Reviewer’s report

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

Version: 1 Date: 16 May 2017

Reviewer: Justin White

Reviewer’s report:

The authors have satisfactorily addressed most of my concerns, although there are two issues that need further action. The first one in particular has implications for the entire analysis.

1) I am still concerned about the authors' choice of scenarios for estimating the elasticities by income group. This is one of the key inputs into their model. To recap their approach, in the first scenario, the authors rely on estimated differences between high-income countries and low-income countries. In the second scenario, the authors rely on estimated differences by income in Mexico, based on a single study. Given that the authors’ study is focused on the US, I believe that the authors need to rely on US evidence, where available.

I think that that the authors may have overlooked some of the available US-based evidence. The authors state in their rebuttal: "In Lin et al. (2011) [4], we could not find estimates for variation by SES." I just reviewed the Lin paper again, and the authors are mistaken. Table 3 has elasticity estimates for various beverage categories among low-income individuals. Table 4 has elasticity estimates among high-income individuals. These estimates are drawn from national data sets (NHANES and the National Consumer Panel) over a 10-year period. While I cannot say that these estimates are authoritative, they seem to me to be more sensible than the authors' use of a single study from Mexico.

There may be other studies as well. To be honest, I have only taken a cursory look. Park et al (1996) have estimates for fruits and vegetables, as did Raper et al. (2002), Huang et al. (2000), and Dong and Lin (2009). These are summarized in Table 2 of Dong and Lin (2009).

I am also not convinced that the authors adequately reviewed the rest of the literature. For example, the authors note that "Clements and Si (2015) [7] utilize international data from Cornelsen et al. (2015) [8], not US specific data." However, that study also used Green et al. (2013), which has a number of US-based studies, noted in that paper's supplement. Have the authors reviewed those studies for estimates? Powell et al. (2013) includes other US-based studies.

I suggest that the authors rerun their model incorporating US-focused estimates, and account for differences in the income gradient by product, rather than assuming just one estimate.
2) The authors state in their rebuttal: "We have clarified in the Methods that while many cross-sectional elasticity papers use income as a measure of SES, we used education as an alternative proxy for SES...." I remain confused about this issue. The results are reported by education level. However, the elasticity estimates are derived by income category. In Table 1, the authors had to make some unstated assumptions mapping their income elasticity estimates onto the education categories. How was this done? The text or supplement should have this detail.

References


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

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