Reviewer’s report

Title: Extending access to a web-based mental health intervention: who wants more, what happens to use over time, and is it helpful? Results of a concealed, randomized controlled extension study

Version: 0 Date: 27 Aug 2018

Reviewer: Kristina Fuhr

Reviewer's report:

This study investigates the effect and engagement of participants with a web-based mental health intervention after the first three months of use of the intervention within an RCT (the experimental group). Participants of the main study were asked at the three-months-assessment if they were interested in another three months access to the intervention and if so were randomized to either receiving the three months additional access or not. After another three months in the present study, the number of logins, and primary and secondary outcomes were assessed again.

The study addresses a very interesting point since adherence in internet interventions is known to be very limited and small, so the question remains who is willing to participate and engage in a specific time frame and why.

However, since the main study results are not published yet, the information about the outcome of the RCT is very raw and it remains unclear what the author's mean by "there was some benefit of access to the intervention". I would recommend to either wait until results are finally published or to clarify the results.

When comparing active intervention to a wait-list control, effect sizes are mostly over-estimated, ....

My two main concerns are that the main results are unclear and also not published yet. The other point is that in the discussion the authors' focus mainly on some subgroup results, that were only mentioned in the chapter BWW Utilization, but no analyses were made also because of small sample sizes (that is also mentioned in the limitations section). I would rather be careful with the interpretation of the usage pattern of only seven (!) participants but focus more on interpretation of the results concerning number of logins and other outcomes. The authors' found that there were no differences in the outcomes between the groups after another three months. However, the number of logins was extremely small. What conclusions can be drawn?
I have also some minor comments regarding the manuscript.

Introduction

- On p. 4, line 2, the authors' introduce the concept of "Mental Health Issues", all written in capital letters. Is this a common concept? However, on p. 5, line 2, the concept is repeated again, but this time without capital letters.

- When you report about adherence measures and problems with the engagement of users (p. 4, lines 9-20) you could also cite the literature more elaborately. For example, the problem that in every internet intervention, different measures of adherence were reported, and also definition of logins required for being "per protocol", is mostly not reported, or differs among studies (see for example Donkin et al., 2011, doi: 10.2196/jmir.1772).

- The authors' state that "overall fit between user and the platform components... are likely important factors for ongoing engagement." (p. 4, lines 17-18). Here, the authors' could also insert some reference(s) that supports this statement. Also in the next sentence, it would be interesting to name some examples for implications for payors based on low engagement of users.

Methods

- The authors' cite their own trial protocol. In my opinion I would rather insert a reference here instead of using a link to a web page

- In the statistical analysis section, authors' decide to use repeated measures ANOVA cause of "high degress of missing data at study end" (p. 9, lines 6-8). However, this ANOVA eliminates all cases with missing data so I don't really understand why this was a better choice. Or did the authors' use some kind of method for replacement of missing data?

Results

- When reporting the primary and secondary outcomes on p. 11, lines 4ff., the authors' should make clear that all statistical data that is referred to are reported (also for the secondary outcomes, even if reported in a table). I would also report main effects of the ANOVAs, if available.
Discussion

- The authors' could also discuss advantages and disadvantages of using the number of logins as only measure for adherence.

- In the limitations section, authors' report about "imbalances in baseline variables between groups that was likely due to chanc. We did re-run the analyses...e" (p. 13, lines 19-21). First, I dont find in the results section any of these imbalances mentioned. This result should not be reported the first time in the discussion, but before. Even if they are reported in table 1 (without statistical data), but should also be mentioned in the main text. Second, why are some significant (but not so easy to explain) results due to change and others (for example the engagement of 7 users) are emphasized even if there was not enough power?

- On p. 12, lines 3 ff., it is discussed that the sample of the study is represented by "more treatment-refractory and severely symptomatic individuals..." Where does this information come from? If from the trial protocol, then the original reference could be cited here again.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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Please indicate the quality of language in the manuscript:

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