**Author's response to reviews**

**Title:** Predictors of mortality among elderly people living in a south Indian urban community; a 10/66 Dementia Research Group prospective population-based cohort study

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**Author's response to reviews:** see over
Reviewer 1

We don’t think that the observed difference in waist circumference between those followed up and not followed up, although statistically significant, allows the possibility of much bias. The observed association with waist circumference turned out to be oddly non-linear, with the increased risk concentrated in the second quarter.

Thank you for spotting the two typographical errors, which we have now corrected.

Reviewer 2

Reviewers 1 and 2 disagreed regarding the background and discussion sections. Reviewer 1 described the background as ‘well written, brief, concise, and the objectives well described’ while reviewer 2 felt it was ‘somewhat unclear’ and recommended ‘modifying the section to make it more concise and easier to understand’. Reviewer 1 described the discussion as ‘well written and structured’ while reviewer 2 made the identical observations as for the background.

We have carefully considered reviewer 2’s suggestions for the background and discussion sections, many of which we agree with, others of which we did not, as below

1-1 The point of this introductory sentence was to draw attention to the lack of comprehensive studies of risk factors for mortality among older people in LMIC, since most of the studies that are then cited have addressed only some of these domains. We have now tried to clarify this point, and hope that, with this clarification, the relevance of the first sentence becomes apparent?

1-2 The reviewer is of course quite correct. South Korea is a high income country, but it was until relatively recently a low income country, and this was specified as a socioeconomically deprived catchment area. We have now edited to clarify.

1-3 We disagree with this point. Much of the paper is taken up with the observation that many risk factors consistently observed for younger people seem not to be replicated for older people, in our as well as in others’ studies. The difference in findings between developed countries and LMIC (among older people) is much less striking. It is true that we did not address the issue of effect modification by age directly (all of our participants being aged 65 years or older), but there again neither did we, nor could we examine the issue of LMIC versus developed countries directly.

1-4 Again, I’m afraid we tend to disagree with this suggestion, which seems also to be counter to the recommendation to make the section more concise and easy to understand. Our intention here was clearly to summarise the main issues arising from the review of the relevant literature, before moving on to a description of aims and objectives.

2-1 We have broken this paragraph into two paragraphs, as suggested
2-2 We weren’t clear where or why this would need to be repeated. We do already state as a general limitation, in the first paragraph of the discussion that ‘not all chronic diseases were ascertained with equivalent rigour’. We then go on to discuss the possibility that three self-reported diagnoses in particular - diabetes, stroke and ischaemic heart disease – may have been under-reported and their effects in the multivariate model captured instead through self-reported disability.

3 We do take the point about the conclusion paragraph. Our aim here was to confine ourselves to implications of the findings, and recommended actions with respect to prevention, policy and practice. Following the reviewer’s suggestion, we have now revised this, moving the implications for prevention into the final paragraph of the discussion, and providing a shorter final overarching conclusion.

Other points
4 apologies – we have now inserted these important citations
5 ‘missing values’ we have amended the tables accordingly
6 I think this is done automatically in the PDF conversion?
7 thank you for pointing these out – the problem was caused by a merging of reference manager files, and has now been resolved.

Reviewer 3

We were not fully clear about the nature of the reviewer’s objections to table 1, but have tried to clarify this a little further by changing ‘known’ and ‘not known’ to ‘vital status known’ and ‘vital status not known’.

- We present the participant characteristics by gender (and by assets) in order to understand associations with these important variables and to later analyse confounding and mediation
- We did not only consider demographic and lifestyle variables (this is clear from closer inspection of Table 1)

‘I think that a table presenting the differences between subjects alive and died at follow-up (showing the p-values) could be more readable’
This is a cohort study, and as such, the results are conventionally presented by comparing the outcome (death) between exposed and unexposed rather than comparing the exposure proportions between those that have and have not experienced the outcome.

‘Did you also check that the covariates meet the proportionality assumption for Cox multivariate models?’
We did, and have now included this information in the table 6, and a note to this effect in the text (page 8 line 4 ‘Both models met proportional hazards assumptions, both with respect to the global test (Table 6) and individual covariates). Apologies for omitting this information.

We do not agree with the reviewer’s suggestion that we should check systematically for interaction effects between all main effects included in the model. This would be highly unconventional, and likely to result in type 1 error, and other difficulties in interpretation. A more conservative approach is to base tests for interaction on prior
hypotheses based upon previously observed interactions, or interactions suggested by mechanistic theory.

Regarding the suggestions for additional variables to be studies as possible risk factors for mortality:

Drugs and adherence to drugs – we did not have any data on this

Comorbidity index – We are aware of the reviewer’s interesting paper showing that disability, rather than comorbidity was the stronger independent contributor to mortality. Our approach had been to look at physical, mental and cognitive disorders separately and in multivariate models rather than to use a comorbidity index. We do have such a four point index, based on the number of comorbid conditions (out of stroke, dementia, depression and three or more physical impairments). There was no trend for an association with comorbidity –

hazard ratios
for no conditions 1.00 (reference category)
for any one condition 1.89
two conditions 1.00
three or more conditions 1.44

Sensory impairments – deafness and blindness were included among the list of 11 common physical impairments. We did not look at them, nor at any other of the organ system impairments individually

Health service utilization – we had not looked at this, as any association would have been difficult to interpret regarding direction of causality. The verbal autopsy, as we reported, identified that the majority of those who died had not sought medical attention in their final illness. Therefore, in this setting, being very ill may paradoxically lead to reduced help-seeking, rather than vice versa.

For the reviewer’s information we have now also tested for associations between mortality and visual impairment, hearing impairment, and use of community health services, adjusting for age and gender, and found none.