Title: Stability and variability in income position over time: exploring their role in self-rated health in Swedish survey data

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Author’s response to reviews: see over
Response to reviewers

Referee 1:

Thank you very much for your helpful comments. We considered most of your recommendations in the revised version of the manuscript.

Please find our responses below along with the changes in manuscript. A revised version of the manuscript with highlighted changes is enclosed at the end of this document.

Major Compulsory Revisions

1. Citation of references: Several references were not suitable. P3: McDonough and Berglund 2003 should be numbered. P4: TA Blakely, BP Kennedy, R Glass and I Kawachi should be Blakely et al. JM Mellor and J Milyo should be Mellor and Milyo. P10: GA Kaplan, SJ Shema and CM Leite should be Kaplan et al. M Benzeval and K Judge should be Benzeval and Judge. TA Blakely, BP Kennedy, R Glass and I Kawachi should be Blakely et al. O Lundberg and J Fritzell should be Lundberg and Fritzell.

The citation styles were changed. Some of the phrases without suitable references have been deleted.

E.g. page 3, line 27:

A study by McDonough et al. [7] based on US survey data found increased mortality rates for middle-class individuals who had experienced substantial income losses. For low-income earners, a sharp income decline did not exceed the mortality risk they already had. For high-income earners, no significant increases in mortality risk were found [7].

Earlier studies have almost consistently shown that increasing income is more beneficial for health than falling income [8]. The extent to which income increase influences health is covered only weakly in earlier studies. In comparisons of stable income with increased and decreased income, the health implications are by far less coherent. In a longitudinal survey from New Zealand, Gunasekara et al. [9] found a very weak increase in self-rated health for those with noteworthy income increase of more than NZ$10,000. Only those with low income at baseline and income increase had notable health benefits. Empirical evidence regarding the role of lagged income effects has mainly been provided on the basis of community-level analysis [1]: Investigating state-level inequalities and their lagged effects in US federal states, Blakely et al. could show that income inequality up to 15 years beforehand demonstrated significant associations with self-rated health [10].
The somewhat inconclusive findings resemble a previous study that explored changes in income over time by using an earlier wave of the same data: Lundberg and Fritzell [18] found an association between income change and health with more plausible associations for men than for women.

2. Data and methods: Analytical methods should be mentioned more clearly in the last part of the Data and Methods. Also, ethical issues should be mentioned.

*We added some information regarding methods to the data and method section (page 5, line 3 to 5).*

*With respect to the quasi cross-sectional data and the categorical nature of the variables of interest, the analysis was conducted using logistic regression. In one of the analytical models, OLS regression was additionally applied. All calculations were performed in Stata 11.*

*Ethical issues were not stated because the used data set and descriptive study design do not require an ethical approval. However, we added a note that we obtained permission for using the data (page 4, line 37).*

*Access and permission to use data was given by the LNU Research Group at the Swedish Institute for Social Research.*

3. P8, Model 1: The authors described that “For women, two years in income position below median increases...”. But Table 1 did not show so.

*The statement was corrected (please see page 7, line 23).*

*After three years with low income the estimates increased once more, and remained relatively stable at a high level. For women, five and six years in income position below median increases the association with poor SRH significantly.*

4. P8, Model 2: The authors described “an 81 percent higher probability”. But the Odd ratio did not directly mean the probability.

*We agree with you and replaced “probability” by “risk” (page 8, line 8).*

*Men with at least one period of income increase have an 81 percent higher risk of ending up in poor SRH compared to the reference group.*
5. P10, 2nd Paragraph: The authors mentioned that the critical stages were the first year and after three years. But according to Table 2, I understood the first year and after four years were critical.

Indeed, the phrase was not correct. A revising the section, we dropped this statement.

6. P11 L 10: (? not sure this is what you want to say?)??

The sentence was a left over and not supposed to be there. We have deleted the sentence.

Minor Essential Revisions

7. P3: The first sentence should be accompanied with reference(s).

A reference was added to page 3, line 3.

Extensive research has been dedicated to demonstrating the adverse health consequences of low income position; however, much less attention has been paid to income dynamics and lagged effects of income [1].

8. P5, self-rated health: The authors should mentioned “bad” and “between” were categorized into “poor” health. And the tables’ title said “less than good”.

We agree that we used inconsistent labels. We have replaced “less than good” by “poor” SRH.

Tables 1 to 4.

9. P9: The sentence of "Generally, cross-sectional analysis underestimates the flexible ...“ should be accompanies with reference(s).

A reference was added to page 9, line 5. The formulation has been adjusted slightly.

Generally, cross-sectional analysis underestimates the flexible nature of income and conceals that the aspect of time within the association between income and health might be potentially important [6].

Discretionary Revisions

10. Results of Abstract: A few quantitative results such as OR (95%CI) could be
shown: e.g. 0.22 (1.22-3.55) for men.

*We decided to keep the abstract as before (without OR’s) as we feel that the report of estimates does not suit to the rather general report of findings.*

11. Table 1: Quintiles did not have equal distributions. The higher had larger subjects.

*Indeed, the unequal distributions across the income groups require some explanation: We calculated the income quintiles on the basis of the total survey sample (N=5142) as it is random sample representing the Swedish population. The age restriction of our using sample then resulted in these unequal income groups.*

*This way of coding is justified by the assumption of the income inequality hypothesis – assuming that individuals in our study population relate at least to some degree to the overall income distribution. In other words, we have classified our sample according to the national income distribution.*

*We added a sentence in method section that explains the uneven distribution (please see page 5, line 26).*

*The income quintiles that show the mean level income over time refer to overall income distribution and were therefore calculated on the basis of the total sample.*

12. Table 2: I wonder why this model did not include the disposal income quintile.

*The used income variable in Model 1 (Table 2) was based on disposable income. An additional variable of disposable income would likely result in over-adjustment and collinearity in the model.*
Referee 2:

This paper is an interesting attempt at exploring various dimensions of income change, and I appreciate the authors' attempt to get the most out of combining cross-sectional questionnaire data with register data on income. The authors are well aware of various themes related to income change, and are trying to address many overlapped questions. This, however, I think is also the major drawback of the paper. I feel accuracy and clarity in terms of sketching out the conceptual setting of the paper as well as discussing interpretations can be strengthened. I have a few questions about the analyses as well, which are related to some degree of unclarity around the aim of these analyses. Finally, since data on other dimensions of socioeconomic position is limited, I feel somewhat more attention to possible competing explanations, particularly related to labour market position, is warranted.

Thank you very much for your helpful and constructive comments. We highly appreciate your critique and recommendations. By considering most of them, we have hopefully reduced weaknesses and improved the manuscript.

Please find our responses below along with the changes in manuscript. A revised version of the manuscript with highlighted changes is enclosed at the end of this document.

Major revisions
1. My main critical point is that I feel I did not get a clear enough impression on why the authors are interested in income change (as a determinant of health). I could think of three, to some degree different, justifications: 1) to test whether the income-health association could be causal; 2) to study a possible hypothesis of income change as determinant of health 'in its own right', e.g. that experiencing income loss (or too much fluctuation, etc.) is (equally) bad for one's health regardless of the level of income; or, 3) to study the health-effects of social mobility and change in socioeconomic position, in general. I think these three might lead to slightly different choices in analysis strategies, and the discussion of interpretation may have different emphasis. Presently the manuscript also discusses additional interest on cumulative disadvantage and long-term effects of low income. I feel it would be helpful if the authors could clarify their intentions in this respect.

Thank you for this comment and suggestion: Indeed, our intentions and overall aims of the study could have formulated more clearly. All of the three potential justifications of the study that you figured out apply to our study. However, the major aim of the study was to show that income change as such may be an important determinant of health. We therefore included a sentence at the end of the instruction stating the general aim of the study (page 4, line 20).

The present study hypothesizes that the individual’s current income situation is crucial for health, and further that income changes per se contribute to
social inequalities in health. The specific aim of this paper then is to explore short and longer-term consequences of individual income position on health in Sweden, and further, to account for income variations over a six-year period by combining longitudinal income information with self-rated health from a cross-sectional survey.

For studying the effects of social mobility, we would have chosen other measures (e.g. occupation, employment histories).

We deleted the paragraph on cumulative disadvantage on page 3 as we think it does not contribute that much to our approach. Also, the terms regarding “accumulation” have been removed from the discussion section.

2. One methodological concern regards the distinction between 'absolute' and 'relative' income: to my judgement, absolute income and relative income are merely two different ways to scale the income data, (often, moreover, the same data) and there are not very good justifications to regard them simply as measures of two different mechanisms through which income is assumed to affect health. It is not self-evident why it makes sense to compare results across differently scaled income variables. For this reason, I think it would be helpful if the authors could state more clearly how the use of different scalings in the analyses contributes to interpretation.

Indeed, measures of absolute and relative income usually refer to the same income variable recoded into different scales. However, as the vast of studies on the relevance of relative income suggests, the way of scaling income is important and may lead to different conclusions and interpretations. A justification has been included on page 10, lines 14 to 24.

The study uses one income variable; however it was scaled in different ways throughout the presented analysis. The contrasting findings between absolute income and rank position indicate that the type of scaling used on the income variable is important with respect to the health outcome. For example, previous studies on the causal implications of income on health have shown that the method of scaling income may lead to different conclusions regarding health [25]. The income variable modeled as absolute income draws on material conditions of the individual. Whereas rank positons in income distribution acknowledge the role of social comparisons with significant others, and thus emphasize the psycho-social dimension of income inequality [26, 27]. In practice, however, both types of scaling widely overlap which makes it cumbersome distinguishing between the absolute and relative aspects of income position [28].
3. I think adding some discussion on how overall income development in the entire study population may have affected results is warranted. This is particularly because how a measure of position on the income distribution relates to quantity of income depends on the overall income development.

\textit{We agree that the overall income development in Sweden might have affected our results. A brief explanation was already included in the limitation section. We further added some explanations clarifying how the used measures of absolute and relative (rank) income position were affected by the overall income development. Please see the changes on page 10, line 25 to 31.}

The overall income development in the study population may have influenced the presented results. In particular the findings based on absolute income follow the trends of the overall income development on the population-level. The findings based on relative rank measures, however, are less prone to fluctuations in income on the national level and are more influenced by changes in the income distribution. The compression of the income distribution affects the relative distances between lower and higher income groups and thus the rank positions in the income distribution [3, 29].

4. One of the consequences of the above considerations with regard to analysis I think is the question whether the income-level measure and the several income-change measures end up measuring what they are assumed to measure. In all models, the authors are building both the income-level and the income-change measure on data from the same period – so income-change is analysed from a place where, presumably, the effect of how much income a person has has been removed. Taking into consideration that the data is on individual income, one could ask if an income-change variable adjusted for income-level is then actually measuring employment? I think the paper needs a bit more careful consideration on how confounding from changes in labour market position or other factors related to changes in work life may have influenced the results. (I wonder whether it is helpful to keep the limitations of the study as a separate section; maybe it would be more useful to discuss interpretations and alternative interpretations side by side?)

\textit{Given the used measures and type data, we cannot certainly rule out that confounding has occurred. Indeed, income change likely overlaps with changes in the employment status. That a variable of employment status was not available is certainly a drawback of the study; the same time, the inclusion of employment could lead to over-adjustment of the model (Blakely et al 2004). We added some considerations in the revised manuscript on page 10, starting with line 32.}

\textit{Not to be downplayed, the rise and fall of individual income over time is primarily a consequence of preceding changes in the individual’s employment and labor market position. In the presented analysis,}
confounding from employment status may have occurred and affected the findings because an appropriate control variable was not available in the data material. Previous studies adjusting the income-health relationship for employment showed that magnitudes in poor health become attenuated [30]. However, it has also been reasoned that the adjustment is problematic as employment status can be regarded as a proxy for health. Poor health status is a major cause for interrupted work biographies and non-active labor force [31]. In the same study it was further argued that employment and income belong to distinct causal pathways, with income accounting for material living conditions and employment capturing aspects of health behavior. We therefore regard the omission of employment status as tolerable.

We think that the study design demands a separate limitation section. We therefore kept the limitations in its previous form.

5. I would like to propose to the authors that they consider testing whether using the income data from 1990-1994 as the 'level-adjustment', and that from 1995-1999 for the income change measures, will lead to different results. This treatment would result in an income-change variable that is also measuring for amount of income. Essentially, of course, which is better depends on the exact aims of the study.

This is an interesting suggestion. We performed the test according to your suggestion with income levels based on the years 1991-1994: The income gradients based in income levels during the years 1991 to 1994 is somewhat weaker, but still notable. The coefficients of the income change variable are slightly more pronounced throughout all models and both income change measures.

Given the similar results compared to our current approach, we decided not to present the adjustments from the earlier period. Further, as already mentioned in the previous version of the manuscript, we somewhat distrust the income histories from the early 1990ies as Sweden went through a severe economic crisis during that time. Using income information from that period may lead to potential problems and biases in the results.

6. I am not sure if adding in same model the effects of a person’s average level of income during the study period and the number of years income was below the overall median income (of the study population during the entire period) is completely justified, particularly when average income has been categorised. To my judgement, these variables both measure for the person’s average income level, and it is not easy to judge how they overlap. I think these can be two different models. Furthermore, I think the variable measuring time in income
position below median is somewhat difficult to interpret in and of itself because
the overall median across all time points is somewhat fuzzy reference point in
case median income is rising across time. Perhaps this measure can be defined
in terms of median (or perhaps 60% of median?) for each calendar year
separately.

The variable used in Model 2 was actually based on medians calculated separately for each
calendar year. We added a brief explanation on page 5, line 32.

The median was calculated separately for each calendar year.

7. I think we need a table showing the effect of income level on self-rated health
without any income-change variable in the model. This is crucial for the readers
ability to judge how the income-health relationship overall in these data compare
to the relationship reported among other populations and other data. Presently it
is not possible to tell whether the somewhat inconsistent gradient in the effect
level of income among men is due to income-change variables in the model. In
contrast, I wonder whether showing all results with and without adjustment for
education is useful; perhaps only adjusted results could be reported.

The associations of income-level with the health outcome are included in the tables now
(Table 3 and 4). We further removed some unadjusted models and adjust for educational
attainment throughout all models. Consequently, and in order to save space, we rearranged
the tables showing all “income change” variables in a single table for men and women.

The analysis of income slopes is now included in Table 4/ Model 5.

Please refer to Table 3 and 4.

Minor revisions
8. There are a few passages in the paper where there seems to be some
confusion related to the terms absolute and relative: in the seventh paragraph of
discussion, the authors state that “an increase in relative income position does
not necessarily correspond to an increase in absolute income”; this statement
can only be true if the population average of income decreases over time – which
I think is not the case for the studied population. (In fact, the low number of
persons with decreasing absolute income is most likely related to a rising
average income among the study population as a whole.) The degree to which
measures of position on the income distribution (i.e. relative income) and income
measured in units of money (i.e. absolute income) potentially show different
changes is to a large degree determined by how those measures are constructed
by the researchers. For this reason, broad statements like the above seem
uninformative. Likewise, in the fourth paragraph of introduction, the statement
that “it has been shown that absolute income is more important than relative
income”, seems in same way uninformative – the evidence that this passage
refers to is perhaps about the shape of the income-health association, and how
that affects effects found for differently scaled predictor variables? Lastly, the authors refer to the analyses in Table 2 as relative changes in income position. I wonder whether this is accurate: the measure is actually a fixed percentage unit change on cumulative distribution of income; to me that would read as absolute change in relative measure – but I think it would be best to call this measure change in (rank) position on the income distribution (and leave the ‘absolute’ vs. ‘relative’ terminology out).

We changed the terminology and replaced “relative income” by “rank position”. However, in the introduction and discussion section we still refer to the dichotomy “absolute/relative income” as it represents the standard terminology in literature.

Page 7:

In a set of models, we accounted for short-term income change and income mobility within a six-year period and the subsequent outcomes for SRH. The modeling strategy distinguished between changes in absolute income and changes in the relative rank position in the income distribution, which led partially to fairly different results. Both approaches revealed that a substantial loss of income at one or more occasions during the past six income years is associated with an increased risk of poor health (more for men than for women). Inconsistent rank positions in the income distribution yielded increased estimates of poor SRH for men, whereas inconsistent absolute income disclosed a higher prevalence of poor SRH only among women.

We also deleted the phrase about “an increase in relative income does not necessarily correspond to an increase in absolute income...” in the seventh paragraph of the previous version as we feel that is does not contribute to the discussion.

We also dropped the statement on the effects of absolute/relative income in Sweden in the instruction (on page 4 in the previous version).

Discretionary revisions

9. The authors use quite many different terms in relation to income change, including income dynamics, income stability, income persistency, and income volatility, and it is not always entirely clear for the reader what is meant by each of these. It would be helpful to explain even very briefly what is meant by the terms used. Particularly the second paragraph in the introduction is quite challenging to follow. Partially this is also because there seems to be a ‘conceptual leap’ from cumulative advantage theory to dose-response relationships in determinants of health, which is not very easy to follow.

Indeed, we used many different terms that are probably confusing. As there does not seem to be a standard terminology regarding “income change” in the literature, we used a variation of terms in the previous version of the manuscript that was obviously not helpful.
In order to meet your recommendation, we reduced the number of different terms for income change. We also replaced “volatile” by “inconsistent” which more properly describes the respective category in the income change variable.

Consequently, we also changed the title of the manuscript.

A brief definition of “income dynamics” has been added to page 3, line 3.

The primary reason why income dynamics (i.e. downward and upward trends in income) are relatively unexplored is the common application of cross-sectional data, which are naturally limited when dealing with trends over time.

The term “dose-response relationship” was intended to describe the “temporal” gradient of income position on health. However, we deleted the section on accumulative effects and dose-response relationships in order to avoid misinterpretations.

10. I wonder if the description on previous studies in the introduction could be streamlined a little? I appreciate that the authors are trying to describe very exactly what kinds of income-change measures have been used in previous studies, but the text is fairly confusing to read, particularly as some of the authors’ statements about previous research end up being conflicting with each other (in the fourth paragraph of introduction). Furthermore, I think that changes in area-level income inequalities are a different issue from changes in individual income, and are better left out of the discussion here.

Previous income research has debated whether context-level effects of income inequality on health just reflect the individual level associations. Our impression is that this puzzle is still not solved. We mainly intended to show here that previous studies have identified lagged effects of income on health. Following the stream of research that holds context-level effects as consequence of individual level effects our conclusion is then that lagged effects of income exist. We kept the statement but rephrased the sentence (page 3, lines 38-42).

Empirical evidence regarding the role of lagged income effects has mainly been provided on the basis of community-level analysis [1]: Investigating state-level inequalities and their lagged effects in US federal states, Blakely et al. could show that income inequality up to 15 years beforehand demonstrated significant associations with self-rated health [10].

We deleted two paragraphs in the introduction section in order to streamline the introduction section (page 3 – the section on cumulative effects; page 4 – section on lagged effects and regional levels).

11. I think there a couple of passages where the structure of the paper could be
cleaned up: at the moment results section contains explanation of methods (third and sixth paragraph of results), and the discussion starts with too long description of data and methods (in the first two paragraphs).

_We changed the structure according your recommendations: The description of results in discussion section was removed and is included in the result section now. Correspondingly, the methodological description in the result section was moved to the “Data and method” section._

Page7, Line 17:

_The median threshold was chosen since the analysis with alternative cut-points produced similar magnitudes, but also showed rather inconsistencies in patterns over time. Therefore, only findings below median income were shown._

12. The second, third and fourth sentences in the sixth paragraph of discussion seem unclear and it would be helpful if these could be revised.

_We revised the in particular the second part of the discussion section. We hope that the discussion is more clear now (please see our revisions on page 10, starting with line 14)._  

_The study uses one income variable; however it was scaled in different ways throughout the presented analysis. […]_