Reviewer's report

Title: Psychometric properties of the Flemish translation of the NEECHAM Confusion Scale

Version: 1 Date: 3 January 2005

Reviewer: Elisabeth Hamrin

Reviewer's report:

GENERAL
First I like to anwer the questions given to the reviewers No 1-7
1) IS THE QUESTION POSTED BY THE AUTHORS NEW AND WELL DEFINED?
The research question is clear, to determine the reliability, validity and diagnostic value of the
Flemish edition of the NEECHAM confusion Scale. Missing in the Background is an international
accepted description of the concept "delirium"
such as the ICD-10 of WHO (see F05, page 316).

2) ARE THE METHODS APPROPRIATE AND WELL DESCRIBED, AND ARE SUFFICIENT
DETAILS PROVIDED TO REPLICATE THE WORK AND 3) ARE THE DATA SOUND AND WELL
CONTROLLED?
The problem with the paper is that the sample covers only 54 patients after hip fracture but the data
referred to are observations at five occasions which includes totally 194 observations. The 54
patients are part of an intervention group in a main study (Milisen et al 2001, ref. 29). It must be
stated how many of the 54 patients had complete investigations filled in with the NEECHAM Scale
on occasions 1-5. Since MMSE and CAM were administered on the same measurements points - it
must be stated how the comparisons were done evaluating concurrent validity. It would have been
preferable to present the results for reliability and validity on the different test occasions instead of
"adding" the observations - even with this small sample. The interesting thing is, however, that in the
principal component analysis, the results from the study by Milisen et al (this study) is very similar to
the principal component analysis by Neelon et al 1996 (ref 21).

4) RELEVANT STANDARDS FOR REPORTING AND DATA DISPOSITION
I suggest minor essential revisions according to the following:
TABLE 1: It's well done, but the final amount of patients in the study by Johansson et al 2002 was
n=73
TABLE 2: May be there could be a heading for % (percent) instead of writing for every value
TABLE 3: Add in the table head 54 patients, 194 test occasions
TABLE 4: Suggest instead of "Alpha if item deleted" to write "Total alpha if item vital is deleated"
TABLE 5: Suggest that the level of significance (p-values) is marked for the different coefficients
TABLE 6: Preferably to add also 54 patients, 194 test occasions in the table head
TABLE 7: Very interesting table - but there are according to me 2 cutpoints - 24 and 27 which could
be taken up in the discussion - and in a way follow Neelon et al's recommendations.
FIGURE 1: The ROC-curve - may be a reference. The expression BI should be explained

5. ARE THE DISCUSSION AND CONCLUSIONS WELL BALANCED AND SUPPORTED BY DATA
It should be stated in the discussion when comparing the data by Neelon et al from 1996, that her
samples were 168 and 258 patients respectively and not based on repeated measurements. The
suggestion to delete the physiological items 'vital' and 'oxygen' seems very wrong according to the
theoretical framwork for the scale created by Neelon & Champagne which builds on behavioral
psycho-physiology especially for elderly with some trauma. The discussion and conclusions are
based on the data presented. But if the authors decide to report also on the data when a comparison
between the instruments are done at each of the five test occasions, the scientific value of the article
would improve.

6. IF THE TITLE AND ABSTRACT ACCURATELY CONVEY WHAT HAS BEEN FOUND?
Also in the abstract it should be stated that the result are based on repeated measurements. Both in the abstract and the discussion (conclusion) further validation is suggested in diverse clinical populations. This reviewer would not recommend that the methodology used in this study by "adding" observations should be used.

7. ACCEPTABLE WRITING. The language used in the manuscript seems satisfying (although this reviewer is not native English). I understand that no native English person was used for the back-translation from Flemish to English, which would be preferable. May be that could be done for further studies.

IN SUMMARY. This reviewer is aware of the experience of the first author (K. Milisen) and co-authors within the field of instruments for delirium (through other publications). The dilemma is the methodology in this manuscript the way the studied sample has been used. My MAJOR compulsory revision concerns points No 2, 3 and 5 before deciding on acceptance or rejection. It concerns the need to get report from additional analyses where we also get some results on what happens at the different test occasions comparing NEECHAM SCORE with the different other instruments as well as diagnostic values.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
See under Points 2, 3 and 5 ABOVE and Summary and will include a new abstract.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Is detailed described in Point 4 ABOVE.

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Discretionary Revisions (which the author can choose to ignore)

List of References has followed the recommended style by BMC Nursing Editorial with the exception that abbreviations of Scientific Journals have not been used.

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

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:

I declare that I have no competing interests. I am myself co-author of the paper by Johansson et al 2002 and responsible with the first author for the Swedish edition of the NEECHAM SCORE. I have continuous contact with prof Virginia Neelon. I am principally very happy to know about the work done using the NEECHAM Scale internationally.