Reviewer's report

Title: The effects of psychological interventions on depression and anxiety among Chinese adults with cancer: a meta-analysis of randomized controlled studies

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Reviewer: Michael Schuler

Reviewer's report:

The paper examined the effects of psychological interventions on depression and anxiety among Chinese adults with cancer using meta-analyses. Furthermore, moderator effects were examined. The authors argue persuasively that a lot of studies published in Chinese journals were not included in previous meta-analyses and that there is thus a lack of knowledge about effects of psychological interventions among Chinese adults. The statistical methods used in the study (assessment of heterogeneity, using random effects models and moderator analyses to examine heterogeneity, assessment of publication bias) seem to be appropriate to answer the research question.

The authors found large effects on both depression and anxiety. These high effect sizes are striking, because they are much higher than effects found in other meta-analyses (not constrained to Chinese participants). The authors are aware of this discrepancy and discuss several reasons. However, I have a major and two minor concerns on this topic:

1. The authors examined whether “study quality”, measured on a modified Jadad scale, has a moderating effect on the results. This scale consists of five items, e.g. “appropriate randomization” or “clear description of statistical methods used”. However, in my view, inappropriate randomizations may have a much stronger effect on the results than a less clear description of the statistical methods used. The authors state that 79% of the studies “had the high bias of inappropriate methods of randomization” (page 17, line 20-21). Therefore, I would recommend to do another moderator analysis, using just the item “appropriate randomization yes/no” as a moderator variable.

2. The authors should distinguish more clearly between factors that may have inflated the effect sizes in their study (e.g., not including unpublished work) and factors that might explain why the effects of psychological interventions may “really” be higher among Chinese patients as compared to patients in other countries (e.g., that the interventions were targeting both patients and family members. The former are limitations of the presented study, the latter are factors that may shed light on important differences between Chinese patients/ Chinese health system and the patients/ health systems in other countries.

3. Why should the Self-rating Depression Scale and the Self-rating Anxiety Scale indicate higher effects in depression and anxiety than other well established measurements like the Beck Depression Scale (see page 19, lines 6-11)?
Some other minor comments:
4 On page 5, line 12, the authors cite effect sizes by Faller et al. 2013. However, the cited effect sizes are not correct. The correct effect sizes are the ones cited on page 18, lines 18-20.
5 Page 17, lines 11-12: “P=0.000” would indicate an impossible result. Please correct to “p<0.001”

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

**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