Reviewer’s report

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

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Reviewer: Isabelle Carrard

Reviewer’s report:

In this systematic review, the authors were interested in giving an overview on Internet-based treatment for eating disorders (ED), developed and assessed in randomized controlled studies. As the authors underline, it has been done in other fields of psychopathology but not for ED. This is the main interest of this review, which timing allows a good overview of what happened in the field until now. The method is well described. The authors had to face a large heterogeneity among studies and acknowledge appropriately that this is a limit to what can be concluded from this review. This article should be improved by describing in more detail the different procedures/use of Internet among the studies, giving a real overview of the multiple possibilities brought by new technology, rather than focusing only on interventions efficacy. The discussion could be reoriented with this angle and be more concise.

Minor essential revisions

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.

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

Study characteristics p. 7: Johnston focused on symptoms of bulimia nervosa. It should be specified which studies documented diagnoses and which did not.

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-Oritz, “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.

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 ?
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?

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

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…”.

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?

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

Major compulsory revisions

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.

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.

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.

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.

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.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.