PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Therapist-guided Internet-delivered cognitive behavioural therapy supplemented with group-exposure sessions for adolescents with social anxiety disorder: a feasibility trial
AUTHORS	Nordh, Martina; Vigerland, Sarah; Öst, Lars-Göran; Ljótsson, Brjánn; Mataix-Cols, David; Serlachius, Eva; Högström, Jens

VERSION 1 – REVIEW

REVIEWER	Debbie Spain NIHR Clinical Doctoral Research Fellow, King's College London, UK
REVIEW RETURNED	13-Jul-2017

GENERAL COMMENTS	Many thanks for asking me to review manuscript 2017-018345, "Therapist-guided internet-delivered cognitive behavioural therapy supplemented with group-exposure sessions for adolescents with social anxiety disorder: a feasibility study" The manuscript is easy to read, informative and represents a valuable contribution to the literature. I have a few suggestions for edits, as follows: Why specifically, were participants excluded at initial screening? Were particular modules more popular, or conversely, less likely to be completed? If there was a trend, this could be mentioned in the Discussion. Many participants have co-morbidities: were participants who were more symptomatic less likely to make gains? Are data available about the degree to which other co-morbid symptoms improved post-intervention, e.g. because feasibly, behavioural and cognitive techniques could have impacted on these symptoms. Was age a relevant factor in terms of effectiveness of the intervention? The sample is predominantly female – perhaps mention in the Introduction or Discussion the gender split of social anxiety in sub-clinical and clinical samples, and in turn, how representative the study sample is, e.g. generalisability of study findings. Are any further data available about acceptability and satisfaction with the intervention? If so, perhaps this could be mentioned given that this has relevance for refinement of interventions.

REVIEWER	Sonja March University of Southern Queensland, Australia
REVIEW RETURNED	18-Jul-2017

GENERAL COMMENTS	Thank you for the opportunity to review this paper which reports on a blended therapist-guided plus group CBT intervention for adolescent social anxiety. The paper is well written, with a clear objective and methodology that addresses the research question. The authors have clearly presented this feasibility trial – while it is not a randomised trial, it provides valuable information regarding the treatment of social anxiety disorder in adolescents, and allows insight into how ICBT and face-to-face may complement each other. I had a few minor comments and requests for additional information. Introduction • The authors comment that CBT is effective and the treatment of choice for SAD in adolescents. However, they fail to highlight that SAD is one of the most difficult anxiety disorders to treat in childhood and adolescence. How many sessions are typically required for SAD? Was this achieved in previous ICBT attempts? Previous evidence (including from meta analyses) suggests social skills training is important in enhancing the outcomes of CBT for SAD. How is this tackled in internet therapy? Is this why others have failed? It is a considerable challenge to treat SAD with ICBT, thus a brief discussion of this will ground the study, allow for consideration of how internet-based interventions could help (or not be useful) and also provide further rationale for the blended approach. • The authors seem to suggest that the justification for this study is that perhaps multiple modalities are needed – but with no in depth discussion around this. Further, they argue that blended treatment is needed to ensure exposure is implemented well, but do not mention social skills training in the introduction. Why wouldn't we just focus on face to face approaches then which can deliver these techniques more efficiently? The rationale needs to be more comprehensively communicated. Further, the blended program is 12 weeks (and group sessions were 2 hours) in duration – are there indications of the true cost reductions and other benefits for this type of intervention for SAD? Methods: • What is the justification for the primary outcome? Why not use loss/retention of diagnosis and recovery/remission as the primary outcome? • The kappa for comorbidity was fairly low – where were problems noted? In diagnosing comorbid anxiety or other disorders? • The authors note that parents could send messages to the therapists throughout the program. What was the purpose of this and how did it change the standardised treatment if they did send a message? Results: • The authors mention the session completion rates – are these at the point of post-assessment or 6-month follow up? • "Two thirds of the participants attended two or more group sessions and only 10% attended none." – It would be useful to provide the percentage breakdown across all sessions, or grouped into 0, 1-2, etc. This provides valuable information for understanding how users engage with online programs for SAD.

	For example, did the inclusion of the group sessions increase early completion? Or re-stimulate later completion? The authors comment on these possibilities in the discussion – but by including data in the results section, you could provide more informed suggestion. • With these session completion rates in mind - perhaps worth commenting in the discussion the proportion of people who missed out on various treatment components (e.g. how many actually completed exposure and social skills training?) and what did this mean for treatment outcomes? How many participants did the group exposure sessions without having done the online CBT content? • Therapist time – was there a minimum amount of time that the therapist was required to spend preparing feedback? How much of this time was spent on unsolicited questions from users/parents? • Proportion of adolescents and parents staying home due to anxiety – is this simply a group proportion, or does this reflect a change in whether or not they stayed home. E.g. how many adolescents shifted from previously staying home, to not? • A proportion of participants received additional treatment between post and follow up – was this controlled in the analysis? Who were these participants? Were they the participants who had shown meaningful improvement, or no improvement at post? This is important as there was only little change from post-follow up. In a feasibility trial like this, this type of additional information will add value for clinicians wanting to utilise ICBT, or researchers wanting to examine these types of treatment. The authors comment very briefly in discussion – but would be worth including in results and then including further discussion regarding this.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1: Debbie Spain

1. Why specifically, were participants excluded at initial screening?

Response: Most participants that were excluded at initial screening (telephone screening) fulfilled exclusion criteria such as initiation or dose modification of psychotropic drug within the past six weeks, ≥ 5 sessions of CBT (including exposure) within the last six months or was diagnosed with an autism spectrum disorder. Due to the journals word limitation this was not described further in the manuscript, but on request from the editor we are prepared to add this information.

2. Were particular modules more popular, or conversely, less likely to be completed?

Response: No, since it was not optional for the participants to choose which modules they would work with. The modules were organized in such a manner that the therapist would ”unlock” the next module once the participant completed the prior module, i.e., the modules followed a specific and predetermined order. This was unclear in the manuscript and we have added this sentence in the section Intervention (page 11):

The modules were assigned in a predetermined order and therefore, all modules but the first were initially locked. Once the participant completed a module, the therapist made the next one available.

3. If there was a trend, this could be mentioned in the Discussion.

Response: Following the prior response, no such trend could be observed.

4. Many participants have co-morbidities: were participants who were more symptomatic less likely to make gains? Are data available about the degree to which other co-morbid symptoms improved post-intervention, e.g. because feasibly, behavioural and cognitive techniques could have impacted on these symptoms.

Response: Self-rated levels of comorbid anxiety and depression were measured using the RCADS-C/P, and this measure showed significant reductions of anxiety and depression from pre to post treatment. These changes are reported in the manuscript but we have not conducted analyses on the possible association between number of comorbid disorders and the outcome after treatment. We do find issues related to comorbidity very important, but as this pilot trial had a small sample and limited variation in the comorbidity variable, we are planning to conduct these analyses in an upcoming (and better powered) RCT instead.

5. Was age a relevant factor in terms of effectiveness of the intervention?

Response: This is also an interesting question. Earlier studies conducted by our research group have not found age to be a predictor of treatment outcome in internet-delivered interventions and we chose to not include this analysis in the manuscript as the sample size is small and the age span is quite narrow.

We aim to include age as a possible predictor in the upcoming RCT where we have the possibility to analyze it together with several other potential predictors. The RCT will include children from a greater age span (10-17 years), which provides a greater range from which we may draw more firm conclusions on the relevance of age as a predictor or moderator of treatment outcome.

6. The sample is predominantly female – perhaps mention in the Introduction or Discussion the gender split of social anxiety in sub-clinical and clinical samples, and in turn, how representative the study sample is, e.g. generalisability of study findings.

Response: We have clarified this in the Introduction (page 3):

SAD is more common in adolescent girls than in adolescent boys with a female to male odds ratio of 1.58 (95%CI: 1.18-2.12)2.

(2: Burstein M, He JP, Kattan G, et al. Social phobia and subtypes in the national comorbidity survey-adolescent supplement: prevalence, correlates, and comorbidity. J Am Acad Child Adolesc Psychiatry 2011;50(9):870-80. doi: 10.1016/j.jaac.2011.06.005)

As well as in Limitations (page 19):

Although social anxiety is generally more common among women, the current sample had an overrepresentation of girls. The effect of gender on the results in this trial is unclear and may be further analyzed in future trials with larger samples.

7. Are any further data available about acceptability and satisfaction with the intervention? If so, perhaps this could be mentioned given that this has relevance for refinement of interventions.

Response: We had several measures of acceptability and satisfaction and prioritized some of them. However, we agree that the manuscript would be strengthened with a further addition. We have therefore added the following in the Results (page 13):

Furthermore, most of the participating adolescents found the treatment's online platform easy to use, with a mean rating of 5.6 (range 4-7) on the 7-point TAS scale item (were 7 indicates full agreement with the statement "The program was easy to use").

Reviewer 2: Sonja March (SM)

INTRODUCTION
1. The authors comment that CBT is effective and the treatment of choice or SAD in adolescents. However, they fail to highlight that SAD is one of the most difficult anxiety disorders to treat in childhood and adolescence. How many sessions are typically required for SAD? Was this achieved in previous ICBT attempts? Previous evidence (including from meta analyses) suggests social skills training is important in enhancing the outcomes of CBT for SAD. How is this tackled in internet therapy? Is this why others have failed? It is a considerable challenge to treat SAD with ICBT, thus a brief discussion of this will ground the study, allow for consideration of how internet-based interventions could help (or not be useful) and also provide further rationale for the blended approach.

Response: Thank you for a very important comment, which we thoroughly discussed when we were developing the treatment and later while conducting the trial. We agree that we have not covered the question, whether SAD is one of the most difficult anxiety disorders to treat or not, and that a more covering introduction about this would be desirable (even though the journal's word limitation restricts the possibility to fully cover all aspects of this important issue). When it comes to face-to-face treatment, we agree that generic treatments such as the Cool kids program (Hudson et al., 2015) have shown less effect on SAD compared to for example GAD and separation anxiety. However, with a SAD-specific treatment such as SET-C we can observe good effect sizes. For example, the controlled ES was 1.73 (against waitlist control) and 1.23 (against placebo treatment) respectively for SET-C (Beidel et al 2000, 2007). Our treatment did not include social skills training within the Internet modules, but it was modeled and practiced during the face-to-face group sessions, specifically in the context of exposure.

We are aware of the results from the reviewer's recent study where the internet-delivered generic and the internet-delivered SAD-specific treatments were not significantly different in outcome (Spence et al., 2017). With this in mind and the above question about the role of internet-delivered care for youth with SAD we believe that our trial with blended modalities can add important information, which of course needs to be tested in bigger, controlled trials.

(Ref: Spence SH, Donovan CL, March S, et al. Generic versus disorder specific cognitive behavior therapy for social anxiety disorder in youth: A randomized controlled trial using internet delivery. Behav Res Ther 2017;90:41-57)

We have revised a sentence in the Introduction (page 4):

In face-to-face treatment, generic CBT has shown poorer outcomes for youth with SAD compared to other anxiety disorders14, but when treatments have been tailored to include SAD-specific components, such as social skills training, the reported effects have been larger15 16.

(14: Hudson JL, Rapee RM, Lyneham HJ, et al. Comparing outcomes for children with different anxiety disorders following cognitive behavioural therapy. Behav Res Ther 2015;72:30-37.

15: Beidel DC, Turner SM, Morris TL. Behavioral treatment of childhood social phobia. J Consult Clin Psychol 2000;68(6):1072-80.

16: Beidel DC, Turner SM, Sallee FR, et al. SET-C versus fluoxetine in the treatment of childhood social phobia. J Am Acad Child Adolesc Psychiatry 2007;46(12):1622-32. doi: 10.1097/chi.0b013e318154bb57)

2. The authors seem to suggest that the justification for this study is that perhaps multiple modalities are needed – but with no in depth discussion around this. Further, they argue that blended treatment is needed to ensure exposure is implemented well, but do not mention social skills training in the introduction. Why wouldn't we just focus on face to face approaches then which can deliver these techniques more efficiently? The rationale needs to be more comprehensively communicated. Further, the blended program is 12 weeks (and group sessions were 2 hours) in duration – are there indications of the true cost reductions and other benefits for this type of intervention for SAD?

Response: We believe that one of the most important aims with delivering treatment on the Internet is to make CBT more accessible for the youth population. However, since the literature to some extent suggests that SAD may be more difficult to treat than other anxiety disorders, we had to take this into account when we designed the treatment. When we tried to balance the downside of less accessibility with the possible gains in treatment effect, it seemed motivated to include a limited number of face-to-face sessions. Even though these sessions are longer and involve two therapists (making the sessions more costly), the group format where six adolescents are gathered simultaneously keeps the cost down.

To clarify this point we have now added the following sentence in the Discussion (page 19):

Thus, SAD is a challenging disorder to treat and interventions aspiring to be effective may need to include direct and frequent therapist guidance. On the other hand, development of new treatments should not only consider treatment efficacy, but also accessibility, flexibility and cost effectiveness.

Any extensive health economic analyses could not be provided, due to the limitations of the pilot study design (with no control group to compare with) but we can conclude from our trial that the blended format still consumes less time than a strictly face-to-face delivered treatment. A more exhaustive analysis of health economics will be included in the upcoming RCT and data from the current feasibility trial will be used in an ongoing health economic study, including merged data from several studies, conducted by our research group.

As the reviewer points out, social skills were not mentioned in the Introduction. Therefore, we added "social skills" to the sentence in the Introduction (page 4) that gives a short rationale to the combination of ICBT and face-to-face group sessions:
[…where ICBT is a cost-effective and accessible format and group-sessions may ensure that key treatment components, such as exposure to social situations] and social skills training, [are conveyed properly].

METHODS

3. What is the justification for the primary outcome? Why not use loss/retention of diagnosis and recovery/remission as the primary outcome?

Response: This is a very relevant comment. Since there is really no gold standard measure in the field (as the LSAS for adults, for instance), we decided to choose a continuous measure over a dichotomous one, for strict statistical reasons. Continuous scales tend to carry more information than dichotomous measures (such as presence/non-presence of diagnosis) and similar measures to the one we chose have been used in important works within the social anxiety field, for instance by Beidel and colleagues (2007) who used CGI-I and by Masia-Warner (2007) who used the analogous Clinician's Severity Rating (CSR). However, we fully agree with the reviewer that loss/retention of diagnostic status is an important outcome and we do report this in the manuscript (but as a secondary outcome), as well as the number of participants who scored below the clinical cut-off on the self-rated measure of social anxiety (≤18 p on the SPAI-C), at post-treatment and follow-up.

(Beidel, D. C., Turner, S. M., Sallee, F. R., Ammerman, R. T., Crosby, L. A., & Pathak, S. (2007). SET-C versus fluoxetine in the treatment of childhood social phobia. Journal of the American Academy of Child & Adolescent Psychiatry, 46(12), 1622-1632.

(Masia Warner, C., Fisher, P. H., Shrout, P. E., Rathor, S., & Klein, R. G. (2007). Treating adolescents with social anxiety disorder in school: An attention control trial. Journal of Child Psychology and Psychiatry, 48(7), 676-686.)

4. The kappa for comorbidity was fairly low – where were problems noted? In diagnosing comorbid anxiety or other disorders?

Response: The comorbid diagnoses we calculated kappa for were GAD and depression. The blind assessor accurately diagnosed both GAD and depression in five of the seven assessed films, respectively. Hence, none of them was more problematic to diagnose than the other. However, the assessor commented in one case with comorbid depression and one case with comorbid GAD (that were not matched with the initial assessor) that more information would have been needed for him to reliably establish the diagnoses. This is in line with what clinicians sometimes describe when using the MINI-KID, that it is a bit brief and requires the assessor to supplement the interview with additional questions, to be able to diagnose with certainty.

5. The authors note that parents could send messages to the therapists throughout the program. What was the purpose of this and how did it change the standardised treatment if they did send a message?

Response: The purpose was to keep parents active in the treatment as they had an important role as co-therapists, for example, when the adolescents were conducting exposures. Therapists were instructed to only give support on actual treatment content and instructed to only answer messages about the adolescents (or about parents' relationship with the adolescents) and not regarding parents' own difficulties (such as parental anxiety). Therefore, we believe that the messages did not change the standardization of the treatment.

RESULTS

6. The authors mention the session completion rates – are these at the point of post-assessment or 6-month follow up?

Response: Completion rates are only reported for post-assessment. Participants had access to the treatment platform between post- and follow-up assessment, but the therapists did not monitor their use of the platform during that phase and they received no guidance after the post-assessment. To our awareness, only few of the families chose to log in during the follow-up phase, as most of them seemed rather ”done” with the treatment by then. But there were some exceptions from this and we will keep track of it in our upcoming RCT, which will follow this feasibility trial.

7. “Two thirds of the participants attended two or more group sessions and only 10% attended none.” – It would be useful to provide the percentage breakdown across all sessions, or grouped into 0, 1-2, etc. This provides valuable information for understanding how users engage with online programs for SAD. For example, did the inclusion of the group sessions increase early completion? Or re-stimulate later completion? The authors comment on these possibilities in the discussion – but by including data in the results section, you could provide more informed suggestion.

Response: We welcome this suggestion and data on the frequency distribution of completed modules has been added in the Results section (page 13) on a descriptive group level, as we do not have data on exactly when the modules were completed (before or after the group exposure sessions):

The frequency of completed modules by the adolescents was distributed as follows: 20% (n = 6) completed 2-3 modules, 43% (n = 13) completed 4-6 modules and 37% (n = 11) completed 7-9 modules. None completed fewer than two modules.

Furthermore, there was a significant correlation between number of completed online modules and attendance at group exposure sessions of 0.54, however no causal inferences can be drawn from this.

8. With these session completion rates in mind - perhaps worth commenting in the discussion the proportion of people who missed out on various treatment components (e.g. how many actually completed exposure and social skills training?) and what did this mean for treatment outcomes? How many participants did the group exposure sessions without having done the online CBT content?

Response: None of the participants completed only group exposure sessions as they were introduced at week four of the treatment. All but one participant (n = 29) completed at least the first three online modules, and hence had gotten psychoeducation about social anxiety and exposure as well as had started to build an idiosyncratic exposure hierarchy before attending a group session. A majority of the participants completed a large number of online treatment modules and group sessions, which gave them time to conduct a significant amount of exposure (introduced in online module 3) and social skills training (introduced in group session 1 at week four). However, we did not track the number of completed exposure- and social skills training exercises in other ways than by proxy, through measuring module completion and group attendance, so with our study design, and with the limited data we collected, we are not able to firmly say what impact this could have had on the treatment outcome.

9. Therapist time – was there a minimum amount of time that the therapist was required to spend preparing feedback? How much of this time was spent on unsolicited questions from users/parents?

Response: There was no minimum required therapist time, but the therapists were instructed to log in and provide feedback to their participants three times per week. We have now added a sentence regarding that in the Intervention section (page 11):

Therapists were instructed to log in and provide feedback to their families three times per week.

Since therapist time was logged automatically by the online platform, this includes all types of communication. Information on what kind of communication that consumed therapist's time would require in depth text analysis. Hence, although highly interesting, we are not able to present specific information about communication content or how the therapist time was distributed between different forms of communication.

10. Proportion of adolescents and parents staying home due to anxiety – is this simply a group proportion, or does this reflect a change in whether or not they stayed home. E.g. how many adolescents shifted from previously staying home, to not?

Response: These results were included post hoc and simply reflects group proportions at pre-treatment and follow-up, without any formal test of whether there were significant differences between the time points. In a majority of the cases, adolescents that had stayed home from school at pre-treatment had returned to school at follow-up, even though a few cases were observed with the opposite pattern (not staying home at pre-assessment and staying home at follow-up). The same pattern was observed for the parents, where a majority of those who had stayed home from work at pre-treatment had returned to work at follow-up. We included it as a preliminary indicator of change in school and work absenteeism. However, if the editor finds this too speculative we are willing to remove these figures from the manuscript.

11. A proportion of participants received additional treatment between post and follow up – was this controlled in the analysis? Who were these participants? Were they the participants who had shown meaningful improvement, or no improvement at post? This is important as there was only little change from post-follow up. In a feasibility trial like this, this type of additional information will add value for clinicians wanting to utilise ICBT, or researchers wanting to examine these types of treatment. The authors comment very briefly in discussion – but would be worth including in results and then including further discussion regarding this.

Response: Due to the limited sample size no separate analysis was conducted on these participants. However, we agree that further information about the participants in the results strengthens our reasoning in the discussion. All of the six participants met diagnostic criteria for SAD and were assessed as minimally or not improved, at post-treatment. All but one of the six participants were assessed as still meeting diagnostic criteria for SAD and assessed as minimally or not improved, at follow-up.

Therefore we added this to the manuscript in the Results section (page 15):

All these participants fulfilled diagnostic criteria for SAD at post treatment assessment and five out of six still fulfilled diagnostic criteria for SAD at follow-up.

This also links on to the Limitations section (page 19) where we write:

[A small proportion of the participants did seek additional care between post-treatment and 6-month follow-up, which could have affected the results. However, these participants continued to report high levels of social anxiety at follow-up, implying that additional care had limited impact on the long-term outcome.]

We hope that these responses to the reviewers' valuable comments, and the revisions made, has strengthened the manuscript and we would like to thank the reviewers and the editor for taking the time to thoroughly go through our study in such detail and with such lucidity.

VERSION 2 – REVIEW

REVIEWER	Debbie Spain NIHR clinical doctoral research fellow, King's College London, UK
REVIEW RETURNED	24-Aug-2017

GENERAL COMMENTS	Many thanks to the authors for their response letter and edits. The manuscript reads well. I have no further suggestions.

REVIEWER	Sonja March University of Southern Queensland, Australia
REVIEW RETURNED	07-Sep-2017

GENERAL COMMENTS	The authors have comprehensively addressed the questions raised in the initial manuscript review. While these have been well addressed in the response letter, it is my opinion that the integration of some of this material into the manuscript would be of great use. This information provided justification for many of my questions/concerns. I understand the word limitations of the journal, however if there were some allowance to include extra explanations into the manuscript, this would be beneficial for the readers. For example, those explanations provided to Reviewer 1, in response 1, and to Reviewer 2, in item 1&2 (particularly with respect to the possible unique advantages of blended approaches over standard iCBT - in the introduction), item 5, item 8 (perhaps in discussion) would be useful to incorporate into the main text of the manuscript. If the editor was open to this, I believe this would strengthen the manuscript further.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Debbie Spain
”Many thanks to the authors for their response letter and edits. The manuscript reads well. I have no further suggestions.”

Reviewer: 2
Reviewer Name: Sonja March
Comment: ”The authors have comprehensively addressed the questions raised in the initial manuscript review. While these have been well addressed in the response letter, it is my opinion that the integration of some of this material into the manuscript would be of great use. This information provided justification for many of my questions/concerns. I understand the word limitations of the journal, however if there were some allowance to include extra explanations into the manuscript, this would be beneficial for the readers.

For example, those explanations provided to Reviewer 1, in response 1…”

Response: We have added Response 1 to the Method section (page 6):
Most participants that were excluded at the initial screening fulfilled an exclusion criterion, due to having either initiated SSRI medication (or modified the dose) recently, for having received CBT within the last six months or for being diagnosed with an autism spectrum disorder.

Comment: ”…and to Reviewer 2, in item 1&2 (particularly with respect to the possible unique advantages of blended approaches over standard iCBT - in the introduction)…”

Response: We have added Response 1 and 2 in the Introduction (page 5):
Earlier findings suggest that face-to-face CBT supported by computerized CBT may be more effective than stand-alone ICBT for adolescents and young adults with anxiety disorders31 32. Furthermore, it has been suggested that ICBT combined with face-to-face CBT may be beneficial for adult patients with SAD33 and depression34. Such a treatment has, however, never been developed for adolescents with SAD before and the objective of the current trial…

(31. Sethi S. Treating Youth Depression and Anxiety: A Randomised Controlled Trial Examining the Efficacy of Computerised versus Face-to-face Cognitive Behaviour Therapy. Aust Psychol 2013;48(4):249-57.

32. Sethi S, Campbell AJ, Ellis LA. The Use of Computerized Self-Help Packages to Treat Adolescent Depression and Anxiety. Journal of Technology in Human Services 2010;28(3):144-60.

33. Andersson G, Carlbring P, Holmström A, et al. Internet-Based Self-Help With Therapist Feedback and In Vivo Group Exposure for Social Phobia: A Randomized Controlled Trial. J Consult Clin Psychol 2006;74(4):677-86.

34. Mathiasen K, Andersen TE, Riper H, et al. Blended CBT versus face-to-face CBT: a randomised non-inferiority trial. BMC Psychiatry 2016;16(1):432.)

Comment: ”item 5,”

Response: We have added Response 5 to the Method section (page 11):

…with the purpose to keep parents active as co-therapists. Therapists were instructed to only give support on actual treatment content and to only answer messages about the adolescents (or about parents' relationship with the adolescents) and not regarding parents' own difficulties.

Comment: "…item 8 (perhaps in discussion) would be useful to incorporate into the main text of the manuscript. If the editor was open to this, I believe this would strengthen the manuscript further."

Response: We have added Response 8 to the Discussion (page 18):
A majority of the participants completed a large number of online treatment modules and group sessions, which gave them time to conduct a significant amount of exposure (introduced in online module 3) and social skills training (introduced in group session 1 at week four). However, we did not track the number of completed exposure- and social skills training exercises in other ways than by proxy, through measuring module completion and group attendance.

We hope that these additions are in line with the suggested revisions and would like to thank the reviewers and the editor for the opportunity to further develop the manuscript.

VERSION 3 – REVIEW

REVIEWER	Sonja March University of Southern Queensland
REVIEW RETURNED	04-Oct-2017

GENERAL COMMENTS	Thank you to the authors for including these additional components in the manuscript. The information adds value to the paper and will more clearly guide further research in this field. I have no further requests.