

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Trends of socioeconomic equality in mortality amenable to health care and health policy in 1992–2013 in Finland: a population-based register study
AUTHORS	Lumme, Sonja; Manderbacka, Kristiina; Karvonen, Sakari; Keskimaki, Ilmo

VERSION 1 – REVIEW

REVIEWER	Maria Pia Fantini Alma Mater Studiorum - University of Bologna, Italy
REVIEW RETURNED	11-May-2018

GENERAL COMMENTS	<p>The study by Lumme et al. aims to investigate trends in socioeconomic equality in mortality amenable to health care and health policy interventions in Finland over two decades (years 1992–2013). The authors found that improvement in mortality—especially among younger adults—differed between socioeconomic groups. The paper is well framed: the study aim is clear and interesting, statistical methods are scientifically sound, and results are adequately described. I only have a few comments:</p> <ol style="list-style-type: none"> 1. As far as I know, the “concentration index” is not so widely used to measure the degree of socioeconomic inequality. The authors cite some references (40–42), but they are quite old and I guess that many readers will not know how this measure actually works. I suggest that the authors spend some words on how this index is calculated from individual level data. 2. Instead of a linear regression model over mortality rates and C indexes, I would carry out a log-linear regression, which is useful to detect possible nonlinear relationships and expresses the direction and magnitude of the association in terms of “annual percent change” (APC). Decreasing trends in mortality and socioeconomic equality somewhat slow down over the study period (see, for instance, the C index for alcohol related mortality among younger men), so results from a log-linear regression would be more significant than those from a linear regression. Further, the authors describe mortality decline in terms of percent change instead of absolute difference (page 7, 2nd paragraph), which is indeed a better characterization of trends over time. 3. On page 10, paragraph 2, what the authors say is adequate and somewhat easily conceivable, but no reference is provided in support of their statements.
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	<p>4. On page 10, the sentence “A study by Hoffman [...] mortality by age” is unclear—differences between younger and older adults in equality trends are driven by worsening health? Please clarify.</p> <p>5. In my opinion, this study has a major limitation that is never mentioned. While it is stated that “individual-level data on socioeconomic indicators” (page 2) were used, only income bracket actually has been collected and analysed. Income is known to be insufficient to track socioeconomic status, as other individual and contextual factors contribute to it, such as education, occupation and where the patient lives. Please include this as a study limitation in the Discussion section and in the “Strengths and Limitations” subsection on page 2.</p> <p>6. In general, no study limitation is adequately addressed in the paper. Another pitfall that the authors only sketch is that, given the study design, inferences to be drawn from temporal trends are inevitably tentative and somewhat speculative.</p> <p>7. Different Y-axis scales can lead to data misinterpretation. If possible, I suggest using the same scale for younger and older age groups (Figures 1 vs. 2 and 3 vs. 4).</p>
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REVIEWER	Katrien Vanthomme Vrije Universiteit Brussel
REVIEW RETURNED	18-May-2018

GENERAL COMMENTS	<p>This is a very interesting article, well-written and very clear. It contains all important parts of a scientific article. The authors give a clear overview of the literature, the study aims, methods and findings.</p> <p>Major comments: Since this paper is about socioeconomic inequalities in mortality amenable to health care and health policy in Finland, I think it is crucial to inform the readers about the health care system in Finland. How does it work? Is it highly accessible? Is it a universal, pay-for-fee system,...? This kind of knowledge is crucial to interpret the results.</p> <p>Furthermore, I would like to ask the authors to think about intersectionality: what about differences by age, gender or migrant background? The first two are analysed, yet to my opinion, differences by age or gender are not discussed thoroughly. Is it possible to include differences by migrant background as well?</p> <p>Minor comments: - p. 3, line 30: conditions instead of condition. - p. 3, line 42: variations instead of variation. - p. 4, line 2: write IHD in full spell before using the abbreviation. - p. 7, lines 5-17: I would suggest providing a table for these figures.</p>
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REVIEWER	Bjørn Heine Strand Norwegian Institute of Public Health, Norway
REVIEW RETURNED	20-May-2018

GENERAL COMMENTS	<p>This is a well written paper on trends in income inequalities in cause specific mortality in Finland during 1992-2013 for adults aged 25-74 years. The paper uses high quality Finnish individual based registry data, which is adequately analysed and interpreted.</p> <p>Aiming at estimating the impact of health care on the mortality inequalities, mortality is grouped into causes "amenable to health care intervention". The idea is that with adequate health care these deaths would not occur. This is a fairly strong assumption, but this method has been applied in numerous papers in a similar way. Furthermore, the paper includes two more sets of causes of death groups: "causes preventable by 1) health policy measures and 2) behavioural interventions". The first group consists of IHD and the latter of alcohol related deaths. This Choice of causes to represent these rather broad concepts could possibly be justified better. IHD could possibly also be a marker for the latter Group? And alcohol related for the former? Thus, the links between these concepts and the selected causes seem rather weak. That said, the results themselves are interesting, especially those for alcohol related causes. Inequalities in alcohol related causes increased substantially during 1992-2013, especially among younger adults.</p> <p>Smoking related deaths were not investigated. I believe this would have strengthened the paper further to include this as a separate group addressing the "behavioural interventions" not only by alcohol related deaths.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1

The study by Lumme et al. aims to investigate trends in socioeconomic equality in mortality amenable to health care and health policy interventions in Finland over two decades (years 1992–2013). The authors found that improvement in mortality—especially among younger adults—differed between socioeconomic groups. The paper is well framed: the study aim is clear and interesting, statistical methods are scientifically sound, and results are adequately described. I only have a few comments:

1. As far as I know, the “concentration index” is not so widely used to measure the degree of socioeconomic inequality. The authors cite some references (40–42), but they are quite old and I guess that many readers will not know how this measure actually works. I suggest that the authors spend some words on how this index is calculated from individual level data.

Our response: We have now added more references which are more recent. Additionally, we have described how the concentration index is estimated using aggregated data.

2. Instead of a linear regression model over mortality rates and C indexes, I would carry out a log-linear regression, which is useful to detect possible nonlinear relationships and expresses the direction and magnitude of the association in terms of “annual percent change” (APC). Decreasing trends in mortality and socioeconomic equality somewhat slow down over the study period (see, for instance, the C index for alcohol related mortality among younger men), so results from a log-linear regression would be more significant than those from a linear regression.

Our response: We agree with the reviewer that it is better to model also non-linear relationships between the outcome variables and time. Thus we have conducted additional statistical tests to find the most appropriate models for the changes. In most of the cases, piecewise polynomial models yielded the best fits for the associations. However, the improvements compared to linear regression were not always significant. The changes in mortality rates were highly significant ($p < 0.001$) when modeling linear or non-linear associations. The changes in C were in some cases more significant when modeling non-linear associations compared to linear. Since C can have negative values we did not model log-linear relationships due to difficulties to interpret to translated and transformed values of C.

We have now also added average annual percent changes (AAPC) for those mortality categories where the relative changes were biggest.

Further, the authors describe mortality decline in terms of percent change instead of absolute difference (page 7, 2nd paragraph), which is indeed a better characterization of trends over time.

Our response: We have now replaced relative changes in mortality rates with absolute changes as suggested by the Reviewer.

3. On page 10, paragraph 2, what the authors say is adequate and somewhat easily conceivable, but no reference is provided in support of their statements.

Our response: We have now reformulated the chapter and provided references to the chapter as indicated by the Reviewer.

4. On page 10, the sentence “A study by Hoffman [...] mortality by age” is unclear—differences between younger and older adults in equality trends are driven by worsening health? Please clarify.

Our response: We have now clarified the sentence as indicated by the Reviewer.

5. In my opinion, this study has a major limitation that is never mentioned. While it is stated that “individual-level data on socioeconomic indicators” (page 2) were used, only income bracket actually has been collected and analysed. Income is known to be insufficient to track socioeconomic status, as other individual and contextual factors contribute to it, such as education, occupation and where the patient lives. Please include this as a study limitation in the Discussion section and in the “Strengths and Limitations” subsection on page 2.

Our response: We agree with the reviewer that the association between socioeconomic position and health outcomes is complex since the relationship between different socioeconomic positions can be divergent. Income does not cover all the dimensions of the socioeconomic position, but it has been found to be most directly associated with material resources and wealth. There are some advantages in using income as a measure of socioeconomic position over occupation and education. Income describes relatively accurately individual's material living conditions which are found to be one of the most important factors affecting health related outcomes and is therefore perhaps the most effective single proxy for overall level of disadvantage (Duncan et al., 2002). Moreover, income is sensitive to changes. Whereas education is a rather rough measure of socioeconomic position and does not change much during adulthood. Categorisation of occupation differs between countries. In Finland, for example, the most commonly used indicators of occupational social class do not include an inherent ranking and thus it cannot be used in using concentration index as an inequity measure. We have now discussed the use of only income as a measure of socioeconomic position in the Discussion.

6. In general, no study limitation is adequately addressed in the paper. Another pitfall that the authors only sketch is that, given the study design, inferences to be drawn from temporal trends are inevitably tentative and somewhat speculative.

Our response: We have now added discussion concerning the limitations of the amenable mortality method as indicated by the Reviewer.

7. Different Y-axis scales can lead to data misinterpretation. If possible, I suggest using the same scale for younger and older age groups (Figures 1 vs. 2 and 3 vs. 4).

Our response: In Figures 3 and 4 we have used the same scale for younger and older age groups, but different scales for men and women due to larger confidence intervals among women. In Figures 1 and 2 we have decided to keep different scales for younger and older age groups since the levels of mortality rates are so different that using the same scales would complicate observing the changes in the levels of mortality rates among younger age groups.

Reviewer: 2

This is a very interesting article, well-written and very clear. It contains all important parts of a scientific article. The authors give a clear overview of the literature, the study aims, methods and findings.

Major comments:

Since this paper is about socioeconomic inequalities in mortality amenable to health care and health policy in Finland, I think it is crucial to inform the readers about the health care system in Finland. How does it work? Is it highly accessible? Is it a universal, pay-for-fee system,...? This kind of knowledge is crucial to interpret the results.

Our response: We have now added a paragraph concerning the health care system in the Introduction section as indicated by the Reviewer.

Furthermore, I would like to ask the authors to think about intersectionality: what about differences by age, gender or migrant background? The first two are analysed, yet to my opinion, differences by age or gender are not discussed thoroughly. Is it possible to include differences by migrant background as well?

Our response: We agree with the Reviewer that intersectionality is an important framework for studies aiming to unravel the complex associations between aspects of social structure and especially how gender, age, and ethnicity intertwine with other social hierarchies. However, the Finnish legislation does not allow registers to contain information concerning ethnic background or migrant status and they could therefore not be analyzed. Additionally, we feel that gender differences or differences by age and gender are beyond the scope of this paper as that would have required an approach more focused on detailed analysis of interactions between different social groups. Further, differences in socioeconomic equality between men and women are rather small and thus we did not discuss these differences in detail. This does not obviously imply that the intersections between gender and other markers of social status would be irrelevant in Finland either.

Minor comments:

- p. 3, line 30: conditions instead of condition.
- p. 3, line 42: variations instead of variation.

- p. 4, line 2: write IHD in full spell before using the abbreviation.

Our response: Corrected as suggested by the Reviewer.

- p. 7, lines 5-17: I would suggest providing a table for these figures.

Our response: We have added a table as suggested by the Reviewer.

Reviewer: 3

Reviewer Name: Bjørn Heine Strand

This is a well written paper on trends in income inequalities in cause specific mortality in Finland during 1992-2013 for adults aged 25-74 years. The paper uses high quality Finnish individual based registry data, which is adequately analysed and interpreted.

Aiming at estimating the impact of health care on the mortality inequalities, mortality is grouped into causes "amenable to health care intervention". The idea is that with adequate health care these deaths would not occur. This is a fairly strong assumption, but this method has been applied in numerous papers in a similar way. Furthermore, the paper includes two more sets of causes of death groups: "causes preventable by 1) health policy measures and 2) behavioural interventions". The first group consists of IHD and the latter of alcohol related deaths. This Choice of causes to represent these rather broad concepts could possibly be justified better. IHD could possibly also be a marker for the latter Group? And alcohol related for the former? Thus, the links between these concepts and the selected causes seem rather weak. That said, the results themselves are interesting, especially those for alcohol related causes. Inequalities in alcohol related causes increased substantially during 1992-2013, especially among younger adults.

Our response: We fully agree with the Reviewer that the neither IHD deaths nor alcohol related deaths can be strictly designated to one group alone. However, policy interventions are a strong measure to control alcohol related deaths in a country with relatively strict distribution and price policies and IHD deaths have in earlier research consistently been decomposed into parts considered to be attributable to health behavior and those attributable to health care interventions (the IMPACT-SEC model). We have now amended the text to make this clear.

Smoking related deaths were not investigated. I believe this would have strengthened the paper further to include this as a separate group addressing the "behavioural interventions" not only by alcohol related deaths.

Our response: We agree with the Reviewer that adding smoking related deaths would have strengthened the paper. However, a part of these deaths is likely to be lung cancer related, a part IHD related and a part are deaths due to COPD. As both IHD and COPD have other causes than smoking as well and we could not separate the proportion of smoking related deaths in each age and income group these could not be analysed in the current study. We have now added discussion concerning this in the discussion section.

VERSION 2 – REVIEW

REVIEWER	Maria Pia Fantini Alma Mater Studiorum – Università di Bologna, Italy
REVIEW RETURNED	09-Jul-2018

GENERAL COMMENTS	Well done, I have no further comments.
REVIEWER	Katrien Vanthomme Vrije Universiteit Brussel
REVIEW RETURNED	10-Aug-2018
GENERAL COMMENTS	I want to thank the authors for their effort and time to the revision. My concerns on the original draft are now resolved. Therefore, I think this draft can go to the next step for publication.
REVIEWER	Bjørn Heine Strand Norwegian Inst of Public Health
REVIEW RETURNED	11-Jul-2018
GENERAL COMMENTS	I have no further comments.