Author's response to reviews

Title: Inequality in oral health related to early- and later-life social conditions: a study of elderly in Norway and Sweden

Authors:

Ferda Gülcan (Ferda.Gulcan@iko.uib.no)
Gunnar Ekbäck (gunnar.ekback@orebroll.se)
Sven Ordell (sven.ordell@lio.se)
Stein Atle Lie (Stein.Lie@iko.uib.no)
Anne Nordrehaug Åstrøm (Anne.Aastrom@iko.uib.no)

Version: 2
Date: 7 November 2014

Author's response to reviews: see over
Thank you for the review of the manuscript “Inequality in oral health related to early- and later-life social conditions: a study of elderly in Norway and Sweden”. We are grateful for the valuable comments from the reviewers. We have revised the manuscript according to their suggestions. Please find attached the revised manuscript. The response to reviewers’ comments is detailed below.

On behalf of the co-authors

Sincerely,

Ferda Gülcan
Referee 1- Roger Keller Celeste

Major Compulsory Revisions

Abstract:
1-There is no reference to analytical methods in the methods section, only in the results section (i.e. GEE). I think this is confusing.
We have added the analytical methods in the methods section.

Introduction:
2- Reference 13 does not clearly describe the social mobility hypothesis, with distinct trajectories (upwards-downwards) as an explanation. This, indeed, came in later publications from the same authors (e.g. Kuh JECH 2003; Hallqvist SSM 2004) and was operationally described by Mishra et al (IJE 2009). As I understand, some models may operate simultaneously, but not any combination. For example, if evidence supports that adult health is directly associated with downward trajectory (poverty in adulthood) and not upward trajectory (poverty in childhood), then it refutes that the critical period model.
We have inserted the reference suggested by the reviewer. Although all life course models were introduced in the introduction, the focus was on the critical period or the latent effect life course model in this study.

3- It seems there are two objectives in the manuscript: a) to examine if inequalities change with ageing; b) to test if early and later life SEP are associated with tooth loss and "quality of life" (OIDP). Considering that they have only a 5-year follow-up with tooth loss data from self-reported, I do not believe there would be changes beyond random error. I would like to see from authors why they think objective "a" is appropriate, I recommend keeping it for a longer follow-up.
We partly agree with this criticism. We do believe that there might be time modifications of the social condition-oral health relationships even if the follow up period was limited, especially in this age group where substantial changes in socioeconomic status may happen with retirement (age 65 in both countries).
We performed statistical analysis (GEE) to assess the change in this period, and the results showed some significant interactions.
We have also modified wording to avoid strong statements of change in social inequality over time.
Methods:
4- State what was the purpose of the surveys in both countries. It they had only descriptive objectives, representativeness is important.
We have stated the purpose of the surveys in the methodology section. We have also accounted for representativeness of the results by weighting (IPW) for non-response and loss to follow up.

5- It is highly discussable that gender, country of birth and marital status could represent socioeconomic position on theoretical grounds. At best, they might be an indirect measure of SEP. I would like to see some good reference for such a choice, but, in the end, I would get stick to the true SEP measure that you have (education and work). The dubious choice of early life SEP measures compromises the interpretation of the results because the pathways between, for example, marital status and tooth loss do not go completely through socioeconomic position.

We agree that the concept of SEP may not be appropriately used in this article. Rather the variables used are just indirect measures of the traditional SEP. Thus we have changed the concept and use social conditions throughout the manuscript instead of SEP. See also how we define the social measures used on page6 revised manuscript.

6- As you have dichotomized you outcome, have you tried to work with the incidence between 2007-2012 to get incidence OR? Did results change?
We did not work with the incidence in this study since we had measure of self-reported tooth loss that did not capture number of teeth lost. However we reanalysed our data with a change variable with (1) being those who moved from having all teeth to having tooth loss and (0) those with persistent all teeth across the survey period. The results of the analyses did not change the results of this study substantially.

Discussion:
7- Overall, discussion could focus more on the objectives and main results. For example, the first paragraph has a lot of interesting comments that do not discuss the main results. Other paragraphs have to some extend the same problem, as some discuss welfare policies that are not at issue. I suggest keeping only the essential parts related to your objectives.
We have tried to modify the discussion part in accordance with comments from reviewer. However, in interpreting the results we also find it important to provide information on the context of the present findings such as for instance time of retirement, factors associated with early and later retirement.
8-In regard to the life course results, I have a different interpretation. GEE did not incorporate incidence of tooth loss from 2007 to 2012; therefore, even accounting for autocorrelation, analysis were about prevalence. The fact that early life SEP (education) was associated with the outcome does not mean that it was associated with incidence of tooth loss in late life, as tooth loss could have happened in early life. This problem occurs in other publications with prevalence (see Peres et AJPH 2011 or Bernabe et all JCP 2011), as prevalence poses difficulties in disentangling early and late life effects.

We are not sure that we interpret this comment correctly. In this study we did not work with incidence as earlier mentioned. Otherwise see our answer to question 6 above. We are fully aware that adjusting for the repeated measurements of the outcome variable does not give us the incidence. The standard logistic regression was run for each survey year and country. GEE with repeated measures was used to account for clustering and to assess the interactions. We have short period of time and only two time-points. The correlation structure has little meaning in this situation.

9-In regard to the increase/decrease in the SEP gap from 2007 to 2012, results should be interpreted in light that only one coefficient was significant out of 24. Please, keep speculation about the significance of social network at minimum. You should also consider that, depending on how you dichotomized the outcome, you may get different trends (see Celeste, Nadanovsky, Fritzell, CDOE, 2011). Did authors performed sensibility analysis?

Thanks for the comment. We are aware of speculations about the significant interactions and try to avoid emphasizing those findings. However, the GEE revealed two significant interactions. No sensibility analysis was performed.

Minor Essential Revisions
Table 1: categories of "Tooth loss" are not clear. Does "All/almost" mean having all/almost teeth? The word "teeth" is in between the two lines.
We have corrected the tooth loss category meaning having all or almost all teeth present in Table 1.

Table 2: p-values are not clear. Do the represent comparison between years? Categories of some variables? Between countries?
The p values relate to the associations between social conditions and oral health indicators separately for each country and survey year. We have modified the heading of the table in order to make this clearer.
Referee 2- Marco Peres

Major compulsory revisions

This is an interesting manuscript addressing a relevant and timely issue. The manuscript is well written and the Introduction section clearly presents the research question and objectives. However, I think the authors should provide in the Introduction section the plausibility of the association between early life and oral health outcomes later on.

We think we have argued the plausibility with presenting the life course hypothesis of the latent effect life course model and referring to the literature that have provided support for this hypothesis (e.g. Poulton et al and Nicolau et al)

The main of shortcoming of the manuscript is the very short period of follow up (2007-2012) and, mainly, the use of participant’s education (higher education versus lower education) as a proxy for socioeconomic environment in early life. Education is the most stable socioeconomic position in the life span reflecting adulthood circumstances. Usually parents’ education is used as a proxy for early life socioeconomic environment. Could the authors justify this option or provide a reference to support it?

We did not have access to parents’ education as a measure of socioeconomic status in early life. We used participants’ level of education as a time invariant (stable) early life predictor referring, not to childhood, but to early adult life when most people achieve their final educational level that remains stable thereafter. In addition we used as early life social conditions; country of birth an aspect associated with the participants from their first day of life.

Why did the authors consider marital status as a SEP variable?

Marital status reflects social support and also wealth and economy. This indicator reflects non-work related social condition (Allin S et al. 2009). Due to comments also from reviewer 1 on this issue we have changed the SEP to social conditions throughout the manuscript. See also that we have commented upon the social variables used in the introduction page 6.
How the sample was selected?

The sample was not selected – all 65 and 70 year olds in the Swedish and Norwegian study regions were asked to participate each survey year. Thus the survey was more like a census.

Most of the variables used are binary regardless the use of large sample size; this may result in loss of information. Should you justify the use of such categorisation?

Binary variables fit the performed statistical analyses in terms of logistic regression and Generalized Estimating Equations. Many variables were originally categorical and assessed with few categories and thus it was most feasible to dichotomize.

Could you provide reliability values of self-reported number of teeth for Swedish participants?

We were not able to validate the measure of self-reported number of teeth in the Swedish study group as we are not in charge of that data collection. However, according to the literature self-reported number of teeth has been found to be closely related to the actual number of teeth (Pitiphat W et al. 2002). We have inserted a notion about this in the methods section of the revised manuscript.

Minor revisions

i) to shorten the results section

ii) refrain from using 95% CI and p values; they are in the tables

iii) refrain repeating what the readers can see in the tables

We have shortened the result section as suggested and addressed the methodological issues in the discussion part.