Reviewer's report

Title: Deriving Utility Scores for Co-Morbid Conditions: A Test of the Multiplicative Model for Combining Individual Condition Scores

Version: Date: 28 June 2006

Reviewer: Kim Moesgaard Iburg

Reviewer's report:

General

1) A very interesting paper on an important topic that recently has been given attention in PHM and this paper follows to some extent up on the other papers. The Canadian survey data and the HUI-3 is a solid background for the paper.

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

2) Considering that the theoretical range of HUI-3 is between -0.36 and 1 (full health) the average HUI-3 scores on all conditions are relatively high in table 1, lowest observed are multiple sclerosis (0.69), effect of stroke (0.69) and Alzheimer/other dementia (0.45). For two comorbid conditions 90% were over 0.80 in HUI-3 score. The authors comment that reported conditions indeed are mild. Is there a ceiling effect in the survey - or possible selection bias of survey respondents - and has there been made an analysis of non-respondents in the survey used? Please comment and report this.

3) The basis for the purification of the HUI-3 observed score is the fact that people reporting no chronic conditions only gives an average score of 0.94 (1.0 was expected in theory). The authors say in the method and discussion sections that this may be due to conditions other than those surveyed, like influenza, or a general state of health associated with ageing and not with any specific conditions. I am not sure that I understand why you want to purify this - and not either why you want to standardize differences away that reflect the actual observed utilities among people, whether young or old, man or woman? Need more argumentation in the text for the purification and standardization. I would like to see the age and sex pattern of the observed HUI-3 score on single and comorbid conditions.

4) The 0.94 observed maximum score is not surprising, however it may be possible to define a "healthier" sub-sample of respondents instead of only using the criteria for chronic conditions yes/no. Suggest to define a sub-sample of respondents having no diseases currently or previously, no chronic conditions, and no symptoms, pain or complaints reported (of course depending on the availability of indicators in the concrete questionnaire).

5) The description of the computation of the synergy coefficient, s, for capturing additional interaction between conditions, is not easy to follow on p5. The synergy coefficient is however found to be of a value of 0.99, both when replicated for another sample and for two and three comorbid conditions, and therefore has no real consequence, and a straightforward multiplicative approach can safely be taken without it. What does this tells us about the previously stated argument about having a "synergy" effect of comorbid conditions? Please comment in the discussion section.

6) The authors quote Mathers et al's paper on comorbidity (PHM 2006), however, without commenting this paper's alternative approach of using dependent comorbidity adjustments of HALE rather than the usual straightforward multiplicative model assuming independence between conditions. The effect of this dependent comorbidity was a reduction in total PYLD across all countries by age and sex ranging from a few percent in younger adults to 8% in the oldest age groups in developed countries and up to 15% in the least developed countries.

7) The prevalence of self-reported conditions is speculated to be under-estimated compared to clinician-reported conditions. No reference to this is given. What is the evidence for this in the literature?
Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

8) Please correct numbering of pages. OK from p1-p6, but then numbering starts again from p2 through p23.

9) Page 19 seems to be a fraction of something else, showing equations on sample weights and age-sex standardization. Is something missing in the uploading - page 18 has probably fallen out?

Discretionary Revisions (which the author can choose to ignore)

10) It is rather unusual to see the person who wrote the introduction, discussion and conclusion - and prepared the final version of the paper to be the last author (no?)

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: Yes, and I have assessed the statistics in my report.