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

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Version: 2
Date: 13 May 2014

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

May 13 2014  

Dear Ms Pafitis,  

Re: Socioeconomic inequalities in chronic conditions among older people in Australia: cross-sectional findings from the 45 and Up Study (MS: 201509563102692)  

Thank you very much for your email and for the 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 (except for changes to the results in the tables and referencing, which are not tracked).  

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. Why only compare lowest and highest income groups? It seems that using <10K may be limiting. According to the 2012 poverty in Australia report, the poverty level was (for a single individual) at 18K. I suggest the authors use <20K versus #70K. One other option would be to look at all categories (income as a continuous) and explore whether the trends in the figures are significant.

We agree with the recommendation of using <20K as the reference group instead of <10K and have re-analysed the results accordingly. Note that while this analysis obviously means the values of PRs and PDs reported throughout the paper have changed, the overall pattern of findings and the conclusions are very similar. (Please note that because of the many changes required in the tables, we have not used tracked changes to note these changes.)

With regard to only comparing the highest and lowest income groups, please see response to Reviewer 3, Comment 7.

2. Limitation: "most population-based cohort studies" should be deleted. Not all national studies have such a low response rate. This may be the case in Australia, but should not be generalized to all surveys. 18% however is very low. It would be useful to provide the distribution of income for these age and gender groups using Australian national estimates as a reference.

The text in the amended manuscript has been changed to “like many population-based cohort studies, the response rate was low.” (p.13, para 3)

A table comparing the income distribution by sex and age from the 45 and Up Study to national estimates from the National Health Survey (2011-12) is below. The most notable difference are that the 45 and Up Study has a higher proportion of people in the higher levels of income in the younger age groups and a higher proportion of low income people in the >80 age group, compared to the distribution seen in the population. However, caution should be used in comparing the distributions because surveys were performed at different times (approximately 5 years apart). Furthermore, the data from the national health survey was weekly household income (cash) which does not directly translate to the exact same yearly income definitions and categories used in the 45 and Up Study. Given the percentages from the National Health Survey are not directly comparable to the 45 and Up Study percentages, we have not included this table in the amended manuscript.

However, we do want to emphasise that the main focus of the paper is on comparing the relative inequalities, based on fixed income levels, which are not dependent on the actual income distribution in the sample – please also see our response to the next comment (Reviewer 1, Comment 3). Please also note we do state in the discussion that the study “is unlikely to be representative of the general population and thus caution should be used when interpreting and generalising absolute prevalence estimates.”
<table>
<thead>
<tr>
<th>Age group (years)</th>
<th>45-64</th>
<th>65-79</th>
<th>≥80</th>
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<td></td>
<td>Pop*</td>
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<td>MALES</td>
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<tr>
<td>&lt;$20K</td>
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<td>12.2</td>
<td>30.7</td>
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<td>$20-&lt;30K</td>
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<td>6.7</td>
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<td>31.3</td>
<td>47.8</td>
<td>5.8</td>
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<td>FEMALES</td>
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<td>29.9</td>
<td>36.5</td>
<td>5.4</td>
</tr>
</tbody>
</table>

*Population data estimated from the Australian National Health Survey

3. Clarify the following "internal comparisons of the relative inequality are valid"

We have now clarified this in the manuscript (see p.14, para 1).

We provide a detailed response below, as this also addresses the related issues raised above, and by Reviewer 3, in Comment 4, regarding the low response rate.

We acknowledge that the participation rate is low and the reviewers’ concerns that the study is not representative of the general population. However, much epidemiological practice, including RCTs and cohort studies, is based around the principle that high response rates and representativeness are not necessary for reliable estimates of relative measures based on internal comparisons within study populations; classic examples of this are the British Doctors’ Study, \(^2\) and the UK Biobank. Examples relevant to Australia include the AusDiab Study (33% response rate) and Australian Longitudinal Study of Women’s Health (44% response rate, or half of this for linkage studies). Furthermore, pooled analyses incorporating cohort studies, case-control and other study designs tend to find materially similar findings between studies with varying response rates.\(^6\)

Aside from these general experiences over time, simulation studies and algebraic work indicate the likely validity of internal comparisons in the face of varying response rates. Because this issue had been raised repeatedly, it has been tested empirically in the 45 and Up Study, by comparing relative risk estimates from the 45 and Up Study (a non-
representative sample, with a response rate of 18%) with relative risk estimates based on highly comparable questionnaire items from the NSW Population Health Survey (a sample that is generally considered representative, with a response rate of 62%).\(^7\) It was found that exposure-outcome relationships did not differ materially between the two surveys. Another key paper in this area, by Anne-Louise Ponsonby and Terry Dwyer, and published in ANZJPH, comes to the same conclusion through a review of classic epidemiological work.\(^3\) (These two papers have been cited in the manuscript, see p.14, para 1).

We do acknowledge that high response rates are generally required for population prevalence estimates, thus we suggest that caution should be applied in generalising the absolute inequalities found in our sample to the general population (as noted in response to the Reviewer 1, Comment 2 above).

4. The discussion could benefit from additional statements. A paragraph discussing the results and implications of higher income-related inequalities among those diagnosed with diabetes, those with severe physical functioning impairment and psychological distress is needed.

The Discussion has been expanded considerably (see also Reviewer 3, Response 2 and next response). Specifically regarding the above, please see Discussion, last sentence of p.10, para 3; and p. 12, para 1.

5. The "novelty" of this study, according to the background, is the ability to detect inequalities in oldest old (\(>=80\)), yet very little is discussed regarding the results in older age.)

Regarding the results in older age, we have now:
- added a statement in the first para of Discussion (p. 10)
- extended the discussion to detail results of previous studies that have quantified inequalities separately in the very old, see p. 11, para 1 (note there is very little literature on this).
- added a discussion point on p.12, para 2.

On page 8, next to the "57646" excluded due to missing income, add the percent of missing income.

The percent of participants excluded due to missing age and missing income have been added to the amended manuscript.

6. On page 8, last paragraph, there are too many numbers making it hard to follow. Delete "where the overall prevalence was ...", this information is in the table.

This text has been deleted from the amended manuscript.

7. The paragraph starting with "the 45 and up study .... in population health" belongs in the method.

This paragraph referred to above discusses the strengths of the study. To make this clearer, we have now amended the relevant paragraph to indicate this, and consequently kept it in the Discussion. Please note that the reference to methods in this paragraph (pertaining to over-sampling), is included in the Methods section (see p.5, para 1).
REVIEWER TWO COMMENTS

There is the possibility of residual confounding when testing the association between income and health because of the choice of measures of income (annual household income), the choice of measures of health (self-reported chronic conditions, physical functioning, and psychological distress), and the lack of control for confounders such as education level and ethnicity/race. Please address these issues in the manuscript:

1. What alternative measures of income might have been used in other studies when examining the relationship between income and health?

Of the previous studies that have assessed the relationship between income and health outcomes similar to those analysed in our study, household income was a common measure used to assess income.\textsuperscript{8, 9} Alternate measures of income used in other studies included equivalised household income\textsuperscript{10, 11} and measures of wealth.\textsuperscript{10, 12} Data on these alternate income measures were not available in the 45 and Up Study so we were unable to assess these income measures in our analyses.

2. Why did the authors not include education and ethnicity/race as covariates in their models? What effect would the inclusion of these measures have on the magnitude of the prevalence differences and ratios?

As outlined in the discussion of the manuscript, the aim of this paper was not to examine a causal relationship between income and chronic health outcomes, which would require a different study design (see also response to Reviewer 2, Comment 7). Instead, the aim of this study was to describe the burden of chronic disease by income, with a particular interest in examining if income-related inequalities in this burden are apparent in the very old, regardless of the underlying mechanisms. Thus descriptive, not explanatory, models were used in this study.

However, we do agree that ethnicity is potentially an important covariate to adjust for in this context, particularly as the distribution of this covariate is likely to be different in our sample than the general population. Thus, we have now included adjustment for this in the models (see Methods, p.7, para 2). Note that this adjustment had little material effect on the results. (Revised results are shown as tracked changes throughout manuscript but are not shown as tracked changes in the Tables).

We have chosen not to adjust for education, however, given the aim of the study is to describe the excess disease associated with lower income, regardless of whether or not this excess may be partly attributable to other socioeconomic factors such as education, which are often closely aligned. Moreover, adjusting for education is likely to lead to an underestimation of the excess disease in low income groups. In doing this, of course, we assume the distribution of education within income groups in our sample is similar to that of the general population.

3. Please provide information about the sensitivities and specificities of self-reported chronic conditions when compared to information in the medical chart or another appropriate “gold standard” source of information about chronic conditions.

Two validation studies comparing self-report measures of diabetes and breast cancer in the 45 and Up study to administrative data have been published. One study examined self-reporting of diabetes compared to diabetes status ascertained from the Admitted Patient Data Collection (APDC), claims for medical services (MBS) and pharmaceutical claims (PBS).\textsuperscript{13} The results showed high sensitivities of: 83.7\% for a diabetes diagnostic code in the APDC; 63.9\% for one claim for a glycosylated haemoglobin test in the MBS (this increased to 80.5\% if claims for two tests were used); and 96.6\% for dispensing of a
diabetes-related medication in the PBS data. The corresponding specificities were 97.7%, 98.4% and 97.1%, respectively. Similarly, high sensitivity (84.7%) and specificity (99.6%) were found in a study comparing self-report of breast cancer on the 45 and Up Study questionnaire to cancer registry data. We were unable to find any Australian validation studies comparing self-report to gold-standard measures for heart disease, arthritis or Parkinson's disease. However, it has been previously shown that self-report measures of health conditions and behaviours generally underestimate that found from gold-standard measures.

4. The Methods section should contain details of the methods and statistics used to assess fit of the Poisson regression model. Would a negative binomial regression model have provided a better fit to the data? The regression method used to analyze the data should be identified in the abstract.

We have now added information about goodness of fit of the Poisson models to the Methods section (see p.7, para 2). We have also added the regression method used (Poisson regression) to the Abstract (see p.2, para 2).

5. Please include a measure of general/overall health in your analysis, such as “How would you rate your overall health?” For example, you might focus on the percentage of respondents who report being in the “excellent” and “very good” categories, or conversely the percentage in the “fair” and “poor” categories.

It is our preference is to only include in this paper specific chronic diseases and associated health outcomes (severe physical functioning limitation and psychological distress). However, we are happy to take editorial advice on whether a measure of general/overall self-rated health should also be included. (Indeed, our analysis of self-rated general health shows the same patterns as seen with the specific health outcomes examined in the paper). Please also note Response to Reviewer 3, Comment 6, regarding self-rated health being reported elsewhere.

6. Please cite one or more references that have use the selected cut-points for the SF-36 physical functioning scale and the Kessler Psychological Distress Scale to define severe physical functional impairment and high psychological distress, respectively.

In the amended manuscript we have added references to previous papers that have used the selected cut-points for physical functioning impairment and psychological distress (see p.7, para 1).

7. Please remove the sentence “It was not the aim of this paper to determine causality between SES and chronic disease, which is not possible with cross-sectional data.” If issues of confounding can be adequately addressed, it may be possible to infer causal relationships with cross-sectional data.

While we agree that addressing confounding is essential for analysing causal relationships, we do not agree that it is possible to infer a causal relationship between SES and chronic disease from cross-sectional data as used in this study, even if fully-adjusted for confounding. To use a simple example, it is clearly not possible to determine from these data, even if adjusted for all possible confounding, whether the higher prevalence of severe physical functioning limitation in low income working age men (45-64) is ‘caused’ by their low income and/or whether the limitation has “caused” them to have low incomes (e.g. because of limitations in ability to work). We have, however, modified the sentence in the amended manuscript to limit the statement to apply our study:
i.e. [changes underlined]: “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.” (p.12, para 3).

8. *Figures 1 and 2 report the health measures using proportions, but in the tables they are reported using percentages. Please correct this inconsistency.*

   **Figure 1 and Figure 2 have been amended to show percentages rather than proportions.**

9. *The abbreviation PR should be defined in the titles of Tables 2 and 3.*

   **PR (prevalence ratios) has now been defined in the titles of Table 2 and Table 3 in the amended manuscript.**

10. *Please ensure that the results are reported using a consistent number of decimal places in Tables 2 and 3.*

   **Table 2 and Table 3 in the amended manuscript have been changed to consistently report to two decimal places.**

11. *Discretionary revision: The title is misleading because physical functioning is not a chronic condition. Please consider changing the title.*

   **We agree, and note that this comment is in line with that of Reviewer 3, who has advised that the health outcomes of interest need to be more clearly defined (please see Reviewer 3, Comment 1). We have changed the title of the paper to [changes underlined ]: “Income-related inequalities in chronic conditions, physical functioning and psychological distress among older people in Australia: cross-sectional findings from the 45 and Up Study”**

**REVIEWER THREE COMMENTS**

1. *The authors should define more clearly the health outcome of interest. The introduction starts with “non-communicable diseases” and “chronic diseases”, while the variables include “functional impairments” and even “psychological distress”. If the authors really aim to focus on non-communicable diseases, the latter two variables could better be omitted from analyses.*

   **We agree that the health outcomes included in this study require greater clarity. To this end we have:**
   - Removed the term “non-communicable disease” from the text (see Introduction, p.4, para 1)
   - Provided a clearer rationale (plus references) for the inclusion of physical functioning and psychological distress as outcomes—see Methods p.6, para 1 and p.7, para 1. (Associated changes also made in Abstract Background, p. 2; Background, p. 4, para 2).
   - Changed the title of the paper (see Reviewer 2, Comment 11, above).

2. *I recommend the authors to give a more detailed account of the relevant literature, and the contribution that this paper could possibly make to that literature. It is incorrect to say that “there is sparse information on socioeconomic inequalities in the prevalence of chronic...***
conditions in older people” especially if we include the many studies on disability prevalence and incidence.

We agree that a more detailed account of the literature was necessary. To this end we have provided more detail (including additional references), in both the Introduction and in the Discussion. Please note that we have focused on the literature describing inequalities in the prevalence of chronic disease, physical functioning limitation and psychological distress, and in older ages, as this is the focus of the paper (rather than on incidence and mortality studies, and studies of younger people). Substantial changes (including additional referencing) have been made in the following places:

- Background, para 3, p. 4-5
- Discussion, para 2 & 3, p. 10-12

Note we have also removed “there is sparse information” from the manuscript, and made more explicit the gaps in the literature (as noted above).

3. I recommend also presenting results for educational level. The authors recognize that income may not be a good measure of SES in older people as in younger people. Moreover, reverse causation processes at younger ages may strongly affect comparisons with older ages. The use of educational level would not suffer from such problems to the same extent, and thus provide important complementary results.

Although not made clear in the original manuscript, the primary aim of this study was to examine the excess chronic disease burden associated with lower income, rather than SES more generally (with a particular interest in describing inequalities in the very old). To make this clearer, we have changed the title of the manuscript (see response to Reviewer 2, Comment 11, above) and other relevant sections in the manuscript (Abstract Background, Method and Results sections, p. 2; Method, p. 7, para 2; Discussion, p.12, para 3). While we agree that education can provide important complementary results, because our aim was to specifically examine income rather than SES inequalities more generally, we have not presented the results for education as well. Please note that comparison of inequality estimates using different measure of SES (including education) is the focus of another paper (include formal testing the magnitude of inequalities across different measures of SES and age groups) and is currently now in press.17

4. The response rate of only 18 percent should be given more attention. This is not “relatively low” but very low in an absolute sense, with great potential for strongly affecting observed disease prevalence rates and inequalities therein. The authors should provide more detailed information on response rates, e.g. according age group and possibly area-level SES.

Please see our detailed response to Reviewer 1, Comment 3. Note we have taken out the work “relatively” (p. para ).

Unfortunately, we do not have data on response rates according to sociodemographic variables so we are unable to provide more detailed information on the response rates in the amended manuscript.

5. The authors should justify the use of Prevalence Ratios (PR) instead of Odds Ratios (OR) as measure of relative inequalities. The (maximum) value of PR is strongly sensitive to the
absolute level of prevalence rates (Houweling et al, Int J Equity Health. 2007 Oct 29;6:15), thus greatly affecting comparisons between age groups.

We acknowledge that the PR is sensitive to the absolute level of prevalence rates, which needs to be kept in mind particularly when the prevalence is high in all groups, creating a ceiling on the PR. Nevertheless, we do not see this as an issue in this context, particularly as we have reported the underlying prevalence and prevalence differences (PDs) alongside the PRs—indeed, an advantage of modelling the data as proportions rather than odds, is we are able to report on absolute inequalities, i.e. prevalence differences, alongside the PRs.

The approach of reporting PR/RRs over ORs, as we have done, is supported in the literature. Moreover, as this is descriptive study of inequalities in chronic disease, and prevalence is the primary outcome of interest, the prevalence ratio rather than odds ratio is the measure of interest and, unlike the OR, is directly interpretable (which is particularly important if conveying results to a wider audience), i.e. if, for instance, the prevalence is three time higher the PR will equal 3, whereas the corresponding OR will equal anything from 3 or higher (and often much higher), depending on the overall prevalence of the outcome. Further, ORs cannot be used to directly compare relative inequalities in prevalence across age groups because they are sensitive to the overall prevalence in each age group. For example, take the situation where the prevalence of disease is twice as high in the low income group than the high income group, in both younger (20% vs 10%, PR=2) and older people (40% vs 20%, PR=2) ; for the same disease, if using the OR to measure relative inequality, the ORs for young and old respectively will be 2.25 and 2.67, i.e., if using ORs to measure relative inequality in prevalence, relative inequality in prevalence “will appear” to be higher in the older group than the younger group, (even though it is not), purely because the overall disease prevalence is higher in the older group.

6. The authors should justify the lack of tests on interaction between age and SES. Such tests would be expected given their aim to assess whether the relationship between SES and health varies according to age.

The aim of this study was to describe the excess chronic disease in low income compared to high income groups in different age groups (with a particular emphasis on older people (≥80), as this is not commonly reported). We did not set out to test hypotheses about age differences in inequalities. This latter aim is a specific focus of recent research (involving different methods to model inequalities in two health outcomes– self-rated health and heart disease – formally testing the magnitude of inequalities across different measures of SES and age groups) and is currently now in press (see also response to comment 7 below). Note that the results from that paper regarding age differences in inequalities are consistent with the findings described in this study.

Accordingly, we have now clarified the aim of the study by removing the word ‘compares’ in the aim stated in the Abstract and Introduction. The aim in the amended manuscript now reads: “The aim of this paper is to quantify absolute and relative income-related inequalities in chronic diseases, physical functioning and psychological distress in mid-age and older Australians, using large-scale population-based study data that includes a large sample of people aged 80 years and over.” (p.5, para 2 ) We have also deleted the following from the first sentence of the Conclusion of the Abstract “Although the magnitude of relative and, to a lesser extent, absolute inequalities decreases with age” (p.3), and deleted the following from the Conclusion of the
discussion “Although the extent of relative income-related inequality in chronic conditions, and for some conditions also absolute inequality, diminishes with age” (p 14).

7. Finally, the authors should justify their use of the simple ratio comparing highest to lowest income groups. This ratio discards information on intermediate income groups, and it is sensitive to the large age differences in % people in the highest and lowest groups. It seems more useful to use the RII and SII, or their equivalents based on Lorentz curse (e.g. Concentration Index).

We have used the RII and SII in other inequalities research (including the paper referred to above in point 6[^17]), and agree it is a very useful measure, particularly when comparing different SES measures or changes over time in inequalities. We chose not to use the RII (or concentration indexes) in this study for three reasons: (1) given this is a descriptive study, we wished to present the data in an way that was easily interpretable for a wide audience (i.e. PRs/PDs are easier to interpret than the more nuanced RII/SII or concentration index measures); (1) the RII requires that the association between the exposure (SES) and the outcome be linear, and this assumption was not always met across the different conditions; and (3) as the reviewer has noted, the RII (and concentration index) is sensitive to the proportion of people in the low and high income groups, thus all else being equal (i.e. where there are the same PRs across age groups), the magnitude of the inequalities based on RII or concentration indexes will be higher where there is a greater proportion of people in the lower income groups. While this is important information, this is not what we aimed to report on in this particular study.

While, for simplicity and clarity, we only report on PRs and PDs for the highest and lowest income groups, as is common practice, the prevalence for all income groups are reported in the figures, clearly showing SES gradients. We are happy to take editorial advice on whether supplementary tables should also be supplied showing PRs and PDs for each level of income.

References