Author's response to reviews

Title: Identifying common impairments in frail and dependent older people: Validation of the COPE assessment for non-specialised health workers in low resource primary health care settings

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Dear Editor,

We would like to thank you very much for considering our manuscript entitled: “Identifying common impairments in frail and dependent older people: Validation of the COPE assessment for non-specialized health workers in low-resource primary health care settings ”, for publication as an original article.

Please see my responses to the reviewer’s comments in detail below. I will certainly happy to give more clarity if necessary.

First reviewer ( Dr. Richard Biram):

On behalf of all authors, I would like to thank you for your time and valuable comments, which certainly improved the quality of the manuscript. I have listed my response to specific comments below.

Comment 1: I did not identify a section describing any ethical considerations which were undertaken prior to the undertaking of the study, and a note to the authors would be to ensure that the adherence to any local ethical guidelines for the undertaking of the study is described.

My response: Thanks for pointing out this. I assure that we followed ethics protocol in accordance with local and UK ethics committees’ recommendation. I have added a separate paragraph in the revised manuscript submitted (see lines 137 to 142) with details of the ethical consideration that was carefully dealt before commencing the study.

Comment 2: I would recommend the authors review the use of the word ‘secular’ (line 88) – is this the correct term? It does not seem to make sense in the context of the rest of the sentence and I suspect it may be an autocorrect error.

My response: Thanks for the suggestion. I had amended this in the revised manuscript (please see line 88).

Comments 3: If I could make a further minor suggestion to the authors, as a matter of personal taste. Throughout the document, there are used interchangeably the terms COPE assessment or CHW assessment, and/or clinician assessment or Easy-care assessment. At times, this can make it quite difficult to understand which group is being described, as the terminology is prone to change from one sentence to the next. It would be useful to the reader to try to keep to one format or the other when describing the outcomes from each group for ease of understanding.
My response: We thank you for this suggestion. I have managed to address this issue of inconsistency in the revised manuscript. It would be appropriate to use COPE assessment instead of CHW and doctors’ assessment instead of Easy-Care assessment except in the circumstance where Easy-Care independence scale was compared with COPE. Please see the changes in the following lines (301,305,306,308,310,312,368,378,465,470,476).

Second reviewer (Dr. Rhian Simpson):

We felt the second reviewer’s comments were very useful. Please see my response below.

Comment 1: Question well defined. The question was not particularly well defined and I had to read the paper several times for clarification. It would be clearer if the focus is on the COPE tool itself rather than the process around the tool. For example in section 119 to 121 it says the aim is to develop tool but then says feasibility and acceptability of this approach.

My response: This is a pragmatic study; the COPE (geriatric assessment) was tested in the real world within government health system, using existing primary health care professionals. Testing the just the COPE tool alone is not our aim, in fact, individual measurements in COPE tool had been tested in LAMICs (we mentioned this clearly mentioned in the method section with references). The question is whether existing public health workers can use COPE in routine practice. These workers are already burden with heavy workload (includes vaccination, maternal health, HIV, TB care etc), and older people in need of care are not prioritised in their day-to-day work: due to lack to knowledge and training in assessment. Therefore, it was not clear whether it is feasible to perform geriatric assessment as part of routine primary care practise. And whether these workers will value COPE assessment was also doubtful. This has been augmented in the background section and implications of the work for practice was reiterated in the discussion. In this given context, your suggestion to twist the paper on COPE tool makes us feel we are not answering the real world question that can change the practice and policy.

Comment 2: Sample: selection bias as CHW were asked to choose those that they knew from their practice who were already frail. It does not say how many the 159 were chosen from or why this number was chosen. If you were to use the tool in a more general population would it be as good at measuring impairments? This limitation is not really addressed in the paper.

My response: Routine screening for care needs of older people do not exist in India or any other LAMICs. Most screening programmes is opportunistic and based on people’s concern (service provider and seeker). As reviewers suggested, using this tool in a more general population would be good but currently there are no policies to encourage public health workers – we are keen to bring change in the routine care of primary health care system hence this study was a demonstration that primary health care workers can effectively perform the assessment for older people. We have performed sample size calculation to achieve this n=159. Details of this has been mentioned in the reference we cited; also there is another subsequent paper which was submitted to BMC geriatrics. This has more information on the
sample size calculation and rational for opportunistic screening. Unfortunately, this paper was rejected now and we hope to submit elsewhere and cite COPE assessment paper.

Comment 3: There is no detail re how the participants were approached or consented.
My response: I have added a separate paragraph in the revised manuscript that explains ethics and consent process. Please see the second paragraph of the method section.

Comment 4: Training of CHW was well described, but there was no discussion around the reliability of the COPE as a test including inter-rater reliability.

My response: As we described in the introduction and method section, the COPE assessment was developed to guide health workers in organising the care. Testing the reliability was not the objective of this pragmatic study.

Comment 5: The biggest problem with the study for me is the clinician assessment using EASY Care that was used to validate the COPE. It is acknowledged that this is not a gold standard (123) and in the discussion (458). 124 it states that Drs was not specialist, lacked time for a rigorous assessment. There is nothing describing how these clinicians were chosen, trained and supported re using the tool or its reliability.

My response: We agree, this limitation has been clearly brought in the discussion section. We did not select clinicians; we trained all clinicians working in the primary care facility chosen for this work. The details of how the clinicians were trained will be mentioned in the subsequent publication as stated earlier (see my response to comment 2).

Comment 6: I think the first objective should be that the COPE assessment tool is a valid tool as per title.

My response: As mentioned earlier, the main objective is to test the feasibility of administering COPE assessment and acceptability to primary health care professionals. However, I believe we have provided enough information on the validity of COPE when used in real world resource-poor settings.

Comment 7: There was only moderate agreement between the COPE and clinician judgment and this is discussed 469. It is recognised that without a gold standard 479 it is difficult to interpret the findings. This is the main difficulty I feel when drawing conclusions from the study.

My response: The ideal way to test the validity of this assessment, administered by CHWs, against specialist assessment (geriatricians, physiotherapist, nutritionist, psychiatrist/psychologist). But in India, no such team exist in the public health system at the community level; therefore, this is impossible. Even if we carry out such work by employing a separate specialist team for gold standard assessment, the implications will have little meaning to primary doctors in the public health system.
Comment 8: With any assessment tool the acceptability to the individual being assessed is important and should also maybe be considered for future work.

My response: I strongly agree, this is an ongoing study and next stage of intervention trial following the COPE assessment is well underway. We will certainly investigate service receiver’s perspective in future work.

Comment 9: The conclusion should centre around the validity of the COPE tool.

My response: I assure you, this has been well augmented in the discussion section.

Comment 10: For me the limitations of the study and the uncertainty re the validity of the COPE tool need to be better described if it is to be published.

My response: As stated earlier, testing the validity of the COPE is not our main objective. This study finding has direct implication for primary care practice. To our best knowledge, this is the first study in resource-poor settings; and publishes this work will change primary care policy in India. As we are working closely with member state of WHO, this evidence will be used for instigating programme for older people.

Comment 11: Future work should be addressing some of these limitations e.g. can it be used in a more general population, how to improve the EASY CARE assessment so that there is a greater confidence that it is a true measure of common impairments.

My response: Thank for this suggestion. We have submitted a short report on the validity of EASY care assessment in resource-poor setting to Age and Ageing oxford Journal; this paper is currently under review.

Sincerely,

Corresponding author

Dr. A. T. Jotheeswaran