Reviewer’s report

Title: Functional health state description and valuation by people aged 65 and over; a pilot study

Version: 1 Date: 20 Jan 2017

Reviewer: Paul Kind

Reviewer's report:

My thanks to the authors for taking previous comments into account and I appreciate that they have made some efforts to respond to them. However, there remain some critical elements that still need attention.

1. Distinguishing QoL HrQoL and capabilities

It would be really much better if the introductory section of the manuscript avoided reference to what is/isn't meant by capabilities, HrQoL or anything else. You would be strictly speaking rather more accurate if you acknowledged that there is some uncertainty about how to identify/describe/value the benefits of health/social care interventions in the elderly - especially when it comes to economic evaluation. The opening statement is fine in that respect. Leave out the window-dressing though.

Lines 77 - 83 remain entirely opaque. I simply do not understand the confused text here - even after several readings. Please imagine that you are describing this for the benefit of one of the subjects in your feasibility study and try again.

Lines 101 - 104 belong in the discussion section. They are a statement of belief that lacks substance.

Please check statement in Lines 107 - 109.

First we aim to study the feasibility of allowing elderly, from South-Africa and the Netherlands, to use both questionnaires to report on their own health and QoL. Ultimately
to provide appropriate information on quality of life for use in a future study among elderly.

You are not "allowing" you are administering questionnaires

This is a feasibility study - it does not provide definitive information on QoL, it is reporting on methods.

Line 110 - 112

Secondly to determine whether of the two methods to elucidate health state valuations, i.e., using a visual analogue scale (VAS) or applying the Time Trade Of method (TTO), would be appropriate for the elderly.

WHICH of two methods …

2. EQ-5D+C

EQ-5D+C is not as variously described in the manuscript

* a standard utility instrument

* a well-known and validated questionnaire

Firstly, EQ-5D can be represented in several formats - descriptively in terms of its individual dimensions or as a summary index. In this latter format responses to the EQ-5D components are scored using one of several different methods. ONE approach is to use utility weights and this type of scoring is primarily intended to provide a quality-adjustment factor when computing QALYs. It has absolutely nothing to do with HrQoL in any other context and certainly not when it comes to measuring patient self-reported health status. I must insist that this "utility" definition is corrected.

Secondly, if EQ-5D+C is indeed a "validated" questionnaire then please provide some/any evidence to support this description. Furthermore, please note that there is at least one other published paper in HQLO relating to EQ-5D+C which was compared against EQ-5D in elderly
people. This paper concluded both the EQ-5D and the EQ-5D+C were responsive to changes in the MMSE, with the EQ-5D performing slightly better.

Lastly, the nomenclature EQ-5D+C is mostly likely an invented one as there is absolutely no mention of it on the EQ-5D website (www.euroqol.org). I strongly urge that you contact them to ask for advice on this matter.

EQ-5D is probably better described as a generic measure of health-related quality of life with the capacity of being represented as a single index score. BTW it should be noted that the 0-100 VAS rating of self-assessed health status is an integral part of EQ-5D.

Please note also that references 2 and 7 are identical. One should be deleted.

3. CAF

Whilst appreciating that this is effectively a work in progress and presumably part of PhD development I find it very concerning that it should have a pivotal role in a study (even a feasibility study) without apparent documentation regarding its provenance.

In particular I am troubled by some aspects of its design, for example, dimension 1 (Attachment) is described as

Feelings of love, affection, companionship and friendship from your partner, family, friends and pets

I find it troubling that all these sources are conflated and even designated as such.

Something needs to be said about development/testing of this questionnaire, if only to refer to other separate work relating to it.

In addition, there must be something in the manuscript relating to the (presumed) translation of the questionnaire from Dutch / Afrikaans / English ??
4. Valuation method(s)

There is a great deal of confusion in the text about the number/selection of health states used.

Line 187 cites the same 10

Line 192 list 9

Line 200 cites 18 (the same as in group #1) - really?

Line 203 we are back to 10 listed states

This is a mess. It might be better handled by providing the information in tabular form.

The so-called TTO procedure given in the Appendix is non-standard and you will need to find suitable references to justify its selection - how/why did you choose it?

TTO normally involves an interview situation in which respondents iterate through choices before reaching an indifference point. This departure from conventional TTO must be clarified for the reader.

How were health states selected for valuation study?

Did you choose any that were already published in the literature - if not, then you missed a chance to say something about how robust your results are in fact.

More critically, the SA study included a form of TTO whereas this was dropped from the Dutch follow-up. This is an absolutely critical part of your research material and MUST be properly accounted for in the manuscript. Respondents did not like TTO (or preferred VAS) so we excluded it from the 2nd study is not acceptable. Others need to be able to learn from your experience. You need to explain this choice process. You may reject this criticism, but if you do then I must insist that you remove all reference to TTO from this paper since it neither contributes any useful information nor are any results properly reported.
5. Results

Mean VAS scores are presented as decimals when in fact they come from a 0-100 rating scale. The reported numbers therefore should be in this range too. There is no reason whatsoever for this "rescaling" since no TTO data are presented in this paper.

Frequencies are presented as %. The numbers are small and should be shown as such - 6 people may be 60% but you can safely leave this to the reader I think

Is there an error in Table 3 for the SA value for 55555 (value given is 0.06) which is WAY out compared to everything else

You might wish to consider a Friedman test (non-parametric) on the ranking of CAF state values across the samples.

You choose to present health state values in graphical form but you might want to state how the rank order of states in Figure 1 was determined (sorted by one subgroup probably) and that the Dutch semi group displays a single marked divergence from the other subgroups.

6. Spelling

A couple of spelling errors - notably SATES instead of STATES.

Please check more carefully

All in all, some further (minor) revisions are needed
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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