

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Shape of the association between income and mortality. A cohort study of Denmark, Finland, Norway and Sweden in 1995 and 2003
AUTHORS	Mortensen, Laust; Rehnberg, Johan; Dahl, Esben; Diderichsen, Finn; Elstad, Jon Ivar; Martikainen, Pekka; Rehkopf, David; Tarkiainen, Lasse; Fritzell, Johan

VERSION 1 - REVIEW

REVIEWER	George A. Kaplan Center for Social Epidemiology and Population Health University of Michigan School of Public Health Ann Arbor, Michigan, USA
REVIEW RETURNED	07-Jan-2016

GENERAL COMMENTS	<p>This is a well-done and useful manuscript that compares the shape of the income-all cause mortality association between four Nordic countries and across two time periods when there were increases in income inequality. The data are population-based, come from well-documented national data files, and are sufficiently harmonized across countries to permit comparisons between countries.</p> <p>For the most part, the analyses confirm the growing recognition that the non-linearity of income-mortality association is such that it is misleading to imply, as is often done, a simple linear gradient. Furthermore, they also provide support for the assertion that there are secular trends (in this case based on just two points in time) in the association with those well off experiencing greater declines in mortality over time than those less well-off. That is an observation with important policy ramifications.</p> <p>It is a careful analysis, and I commend the authors for using both relative and absolute income measures. I also think they could make the argument that the shape of the association and change over time and between age and sex groups can serve as a reality check for various causal and non-causal discussion of the income-mortality association.</p> <p>Comments that could be addressed:</p> <ol style="list-style-type: none"> 1) Why were 1995 and 2003 selected and not other years, or more years? 2) Can they examine both pre- and post-tax/transfer income? 3) Are there any data available across the four countries that would inform their speculation as to the reasons for the decreased mortality at very low incomes for working age (mostly men) in three of the countries? 4) They should balance the Gravelle cite with Wolfson et al.'s BMJ paper that tested the 'artifact' explanation of the income inequality-
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	<p>mortality association. (disclosure--I am one of the authors of that paper)</p> <p>The results are useful and important but as is often the case raise a number of questions which the study design and data sources do not allow them to be answered -- not so much a failure of the analysis as much as a more general lack of available data and analytic techniques to answer them. The manuscript is written carefully and without much speculation, but within length constraints, the authors might, if they choose to, speculate on what would be needed to address them. For example,</p> <p>1) Why would one expect the these non-linearities and changes in them over relatively short periods of time? Would they be more likely to be seen for particular causes of death?</p> <p>2) How does the non-linearity they show compare to the shape of the association in countries with less generous redistributive policies?</p> <p>3) Multiple factors can contribute to the changes in income inequality. Do the variations over time between these four countries in these factors shed any light, even impressionistically, on the contribution of various factors?</p> <p>4) What might account for the much shallower associations in women?</p>
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REVIEWER	Adriaan Kalwij Utrecht University
REVIEW RETURNED	26-Feb-2016

GENERAL COMMENTS	<p>Discussion</p> <p>This is an interesting study on the shapes of the mortality-income gradients in Denmark, Finland, Norway and Sweden and how these shapes have changed between 1995 and 2003. The motivation given for why one would expect a change in these shapes is that income inequality has increased over this period. It is not entirely clear why that would be the case. One could argue that if the relative income positions of individuals matter for mortality, and that these have not changed, that increasing income inequality as such would not change the shape of the mortality-(relative) income gradient. Nevertheless, the authors find some changes as mortality decreased more in the upper part of the income distribution than in the lower part. Was this result expected beforehand and why? Related to this is the authors' remark that the shape may be influenced by macro trends. Which macro trends have the authors in mind? Answers to these question may provide insights into if the changes in the shapes of the associations (once controlled for the levels) might be related to changes in individuals' (relative) health.</p> <p>My main concern is the lack of statistical tests to support the main conclusions. It is mentioned that the shapes of the associations have been investigated in previous studies and that the main advantage of this study is that it provides the opportunity for direct comparisons of the shapes between these four countries, across age groups and how these shapes changed over time. I fully agree but the conclusions are based on visual inspections of graphs. The authors do not take full advantage of the abovementioned opportunity. One could, for instance, test if the shapes are equal or that the changes in the shapes of the associations are equal across countries. The same can said about the comparisons across age groups and years. For instance, Belloni et al. (2013, Demographic Research, Volume</p>
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	29, Article 45, pp. 1261-1298) test for no change in the income-mortality gradient over time in Italy. Given the excellent data and the high standard of the statistical analyses already performed, conclusions can and should be supported by statistical test results.
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REVIEWER	Dugravot Aline French National Institute of Health and Medical Research, France
REVIEW RETURNED	03-Mar-2016

GENERAL COMMENTS	<p>The paper examines the shape of the association between income and mortality in four Nordic countries in 1995 and 2003. The objectives of the study are clearly defined, the choice of the statistical methods is appropriate but the results are mainly descriptive. There are major problems in the design and presentation of the results.</p> <p>Major comments:</p> <p>The cohorts were followed for five years for mortality. For individuals aged 25-64 years at baseline, this 5-year period is likely to be too short. At these ages a reverse causation explanation cannot be excluded as income can decrease after the onset of disease, which then is followed by death. This can explain in part the income gradient in mortality.</p> <p>It is surprising not to have confidence intervals around the predicted mortality rates. They are necessary to be able to show change in the income gradient over time, the main objective of the present study.</p> <p>The authors present their results only in the figures. This can be misleading. Indeed, the authors compare figures 1 and 2 and write: « When we looked at individuals aged 65 or more at baseline, we observed a much less steep income gradient in mortality », but the scale for mortality rate in the two figures is not the same. For men aged 25-64 in 2003, the absolute difference of mortality rate between bottom and top income percentile is around 1000 per 100,000. This difference is around 4000 per 100,000 among those aged 65 and more. In absolute terms, the gradient is steeper in older persons. However, in a relative approach, the gradient is effectively less steep in the oldest group: decrease of $1000/1500=66\%$ in youngest group v.s. $4000/16000=25\%$ in oldest group. However the comparisons are based only on the figures with no systematic testing of differences. It would be good to clarify this issue.</p> <p>Minor comments:</p> <p>In the method section, please clarify that the different databases used for each country are linked to mortality outcomes.</p> <p>It would be helpful to specify in the methods section that the analyses are undertaken separately among individuals aged 25-64 and individuals aged 65 and more.</p> <p>The supplementary figures 1 and 2 are the same than figures 4 and 5.</p>
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	<p>In the discussion, the authors explain the finding at the low end of the income distribution by the highly variable income of some individuals. This assumption could be verified by taking the average income over 3 years for example.</p> <p>The authors explain that “the similarity of the shift in shape over time in the four countries [...] may indicate that there are cohort or period effects”. I don’t agree with this comment. Please support this statement with results.</p> <p>Please include in table 1 the numbers for the group of persons aged 80 and more by 5-year groups until 95 years or higher.</p> <p>In figures 4 and 5, there appear to be data for nine age-groups whereas there are only eight possible five-year age groups between the ages of 25 and 64 years – please verify.</p> <p>Typo:</p> <p>Table 2: Inflection instead of infection. Figure 5: mortality is written twice.</p>
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REVIEWER	Rasmus Hoffmann Professor European University Institute Italy
REVIEW RETURNED	03-Mar-2016

GENERAL COMMENTS	<p>Review of Mortensen et al. : “Shape of the association between income and mortality. A cohort study of Denmark, Finland, Norway and Sweden in 1995 and 2003”</p> <p>This study compares the shape of the relationship between household-size equivalized disposable income and all-cause mortality between four Scandinavian countries and between 1995 and 2003 using Poisson regression and smoothing methods. They show a strong non-linear relation and an increasing slope and increasing non-linearity over time. This is a good fundamental article that is worth publishing after the following minor comments have been taken into account:</p> <ol style="list-style-type: none"> 1. p3: 1st bullet point: “income gradients IN MORTALITY” 2. “Based on the findings of this study little is known” sounds strange 3. the word “link” is wrong 4. p4, middle: “Between1995” 5. “less attention to mortality at older ages” is true but there are studies doing exactly this, e.g. mortality differences between income percentile groups based on Danish register data for ages 59+ in Hoffmann R 2011 „Socioeconomic inequalities in old-age mortality: A comparison of Denmark and the USA” Social Science & Medicine, 72(12):1986-1992. 6. p7: “age- income-specific” 7. p8: I am not a mathematician but if the inflection point is where the curve is steepest then this is not the point where the curve is most convex. 8. I don’t understand why a combination of all age groups results in
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a pattern similar to age 25-64. Pattern should be dominated by the majority of deaths at ages 65+. Please explain. Maybe it would be good to differentiate number of deaths in Table 1 between 25-64 and 65+

9. "Since we so far HAVE"

10. p9: The first sentence of the second paragraph sounds as if you refer to the differentiation between 25-64 and 65+ (which should have been mentioned earlier). If you want to refer to the shape differences between age groups, the sentence better starts with "To compare the shape across age groups ..."

11. Since the age-specific results for 25-64 are already shown in the main results, they should not be repeated in the appendix.

12. "countries:" should be "countries."

13. An interesting finding that could be mentioned and discussed is that men have a much steeper gradient than women, i.e. poor men are a special group and contribute much to the overall income mortality gradient.

14. p11, line 3: I think "and increasing mortality" can be deleted. Only age is important here.

15. what is "age retirement"?

16. "cohortS"

17. "changing in the association" – I don't understand this sentence.

18. An overview of mortality differences between income percentile groups for 10 causes of death among Danes aged 59+ (1980-2000) can be found on page 174 in: Hoffmann R 2008 „Socioeconomic Differences in Old Age Mortality“, Monograph in the series Demographic Methods and Population Analysis“, K. Land (Ed.), Vol. 25, Springer, Dordrecht. It shows (1) a relatively similar gradient for all causes of death, (2) it confirms the non-linear relationship with huge differences below median income and almost no differences above median income (see the below two graphs).

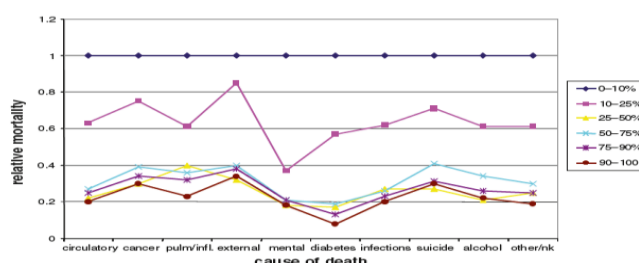


Fig. 8.30 Income mortality gradient for different causes of death, Danish men

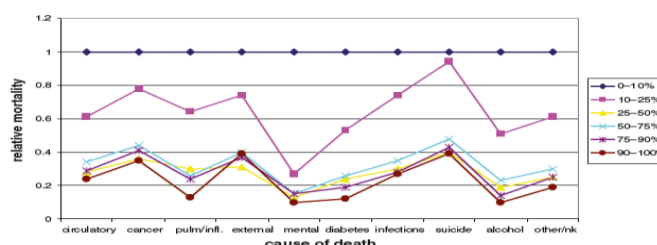


Fig. 8.31 Income mortality gradient for different causes of death, Danish women

19. "I don't know if "mortality TO cardiovascular diseases" is correct English.

20. p12: To refer to references 12-28 is probably wrong.

21. I would encourage the authors to speculate about reasons for their interesting findings (increasing steepness and increasing non-linearity and differences between countries). The uncertainty about

	the question whether the associations are causal at all and, if yes, in which direction between income and health the causality goes increases the uncertainty of possible explanations. But both pathways probably originate in a kind of welfare state failure so that these new findings are worth a short sociological and political discussion.
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VERSION 1 – AUTHOR RESPONSE

Reviewer 1 George A. Kaplan

1) Why were 1995 and 2003 selected and not other years, or more years?

We use these years as our years for income measurements partly due to the reason that the growing income inequalities in the Nordic countries would then be captured, the exact years were selected due to data availability in all countries. It is important to bear in mind that our mortality follow up period is from 1996-2000 and 2004-2008, respectively, so in terms of mortality we do indeed use more years.

We have clarified our follow-up period in the revised manuscript.

2) Can they examine both pre- and post-tax/transfer income?

It might be possible to do this with the original data, but the aggregated income data that we have pooled from all countries do not contain both pre- and post-tax income. We choose to focus on post tax/transfer income because we believe this is the most appropriate measure used for these comparisons.

3) Are there any data available across the four countries that would inform their speculation as to the reasons for the decreased mortality at very low incomes for working age (mostly men) in three of the countries?

The curvilinearity at the bottom of the income distribution has been found earlier (e.g. Ecob & Davey Smith 1999; Fritzell et al. 2004) but we have not been able to find data that really inform the suggested speculative reasons. The reasoning about this has been updated in the manuscript.

4) They should balance the Gravelle cite with Wolfson et al.'s BMJ paper that tested the 'artifact' explanation of the income inequality-mortality association. (disclosure--I am one of the authors of that paper)

We thank the reviewer for reminding us of that paper. Indeed, it is a fitting reference and a good balance to the Gravelle paper, and has now been added.

Result section

We thank the reviewer for his appreciation of our study.

1) Why would one expect the these non-linearities and changes in them over relatively short periods of time? Would they be more likely to be seen for particular causes of death?

One could of course argue that changes in the income-mortality association require a longer periods of time to appear. Still, quite many societal macro changes appears to have surprisingly sudden effects on mortality. All Nordic countries has been experiencing macro-economic changes and increasing inequalities during the 1990s, we believe that these societal changes could have had significant impact on the income-mortality association.

Given that certain causes of death have stronger socioeconomic relations than others (cardiovascular, external causes etc.) it seems reasonable to assume that they would be more likely to be found. Our aggregated data did not allow us to perform analysis of particular causes of death.

2) How does the non-linearity they show compare to the shape of the association in countries with less generous redistributive policies?

The question is fundamental but given data differences, differences in follow-up time etc. we refrain from detailed comparisons with earlier research. At a more general level we can see that earlier research in other countries has shown similar shapes, with diminishing returns of increased income in the association with mortality (for example see: Mackenbach et.al 2005). What is crucial is that our findings show that changes in the income and mortality associations is not only found in the US (Dowd,...Kaplan 2011), but also seen in the Nordic countries following the clear increase of income inequality in itself.

3) Multiple factors can contribute to the changes in income inequality. Do the variations over time between these four countries in these factors shed any light, even impressionistically, on the contribution of various factors?

Our study do not primarily focus upon reasons for increasing income inequalities but on the income-mortality association. Earlier studies suggest that policy changes, rather than labour market changes, lies behind growing income inequalities at least in Finland and Denmark (see e.g. chapters by Fritzell et. al and Kangas et al. in Nolan, Salverda et al (2014) *Changing Inequalities and Societal Impacts in Rich Countries: Thirty Countries' Experiences*. Oxford, Oxford University Press)

4) What might account for the much shallower associations in women?

We believe that the difference in effect is partly caused by lower absolute mortality for women. But there could also be because the casual relationship between income and mortality is weaker for women, the association has been found to be weaker in previous studies (see for example Backlund, Sorlie, & Johnson 1996, Martelin 1994 Kondo, Rostila & Åberg Yngwe 2013). A section discussing the difference between men and women has been added in the manuscript.

Reviewer 2 Adriaan Kalwij

1) This is an interesting study on the shapes of the mortality-income gradients in Denmark, Finland, Norway and Sweden and how these shapes have changed between 1995 and 2003. The motivation given for why one would expect a change in these shapes is that income inequality has increased over this period. It is not entirely clear why that would be the case. One could argue that if the relative income positions of individuals matter for mortality, and that these have not changed, that increasing income inequality as such would not change the shape of the mortality-(relative) income gradient. We thank the reviewer for explicitly making this point, which of course is correct. We have included a sentence suggesting that the changes we find indicatively suggest that it is not only the relative income position that matters.

2) Was this result expected beforehand and why?

One reason to expect a change of the shape due to increasing income inequality is the study by Dowd et al. (2011) which found such a change in the US.

3) My main concern is the lack of statistical tests to support the main conclusions. It is mentioned that the shapes of the associations have been investigated in previous studies and that the main advantage of this study is that it provides the opportunity for direct comparisons of the shapes between these four countries, across age groups and how these shapes changed over time. I fully agree but the conclusions are based on visual inspections of graphs. The authors do not take full advantage of the abovementioned opportunity. One could, for instance, test if the shapes are equal or that the changes in the shapes of the associations are equal across countries. The same can said about the comparisons across age groups and years. For instance, Belloni et al. (2013, *Demographic Research*, Volume 29, Article 45, pp. 1261-1298) test for no change in the income-mortality gradient over time in Italy. Given the excellent data and the high standard of the statistical analyses already

performed, conclusions can and should be supported by statistical test results.

It is true that the inference is based on visual inspection of the shapes without support from a statistical test. There are two reasons for this. First, separate models are estimated for each country, sex and year. This means that the different estimates do not arise from a single model, which means that there is no easy way to test the differences in the shapes which is comparatively easy in a single-model framework, where the nesting of models makes this fairly straight forward. Secondly, the test is complicated by the fact that we are modelling jointly association between age and income in relation to mortality. This means that there is not two separate sets of parameters that describe age-mortality and income-mortality, but rather a two-dimensional plane that describes the association. While it in principle is possible to extract the parameters and test if the differences in shape are statistically significant, we would not know how to do this. We tried to circumvent the problem of testing the parameters directly by implementing a bootstrap, which should in principle provide a measure of the variability, but found that it was difficult to implement for the aggregated data used here. We do note that since our analysis is conducted on full populations and since the strategy of jointly modelling age and income, the precision of the parameters will be very low when compared to the existing literature. We are, however, willing to provide the model parameters to the readership of the journal so that a test can be made. We also provide the figures with added confidence intervals, while they still rely on visual inspection they do strengthen the interpretation of changes in the shape of the associations. We did not replace the original figures with confidence intervals in the manuscript, but offer them to the reviewers and if desired as supplementary files.

Reviewer 3 Dugravot Aline

1) The cohorts were followed for five years for mortality. For individuals aged 25-64 years at baseline, this 5-year period is likely to be too short. At these ages a reverse causation explanation cannot be excluded as income can decrease after the onset of disease, which then is followed by death. This can explain in part the income gradient in mortality.

Concerning the problems of reversed association we agree that it is always present when exploring the income-mortality association. However, we believe that a 5 year follow-up is still valid at ages 25-64. If we were to increase the time window between income and the starting time of mortality other problems are introduced as those individuals who were not contributing to reversed causality would also be excluded. Still, we of course cannot rule out reverse causation and this is mentioned in the discussion section of the paper.

2) It is surprising not to have confidence intervals around the predicted mortality rates. They are necessary to be able to show change in the income gradient over time, the main objective of the present study.

We agree with the reviewer and have now included figures with confidence intervals as additional files to the reviewers. We did not replace the original figures with confidence intervals in the manuscript, but offer them to the reviewers and if desired as supplementary files. In addition to the figures with added confidence intervals we also provide excel files with the model parameters used in the analyses.

3) The authors present their results only in the figures. This can be misleading. Indeed, the authors compare figures 1 and 2 and write: « When we looked at individuals aged 65 or more at baseline, we observed a much less steep income gradient in mortality », but the scale for mortality rate in the two figures is not the same. For men aged 25-64 in 2003, the absolute difference of mortality rate between bottom and top income percentile is around 1000 per 100,000. This difference is around 4000 per 100,000 among those aged 65 and more. In absolute terms, the gradient is steeper in older persons. However, in a relative approach, the gradient is effectively less steep in the oldest group: decrease of $1000/1500=66\%$ in youngest group v.s. $4000/16000=25\%$ in oldest group. However the comparisons are based only on the figures with no systematic testing of differences. It would be good

to clarify this issue.

We thank the reviewer for making this important comment and we fully agree with the reviewer. In the revised manuscript we have added a sentence in the result section in which we clarified that absolute inequality is larger in old age, but comparisons of the gradients here are done on a relative scale.

Minor comments:

1) In the method section, please clarify that the different databases used for each country are linked to mortality outcomes.

We have clarified this in the revised manuscript

2) It would be helpful to specify in the methods section that the analyses are undertaken separately among individuals aged 25-64 and individuals aged 65 and more.

We have clarified this in the revised manuscript

3) The supplementary figures 1 and 2 are the same than figures 4 and 5.

This was a mistake and has now been fixed.

4) In the discussion, the authors explain the finding at the low end of the income distribution by the highly variable income of some individuals. This assumption could be verified by taking the average income over 3 years for example.

In fact we had the possibility to test this with the Swedish data and it turned that the results were similar with a 3 year average. We have therefore modified this comment in the revised manuscript. We cannot give a satisfactory explanation to these patterns at the moment, but still offer our speculations.

5) The authors explain that “the similarity of the shift in shape over time in the four countries [...] may indicate that there are cohort or period effects”. I don’t agree with this comment. Please support this statement with results.

We simply suggest that our shifts are due to either cohort or period effects. Our analysis cannot really separate these two even though our initial hypothesis of changes due to increasing income inequalities suggest a period effect.

6) Please include in table 1 the numbers for the group of persons aged 80 and more by 5-year groups until 95 years or higher.

Sample characteristics for all age categories up to age 95 and above has been added in table 1.

7) In figures 4 and 5, there appear to be data for nine age-groups whereas there are only eight possible five-year age groups between the ages of 25 and 64 years – please verify.

We thank the reviewer for noting this error which has now been corrected.

Finally we have also corrected the typos noted by the reviewer.

Reviewer 4 Rasmus Hoffmann

This study compares the shape of the relationship between household-size equivalized disposable income and all-cause mortality between four Scandinavian countries and between 1995 and 2003 using Poisson regression and smoothing methods. They show a strong non-linear relation and an increasing slope and increasing non-linearity over time. This is a good fundamental article that is worth publishing after the following minor comments have been taken into account:

We thank the reviewer for his positive evaluation of our article.

Minor comments

1. p3: 1st bullet point: "income gradients IN MORTALITY"
2. "Based on the findings of this study little is known" sounds strange
3. the word "link" is wrong
4. p4, middle: "Between1995"

We have corrected the manuscript accordingly.

5 "less attention to mortality at older ages" is true but there are studies doing exactly this, e.g. mortality differences between income percentile groups based on Danish register data for ages 59+ in Hoffmann R 2011 „Socioeconomic inequalities in old-age mortality: A comparison of Denmark and the USA" Social Science & Medicine, 72(12):1986-1992.

We agree. A sentence acknowledging that there is evidence for inequality in mortality in old age has been added with citations to this paper as well as another.

- 6 p7: "age- income-specific"

We have corrected the manuscript.

- 7) p8: I am not a mathematician but if the inflection point is where the curve is steepest then this is not the point where the curve is most convex.

We fully agree with the reviewer and have now revised and clarified the definition of the inflection point.

- 8) I don't understand why a combination of all age groups results in a pattern similar to age 25-64. Pattern should be dominated by the majority of deaths at ages 65+. Please explain. Maybe it would be good to differentiate number of deaths in Table 1 between 25-64 and 65+

We thank the reviewer for this remark, it is correct that the association when all age groups are included is more similar to the association among those aged 65+ due to the higher mortality in that group. Additional figures (not provided) from our data also confirm this. The sentence in the manuscript has been revised to correctly convey this point.

- 9) . "Since we so far HAVE"

- 10) p9: The first sentence of the second paragraph sounds as if you refer to the differentiation between 25-64 and 65+ (which should have been mentioned earlier). If you want to refer to the shape differences between age groups, the sentence better starts with "To compare the shape across age groups ..."

- 11). Since the age-specific results for 25-64 are already shown in the main results, they should not be repeated in the appendix.

- 12) "countries:" should be "countries."

We have made the corrections pointed out by the reviewer.

- 13) An interesting finding that could be mentioned and discussed is that men have a much steeper gradient than women, i.e. poor men are a special group and contribute much to the overall income mortality gradient.

We agree and have now included a discussion about the steeper gradient among men in the revised manuscript. Although we would argue that it is not only among the poor but also among men with a relatively low income above any reasonable poverty line.

- 14) p11, line 3: I think "and increasing mortality" can be deleted. Only age is important here.

15. what is "age retirement"?

16. "cohortS"

17. "changing in the association" – I don't understand this sentence.

We have clarified and/or changed in accordance with the suggestion.

18) An overview of mortality differences between income percentile groups for 10 causes of death among Danes aged 59+ (1980-2000) can be found on page 174 in: Hoffmann R 2008 „Socioeconomic Differences in Old Age Mortality“, Monograph in the series Demographic Methods and Population Analysis“, K. Land (Ed.), Vol. 25, Springer, Dordrecht. It shows (1) a relatively similar gradient for all causes of death, (2) it confirms the non-linear relationship with huge differences below median income and almost no differences above median income (see the attached two graphs). We thank the reviewer for informing us about these findings which now are included in the revised manuscript.

19. "I don't know if "mortality TO cardiovascular diseases" is correct English.

20. p12: To refer to references 12-28 is probably wrong.

We have corrected this in the revised manuscript.

21) I would encourage the authors to speculate about reasons for their interesting findings (increasing steepness and increasing non-linearity and differences between countries). The uncertainty about the question whether the associations are causal at all and, if yes, in which direction between income and health the causality goes increases the uncertainty of possible explanations. But both pathways probably originate in a kind of welfare state failure so that these new findings are worth a short sociological and political discussion.

The reviewer has correctly noted that we did not speculate that much about our findings. We have now included some more speculative sentences in the discussion section. However, we decided to be somewhat moderate in our speculations because of the extensive literature that already exist on the policy implications and causality questions for the income-health relationship.

VERSION 2 – REVIEW

REVIEWER	Adriaan Kalwij Utrecht University, the Netherlands
REVIEW RETURNED	15-Jul-2016

GENERAL COMMENTS	I am happy with the responses. In the end it is a descriptive analysis. A final perhaps minor comment is that I find how the term non-linearity is used often confusing. Not sure why the word is used at all, to be honest. It is well known the income-mortality gradient is non-linear as, for instance, a standard Cox type of model would allow for in a statistical analysis. I therefore fail to see why this is emphasized as a finding. Anyway, what I find a bit confusing is when, for instance, mortality drops more between two periods and the income-mortality gradient becomes steeper it is referred to as a stronger non-linearity, phrases like the strength of the gradient is stronger and an increase in the gradient. In my opinion the wordings concerning the gradients and if it becomes steeper or less steep (or a stronger or weaker gradient, also fine) needs to be improved a bit.
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VERSION 2 – AUTHOR RESPONSE

The reviewer has a single comment ("A final perhaps minor comment") where he questions the use on 'non-linear' in the manuscript. We take the point and have revised the wording on shape of the

gradients throughout the manuscript so that it is either replaced or if the phrase is used, it is accompanied with a description of what we mean by it. The following changes have been made (also documented in the main document with tracked changes, which - along with a clean version - has been updated as part of the revision):

1. Page 1, abstract: "A strong and largely non-linear association was observed between income and mortality" changed to "A steep income gradient in mortality"
2. Page 1, abstract: "A non-linear gradient exists between income and mortality in most cases and because of a more rapid mortality decline among those with high income this non-linearity has become stronger over time" was changed to "A non-linear gradient exists between income and mortality in most cases and because of a more rapid mortality decline among those with high income the income gradient has become steeper over time"
3. Page 2, key points: From "From the 90s to the 00s the non-linear income-mortality association generally became stronger" to "From the 90s to the 00s the income-mortality gradient generally became steeper"
4. Page 3, introduction. "i.e. non-linear" is deleted.
5. Page 5, introduction. From "(i) Do we find evidence for a non-linear association between income and mortality in the Nordic countries?" to "(i) Do we find evidence for a non-linear association with steeper income gradients in mortality at the low end of the income distribution as compared to at the high end in the Nordic countries?"
6. Page 11, discussion. From "A strong and largely non-linear association exists between income and mortality across age-groups and between genders in Denmark, Finland, Norway and Sweden" to "A steep income gradient in mortality exists across age-groups and between genders in Denmark, Finland, Norway and Sweden"
7. Page 13, conclusion. From "In conclusion, we observe a non-linear association between income and mortality in most cases up until the oldest old." to "In conclusion, we observe an income gradient in mortality in most cases up until the oldest old"