the GP. Were GPs trained in screening for depressive symptoms? To assess eligibility for inclusion into the trial as outlined?

The GPs screened only for the exclusion criteria, as explained in the text: “The list of potentially eligible participants identified at the practice was subsequently checked by the GP to identify anyone who met the exclusion criteria for the study”. We have changed the word ‘screened’ to ‘checked’ to clarify this.

At this point the author(s) report n=29 were excluded; what were the reasons for exclusion at this point? Could they be included into the flow under the category – excluded by GP?

Unfortunately, the GPs did not report back to us which of the specific exclusion criteria were used for each potential participant. Therefore, we are unable to present this data.

g. Similarly, what were the reasons for declined at baseline visit?

We did not formally record the reasons why participants declined at baseline visits, and therefore cannot report this information.

h. In flow diagram, reasons for exclusion at baseline visit n=22, but number of reasons adds to n=24: is this because of multi-reasons for some participants?

Yes, that is correct.

i. The intervention for face-to-face is described adequately. The intervention for bibliotherapy is described, but raises a complication: was there a third intervention, an online intervention. Did participants in one arm receive two low-intensity interventions? Or a blended bibliotherapy/telephone/online low-intensity intervention? The authors need to clarify the interventions under investigation, and the place of any confounding low-intensity intervention?

The Li-CBT online material provided a blended learning approach, supplementing the same content as in the books and worksheets, but as online audio modules. The participants had the option of accessing this material online in addition to the printed material.

j. The type of support used in the different interventions is described and very different. Has this point been considered fully in the article and especially the discussion of the results?

We accept that these two interventions are very different. However, this study is piloting a method to test the effectiveness of two specific intervention packages. As such, we are interested in the macro level outcomes of these interventions, and not in the particular components of the intervention that are effective. However, in the write up to our fully-powered trial, it would be a useful point to make that any differences in macro-level outcomes could be due to a range of components of these interventions, as there are multiple differences between them. We will therefore aim
to consider process issues in each intervention in greater detail in that planned study.

k. In the section on outcome measures please specify at what point the economic data was collected, e.g. CSRI, EQ5D5L?
   The economic data was collected at all assessment points. We clarified this information in the text.

l. You might mention what % was the composed random sample of participant therapist/ supporters were included in the adherence checks for both measures?
   This information is now included in the results.

m. Also I have questions over the use of these adherence measures to assess the adherence of: “guidance delivered by trained support staff,” not their ability to be supporters but the ability of the measures to accurately assess what their trained guidance was? Is it appropriate to rate them again a therapy manual, the one designed for the study?
   The Guided CBT Rating Scale is part of the therapy manual of the Li-CBT intervention. Both scale and therapy manual were developed by CW, one of the authors of the study. It provides a structure for a rater to assess the quality of the structured support and the quality of relationship achieved in Low intensity delivery.

RESULTS

d. Good research retention at 6-months, was any particular protocol employed/incentives used?
   No, we just had a wonderful research assistant, very efficient who established an excellent rapport with the participants!

DISCUSSION

a. The first paragraph talks about the findings here might be the first steps in contributing to a full-scale comparative trial. I would temper this with some consideration of the points raised earlier in terms of
   a. The different types of supporters
      As argued above, we feel that it is premature to discuss the multi-component nature of the interventions.

b. Clarification about the use of 1 or 2 low-intensity interventions
   We have clarified this in the methods section.

b. It would seem that the cost of recruitment is prohibitive and that a different strategy (ies) would need to be explored to reduce costs and increase participation?
   We have recommended in the discussion that “for a definitive RCT, we would recommend utilising wider recruitment methods, such as notices in local media sites and routes for self-referral” and use wider community based recruitment as advocated in reference 30.
d. Nothing is mentioned of the impact of ‘the problem with adherence that was identified in the PCC arm.’

We have expanded the discussion of the problem of adherence in this section.