Author's response to reviews

Title: Study protocol Efficacy of immediate patient feedback in emergency psychiatry: A randomized controlled trial in a Crisis Intervention & Brief Therapy Team

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Author's response to reviews: see over
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Study protocol Efficacy of immediate patient feedback in emergency psychiatry: A randomized controlled trial in a Crisis Intervention & Brief Therapy Team

25-4-2013

Dear Deesha,
Hereby we send you a note with further information about the revision of our manuscript. We have enclosed the original comments of the reviewers and have inserted a point-by-point response to the concerns. We have addressed the comments of the reviewers in the revised manuscript, which has been uploaded to your site.
We hope and trust this will be satisfactory. If not, please let us know.

Best regards,
Flip Jan van Oenen

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Review 1

Title: Study protocol Efficacy of immediate patient feedback in emergency: A randomized controlled trial in a Crisis Intervention & Brief Therapy Team

Version: 1 Date: 8 January 2013

Reviewer: Christoph Flückiger

Reviewer’s report:
It might strengthen the theoretical introduction if you refer to a broader community that developed feedback systems (e.g introduced by Lambert, Lutz or Pinsof)

The article of Lambert (2007) was already part of the literature review. The other articles are being referred to in the revised version.


Further, it might be helpful refer to other outcome predictions from early session reports :

In addition: Other studies that refer to feedback and alliance

The only essential minor that has to be revised imho:
Statistical analyses: There is a lack how the authors intend to run the statistical analyses.
Level of interest: An article of importance in its field
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.
Reviewer's report
Title: Study protocol Efficacy of immediate patient feedback in emergency: A randomized controlled trial in a Crisis Intervention & Brief Therapy Team
Version: 1 Date: 15 January 2013
Reviewer: Bruce Wampold

Reviewer's report:
The design of this feedback clinical trial is basically sound. The application to a naturalistic setting with crisis intervention is novel. The use of outcome measures other than that used for feedback is commendable and necessary.

Major Compulsory Revisions

1. The literature review suffers from a number of problems.
   a. It is much too long and needs to be more focused on the essentials. Many trials of feedback have been conducted. Focus on how this trial extends the literature.

   The literature review has been shortened and focused more on how this trial extends the literature, focusing on the two novelties this study offers:
   - This study is the first to examine feedback in a psychiatric crisis population
   - This study adds supplementary outcome measures other than those used for feedback, measuring outcome as well as alliance with an added separate instrument.

   b. The review omits discussion of a major feedback study by K. de Jong recently published, which is notable because it was conducted in the Netherlands and because it found no effect, for either signal or non-signal cases.

   This study has been integrated in the literature review in the introduction.

2. Design
   a. As indicated above, it is commendable that multiple outcome measures were being used. However, insufficient psychometric information was given relative to several of the measures. More importantly, it was not indicated when the measures would be given (pretreatment, posttreatment, periodically?).

   Supplementary information is provided about the measures.
   The time points on which the measures would be given were specified on page 27: ‘Time points: every 6 weeks’ and page 24: ‘Experimental intervention: every session’.
   A more elaborate specification is added.

   b. The patients are randomly assigned to condition, but it was not mentioned how
the patients were randomized to therapists. There are two ways to do this. First, and presumably the way in which this study proposes to assign patients, is that patients are randomly assigned to therapists so that therapists will have patients for whom they will receive feedback as well as have patients for whom no feedback will be received. This is the typical feedback design, but is problematic because there is a contrast effect. Therapists simply may attention more to patients in the feedback condition simply because these patients are more salient to them. The alternative is to randomly assign therapists to conditions and then randomly assign patients to therapists. This design has its own disadvantages (therapists are different in the two conditions, but the differences are accommodated in design by random assignment of therapists), but because it is rarely used it has the advantage of ruling out the contrast effect, a perspicuous threat to other studies. Of course, depends on there being sufficient number of therapists.

Both options for the study design as described have been thought through thoroughly, resulting in the choice for the design described. The possibility of a contrast effect is referred to in the discussion.

c. In any event, patients are nested within therapists and this must be accommodated in the analysis. Good examples of studies that have done this correctly are the de Jong study and the Anker study. Consequently, in the study therapists effects can and should be examined. This study may be able to answer one question that has not been addressed in the literature—does feedback improve outcomes for all therapists equally or are do some therapists (perhaps the better therapists) benefit more from feedback than others?

A description is added how the fact that patients are nested within therapists is been accommodated in the analysis.

3. Analysis. The authors do not fully describe the analysis to be conducted. What is the structure of the data? What analysis will be conducted and what tests of hypotheses will be used? What are covariates. The authors talk about examining how feedback works, by collecting information on the alliance. This suggests a mediation model, but he model must be set up and the analysis described (if alliance is conducted every session, then a longitudinal design could be used, but much needs to be said about this).

A more detailed description about the analysis to be conducted is provided.

Minor Essential Revisions
4. The authors seem to confuse an effect with a complete causal model. The authors state, “For example, Whipple et al. [41] found that all patients who are
not ‘on track’ benefit from feedback” (p. 8); this is not so, on average patients benefited, but not ALL patients.

This is corrected.

5. The term “unrestricted randomization” on p. 19 is not a standard one and it is not clear what is meant.

This is corrected.

6. What proportion of patients are referred to other services, historically at this clinic.

This information is added in the text.

Discretionary Revisions

7. Whether the design involves randomly assigning therapists to condition or not (see 2b) is discretionary. However, this reviewer recommends it.

Level of interest: An article of importance in its field

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

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests: No competing interests