Reviewer's report

Title: Internet-based cognitive behavior therapy for obsessive compulsive disorder: A pilot study

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Reviewer: alfred lange

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Review Manuscript E. Andersson et al., July 3, 2011-07-02

The manuscript describes a pilot study aiming to provide evidence for the suitability of online CBT in the treatment of OCD. The main question is well described and well defined. Overall, the manuscript is well written and easy to follow. This includes the description of the 15 modules constituting to the treatment.

There are some interesting findings, but there are also weaknesses in the description and interpretations that should and could be addressed.

1. There are many measures of the effects but there is no (Bonferroni) correction for findings by chance.

2. The authors define clinical significant reduction as the % of patients with 30% reduction in symptomatology. This is not the correct way to define clinical significance. This percentage should be described as meaningful response. It would be correct to use the more conservative Jacobson and Truax criterion: two standard error reductions in combination with change from above the cut-off score of clinical symptomatology to below.

3. The suggestion of cost-effectiveness seems too self-serving. There are 5 measures in the TIC-P, only one of them (hours of help from the family) shows a nearly significant reduction between pre- and posttest. If the Bonferroni correction would have been applied, even that measure would not show a trend in cost-effectiveness. Yet, the means of the TIC-P measures suggest improvements, but they do not come close to significance. This might be related to the large SD’s at pretest. The authors might reflect on this: were the SD’s in this study larger than in other studies, or do the scores reflect a psychometric problem of TIC-P?

4. The study is an open pilot trial with only 23 participants. In the discussion, the authors state this as limitations. I would not call it that way. Pilot studies as these can be helpful and even necessary. But even an open pilot study should include a follow-up period, in order to investigate to what degree improvements sustain. Especially since an unknown but probably considerable percentage of the patients did not complete treatment and accordingly did not follow the relapse prevention module.

5. Due to the omission of a follow-up period the interpretation of the cost-
effectiveness is also questionable. This should not be measured during the treatment but in a (longer) follow-up period.

6. The authors mention an average completion of slightly less than 2/3 of the 15 modules. What does this mean? After a pilot, we would expect a detailed description on where the people stopped and why? Didn’t they need further response prevention exercises? It would be interestingly to explore whether there are indications (not tests) whether the ‘completers’ (a term they do not use) have a higher rate of reduction in symptoms in comparison to those who left the programme.

7. The self-help treatment is supported guidance by one psychologist (not clear whether this was done by email and/or telephone contact). Altogether 92 minutes during the treatment on average. This amounts to an average of about 6 minutes per module. Since patients could seek contact whenever and how often they wanted this could happen a few times during one module. This calls for more description and more reflection than the authors provide: To what degree may the results of this psychologist be generalized to other psychologists as he/she must have been a sort of genius. After all, in maybe two or three minutes he must have been able to read the report of the patient, think about it, give meaningful feedback and motivate the patients to proceed. The description in the manuscript should also be clear about the way of measuring the support time, exactly where did it start and end and how reliable was it measured. The more so, since only one psychologist was involved.

8. Finally, the programme was mainly online, but the patients were seen by a psychiatrist. The authors sure have their reasons for this but it also constitutes a limitation in a study on pure online treatment. What is the effect of this face to face intake? How much of a threshold it causes to participate in the online programme? How does it affect the outcome? Does the client/therapist interaction during the diagnostic interview influences the outcome? Shouldn’t we add the time of the diagnostic interview to the 92 minutes? Wouldn’t it be possible to conduct the screening entirely online and telephone?

Conclusion: The points made above could and should be addressed in the description of the study, in the abstract that should be more cautious, and in reflections in the discussion. The discussion should focus more on what the authors learned from this pilot than on the outcome. The authors should focus on the real limitations and not on the limitations that are not real (pilot study, no randomization)

I would suggest a thorough revision and then rereview to decide on acceptance or rejection.

**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.

**Declaration of competing interests:**

I declare that I don’t have competing interests, although I am advisor to Interapy (internet treatment) in generating online treatment and the studies into effectivity. I have no shares and I am not on the payroll of Interapy.