Author's response to reviews

Title: The working alliance in a randomized controlled trial comparing online with face-to-face cognitive-behavioral therapy for depression

Authors:

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Version: 3 Date: 3 November 2011

Author's response to reviews: see over
Dear Dr. Olino,

Many thanks for sending us the valuable comments and reviews. We have incorporated all of the reviewers’ suggestions in the new version of the manuscript, as you will see in the attachment.

Please let us know if you have any further questions.

Kind regards,

Barbara Preschl
Reviewer 1

1. Abstract (Method): It could be mentioned that therapist ratings of the WAI were also administered.

We now mention in the Abstract that the WAI for therapists was administered at post-treatment.

2. Abstract (Results): The statement that "significant correlations were found between clients' ratings of the working alliance and therapy outcome in both groups" could be more specific. Results show that only the working alliance at post-treatment is significantly associated with outcome in both groups.

We specified this statement as recommended by the reviewer.

3. Abstract (Results): The sentence that "results from multiple regression analysis showed a significant influence of the pre-treatment depression score on the post-treatment depression score in the face-to-face group and a significant influence of the pre-treatment depression score on the depression change score in the online group" could also be reformulated or even deleted. Just by reading the abstract it does not become clear why the authors investigated the pre-treatment score as a potential predictor since the whole paper is about the alliance. It should become clear that WAI scores were not predicting post-test or change scores anymore, when controlling for pre-treatment scores.

The sentence has been deleted as suggested by the reviewer.

4. Method (Procedure / Exclusion criteria): One of the exclusion criterion was: "other psychiatric disorder". Which one? Others than psychotic and bipolar disorders? If yes, how were they assessed? Low symptom severity is another exclusion criterion. The reader could be provided with the cut-off score.

“Other psychiatric disorders”: We now specify that these were post-traumatic stress disorder, anxiety, and phobia. We have also added the BDI cut-off score for low symptom severity.

5. Method (Outcome measures): I am not sure if it is good to present the WAI under the title "outcome measure" because by adding WAI mid-treatment scores, the WAI is not just an outcome measure anymore.

We would still consider the WAI as our outcome measure and would prefer not to make any changes here.

6. Discussion: From my point of view, the fact that the WAI at mid-treatment was not substantially and significantly correlated with outcome in the online group should be discussed in more detail. To some extend, the discussion ignores these results. For instance, the authors write that "our results are in line with previous research by Knaevelsrud and Maercker, who found correlations
We now discuss the finding that there was no significant correlation between WAI at mid-treatment and BDI scores at post-treatment in the online group in more detail in the Discussion section.

**Reviewer 2:**

The reviewer asked why we did not provide a manuscript showing “tracked changes.” To our knowledge, this would not have been consistent with the authors’ instructions concerning re-submission of manuscripts.

**MAJOR COMPULSORY REVISIONS**

p16. Second paragraph & Table 4. It doesn't make sense to add all three WAI composite scores (mid-treatment, post-treatment and therapist post-treatment) into the same regression model. They should not compete with each other. I agree with the other reviewer's point that the simplest solution would be to let the different WAI scores predict the residual BDI gain scores, then the multiple regressions could be dropped and also the BDI change score as outcome. Table 3 with BDI-pre as covariate in all correlations between the WAI-scores and BDI-post would suffice to present the results. It would make the data interpretation much more straightforward without losing any informative value (following this recommendation would of course require some rewriting).

We have changed Table 3 and have removed Table 4 and the multiple regression analysis as suggested. Further, we have calculated the BDI residual gain score and dropped the BDI change score as suggested. In addition, we have slightly reworked the corresponding parts of the Data Analysis, Results, and Discussion sections, as well as the Abstract.

**MINOR ESSENTIAL REVISIONS**

Abstract. Results: Instead of writing about the influence of pre-treatment score BDI on post-treatment score (not part of any research question), it is enough to state that WAI did not predict outcome when entered into a multiple regression analysis together with BDI baseline score.

We have changed the Results section of the Abstract as suggested by Reviewers 1 & 2

Abstract. Conclusions: Add a sentence about the failure of the WAI to predict outcome.

This additional comment has been added.

p7. Second paragraph I am unsure how to interpret the second part of the second objective of the study, specifically "which treatment was the better predictor of the therapeutic alliance". I am guessing that what the authors mean to say is "if the predictive value of the alliance on the treatment outcome differed between the two treatments". This is never formally tested. Perhaps skip this part of the second objective?

We have deleted the phrase “and which treatment was the better predictor of the therapeutic alliance” as suggested.
We have removed the paragraph as suggested.

This section has also been removed as suggested.

We have removed the BDI-pre correlations from Table 3 as suggested.

Double reporting of correlations has been removed from the text as suggested.

The aspect of romantic relationships has been added.

**DISCRETIONARY REVISIONS**

We have reworded the phrase as suggested.

We have also implemented this suggestion.

We assessed biographical data on the same basis as the study by Lange et al. (2000). However, it as it is not a specific biographical assessment instrument, we have deleted this confusing reference.

"Life-review" is written with and without hyphen.
In line with standard usage, we use a hyphen in formulations involving a compound adjective (e.g., “life-review module/intervention,” but not when “life review” is used as a noun: “The aims of life review are …”

p14. Third paragraph. "Depression levels were measured in terms of…" -> "Treatment outcome was assessed as…” (or similar)

We have corrected this sentence according to the Reviewer’s suggestion.