Reviewer’s report

Title: Experiences of guided Internet-based cognitive-behavioural treatment for depression: A qualitative study

Version: 2 Date: 14 May 2011

Reviewer: Karina Lovell

Reviewer’s report:

This is an interesting paper but I am unsure if it significantly adds anything to the current literature on computerised CBT (CCBT). The overall aim of the study was to obtain a detailed understanding of participants’ experience of the treatment that could contribute to improve the effectiveness and reduce dropouts in Internet-based depression treatments.

The authors have argued that there is a paucity of literature on the user experience of internet delivered CBT. The authors failed to mention 3 key papers (Gerhards et al (2011) Improving adherence and effectiveness of computerised cognitive behavioural therapy without support for depression: A qualitative study on patient experiences. Journal of Affective Disorders, 129, 117-125; Kaltenthaler et (2008)The acceptability to patients of computerised cognitive behaviour therapy for depression: a systematic review. Psychological Medicine ; R. Waller and S. Gilbody (2009)Barriers to the uptake of computerized cognitive behavioural therapy: a systematic review of the quantitative and qualitative evidence. Psychological Medicine. Although it is probable that the Gerhard (2011) was published after submission of this paper the other two papers should have been referenced as they have direct relevance to the aim of this study.

The authors have failed to provide a coherent rationale for their sample size of 12. Given that 48 agreed to participate it would have been advantageous to increase the sample size (the authors would still have been able to incorporate purposeful sampling).

The authors state ‘The 36% who declined taking part of the study did not differ systematically from the larger group, and the main reason was lack of time’ – it is unclear what is meant by this – I am assuming that they are saying that they did not differ from those who did agree but the variables that this was tested on would need to be included.

It is also unclear why the total sample were not invited to interview (total sample was 88 only 74 were contacted).

The authors have argued that they used purposive sampling according to the maximum variation strategy using 2 variables (treatment group and overall improvement). The authors state ‘Unfortunately, only one participant whose improvement was graded as “no change” wanted to participate in the study. This latter statement implies that only one participant of 48 reported no change on CGI – this puzzles me as if this is correct- then they either have a biased sample
(ie the respondents did in fact differ from those who did not participate in the study or there were so few people reporting no change then this was an unsuitable variable to use for maximum variation (I did try to check this with the main trial outcome date but the paper does not report the numbers reporting change on the CGI).

No rationale is provided for the analysis used (i.e. thematic and a grounded theory approach). It is particularly important that a rationale for analysis is provided as most readers would want to know why this approach was used in favour of other approaches such as framework analysis. Given the small numbers in each of the 3 categories (Readers, strivers and doers) and the failure to note whether theoretical saturation was achieved it is difficult to have confidence in these categories.

**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 am a co investigator on a large trial comparing CCBT with TAU which has a nested qualitative study within the larger trial - but dont feel that this has influenced my decision