Author’s response to reviews

Title: Feasibility and acceptability of two incentive-based implementation strategies for mental health therapists implementing cognitive-behavioral therapy: a pilot study to inform a randomized controlled trial

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Author’s response to reviews:

Dear Dr. Pressau,

Thank you for the opportunity to revise and resubmit our manuscript “Feasibility and acceptability of two incentive-based implementation strategies for mental health therapists implementing cognitive-behavioral therapy: a pilot study to inform a randomized controlled trial” for consideration for publication in Implementation Science. We thank the reviewers for their thoughtful comments. We have revised the manuscript to incorporate the reviewers’ suggestions, and believe that the revisions have helped make this a stronger manuscript. Below is an itemized list of each review item with a direct response to each concern.
We have submitted 2 versions of our manuscript – one with highlighting to make it easier to review the additions and one clean version.

Editor’s Comments

1. I suggest completing and adding a TIDIER (http://www.bmj.com/content/348/bmj.g1687) and STARI (http://www.bmj.com/content/356/bmj.i6795/) checklist to enhance the clarity of the intervention description reporting of essential elements of the analyses.

   Thank you for this suggestion. We have added a TIDIER and STARI checklist as suggested.

2. It might help to substantiate the selection of tested incentive-based strategies using evidence from the EPOC Cochrane review literature on financial incentives if possible

   We have added a discussion of the EPOC Cochrane review literature on financial incentives to the section describing the incentives used on page 5.

3. Consider distinguishing incentives (the promise of future delivery if and only if criterion achieved) from reward (the actual delivery of reinforcement when criterion achieved). Your operationalization seems to rightly focus on incentives, but as these terms are often (in my view, inappropriately) used synonymously it would be useful to clarify the distinction early in the manuscript. I find this to be helpfully distinguished in Michie et al’s taxonomy (https://www.ncbi.nlm.nih.gov/pubmed/23512568 - see appendix). This being said, it would help to clarify on Page 8 whether therapists were aware of the incentive in the first place (i.e. were promised the social or financial outcome if they met the criterion), or whether the reward was only provided after successful attainment of the criterion (I suspect the former, but would help to be clear).

   We thank you for this point – precision in terminology is critical. We have closely reviewed the manuscript to ensure that we operationally define the terms using Michie’s work on page 5 and use the terms appropriately throughout; we have described any time the therapists received tangible objects as “reward” rather than incentive. We also clarified on page 7 that therapists were aware of the incentive before beginning the study.
4. How does this intervention address the claim of lack of lasting change achieved by training and consultation made in the abstract (particularly given the findings in Figure 3). Perhaps address in discussion.

We have removed this description in the abstract as it detracts from the main point of the paper. We have added commentary of the brief duration of the intervention per Reviewer 1, Comment 1 in the discussion.

5. Consider adding discussion related to Self-Determination Theory and implication incentives on shifting from intrinsic vs extrinsic motivation.

We have added a brief discussion of STD and the implications of shifting from intrinsic to extrinsic motivation in the discussion on page 19.

6. Please clarify what justified the number of agencies and therapists approached.

We have clarified this justification on page 6.

7. The expressed aim of the “pilot study seeks to understand the comparative effectiveness” does not seem appropriate for the nature of the study design (i.e. pilot studies are not designed to answer questions of comparative effectiveness). I suggest tempering to be more consistent with the scope of the study.

We have tempered our language on page 4.

8. Please clarify what “psychological biases” incentives are designed to address. (line 367)

We have clarified this on page 17 by removing this term as it was confusing.

9. Please complete and include a CONSORT checklist (using extension for randomized pilot and feasibility trials) http://www.equator-network.org/reporting-guidelines/consort-2010-statement-extension-to-randomised-pilot-and-feasibility-trials/ see also: http://www.bmj.com/content/355/bmj.i5239.long

As suggested, we have completed a CONSORT checklist.
10. I suggest avoiding language of ‘marginally significant’ (line 313). Standards for statistical significance are (rightly or wrongly) set at p<0.05 and that is the thresholds at which I recommend judging.

We have removed this language.

11. Figure 3 would benefit from subtitles on axes and clarity on what ‘block 1’ and ‘block 2’ refer to (Figures should stand alone without need for main text to interpret)

We have made the changes suggested to Figure 3.

12. Please add a section in the discussion on implications for implementation science.

We have added a section in the discussion on implications for implementation science – see pages 23-24.

Reviewer 1

1. I have a few concerns about particular elements of the study design and how the affect the interpretation of results. The implementation study took place over a very short period of time. The results seem to suggest very high initial adherence to EBT with a steady and significant downward trend over time. This raises significant questions whether either financial or social rewards could be sustained over a realistic time period. It may be that initial high rates of adherence represent the change in behavior due to being observed by a research team. The time period of the implementation study may not have been long enough to see these effects stabilize as documented in previous studies (Leonard and Masatu 2006). The time period of data collection is something to consider for the larger trial.

We thank the reviewer for this insightful comment. We have added a discussion of this on pages 21-22.

2. Relatedly, in the discussion section the paper asserts that when incentives are removed behavior declines. This conclusion does not seem warranted given the strong overall downward trend over time. Without a proper control group this conclusion is not warranted.

We have removed this conclusion as suggested.
3. The finding that the therapists responded meaningful to the feedback is something to carefully consider for the design of future trials. The fact that the trial combined feedback and incentives makes it difficult to know if incentives would be seen as valuable without the feedback. The feedback would seem to be a treatment worth evaluating on its own in future research. It would be useful to discuss how this design choice affects results more thoroughly.

We have added a discussion of this on pages 18 and 21.

4. The incentives provided in this study are quite large and the implementation of the incentives would appear to be quite costly since it requires recording sessions and hiring trained staff to analyze them. Given these significant costs it would be important to consider the need to document program costs and incorporate cost effectiveness analysis in future trials.

We have added a discussion of this on page 22.

5. It would be useful to clarify whether clinicians received the feedback reports in the post period since this affects the interpretation of results.

We have clarified this in the Methods section.

6. It would be useful to provide more thorough discussion of previous literature on poor adherence to EBT. In particularly it would be useful to compare the rates of adherence observed in this study to prior evidence.

We have added a discussion of this on page 21 and also have included extensiveness scores in Table 1 in the manuscript to address this point.

7. It would be useful to compare the form of social recognition in this study to other behavioral economics studies.

We have added a discussion of this on page 20.
Reviewer 2

1. One point to note is that the client sample was already highly experienced in therapy, having on average attended 10 sessions. This degree of prior contact with therapy is highly skewed towards the high end; indeed, the median number of individual sessions nationally is one. Thus the current study provides little information about engagement and retention in an evidence-based practice. Other types of therapeutic modalities besides CBT, such as family therapy, place emphasis on early engagement, and thus the value of using CBT practices for engagement and retention would be missed unless the full study includes a high proportion of new clients.

We thank the reviewer for this comment. We added a discussion of this on page 19.

2. Of note is the authors' own study (Bedias et al., 2015 JAMA Peds) that pointed out higher influences from organizational, rather than individual therapist measures in predicting the use of EBP related to CBT. This could have relevance to the planned larger study, as the authors' state that one of the agencies already used social incentives and did not think the ones added by this study made much difference. One would need to measure the degree to which social incentives already existed in agencies in the larger study. Also, feedback about how well the therapist followed CBT was deemed important in this study, but its value would likely depend on the existing level of supervision.

We have added a discussion of this on pages 20-21.

3. The authors noted important lessons learned in this pilot study that would help inform the larger trial. Specifically, there were ethical issues raised about financial incentives, and it made sense to provide the opportunity for everyone within an agency to have the same opportunity to earn extra money, particularly since therapists often feel underpaid. It would suggest that the larger design would need to involve a group randomization, or a roll-out or stepped-wedge type of design. The "cluster randomized design" planned and noted on line 396 is presumably due to this and other factors, but not stated directly. Indeed, statistical power is known to be much higher if individual therapists can be randomized within agencies that serve as blocks, and leakage of the intervention is not large. It would be useful in this paper to point out the reasons the authors want to conduct this as a (presumably agency-based) cluster randomized trial.

Thank you for this comment. We added a discussion of this on page 22.
4. Finally, the authors should note that therapeutic principles for CBT are sometimes at odds with those of other modalities, particularly family therapy, which is indeed considered having considerable evidence behind it, especially for externalizing behaviors. Given that this project takes place in Philadelphia, which has long been known as a home for family therapy (as well as CBT), there may be directors of agencies that do not completely endorse CBT in all its components. It would be useful to close the paper with a short discussion of what agency-level factors may be important to measure in the trial and what moderating variables are hypothesized to have effects.

We have added a discussion of this on page 23.