Author's response to reviews

Title: Internet-based interventions for eating disorders in adults: a systematic review

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

Thank you very much for sending us the reviewers’ valuable comments on our manuscript Nr. 1517747769788941, entitled “Internet-based interventions for eating disorders in adults: a systematic review”. We have incorporated the suggestions made by the reviewers into the latest version of the manuscript. As you can see, we have adhered to PRISMA Systematic Reviews and major changes regarding inclusion criteria and the number of included studies have been made. Furthermore, an extra figure has been added, showing effect sizes and corresponding confidence intervals.

Below is a list of each reviewer’s original comment and our comments regarding our changes in response to the reviewers’ suggestions. The manuscript has now been edited by a native speaker.

We hope that these revisions make a substantial contribution to improving the quality of our manuscript, and we would be delighted if you now consider it suitable for publication in *BMC Psychiatry*.

Please let us know if you have any further questions.

Kind regards,
Ruth Dölemeyer

**Reviewer: Ina Beintner**

#1 You did not include one study by Carrard et al. in the review. Why?

As suggested by the reviewer, this study was included, after selection criteria were broadened to include controlled rather than just randomized controlled studies.
#2 The questions addressed in the review are buried in the method section and would be much more prominent at the end of the introduction.

As suggested, the questions addressed are now stated at the end of the introduction (see page 4).

#3 In the method section, you did not report whether you calculated effect sizes yourselves, how exactly you calculated effect sizes (i.e. what standard deviation or pooled standard deviation you used to standardize mean differences) or if you took them directly from the original manuscripts (which could be disadvantageous due to different means of calculations used in original studies).
Also, please indicate if and how you adjusted effect sizes for sample size.

A more detailed description of calculation effect sizes is given as suggested by the reviewer on page 7 line 6ff.

#4. You should explain why you preferred effects for completer samples over ITT effects. If only completer data is provided in the original manuscripts, ITT effects can be estimated by assuming a zero effect for study dropouts (dITT=dCompleter*NCompleter/NITT).

As suggested, ITT data are now used for calculating effect sizes and an estimation has been calculated for studies only providing completer data. This information is included on page 7 line 9ff.

#5. When means and standard deviations are not available, Cohens d can be estimated from F-values (for a German instruction, see Rustenbach, 2003, p. 95). This should be done for data from Robinson and Serfaty.

We found this comment and the literature very valuable and used literature to calculate confidence intervals in a more exact manner. Unfortunately for data of Robinson and Serfaty, Cohen’s d could not be estimated, as their study design used three rather than two groups. Effect sizes for the study by Fernandez-Aranda and colleagues – also now included – could not be estimated, because relevant information was missing.

#6. Please introduce all outcomes in the method section. Also, I would advise to include binge eating frequencies and abstinence rates (defined as total absence of binge eating over a set period of time) as main outcomes and EDI/EDE-Q subscales as secondary outcomes.

As suggested, outcomes are now introduced in the method section and disordered eating behaviour (e.g. bingeing and purging) and abstinence rates have been included as primary outcome. Furthermore, EDI/EDE-Q outcomes are defined as secondary outcomes, where assessed.

#7. Study dropout (i.e. failure to attend post treatment assessment) and treatment dropout (i.e. failure to complete treatment) should be clearly distinguished and analyzed separately from each other.
As suggested by the reviewer, study dropout and treatment dropout have been defined (see page 14 line 6ff) and analyzed separately from each other.

# 8. Please check table 2a for missing information on mean age

Mean age has been added to the table, where necessary.

# 9. Both the results section and the discussion are confusing and would benefit from a clearer structure and some reduction of information that can be obtained from the tables. In the results section, always specify which EDI/EDE-Q subscales you are referring to and maybe group subscales based on what they measure rather than what questionnaire they come from.

As suggested, the volume of information has been reduced in the results section and the discussion and a clearer structure has been implemented in each.

#10. The whole manuscript would benefit from editing, preferably by a native speaker.

The manuscript has now been edited by a native speaker.

Reviewer: Isabelle Carrard

Minor essential revisions

#1 In the abstract p.2: the background states that this systematic review evaluates the efficacy of internet-based intervention programs. To avoid confusions, the use of the word “program” should be kept for Internet-based program. Among the five studies analyzed in this review, only two were real Internet-based intervention programs (Sanchez-Ortiz et al., Carrard et al.). Robinson and Serfaty evaluated the use of e-mails, Ljotsson et al. evaluated the effect of e-mail support with a self-help book, and Johnston et al. evaluated the efficacy of therapeutic writing for bulimic symptoms.

As suggested, the intervention used in the different studies has been described and differentiated in more detail in table 2 and on pages 9 and 10.

#2 Methods p. 5 (and after): Cohen’s d instead of Cohens’ d

This correction has been included.

#3 Study characteristics p. 7: Johnston focused on symptoms of bulimia nervosa. It should be specified which studies documented diagnoses and which did not.
As suggested, documentation of diagnoses in the different studies has been specified on page 9 and in table 2.

#4 Table 2a: in the interventions, it should be specified that Ljotsson et al. used a self-help book and that Carrard et al. and Sanchez-Ortiz et al. developed and used internet-based intervention programs. In the "number and diagnosis of participants" for Carrard and Sanchez-Ortiz, "mean age" and "women" should be erased since they are specified the line below. The N of the control group should be stated for all studies.

As suggested, relevant information has been added and information that was given twice has been erased.

#5 Table 2b: abbreviations should be defined under the table (questionnaires and OBE). In the study by Carrard et al., the definition of abstinence is more precise: abstinence from OBEs for the last 28 days. Are the rates of abstinence mentioned for the Johnston et al. study in the intervention group only?

As suggested by the reviewer, the definition of abstinence in the study by Carrard et al. has been corrected. Unfortunately, in the study of Johnston rate of abstinence was mentioned for the intervention group only but not for the control group. Additionally, abbreviations have been defined under table 2a and b.

#6 Main outcome of studies, starting p.10: the studies are sometimes called by their authors’ names and sometimes by one of their characteristics (for ex. “the study assessing BED” or “the study utilizing the writing task”…), making this part a bit confusing. Could the authors stick on one denomination by study?

As suggested, studies are now referred to by their authors’ name to avoid confusion.

#7 Table 3a and 3b, table 4: the notes under the tables should be checked.

As suggest, notes have been checked and adapted.

#8 Abstinence rate p.12: “Robinson and Serfaty found the increased rates significantly different from chance…” I am not sure of how to understand this sentence. In the study of Carrard et al., the criterion was abstinence of OBE for 28 days (OBE has to be defined). To make the sentence clearer, it should be specified “the abstinence rate after six-months of treatment was only...”.

As suggested, the criterion for abstinence in the study of Carrard et al. is defined in more detail (see table 2) and the sentence concerning the study of Robinson and Serfaty has been clarified (see page 12).

# 9 p. 13: Is the section on diagnosis improvement necessary (if yes it has to be clarified) and has it to be in the abstinence rate paragraph?
Since diagnosis improvement was not part of definition of abstinence, it has been erased.

#10 References: names of journals should be either entirely written or abbreviated. A mix of both options was taken and should be changed.

Names of journals are now written out in full for all references.

**Major compulsory revisions**

# 1 Introduction p.3: the paragraph “in the past years” should be divided into two parts, the first one on what has been done in the field of eating disorders (references are missing) and the second one on advantages and characteristics of Internet interventions.

The paragraph has been divided in the two parts, as suggested by the reviewer, and references have been added (see page 3).

# 2 Intervention characteristics p. 7: the descriptions of the interventions are not clear. Structured self-help interventions were offered by Ljotsson et al., Carrard et al., and Sanchez-Ortiz et al., but it was delivered by book for Ljotsson and by Internet-based programs for the two others. Robinsons and Serfaty used e-mail therapy, and not a structured program. E-mail was the therapy in this latter study, whereas e-mails were supporting the use of self-help in the three others. Rather than mentioning the face-to-face contact as intervention parts, which they were not, since face-to-face interviews were for evaluations, why not mentioning when participants could stay anonymous or not (also in table 2a). Anonymity has been shown to be crucial for the retention rate of studies in Internet-based intervention in other fields of psychopathology.

As suggested by the reviewer, the interventions used in the different studies are now more precisely described (see pages 9 and 10). Furthermore, anonymity has now been included in table 2 rather than mentioning whether face-to-face contact was part of an intervention.

# 3 Control group characteristics p.8: the control group in the study of Johnston et al. had to write about superficial topics, in a factual manner, without exploring thoughts or feelings.

The word ‘without’ has been added on page 10, line 26.

# 4 Discussion: the authors start with describing the heterogeneity of the studies. This is good to underline in order to interpret the results. But I think that the results might be easier to understand if the similarities between the three self-help interventions (book or Internet-based program) based on CBT were underlined as well, and then studies put together as a whole for interpretation. The two other studies can be described individually with their strengths and weaknesses. In Johnston et al. the Internet is not the focus, this study evaluates a paradigm of intervention which has not been evaluated with ED before. In Robinson and Serfaty, CBT is not the only approach used by therapists and the lack of standardization may explain the lack
of significant results. But this is a unique and original study on the use of e-mails in therapy, whereas e-mails are frequently used by clinicians. Regarding the three remaining studies, results show that structured programs based on CBT can be transferred on the Internet, and that e-mail can be a valuable way of supporting participants using self-help. I think that the authors could use the heterogeneity also in a positive way, to show all that has been done with the Internet until now in RCT, and summarize the results more efficiently to draw conclusions despite heterogeneity.

As we found this comment very valuable, similarities between the studies addressing self help interventions are now highlighted and conclusions have been drawn with respect to these similarities.

#5 Drop-out rates p.18-19: as said by the authors, the diagnosis is certainly an important factor explaining the dropout rates. But anonymity can also explain why the dropout rates varied between studies. This was firstly shown in reviews of Internet-based interventions on anxiety and depression.

As suggested, anonymity has been included as one additional possible explanation for varying dropout rates. No connection between anonymity and treatment drop out was found in this review (page 18, line 19ff).

**Reviewer: Fernando Fernandez-Aranda**

#1 In the introductory part some general references need to be updated.

As suggested by the Reviewer, references have been updated.

#2 Please consider including some additional articles in this topic, namely:


As suggested, these studies have been included after broadening the selection criteria (please see reply #1 to Ina Beintner for further details).

#3 Regarding results, please consider including figures with effect size. Although tables are informative, the figures might be of interest at the first view.
As suggested, figures including effect sizes and corresponding confidence intervals have been included (see page 11, line 15).

# 4 Since there was heterogeneity in the studies used (type of program used, diagnostic criteria, samples selection criteria,...), why not considering including a more flexible criteria for selecting eligibility rules? Otherwise, the conclusion are limited.

As suggested, criteria for selecting eligibility rules have been made more flexible (including controlled instead of only randomized controlled designs and additionally including studies referring to participants of 16 years or older). These changes resulted in the inclusion of three additional studies.

Reviewer: Manfred Fichter

A Major Compulsory Revisions

#1 The criteria for studies to be included in this review are not wisely chosen. Why did the authors exclude studies on relapse prevention and why did they include as one of five studies the study by Johnston et al., 2010, where the duration at the intervention was only three days, no follow-up, no CBT, almost zero effect size.

#2 No reason is given, why relapse prevention studies are excluded. Their effect sizes may be smaller or even negative, but the basics are very similar: to do something which helps to improve or prevents deterioration of the eating disorder of the patient.

@ #1 & #2: Studies of relapse prevention are aimed at a different target group of participants: those who have just finished treatment for their eating disorder. Comparison of treatment interventions with relapse interventions makes it very difficult to draw conclusions. Although the study of Johnston et al. differs in many ways from the other studies included in this review, it fulfilled all selection criteria and it was therefore necessary to include it in the review (see page 21, line 4ff).

#3: The authors cite the recent study by Wilson & Zandberg on cognitive-behavioral guided self-help for eating disorders. Wilson & Zandberg had other (better) criteria for selecting study for their review: CBT-studies. The Wilson & Zandberg review I found very interesting. What does the current study by Doelemeyer et al. add to the review by Wilson & Zandberg? Four of all five reviewed studies by Doelemeyer et al. were CBT-studies and in their review they admitted that the study by Johnston was not really appropriate for having been included in the review. At this stage I see no further need for a review concerning the evidence for the value of internet-based interventions for the treatment of eating disorders.

We agree with the reviewer that the review by Wilson & Zandberg is very interesting and valuable. But, as stated in the discussion, to our knowledge, our review is the first examining the standardized results that treatment delivered through the internet can have for participants suffering from different kinds of eating disorders. In the study of Wilson and Zandberg, no standardized effect sizes were assessed.
#4: The number of reviews included (n = 5) is very low as the authors admit on page 22. Starting out with 590 studies 23 studies were considered more closely and they finally included 5 studies in their review of which one study (Johnston et al.) does not really fit and others might have been included (see above 1 inappropriate criteria for study selection for review).

As suggested by the reviewers, selection criteria have been broadened, leading to inclusion of three additional studies (see replies to the comments by Fernandez-Aranda).

#5: The review bei Doelemeyer et al. includes studies which have been printed or e-published until July 2011. Now clearly more than a year has gone by and studies published until July 2012 should have been included.

As suggested, studies that were published since July 2011 were screened and one additional study (conducted by Ruwaard et al.) met (broadened) selection criteria. This is now included in the review.

#6: The discussion is rather lengthy and deals with issues like drop-out rates and rates of abstinence. A basis of four or five studies is too small to come to a concluding statement whether the rates found are high or low.

As suggested, a clearer structure has been implemented in the discussion and information has been reduced. Nonetheless, drop-out rates and abstinence rates are considered to be important issues for discussion in a review concerning the treatment of eating disorders.

B Minor Essential Revisions

#7 In the introduction reasons are given why a review is wanted. Here it is not mentioned that some eating disorders (anorexia nervosa) are among the most difficult to treat mental disorders and one with the highest mortality rate so that treatment research whether delivered over internet or other means is secondary.

As suggested, this information has been added to the introduction (see page 3, line 14).

#8 Throughout the text (page 3, page 17, page 21) the authors seem to argument to much and without true support by the data that effect sizes for internet-based studies for eating disorders are high and they even come to the conclusion that they are as good as studies based on face to face contact in treatment. Instead in this conclusion they do not sufficiently take into account that samples in different studies differ (not treated, inpatient, outpatient), the severity of illness in samples can differ highly, the stage of illness and the stage of treatment do play a role (to name just a few areas).

This important aspect has now been addressed in the limitation section (see page 21, line 7).
Discussion too long and throughout the manuscript many repetition of arguments and statements.

As suggested, a clearer structure has now been implemented in the discussion and repetitions have been reduced.