Reviewer’s report

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

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Reviewer: Roland Rau

Reviewer's report:

The article addresses a research question, which is of interest to science, individuals as well as the media: What can you do (on average) to improve your survival chances? Or: How many (healthy) life years can be gained by lifestyle changes.

The three lifestyles addressed are smoking, alcohol consumption, and being overweight / obesity.

As interesting as the paper is in itself, I think it requires some revisions and clarifications:

The main issue I see is already acknowledged by the authors: The rather negligible effect of alcohol on mortality (see Table 2). This is in stark contrast to what has been discussed in many papers, most recently probably by Trias Llimós et al. in IJE. [1]

As far as I understood, these risk factors were obtained from the DYNAMO-HIA model/software. It seems that those risk factors were not very good for the impact of alcohol on mortality. But how can we be sure that the risk estimates are better for the impact of other risk factors on mortality?

Many estimates rely on data from surveys (e.g., EU-SILC) as input. Thus, we may have some issues with sampling size for the used estimates. Could you please address this issue by showing how much your final estimates could vary due to the uncertainty of estimates derived from (potentially) small samples? Or, alternatively, say something about the sample sizes used for the risk estimates and point out that it would not affect the final estimates.

Throughout the text BMI, overweight and obesity are somewhat used interchangeably. For instance, in the introduction the authors write that "[...] BMI, smoking and alcohol are among the
most important risk factors ...". I would argue that BMI is not a risk factor in itself but rather being overweight or obese. I also think that the categorization of BMI needs to be improved. The lowest category seems to be "lower than 25". It is well known that mortality is highly elevated if BMI is below 18.5 (common threshold for being underweight). Obviously, there is a selection effect (or reverse causation): The reason for a relatively low BMI could be pre-existing health conditions, of course. But this would apply similarly to zero alcohol consumption.

Smaller issues:

- The authors write that "[O]dds ratios were adjusted for age, sex, ...". How did you adjust for age (categorical variable, linearly, ...)?

- Population data and death counts were used from two data sources. Why?

- The authors write that a "pooled model across all countries was used..." What kind of model do you refer to?

- At the end of the discussion section there is a "d" missing in "...compared to current drinkers."

- I think that the need for better data gathering on the European level is a good idea. The beginning of the sentence "We conclude that more research is needed" is not really necessary as this could be (and is often) written at the end of every research article. This does not add anything.

- In the list of abbreviations a "s" is missing in "Assessment"

- The authors mention it only briefly that "it was assumed that smoking, BMI and alcohol were independent." Maybe the authors could point out how this might be not correct and what kind of influence it may have on their estimates (no quantitative assessment, of course).

A suggestion (not necessary since no forecasts are made in the article):

As far as I know, and in contrast to the impact of smoking, the peak of the impact of the obesity epidemic on health and mortality has not yet been reached. Maybe the authors can speculate how this might affect their findings if they repeated it some time in the future. The authors might also
consider to discuss this in the framework of cohort effects (such as smoking) in their period mortality perspective.


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