Author's response to reviews

Title: Educational inequalities in mortality and associated risk factors: German-versus French-speaking Switzerland

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Author's response to reviews: see over
Submission of a paper (revision): “Educational inequalities in mortality and associated risk factors: German- versus French-speaking Switzerland” (ID 1456818354303639).

Dear Melissa Norton

We believe that we could answer all points addressed by the reviewers. Following their suggestions we shortened the paper by focusing more strongly on mortality inequality patterns. Moreover, we have reduced the discussion of gender issues since it is not a central issue. A native English speaking epidemiologist has carefully corrected grammar and wording.

Our paper has been labeled as “high quality article with an interesting question, and appropriate data and methods” by one reviewer (Rana Charafeddine). This extensive review further improved our manuscript. We hope that this profoundly revised and much more concise version is now suitable for publication.

Sincerely yours

Matthias Bopp
Reviewer: Tony Blakely

We wish to thank Tony Blakely for his readiness to review as well as for his comments helping us to further improve our manuscript. The changes are highlighted with the MS word track changed mode in the manuscript.

This paper is interesting, but lacking in many respects. It promises an interesting comparison of mortality inequalities with parallel inequalities in risk factors, but then finds many reasons (often probably plausible) to dismiss non-consistent data as due to errors in the health survey. Thus, the paper inhabits an uncomfortable middle zone: it does not live up to its promise; and it is not a thorough methodological critique of the health survey data. For these reasons, I do not think it is publishable in its current form. It simply does not get together. Nevertheless, the mortality data is strong – and interesting. I wonder, therefore, if a paper focusing more on the mortality differences might be more appropriate. It would be even better if it could then be compared against health survey data that is corrected for many of the biases that are suspected in the current paper, or older health survey data that better allows for time lags. Or a shorter paper pared back to the interesting mortality differences by education might be interesting.

As mentioned in the manuscript, we recently published a paper that assessed variations in mortality in German and French-Speaking Switzerland [1]. This current manuscript focuses on corresponding educational inequalities. Our main problem was that the mortality and the health data stem from sources with completely different characteristics and corresponding data quality (mandatory complete registration vs. sample with self-reports and rather limited participation rate). We agree with the reviewer that the health survey data have shortcomings in many respects and that we should not overstate dissenting results. We thus now focus more strongly on the mortality inequality data and on the variation between the Swiss regions.

Another approach would be to use the 1992/93 health survey data as just one point of comparison, and also look at earlier health survey data, official statistics on tobacco consumption (if available), and historical considerations of the differences between FS and GS as possible determinants of the current variations in mortality inequalities.

The SHS 1992/3 was the first official national health survey conducted in Switzerland. There has been a more or less nationally representative precursor of this survey (SOMIPOPS, 1981/2) and a special survey in four small Swiss cities (NFP1a, 1977) we could also have used, but the number of participants (in both surveys n<3500 for our age span) was too small for robust analyses. However, these older surveys as well as the more recent national health surveys (1997, 2002, 2007) generally show quite similar risk factor patterns. We already stated this in the manuscript:

“However, comparisons with earlier (1977) and later (2002) assessments suggest that prevalence in many major risk factors remained quite stable in the Swiss population [2, 3].“

For example, prevalence of overweight and obesity between 1977 and 2007 were published previously [4] [5]. Moreover, in a paper which is in press, we could show that SES inequalities (measured by education, income and occupational class) remained fairly unchanged in men and women between 1992 and 2007 [6]. Besides overweight / obesity, also smoking prevalence did not substantially differ between
1977 and 1992 [2]. We thus believe that the SHS 1992 appropriately represents the population that provided mortality data for our paper. Of course, the inherent limitations remain (e.g. self-report, limited coverage, selection bias).

There are some minor issues:

• on page 8 it is stated that 10.51% * 11 = 115% difference across education. True, but if education is in years in the Cox Prop Hazards model, then the difference from least to most education will be $1.105^{11} = 2.99$ fold difference, or a 199% increase

  Thank you, this was indeed an error. We corrected this as follows:

  “In GS men aged 50-59 years, those with the lowest education (11 years of maximum education difference with 10.51% increase per year of education: $1.1051^{11} = 3.0$) had a three times higher all-cause mortality risk compared to those with the highest education.”

• there are several grammatical errors

  A native English speaking scientist has carefully read and corrected the entire manuscript.

• the Discussion is quite stilted in its flow.

  We substantially revised the discussion part. We are confident that it is now much more concise and reader friendly.
Reviewer: Rana Charafeddine

It is a high quality article with an interesting question, and appropriate data and methods. Below are few minor comments:

We thank Rana Charafeddine for the valuable comments allowing us to further improve our manuscript. The changes are highlighted with the MS word track changed mode in the manuscript.

Minor Essential Revisions

Abstract:
What does “cultural inequality difference” means? It is not clear.

We deleted this sentence

“Cultural inequality differences could help elucidating unexplained variations found in Europe.

Background:
The background section lacks a description of the characteristics of the two regions. Why do the authors suspect different SES inequalities patterns? Is it because of a different policy structures? different income redistribution patterns (more egalitarian society)? different poverty and unemployment patterns?

We hypothesized that the mortality and risk factor differences between the two regions found in our first paper could root in varying inequality in risk factors and mortality rates [1]. We did not state this clearly enough in the previous version. Regional differences in socioeconomic characteristics (see for example education in table A2) are rather small, and based on this, large variations in inequality between GS and FS cannot be expected. We are interested in the association between risk factors and mortality on the one hand and education on the other hand irrespective of absolute levels of socioeconomic characteristics.

We added the following sentences to the background section:

“Such variations indicate unexploited potential for reduction of health inequalities. Regional differences in socioeconomic characteristics are minor. Compared to the German-speaking part, in French-speaking Switzerland there is a slightly lower proportion of persons with intermediate education and a higher proportion with high and low education (see additional table A2). Economic and wealth parameters are comparable, except of a slightly higher unemployment rate in French-speaking Switzerland.”

Results:
P8 line 10: figure 1 (the figure should be numbered).

Done
General:
There are some typing errors in the manuscript. For instance: larger and not lager (p. 12 line 15). Please revise.

Done, the entire manuscript has been carefully read and corrected by a native English speaking scientist.

Discretionary Revisions

Methods:
As it is well specified in this article, daily alcohol consumption may also be a protective factor. Is it possible to recode the alcohol consumption variable in order to reflect alcohol abuse rather than solely use of alcohol? For instance you use binge drinking.

Unfortunately, the information about the amount (grams) of alcohol consumed in the Swiss Health Survey 1992 is only available from those who also filled in the written questionnaire (about 70%). In this smaller sample, only 2.7% (n=135) of persons reported that they had social problems because of alcohol consumption and only 3.4% reported a daily intake that exceeded 60 g of ethanol. These numbers do not allow valid analyses. In order to have the same n for all variables and because there was no substantial difference, we preferred to use daily consumption (larger sample) instead of amount in gram (smaller sample).

We already stated in the manuscript:
“Moreover, persons with hazardous alcohol consumption may more often refuse to participate in health surveys than moderate drinkers or abstainers. In our study, we used inequalities in daily alcohol consumption (yes or no). However, for a part of the participants (72%), the amount of ethanol consumed daily was available. A mere 3.4% reported daily intake that exceeded 60 g of ethanol, and 16% were abstainers. The inequality pattern resulting from alcohol consumers in the upper quintile (men >71 g, women >27 g) was similar to that of daily alcohol consumption (table 2) in men but not in women.”

and

“Hazardous drinking is only poorly captured by a questionnaire used in a health survey and reporting probably differs substantially, dependent on cultural and social norms [7].”

The authors have opted not to use the RII. Yet in the limitations of the study they present as a potential source of bias the different educational systems within the country. So, isn’t the RII a good way to overcome these intra-country differences in the educational system?

The reviewer is right in that the RII can help to partially overcome variations in educational systems. However RII’s are based on stratified data (often with collapsed educational categories) and thus do not allow for multivariate modeling of individual data. Since the differences in educational systems within Switzerland are small compared to between-country variations and only partially coincide with language regions, use of the RII for the comparison of FS and GS would be rather a disadvantage than an improvement.

We already stated in the manuscript:
“Due to the federal system of Switzerland, there are in fact not only one but 26 educational systems. However, these variations do only partially coincide with language regions and are much smaller than between countries.”

Results:
In the figure, the authors state that inequalities appeared to decrease with age. But also, it is interesting to note that regional difference appear to subside with age as well.

Based on the – merely descriptive – figure, a decrease in regional inequality differences in men with increasing age can be assumed. However, this does definitely not apply to women and the p-values for this difference in men remain statistically significant for all age groups. Thus, the assumption of a decrease of regional difference in inequality over age class appears rather speculative.

Discussion:
P. 11, paragraph three, there is a discussion about the higher mortality rate from CHD mortality between FS and GS in women and how this may relate to different risk factors. However, the difference in mortality inequality in CHD is not statistically significant between the two region (p=0.160).

Correct, we changed the text accordingly:

,, In contrast to smoking and obesity, alcohol consumption can also be regarded as a protective factor against CVD, particularly when consumed in a context of a healthy lifestyle [8, 9]. However, when comparing FS and GS, inequalities in these risk factors did not consistently fit to inequalities in CHD mortality.”

In the same paragraph, the sentence starting by “In accordance with our results” is not clear.

We deleted the sentence also in accordance with your legitimate remark about discussion of gender differences (see below).

P. 14:
It would be interesting to expand on cultural differences in reporting bias for risk factors in the limitations.

We added a reference and an additional sentence:
“Hazardous drinking is only poorly captured by a questionnaire used in a health survey and reporting probably differs substantially, dependent on cultural and social norms [10]. Besides alcohol consumption, also self rated health as well as reporting height and weight (influencing obesity prevalence) may strongly vary between cultures [7][11].“

General:
Sometimes, the writing style is confusing because the authors write in a comparative way without citing the group they are comparing with. As if it is assumed, but this is not clear all the time. For instance: Gradients in all cause-mortality were more pronounced in younger and middle aged men compared to? Here it is clear that it is older group, but still it needs to be mentioned. This is just one example.
We clarified several sentences accordingly.

Often, the authors compare SES inequalities between men and women. As the article is already rich, and the purpose is to compare between regions, it would be less confusing for the reader to reduce gender comparison to the necessary points solely.

Thank you for this valuable hint. We substantially reduced gender comparisons.

References