Some comments on the paper by Sipilä and Martikainen

In a recent paper, Sipilä and Martikainen examine the mortality differential between Swedish speakers and Finnish speakers in Finland. Having read the paper, we feel obliged to provide some comments related to the (poor) literature coverage, the conclusions made, and the general approach of the analyses.

For some reason, the authors neglect the results of a number of studies we have published, as they state that the most recent documentation on the language-group mortality differential extends only to the mid-1980s.

In the discussion, almost all interest is devoted to alcohol-related mortality, for which the largest relative difference between the language groups is observed. These causes account for only 13% of the total difference in age-standardized death rates, however. Cardiovascular diseases contribute to almost 60% and should have deserved at least equally much attention, particularly ischemic heart disease.

A factor that is very unequally treated, and only briefly mentioned in the introduction, is the potential role of hereditary factors. There exist no data that combine mortality records and information on genetics or biomarkers, but there are strong reasons to believe that the population development of the country underlies the persistent regional mortality differences. Several genetic and medical studies also support this view. Hence, one cannot disregard the possibility that hereditary factors and socially determined behaviours go together. Difficulties in explicitly measuring epigenetic mechanisms do not mean that they do not exist.

Our analyses show a notable effect of birth region, which is a variable that provides an even better statistical fit than current region of residence. We also illustrate that persons do not reduce their mortality risk by moving from high-mortality to low-mortality regions. This is true also for elderly, who have lived in the low-mortality areas for several decades.

In our opinion, the language-group mortality differential should be analysed and understood in the same context as the regional mortality differences. This is not, however, equivalent to just including region as a main factor in a multivariate model, like Sipilä and Martikainen do. Considering that practically all Swedish speakers live in the low-mortality regions, their approach is also methodologically inappropriate.

Finally, we question inference drawn based on analyses that attempt to grasp the whole age span, i.e. by including all people aged ≥30 in the same models. Causes of death differ greatly across age and factors that correlate with mortality are age dependent. For instance, a variable that reflects family structure, with categories such as ‘spouses without children’, ‘spouses with children’, ‘single parents with children’ and ‘living alone or others’, evidently measures totally different aspects in the lower and the upper parts of the age span. Since such a classification is applicable mainly to rather low ages, the present estimates must be close to non-interpretable. An evident solution would have been to split the age span into shorter intervals and conduct analyses on specific causes, with interpretable controls.

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