Reviewer's report

Title: A Systematic Review of Evidence-based Treatment for Depersonalization-derealization Disorder (DPRD)

Version: 2 Date: 1 July 2013

Reviewer: Jürgen Barth

Reviewer's report:

The MS summarises the evidence base for the treatment of Depersonalisation-Derealisation Disorder (DPRD) from 1980 until now. The research question is very focused on the efficacy of any type of intervention for the treatment of DPRD patients. Such a focused approach has the advantage that the research question seems initially quite clear, however the variety of intervention strategies and heterogeneity in outcome measures does give rather a blurred picture at the end.

The reviewer had in some parts problems to follow the arguments and the logic of this efficacy paper. Some parts have just a weak connection to this topic or are unrelated. A rewriting with a stronger focus to the research question would be of help.

Major comments

In the search strategy does not include the term “psychotherapy” or “psychological intervention”. Nevertheless this was one of the aims of the study. “Treatment” as search term might not cover such types of interventions adequately.

It is an excellent idea to avoid a pooling of data in meta-analysis. The authors report in a narrative review the findings of the single studies. However, a critical appraisal is missing for each study. The general discussion at the end of the MS is not clearly related to the findings. A closer look at table 2 shows that the presented results does not match with the description in the text. In the text the authors report that each trial is reported as low in outcome assessment. In the table the opposite is the case. This issue should be clarified before the appropriateness of the interpretation of the general findings can be done.

The authors report only about limitations of the psychotherapy study. However, the limitations section should be related the study of the authors itself – not to one single primary study.

The conclusions are not related to the findings. Why is psychobiological research indicated? Why should one study etiology of DPRD after these results? This comment is also connected to the research aim. The interest in psychobiology is a weak argument for conducting this study.

The authors state a high prevalence of DPRD which is disputable given a
reference of 1970 as argument for the clinical importance of the topic.

The reviewer has strong concerns to recommend mindfulness interventions in this group. Is this a random choice or is there any evidence for this recommendation. From a clinical perspective DPRD is rather a contraindication for MBSR.

The flow chart of study inclusion is inconclusive. How can the authors decide, whether an appropriate outcome was assessed from the abstract? It is unusual to mix categories within one global “exclusion rating”. What is the difference between “or” and “and” in the fulltext screening.

Minor comments
The use of PRISMA might help for a stronger focus of the paper. The authors should be encouraged to state more precisely, whether they had been involved in trials on the study aim. The reviewer is not clear, whether the assessment of performance bias is adequate. The assessor does not necessarily have to be blind. It is the treatment provider (performance). Assessment is covered by the next item. What is the difference between “BDI” and “BDI scores” (p.15). Content of table 1 should be improved by more relevant information (Setting, inclusion criteria, exclusion etc.). The authors do not want to run a meta-analysis which is reasonable, however the narrative description of the studies can be improved to make it accessible for readers.

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 interest