Reviewer’s report

**Title:** The Beijing Version of the Montreal Cognitive Assessment as a Brief Screening Tool for Mild Cognitive Impairment: A Community-based Study

**Version:** 2  **Date:** 15 June 2012

**Reviewer:** Edmond Teng

**Reviewer’s report:**

In this revised manuscript, the authors have addressed many of the more critical issues raised by myself and the other reviewer regarding the original version of the manuscript. My few remaining (and relatively minor) concerns are detailed below:

**Major Compulsory Revisions:**

None.

**Minor Essential Revisions:**

1. In the Methods (p. 7), the authors note that “Arabic numbers” were used for the auditory vigilance task. Since presumably those numbers were heard, as opposed to read, but study participants, it would seem that the “Arabic” descriptor is unnecessary and confusing.

2. In “Procedures and Participants,” reference is made to other (unspecified) neuropsychological instruments (p. 8). Did the results of these instruments affect group assignment? If so, which tests and how they affected group assignment should be included. If not, the reference to other neuropsychological testing could probably be eliminated. Additionally, the correct name for the GDS is the “Global Deterioration Scale,” not the “Global Degenerative Scale” (p. 8).

3. The word “District” is spelled incorrectly multiple times in the “Data Collection” section of the Methods (p. 7).

**Discretionary Revisions:**

1. In the first paragraph of the Background (p. 4), the authors assert that “diagnosis and monitoring of MCI could be an effective way for early detection and decelerating the progress of dementia.” I am not sure that the case can be made yet that the identification of MCI leads to “deceleration” of progress to dementia at this point, especially in the absence of any proven therapeutic interventions.

2. In the first paragraph of the Background (p. 4), the authors also state that the MMSE is “relatively insensitive to MCI.” The data presented in this manuscript suggests that the MoCA-BJ, in their hands, is essentially equivalent to the MMSE for distinguishing MCI from normal cognition. Therefore, it is arguable that that
the statement in the Conclusion (p. 18), that the MoCA-BJ is an “acceptable” screening tool, still overstates its utility (even though the authors have already watered down this statement relative to the original version of the manuscript). Since the MMSE can be administered more quickly and easily than the MoCA, one view would be that, in this population, it might be the superior screening tool.

3. As stated in my comments on the original version of the manuscript, the MoCA was designed to distinguish cognitively impaired elderly (either MCI or dementia) from cognitively normal elderly— and the original test was never designed to discriminate MCI from dementia. The authors state in their response that the focus of the current manuscript is to detect MCI, and their inclusion of a small dementia group does not seem to add much additional insight. Given the small size of the dementia group, perhaps it could be eliminated from the manuscript altogether without affecting its overall impact.

4. The Results may read more logically if the global MoCA-BJ data, which are the primary results mentioned in the Abstract, are presented prior to subanalyses of individual cognitive domains of the MoCA-BJ.

5. The “Applicability if the MoCA-BJ in Different Regions of Beijing” section in the Results (p. 12) could be shortened significantly by referring readers to the tables (i.e. not recapitulating the findings in the text) and simply reporting that after statistical adjustments for demographic differences, there were no regional effects. Furthermore, the inclusion of MoCA and MMSE differences between regions in Table 2 seems a little misleading given the very different demographic compositions of the regions. I appreciate that the authors explored the issue of using more demographically sensitive cut-points and found no improvement in the utility of the MoCA-BJ for predicting group identity. Given the strong demographic effects on MoCA scores however, explicitly mentioning this finding in the Discussion would help readers interpret the overall results of this manuscript.

6. In the Discussion (p. 17), the authors distinguish their study from the Lu et al study by stating that they used more stringent criteria— beyond just the CDR. How do the current study’s results change if the group assignments are based simply on CDR? Would the prevalence of MCI and/or discriminative abilities of the MoCA-BJ change?

**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 declare that I have no competing interests.