Reviewer’s report

Title: Prediction of symptomatic improvement after exposure-based treatment for irritable bowel syndrome.

Version: 1 Date: 19 July 2013

Reviewer: Wijnand Laan

Reviewer’s report:

Major Compulsory Revisions

1. The following line in the introduction (?), page 5 is not clear to me:
As almost no studies of psychological treatment of IBS have used the presence of psychological distress as inclusion criteria<....>. This has allowed for several studies where measures of psychological distress have been used as predictors of outcome after psychological treatment.
What are the authors trying to say, what has allowed for several studies?

2. The various ‘Measures’ sections on pages 9 to 12 are too long. Please list them and add a reference for the readers who are interested in the individual Chronbach alphas and possible scores but do not add all this information to the body of the text.

3. A lot of participants suffer from co-morbid psychiatric disorders, approximately 71% ((15+39)/76=71%) of the participants suffered from a serious psychiatric disorder. Given this information, how valid are the IBS diagnosis? Where they made using Rome–III diagnostic criteria? Without proper diagnostic criteria I fear there might be many ‘false positive’ patients suffering from IBS in the sample.

4. Many papers have yet been written on the subject, see the introduction:
We have found four studies that reported a positive association <....>. Eight studies have reported the reverse association<....>. Finally, three studies reported no association <....>.
The authors even state:
Thus, the results in this study largely follow the same pattern of previous prediction studies in the IBS field, with no variable appearing as a clear and consistent predictor of treatment outcome.
The question arises therefore what this study adds the literature, why should it be published?

5. In the conclusion the authors state that:
This is information of high clinical relevance as it supports the use of ICBT for a wide range of IBS patients.
I think this is an overstatement, also given point 6.

6. The authors did not write a review article but nevertheless reference 78
articles. I think this is too much for an original research article.

**Minor Essential Revisions**

1. The data was collected in a previously published RCT. Please add the Trial Register number in the method sections so readers can quickly assess the aim and methods as originally formulated.
2. In the ‘Participants’ section: it is not clear why only 93% of the original study subjects were included.
3. Have the paper read by a native English speaker. The paper is not always easy to read.

**Discretionary Revisions**

1. Please add line numbers when submitting an article, this makes it easier for a reviewer to point to a phrase that is not clear.
2. The ‘Introduction’ heading is missing.

**Level of interest:** An article of limited interest

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests