Reviewer’s report

Title: The accuracy of the MMSE in detecting cognitive impairment when administered by general practitioners

Version: 2 Date: 7 March 2008

Reviewer: Hein van Hout

Reviewer’s report:

Dear editor and authors,

Most of my comments were discussed satisfactory.
However, I strongly feel that the authors unjustly disagreed with two methodological issues I made. These issues strongly influence the interpretation of the results.

Maybe I have not explained my points clearly enough in the first review. My concerns are based on official evaluation criteria for diagnostic studies (STARD). The authors may want to check the official STARD criteria of the consort statement on:

As you will understand points 5 and 11 remain insufficiently addressed in this revised paper.

Point 5 of the STARD statement concerns the sampling procedure. The GPs in the study purposefully sampled patients they suspected or diagnosed already. The problem that arises here is that we do not know the magnitude of missed patients. In my earlier review I referred to this point as verification bias. This a major methodological limitation which was not discussed at all and has consequences for the main interpretation. For example the abstract's conclusion could be rewritten to:
'the MMSE could be used in general medicine for the early detection of cognitive impairment IN SUSPECTED PATIENTS'. Consequently the interpretation in the discussion part should be rearranged to take this point into account.

To get a feel of the 'missed portion' one could think further analyses i.e. compare the portion the GPs detected with official prevalence statistics. Also it is possible to compare detection portions across the GPs. Likely some will do 'better' than others. In addition, if measured, the detected portion could be related to GPs characteristics.

Point 11 concerns of STARD concerns the blindness or independency of index and referende test. However, it is bizarre to evaluate the UVA derived MMSE score, which completely unblinded, to the UVA's own clinical diagnosis.
Obviously their own MMSE scores perform better than those of the GPs. Moreover, in my opinion this part of the paper does not contribute to the objectives of the study at all and should be omitted. Despite being problematic, a reader may accept the limited influence of GPs derived MMSE scores on the UVA’s clinical diagnosis. So this part could be preserved.

I hope I explained my concerns in a better way, and I hope you can improve your paper with it.

regards,
Hein van Hout

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

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

Quality of written English: Acceptable

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’