Author's response to reviews

Title: Understanding the acceptability of e-mental health - attitudes and expectations towards computerised self-help treatments for mental health problems

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

RE: MS: 6394154341135849 - Understanding the acceptability of e-mental health - attitudes and expectations towards computerised self-help treatments for mental health problems

Thank you for the opportunity to revise our manuscript for possible publication in BMC Psychiatry. We have now addressed the points raised by the reviewers and have outlined the changes point by point in this letter.

**Reviewer 1**

*Feedback: I wonder about the rational for choosing repeated measures MANOVA? Could you please explain more in detail the rational and exact procedure (dependent, independent variables, what were the repeated measures etc)?*

Response: Given that this study had multiple dependent variables (expectancy ratings on 12 domains + likelihood of use for 4 interventions), each of which was assessed within each individual (i.e. repeated measures), and included a number of covariates (computer literacy) or between subject effects (gender, previous mental health problems, etc.), a MANCOVA was conducted. MANCOVAs also are suitable when the dependent variables are moderately correlated, as was the case in this study. We revised the statistical analysis section to provide more information about the procedure used.

*Feedback: In the result section/paragraph “likelihood of use” the authors report results of univariate tests. This information is missing in the statistical analyses plan in the method section.*

Response: We thank the reviewer for spotting this and included a sentence in the statistical analysis section outlining the procedure.

*Feedback: In the method section the authors report that they used only frequency of technology as covariate. In the results section for “acceptability” the authors report results for sex, previous and current mental health problems, previous help seeking, but not for technology. Please clarify.*

Response: The variables gender, previous and current mental health problems and previous help seeking were included as between-subject factors in the
We have revised the analysis section to add more clarity about the variables included in the analysis (please also see response above). Frequency of technology used was assessed as an indicator of computer literacy (please also see responses below) and results are reported in a separate section.

**Feedback:** I am also confused about whether or not the analyses in the result section “acceptability” and “likelihood” were controlled for “frequency of technology used” or not. Please clarify.

Response: Yes, results were controlled for computer literacy, i.e. frequency of technology used, please also see our response below.

**Feedback:** 6. The authors report in the method section, that they used “frequency of technology as covariate”. In the results section, paragraph “computer literacy” they report interaction-tests for computer literacy x intervention type. This is missing in the method section - statistical analyses plan.

Response: As outlined in the measures section, we assessed how frequently participants use different forms of technology and used this information as an indicator of computer literacy. Hence, “frequency of technology as covariate” and “computer literacy” refer to the same covariate. We revised the statistical analysis section to clarify this.

**Feedback:** 7. Moreover I miss the statistics on post hoc comparisons in the result section, paragraphs acceptability.

Response: In the post-hoc comparisons, each intervention is compared to each other intervention on all domains of acceptability. As a result, a total of 12 x 6 comparison would have to be reported. To make it easier for the reader to digest this information, we decided to report the results in an aggregated form, e.g. by reporting domains where similar results where found together. Therefore it is not possible to add statistics here without majorly expanding the section. On balance, we felt that this level of detail is probably sufficient while being less confusing.

**Feedback:** 8. The authors report subgroup analyses for high vs. low computer literacy, but only report results with regard to smart-phone use and not for other intervention-types.

Response: There were no significant effects with regard to other intervention types. We revised the sentence to highlight this more clearly. It reads: “Individuals with higher computer literacy reported a significantly higher likelihood (M=1.37, SD=1.27 compared to M=1.08, SD=1.24) of using a smartphone app for mental health problems (t(485)= -2.581, p<.05, r=.12), there
were no differences between groups with regard to the likelihood of use for other interventions.”

**Feedback:** I also wonder about the rational for conducting subgroup analyses only for computer literacy and not also for other potential variables of interest?

Response: Subgroup analyses were conducted for other variables of interest (gender, previous mental health problem, current mental health problem, past helpseeking), but no differences between groups were found. We revised the results section to include results for these comparisons.

**Feedback:** I miss references about the author’s core assumption that transition of E-Mental Health in routine care is low.

Response: In addition to the references of the current treatment guidelines for the UK, we have added a reference to the paper Whitfield, G., & Williams, C., Beh Cogn Psych (2004).

**Feedback:** Methods, paragraph measures: “metal health” should probably mean “mental health”

Response: We have fixed this typo.

**Feedback:** Discussion, fourth paragraph: “advantaged” should probably mean “advantages”

Response: We have fixed this typo.

- Discretionary Revisions

**Feedback:** The authors assume that the 12 investigated domains of acceptability are crucial for the uptake of an intervention. Wouldn’t it be interesting to test empirically which of the factors drives in fact the intention to use an intervention? e.g. by predicting intention to use in a regression model by the investigated factors? This would provide relevant information, necessary for defining strategies to increase the acceptance of internet-based interventions. This seems especially important as some factors seem, despite being rated as important by the participants (e.g. convenience with regard to time and location of treatment), to be weakly related to the likelihood to use an intervention.

Response: We agree that this would be a very interesting research question to investigate. In the current study, an analysis of the data towards that question was not possible, as 1) the outcome dimensions were highly correlated therefore
limiting the applicability of regression analyses (multicollinearity) and 2) this study only assessed a hypothetical scenario. We added a sentence in the discussion section to highlight that the dimensions identified in this study could be used to investigate this question.

14. I wonder about the author’s distinction between E-mental health and m-mental health. Isn’t M-health more a kind of a subcategory of E-Health?

Response: We agree that m-health is a subcategory of e-health. We included the term m-mental health to highlight that we also investigated the acceptability of smartphone apps for mental health. This makes it easier for individuals particularly into m-health to find and evaluate our paper.

Feedback: There are more recent Meta-analyses the authors could cite: e.g. Richards & Richardson, 2012 instead of Spek 2007; Mayo-Wilson & Montgomery, 2014 instead of Lewis, C., Pearce, J., & Bisson, J. I. (2012).

Response: We thank the reviewer for this suggestion and changed the references accordingly.

Feedback: As the cited reference is not accessible by now, the authors could explain more in detail what they mean with “In addition, we have recently found that the current evidence base for these added benefits or ‘collateral outcomes’ is sparse (Musiat & Tarrier, under review)” Introduction 2nd paragraph

Response: We revised the reference in this sentence, as the article has been published in the meantime.

Feedback: How did the authors assess “mental health problem”. Does this mean, diagnosed mental disorder?

Response: The assessment as to whether participants suffered from a mental health problem was based on their personal opinion, as individuals commonly experience mental health problems without receiving a formal diagnosis. The phrasing for this item was “In your opinion, have you ever suffered from a mental health problem?”. In fact, we also assessed whether individuals were given a formal diagnosis. We added this information both in the methods section as well as the results section.

18. The questions to assess “Factors influencing the decision to engage in treatment” were derived by conducting a focus group. I am wondering about what do we know from face-to-face-treatment literature on an empirically basis about factors influencing intention to engage in treatment?
Response: There is some overlap between the dimensions identified by service users in this study and factors influencing help-seeking behaviour previously identifies in the literature (e.g. Avoidance of counseling: Psychological factors that inhibit seeking help. DL Vogel, SR Wester, LM Larson - Journal of Counseling & …, 2007 - Wiley Online Library). We decided not to go into depth about the face-to-face counselling literature for two reasons: 1) Most of the literature on barriers for help-seeking and treatment uptake focus on environmental factors and much less on aspects of the treatment itself. 2) The domains identified by our focus groups are not considered an outcome in its own right or an exhaustive set of potential criteria.

Reviewer 2

Feedback: My main concern is the design of this study. The authors’ conclusions, in my view, go beyond their data. This is a problem, as for abstract readers these conclusions may be inadvertently misleading.

The authors state: "Overall, results suggest a poor acceptability for e-mental health and m-mental health in the general population". On my reading, I think what the paper actually shows, in sum, is that a sample consisting of mostly young educated females, expect e/m-mental health interventions to be less helpful than face to face therapy after reading 2-3 sentences describing each treatment. To quote the authors:

"Each type of treatment was briefly explained with a few sentences, as it was possible that not all participants were familiar with the different options"

The core issue with studies such as those presented the current paper, is that participants (if they have not experienced the treatments personally) will be making judgements on very little information. In fact, participants' expectations may be based almost entirely on the description provided by the researcher. An additional sentence may influence expectations one way or another. I think the authors need to present the descriptions of each treatment provided to patients. Still, I am unsure of the broader merit of a samples' perceptions of a minimally described treatment.

An alternative method that may have provided a more useful description, would have been to create a website where participants 1) watched a short clip of face-to-face therapy with a brief explanation, 2) watched a short clip of someone using a internet intervention with an explanation of the features, 3) a clip of someone reading a self-help book and an explanation, 4) the same as the above but with an app. This would have allowed participants to make judgements based on rich data.
Response: We agree that providing participant with more information would have likely affected the results. However, this would also be an entirely different study (or experiment in this case) and beyond they scope of this study. One of the putative advantages of e-mental health is the possibility to access care anonymously and without the need to contact primary care. This also means that most potential users will not have had experience with the type of intervention previously and will only have access to limited information about how these interventions work. The aim of this study was to investigate this unbiased perception of e-mental health interventions. We think that it has been relatively well established in the literature that those with prior experience of e.g. cCBT have much more positive attitudes towards such interventions. Similarly, there is a plethora of trials investigating how uptake for mental and physical treatments can be improved by pre-interventions and/or information material. We think that only by understanding the reservations of potential users towards technology-mediated intervention will we be able to design information material that improves uptake in e-mental health.

I agree with the authors that acceptability might be an issue with uptake of e-mental health beyond randomised controlled trials. However, whether this paper with the current design and methodology contributes to our understanding of acceptability, is unclear. If we agree that expectations of benefit will be influenced by the description provided, which is a fairly robust hypothesis, to imply that questionnaires like this are accessing/tapping into an already held belief about e-health holds less weight. Expectancies and attitudes may be easily manipulated based on who wrote the descriptions and the details they contained. If this paper is taken further, the conclusions must be toned down to accurately reflect the limitations of the methodology.

It has to be noted that we deliberately chose a three-step approach in this study. The first step was to identify important domains in decision making about mental health treatments. This was done with a service user group and detached from the mode of delivery. The second step involved having the importance of these dimension rated the study population. Again, this was irrespective of mode of delivery and detailed treatment descriptions would not have affected the results. Only in the third step were participants’ ratings potentially affected by the description of the interventions, which we kept deliberately brief to obtain an unbiased view of individuals. These elements were deliberate methodological considerations and not limitations given the aim and research question of the study. However, we understand how our conclusion could be interpreted as going beyond what the obtained data tells us. We revised the abstract and discussion section to highlight the issues raised by the reviewer. For example: “Individuals’ views on different treatment options are likely to be affected by the amount and quality of information provided on these options. In this study, only
brief descriptions were provided for each treatment, to obtain an unbiased view on the acceptability of face-to-face and self-help interventions. In addition, as e- and m-mental health interventions are often designed to be accessible without contact to a health care professional, individuals will likely have only very limited information available and have to base their decision on whether to engage with the treatment on these information.” and “As the attitudes towards different treatment options for mental health are likely subject to the amount and quality of information available to the user, an effort should be made in improving the image of e- and m-mental health, as well as providing users with sufficient information to make an informed choice.”

We thank all reviewers for their valuable feedback, which we believe we have now addressed in the revised manuscript. We appreciate your consideration of our revised manuscript and would be grateful if the article is now suitable for publication.

Yours sincerely,

Peter Musiat (on behalf of the authors)