Reviewer's report

Title: Delivering internet-based exposure treatment for irritable bowel syndrome in a clinical setting: a randomized controlled trial.

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Reviewer: Melissa Hunt

Reviewer's report:

This manuscript, titled “Delivering internet-based exposure treatment for irritable bowel syndrome in a clinical setting: a randomized controlled trial” is a nice extension/replication of the authors previous work on their ICBT intervention for IBS (16 in their references). The question posed by the authors is well defined – namely will the intervention work (i.e. be “effective”) in a representative clinical sample of IBS patients (as opposed to a screened or self-referred sample). The methods used are appropriate and well described. The data appear to be quite sound. Indeed, they have used a sophisticated set of appropriate outcome measures that go well beyond the ludicrously inadequate single question binary outcome measure of “adequate relief” used in most pharmacological intervention studies. They have also used a sophisticated data analytic strategy that is appropriate for the sort of data they have. The introduction is thorough and reviews the prior work on which they are building. The discussion and conclusions are well-balanced, adequately supported by the data, and clear about the limitations of the work. The abstract, however, is MISSING, and needs to be added to the ms. The writing is acceptable and indeed the ms is better written than many by American and British authors.

There is only one discretionary revision that I would like to see: the authors point out that their effect sizes were lower than in their own previous trial, but they do not make any effort to calculate or report on the clinical significance of their findings. This is particularly important given the nature of the sample. They point out that the mean baseline scores on the VSI and the IBS-QOL were “markedly” less severe than in prior physician referred or self-referred samples. It would be quite interesting to know several things. First, what percentage of the entire sample achieved clinically significant symptom reduction. Second, if you divide the sample into more and less severe groups (based simplistically on a median split, or better on 1 SD below the mean of the symptom inventories from their prior study) would the percentage of individuals achieving clinically significant change look dramatically higher for the individuals who were more impaired to begin with? That is, simply giving us group means and effect sizes for treatment versus control doesn’t give us enough detail about actual symptom change across the range of patients enrolled.

The authors conclude, rightly, that we need more research on which to base recommendations about which IBS patients stand to benefit from receiving psychological treatment. Ironically, they may have data from this study which
speaks to that point but which they have not fully exploited. In the US, at least, it is very difficult to get gastroenterologists to refer for psychological treatment at all! If we had clear cut clinical guidelines that could foster a better referral pipeline, all the stakeholders would benefit.

**Level of interest:** An article of importance in its field

**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 have no competing interests