Reviewer's report

Title: Development of Abbreviated Measures to Assess Patient Trust in a Physician, the Medical Profession, and Health Insurers

Version: 1 Date: 15 April 2005

Reviewer: Robert K McKinley

Reviewer's report:

General
This is an interesting paper which may well make an important contribution to research metrics.

The authors have set out to and have probably succeeded in developing abbreviated trust in physician, medical profession and insurer scales. Although the research is situated in the USA, the physician and profession trust scales are likely to be of interest and of use to researchers internationally, the trust in insurer scale is less likely to be so.

Their basic research methodology of obtaining two large samples, one local and one national identified by random telephone dialling, is sound within its own constraints. The interview schedules seem to have been carefully constructed and applied gathering data on demographics, trust and additional variables designed to test validity of the eventual scales. The overall approach taken to scale reduction is logical in that items with greatest theoretical contribution were considered first then their item response characteristics examined and finally their factor loadings considered.

The authors produce impressive data on internal consistency of the scales both globally and within demographic sub-groups, test-retest reliability when data was available and construct and congruent validity.

The authors have identified, acknowledged and appropriately discussed a number of limitations. These are that the data were gathered from English speaking people with telephones which they answer and the relative paucity of older people in their sample. Both issues limit generalisability of the conclusions.

My biggest concern with this paper is the analysis and my uncertainty of how exactly this was performed as there are some omissions and apparent duplications in the 'statistical approach'.

The description in the first paragraph of this section is clear describing selection of items and testing their item response characteristics in the full sample (which we must assume is the local and national samples combined ) and then by sub-group (by which we must assume they mean demographic sub-groups).

The second paragraph is clear.

The third paragraph is less so. It appears that scale scores were calculated for each sub-group and their correlations with sub-group scale scores for the original longer scales examined. They then state that 'items with low feasibility were dropped' which seems to duplicate a previous step. I could not follow the description in the last part of this paragraph.

Notwithstanding these issues, the preferred way to undertake this work is to split the dataset into two subsets, develop the scales with one subset and then test the scales identified using the
‘independent’ data in the other subset. This appears not to have been done and, if not, significantly weakens the conclusions. My reason for reaching this conclusion about the design is that the authors state that they used the ‘full sample’ in the first paragraph of the statistical approach. If this is indeed the case, the authors need to discuss the impact that this decision has made on the security of their conclusions. If it is not the case and they have employed a ‘split design’, this should be made clear in the ‘statistical approach’ section.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

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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)

· Clarify the issues in the ‘Statistical approach’ section identified above
· If a split design was not employed, discuss the reasons for not doing so and the impact on confidence in the scales
· The authors did not present any data from the binary response variables (dispute, change of physician, choice, etc.) and it would be preferable that they did.

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Discretionary Revisions (which the author can choose to ignore)

· Data from 5% and 13% of interviewees from the national and local sample were not utilised. It would be useful if the authors outlined why this was so.
· The authors have given no indication of the nature of the questions used to collect the construct validation data (e.g. satisfaction with the doctor, adherence, recommendation) and it would be useful if these were supplied as an appendix.

What next?: Accept after minor essential 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