Reviewer’s report

Title: Potential gains in health expectancy by improving lifestyle: an application for European regions.

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Reviewer: Martin Mckee

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This paper begins by noting the growing attention paid to healthy life expectancy as a summary measure of population health. It takes advantage of the natural laboratory represented by Europe.

The authors take their relative risks from the DYNAMO-HIA model. For those unfamiliar with this model, it might be useful to add a sentence about how these were derived.

This study uses a limited number of risk factors, but they are some of the most important. Perhaps the authors could comment on what others might feasibly be studied in this way, given the limitations of data and information on relative risks.

An obvious limitation is the age of the data, with BMI from 2005. As these data were from Eurobarometer, they must be self-reported. This is something to discuss in the limitations (as they do for alcohol).

My greatest concerns relate to their alcohol data and analyses. The measure of alcohol consumption is based purely on quantity, and does not take account of pattern. Again, given the marked differences in drinking patterns across Europe, this should be discussed as a limitation, especially as the pattern of drinking scene in the Baltic states and Poland will be associated with a high rate of sudden death due to injuries and arrhythmias while that observed in central Europe, for example in Hungary and Slovenia, will be associated with much higher rates of cirrhosis. This has obvious implications for a study of healthy life expectancy.

Linked to this, the failure to identify a higher rate of alcohol consumption in Eastern Europe in the Eurobarometer data is a cause for considerable concern, given that almost every other study finds this. This leads me to think that perhaps some more caution should be expressed about this data source, which includes only 1000 respondents in each countries, and 500 and the smaller ones, and is known to be subject to considerable sampling bias in some. Similarly, the strong U-shaped curve seen with alcohol might also raise some concern, as it is now well established that this is largely due to a selection effect, as alluded to in the discussion.

While accepting the need for brevity, surely a little more detail is required on how they developed the odds ratios for health status. At least one of the data sources is longitudinal. Did
they use that property? I was expecting to see something more in the online material but it simply referred the reader to another report. Similarly, it was not at all clear how the relative risks for the three factors combined were estimated, as I would expect that this could be quite complex, especially, for example, with BMI and alcohol consumption where I can see lots of scope for confounding and non-linear interactions.

The reasons that certain countries were excluded from the analysis, shown in square brackets on page 9, will be obvious to European readers but not necessarily to others. A brief note might help.

The observation that the gender gap and healthy life expectancy is much narrower than for life expectancy is actually well known and explains, in part, the common finding of worse self perceived health among women in surveys, which is in part a function of survivorship bias.

Overall, this study has quite a few important limitations, in relation to the data sources, the estimation of relative risks, and the prevalence of different outcomes. As such, the findings should be interpreted with quite considerable caution. Nonetheless, given the material available to the authors, they have done as much as is reasonably possible in my view. Thus, given the high policy relevance of the paper, with healthy life expectancy high on the political agenda, I believe that this paper makes a sufficiently important contribution, albeit with many caveats. However, these caveats really should be spelt out in a little bit more detail, as should some aspects of the methods. This would also suggest that they may wish to frame the paper as showing what could be done with much better data, rather than relying on the figures produced here.

Minor point. LEPGH should be spelled out in full in the main text and not just in the abstract.

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I have previously published with 3 of the authors and was involved in the development of the DYNAMO-HIA model used in the study.

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