Reviewer’s report

Title: Falling behind: life expectancy in US counties from 2000 to 2007 in an international context

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Reviewer: Luc Bonneux

Reviewers report:

The authors determine US life expectancy at county level (2357 clusters) and document the well known large mortality disparities in the USA. They suggest that these are growing in recent years, although at the end, the authors drown in speculations and lose their main point. With the economic crisis, it is not unreasonable to expect this.

Introduction

(minor) This is a very ‘low’ level of analysis. The rationale provided in this paper is limited. In a previous paper of the same group (ref 8), the authors suggest they want to provide information on the distribution of mortality in the USA. This is the most valuable asset of this paper, to be explained more clearly.

Methods

(major) They use life table techniques, which are ‘data hungry’. Part of their methods are heavily relying on prediction models. I would ask a statistics reviewer to check the methods, as the parameters used for prediction come from the same dataset. This might cause a circular model. For larger counties, the observed should be compared to the predicted.

(major) The obvious question is ‘why use life tables and a technique that is statistically not parsimonious’. The good old standardised mortality ratio only needs total death counts and an age population distribution. It would give the same answer about the regional variance of mortality without the need for prediction models. A quick look at the CDC showed (old) maps of CMR (SMR, but directly standardised and statistically comparable). The CDC also documented the reason of the high mortality: high lung cancer rates, suggesting historical tobacco use as major cause of increased mortality.

(minor/major: I would suggest to greatly simplify the analysis and present the data as CMR – however, the authors might correctly argue that the life expectancy is more complicated but does the same thing) I agree life expectancies are more transparent for policy makers, but one might limit the calculations of life expectancies to a set of percentiles (0, 5, 25, 50, 75, 95, 100). The heavy “data crunching” is now overwhelming, and more confusing than enlightening.

Results

The comparisons with international life expectancy is elegant.
(Major) The top ten countries are not mentioned, neither are the life expectancies or how they are summarized over the ten countries. This should be part of the methods. Some of the usual top ten countries are very small, and contain wealthy and healthy ‘tax pensioners’. The results are then dominated by the large Japan.

(Major) The paper would benefit from comparing directly the national life expectancy of the USA with the top ten and Japan, UK and Canada (UK generally not in the top 10). Trends in the top 10 and in the USA should be shown.

(Major) If I disentangle the main message of this paper in “mean” and “variance” of mortality, the county levels does add nothing to this international comparison. We know that the USA is running behind (the mean), so are the large majority of counties running behind. I checked in the Human mortality database the trends in LE for the USA and the three comparison countries: Japan, UK, and Canada. The trend changes were not very different.

(Minor) It is worthwhile and enlightening to document increasing variability by using GINI coefficients (or other well known indices of disparity) for the considered countries. They will be far higher in the USA, documenting the large disparities formally.

Discussion

(major, simple to correct) A most important cause of regional disparities in life expectancy is not mentioned: regional migration. The rich pensioners go to the sunshine counties, the poor stay behind in increasingly impoverished regions. As mentioned before, a quick look at the CDC showed high heart disease and lung cancer mortality in the high mortality regions.

(Major, and a cause for rejection) The discussion is now many pages of speculation, not supported by this analysis or these data. The paper suddenly starts pushing for an authoritarian and bureaucratic prevention ideology. Imposing stringent health policies might not be welcomed by many US citizens, living in the land of the free (free, also to make unhealthy choices). Speculations about the disease-specific deaths attributable to all “non-optimal” levels of each risk factor exposure, extrapolations from not fully understood observations, are not supported by the principles of evidence based medicine, asking for documented evidence of interventions and policies. However, high mortality coincided with high lung cancer mortality (in the CDC maps), documenting still largely increased mortality by historical tobacco use. The main causes of the mortality decline in the USA are cardiovascular, which in turn are caused by better management of cardiac risk factors and better treatment of heart and other vascular disease. The central hypotheses of high mortality disparities boil then down to high tobacco use and poor uptake of evidence based treatment strategies by a weak primary health care and financial barriers in the uninsured.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable
**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

'I declare that I have no competing interests'