Reviewer’s report

Title: Yoga for schizophrenia: A systematic review and meta-analysis

Version: 5 Date: 15 November 2012

Reviewer: Angus MacBeth

Reviewer’s report:

Major compulsory revisions
I continue to have concern as to whether the reporting of the cognitive function data in meta analytic form is helpful to readers, especially those with only a passing knowledge of meta analysis. It is based on one study and is strictly speaking not a meta analysis, rather it is a reporting of an effect size with a Confidence interval(www-users.york.ac.uk/~mb55/msc/systrev/week6/meta_text.pdf).


I recommend this plot is removed from the results and the authors report within the text that cognitive function was only reported in one study (then include Effect size and confidence interval), thus few conclusions can be drawn regarding effect of yoga on cognitive disorganization symptoms.

Minor Essential Revisions
I also confess that in my earlier review I overlookes and neglected to comment on the treatment of publication bias. The authors state the following: "Risk of bias across studies

If at least 10 studies were included in a meta-analysis, funnel plots were generated using Review Manager 5 software. Publication bias was assessed by visual analysis with roughly symmetrical funnel plots regarded to indicate low risk and asymmetrical funnel plots regarded to indicate high risk of publication bias [40]."

There are not 10 studies in this meta analysis so funnel plots would presumably not have been informative. How did the authors review publications bias? This is pertinent, as we have a meta analysis with few studies, reporting no significant effect of the treatment condition, therefore the file drawer problem/publication bias is relevant here. Could the authors revise this section to clarify there methods here?
Discretionary Revisions

The authors have made efforts to address the main concern raised i.e. the small number of studies used in the meta analysis. They are correct to highlight the high risk of bias in the identified studies. I remain somewhat unconvinced of the robustness of meta analysis on a small number of studies, but I accept that the authors rationale for doing so. I would suggest that the authors highlight somewhere in the limitations section of the discussion that the effect size estimates derived in the meta analysis are also highly unstable due to the small number of studies. These findings still provide a useful impetus for further studies of the efficacy of yoga in schizophrenia.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

I declare that I have no competing interests