Author's response to reviews

Title: Income-related inequalities in chronic conditions, physical functioning and psychological distress among older people in Australia: cross-sectional findings

Authors:

Rosemary J Korda (Rosemary.Korda@anu.edu.au)
Ellie Paige (Ellie.Paige@anu.edu.au)
Vasoontara Yiengprugsawan (Vasoontara.Yieng@anu.edu.au)
Isabel Latz (ilatz@ph.ucla.edu)
Sharon Friel (Sharon.Friel@anu.edu.au)

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Author's response to reviews: see over
Ms Natalie Pafitis  
Executive Editor  
BMC Public Health

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Dear Ms Pafitis,

Re: Income-related inequalities in chronic conditions, physical functioning and psychological distress among older people in Australia: cross-sectional findings (MS: 201509563102692)

Thank you very much for your email and for the further helpful and constructive comments by your reviewers. We appreciate the time they have taken to provide this feedback. We provide a point-by-point response to each of the comments below. The reviewers’ comments are given in italics and our responses are in bold. We have uploaded the amended manuscript, in which we have tracked the changes.

We very much hope you will now find our paper suitable for publication in *BMC Public Health*. Please do not hesitate to contact me at Rosemary.Korda@anu.edu.au or +61 2 6125 5583 if you require anything further.

Yours sincerely,

Dr Rosemary Korda, corresponding author, on behalf of all authors
REVIEWER ONE COMMENTS

1. There are many revised sections of the text which used > rather than # 80 years.

   We thank the reviewer for picking up this error. We have changed instances of “>80” to “≥80” in the amended manuscript.

REVIEWER TWO COMMENTS

1. The Methods section indicates that the analyses are weighted by age, sex, and region of residence. Please provide information on the source of these weights.

   The weights were calculated using age, sex and remoteness data from the Australian Bureau of Statistics 2006 census data. The following text (underlined) has been added to the amended manuscript: “Analyses were weighted for age, sex and region of residence (major city, rural, remote) using data from the 2006 Australian census (30).”

2. The assessment of model fit should make it clear that the Poisson model was an appropriate choice, that is, that there was no over- or under-dispersion. This is not addressed in the current statement about model fit. The ratio of model deviance to degrees of freedom will be close to one if the Poisson distribution is an appropriate choice for the data. If the variance is larger than the mean, which is common in many analyses involving count data, then a negative binomial distribution would be considered.

   It should be noted that a modified Poisson regression approach was used, which estimates robust standard errors to account for the fact that a Poisson model is being used to model binary rather than count data. This approach estimates standard errors from the variability in the data, so produce correct standard errors even if the underlying probability model is wrong.1 We have now added a reference about this approach in the Methods (reference [30], second sentence of Statistical Methods, page 7).

   Regarding testing for model fit, after running the Poisson models in Stata, we tested the fit of the models using goodness of fit tests (deviance and Pearson’s Chi-squared) and examined the deviance graphically using residual plots (see last sentence, paragraph 2, page 7). The residual plots and the deviance statistic and a Pearson statistic all confirmed that the Poisson model fitted the data in all analyses.

   To make this clearer, we have made now changed the wording in the manuscript
   From: “Model fit was confirmed using residual plots and Pearson goodness-of-fit tests.”
   To: “Model fit was tested using residual plots and deviance and Pearson goodness-of-fit tests, which all confirmed appropriateness of fit of the Poisson model, in all analyses.”

   Given the above, it was not necessary to consider a negative binomial model. However, in response to the reviewer’s concern we also ran the analyses using a negative binomial model and performed a likelihood-ratio test of alpha=0, which confirmed there was no overdispersion.

3. In the Discussion section, the authors make the following statement: “The findings also reflect survival, as well as reverse causality.” It is important for the authors to acknowledge that they adjust for a limited set of covariates in their models and that residual confounding may be present in the data, which may account for some of the unexpected findings of the research.

   We had not discussed residual confounding due to limited adjustment for covariates, as this might suggest that we were aiming to attribute causality rather describe the income-
related burden in health. However, to acknowledge that limited adjustment was made in this context, the following text (underlined) has now been added to the amended manuscript: “It was not the aim of this paper to determine causality between income and chronic disease, which is not possible with these cross-sectional data and with limited adjustment of covariates.” (p. 12, last paragraph)

4. The Methods section requires a separate section entitled “Statistical Analysis” or something similar. As the manuscript is currently written, the Methods section only has sections for “Participants” and “Study Variables”. The analyses are inappropriately included in the latter section.

We have added in the sub-title “Statistical methods” to the appropriate section in the methods of the amended paper (p. 7).

REVIEWER THREE COMMENTS
1. As the particular value of this paper is related to the inclusion and distinction of those aged 80+ years, the abstract may pay greater attention to the results observed for this age group specifically.

We have added the following to results section of the abstract in the amended manuscript (underlined):

“Absolute and relative inequalities tended to decrease with age, but remained relatively high for diabetes and physical functioning in the elderly (≥80 years).”

2. On page 13, row 13-14, the sentence “The lower relative … people” might be deleted as I found it to be highly speculative.

We have deleted this sentence from the Discussion.

3. Finally I would like to make a note that the author may or may not want to consider. A disadvantage of Prevalence Ratios is that their value strongly depends on how prevalence is defined: do you take the ‘unhealthy’ category (prevalence of ill health) or the ‘healthy’ category (prevalence of good health)? This is an arbitrary choice, but a choice that can greatly affects how the PR varies according to age. Even though I preferred the PR for a long time, in many situations I had to come to realise that the Odds Ratio had the advantage of not being dependent to this arbitrary choice.

Thank you, we note these comments, and acknowledge the potential disadvantage of PRs, particularly when the direction of the outcome is arbitrary (although in terms of the current context, we are clearly interested in quantifying the prevalence (and inequality) of disease rather than prevalence of not having of disease). For the current study, we feel the limitations of PRs are outweighed by the disadvantages of reporting ORs, as outlined in the previous responses.

References