Reviewer's report

Title: Persistence and variability in income: exploring their role in self-rated health in Swedish survey data

Version: 1 Date: 10 October 2014

Reviewer: Akseli Aittomäki

Reviewer's report:

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.

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.

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.

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.

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?)

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 results 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.

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.

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 of 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.
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).

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.

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.

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).
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.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.