Reviewer's report

**Title:** Cognitive deficits in schizophrenia: an updated meta-analysis of the scientific evidence

**Version:** 2 **Date:** 15 December 2011

**Reviewer:** Jenny McCleery

**Reviewer's report:**

The authors have addressed many of the comments. The large amount of work which has gone into the table showing the outcomes contributed by each study is an excellent addition.

I concentrate here on two continuing areas of concern: the search strategy and quality assessment.

1. The first is my comment that the search strategy was not adequate for the (implied) objective of conducting a comprehensive systematic review of all cognitive domains in all patients with schizophrenia. The authors dismiss this concern as a 'personal opinion.' In fact, systematic searching has a well-established set of techniques which can be tested empirically. My concern about the search strategy was such that I consulted a search specialist in this field while preparing my review of the paper – this view confirmed that the search terms were not adequate to retrieve all relevant literature. Replacing the authors' specific search terms with broader terms typically used in such searches (e.g. ((control* adj2 study) or "case control") instead of "controlled study") readily identifies additional papers – this was tested. The authors have indeed found a large number of papers using their search strategy, but this does not guarantee that the search is comprehensive. The mention of the possibility of publication bias is welcome. Reviewer JH's suggestion of funnel plots was a good one and would have been helpful to quantify this likely source of bias.

2. The second area is about quality assessment which, obviously, is always about ensuring the methodological quality of the results. The authors dispute this comment, arguing that a large part of the paper and discussion is concerned with quality. I perhaps was not specific enough in this comment. They do indeed discuss at length the overall quality of the evidence, at least as regards the risk of confounding as a result of baseline imbalances between groups and the impact of this on the estimates of effect size. However, quality of individual studies is not assessed as a source of between-study heterogeneity. It is not possible to tell if there is a subset of high quality, less confounded studies in which appropriate matching of cases and controls was done and in whose results more reliance could be placed. There are other important quality items in studies of this nature which are not mentioned and which could also be very significant contributors to between-study heterogeneity; obvious ones are sampling method, blinding, complete/incomplete outcome reporting. The authors have named assessment of
methodological quality as one of the objectives of the review and I find it impossible to say that this has been achieved.

The authors are keen to have fact separated from opinion; the following is an opinion. Where there is a large literature of generally poor quality, then one of the most useful purposes of a systematic review is to be discriminating on behalf of the reader – to identify what high quality studies exist and use them to produce the best estimates of effect size. Without this step the review is of limited utility. To conduct a proper quality assessment of this number of studies would be a huge undertaking, but when one undertakes a review of such a huge area of literature, it is hard to see how this can be avoided.

Conclusion: In both of the above areas, the limitations of the study are not clearly stated. I would consider this should be addressed before publication – particularly as regards the search. Given the limitations of the study, I consider it to be of limited interest.

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

**Declaration of competing interests:**

I declare that I have no competing interests.