Reviewer’s report

Title: The potential impact of food taxes and subsidies on cardiovascular disease and diabetes burden and disparities in the United States

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Reviewer: Lennert Veerman

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Given what we know about dietary risk factors for health, the higher rates of such diseases among people with lower socio-economic status, and the generally less favourable exposure of these groups, it stands to reason that fiscal measures to improve diet will tend to reduce socio-economic inequalities in health. The great contribution of this paper is that it proves this is indeed likely to be the case, and quantifies the reduction in inequalities for a wide range of the most important dietary risk factors. The authors use appropriate data and methods, and present the results in logical way.

I have not real points of criticism of this study, but only a few suggestions on the presentation and the discussion of limitations.

Presenting findings on health disparities can be done in various ways (for an overview, see Keppel Vital Health Stat 2. 2005) and can present quite a challenge. In this case, I wonder if Figure 2 could be explained in a bit more detail. The text informs us that the figures are adjusted for population size; perhaps that should be repeated in the footnote with the figure. And some further aid in the interpretation could be considered. For instance, a reduction of 100% would mean the rates were the same for the high and low SES groups?

The analysis is limited in the sense that it does not consider any substitution or complement effects. A recent study in the Australian context (Cobiac et al, PLoS Med 14(2): e1002232) suggests that these could potentially change the results considerably. The authors duly mention this limitation, however.

The study also does not take into account the fact that if people are prevented from dying from diet-related CMD, they will die of something else at a later age, creating a dynamic that is not incorporated into the present study; one that would change the absolute numbers of deaths reported. Life table-based analyses could deal with that and expand the range of impacts to include life years gained and reduction in disparities in life expectancy. However, I think it if fine to define that as being beyond the scope of the present paper.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes
Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
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I am able to assess the statistics

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