Reviewer's report

Title: Effectiveness and Cost-Effectiveness of Internet-based Interventions for Children, Youth, and Young Adults with Anxiety and/or Depression: A Systematic Review and Meta-Analysis

Version: 3  Date: 17 March 2014

Reviewer: Caroline Donovan

Reviewer's report:

Review for BMC Health Services Research

Title: Effectiveness and cost-effectiveness of internet-based interventions for children, youth and young adults with anxiety and/or depression: A systematic review and meta-analysis

This paper was relatively well-written and addressed an important need in the literature. Indeed there have been few systematic reviews or meta-analyses conducted in this area. There are a number of concerns I have about this paper however, that I will outline below.

Major Compulsory Revisions

1. Although I understand the word count concerns inherent to writing any manuscript, it seems unusual that earlier reviews of this literature cited were not discussed in any detail as to their findings.

2. I question the sensibility of excluding CD-ROM based interventions from the review. Although they are not ‘internet-based’, they are ‘computer-based’. By excluding these studies, the number of studies was so very small for each disorder, that it was not really worth doing a ‘meta-analysis’. Also, both Camp-Cope-a Lot (Kendall et al) and Cool Teens (Wuthrich et al., 2012) are two of the major contributors to the computer-based therapy literature (as well as BRAVE-ONLINE which IS internet-based and is included in the review). It seems very strange not to include these major contributions just because they are not technically ‘internet-based’. I also wonder why the Spence et al (2006) study conducted on BRAVE-ONLINE was not included.

3. On page 9 it was not clear (in the first paragraph) what the last sentence was about. Was this conducted in the study under review or in previous studies? It was not clear which.

4. The authors claim on page 9 that only one study followed participants for longer than 12 weeks after the intervention. However, this is not true. March et al followed up participants at 6 months and Spence et al followed up participants at 12 months.

5. I question the utility of even talking about cost-effectiveness given that there
were no studies conducted on this issue. I can understand it being placed in the discussion as a suggested area for future research, but I fail to see the point of (particularly) placing it in the title and discussing it at length elsewhere in the manuscript.

6. On page 10 in the first paragraph, the authors put forward four explanations for the effects of internet interventions. However, there is no rationale for these four listed explanations and no references to back them up.

7. There was no discussion of the fact that, in at LEAST the cases of the BRAVE-ONLINE studies, treatment effects were significantly ENHANCED at follow-up compared to post-treatment. This would seem an important omission.

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

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

**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.