Reviewer's report

Title: Biting off more than we can chew? The use and cumulation of evidence from modelling studies to inform policy on food taxes and subsidies.

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Reviewer: Lennert Veerman

Reviewer's report:

Introduction
As my work includes modelling the impact of fiscal interventions in the food environment, I cannot be considered an impartial, disinterested reviewer, though I tried to be objective. (Editor beware.)

General conclusions and recommendation
The issue is an important one, and the central message is a good topic of discussion among a broad public health audience. I did not find any factual errors, but a few statements are unsupported by evidence. I also question the logic in some of the examples. I rate the novelty as rather limited. It is not in all cases clear that the potential problems this article brings us, are in fact problems in practice.

My main recommendation is that the paper be expanded and possibly reframed as a review. This would place several of the arguments in a fitting context, and better support some of the assertions made (or not, depending on the findings).

Review
The central argument of the paper is that models are no more than preliminary explorations of the likely effects of different policy options, that their predictive power should not be overestimated, and that we need more evaluation of actually implemented fiscal interventions, which should be accompanied by rigorous evaluation. All of these points are well taken and assuming the target audience consists of policy makers, probably well worth making. First because it calls upon them to implement these potentially effective measures, and second to increase the chances that plans for evaluation are made and budgets made available. But since some of these interventions have been implemented, there could exist evidence on whether or not such rigorous evaluation was planned and executed. Perhaps the authors can search for such evidence; the findings would add salience to this paper. (Discretionary revision)

For modellers and other public health researchers the messages is rather self-evident; many if not most modelling studies and reviews in this area end with the same recommendations. For example, Eyles et al (ref 67 in the paper), which this paper criticises, conclude with “Robust evaluations built into the implementation of food pricing policies would help to answer some of these questions and engender confidence that such strategies will provide positive
effects on population diets and reduce the global burden of NCDs.”

The discussion section of the paper reviews rightly states that models are simplifications of reality, then makes the point that feedback loops or damping are typically not incorporated in the model structure. While true, I would argue that a good reason for doing so is a lack of evidence to underpin such complexities. Modelling this would require some rather heroic assumptions, and therefore these matters are probably better referred to as limitations in the discussion of modelling papers. Again, if the audience is policy makers, this point may be worth making, but if the audience is public health researchers, I would have appreciated an examination that showed whether or not modellers have indeed stated such caveats in their papers. (Discretionary revision)

The authors give the example of EU sugar policies potentially negating the effects of a tax on sugared products. I do not think this example is very strong. If the tax adds a percentage to the price, then the authors are correct that a lower price resulting from a change in EU policy would diminish the effect of the tax. In contrast, if the tax is implemented on a per quantity basis, the difference in outcomes between the two scenarios (i.e., with and without tax) would not necessarily be much different with the EU policy change compared to without. Neither is a good example of dampening, defined as “the capacity of systems to absorb and accommodate change” unless one considers the EU agricultural policy change to be a response to the tax. (Major compulsory revision)

“Model input parameters are also typically described by unique values, and the impact of uncertainty about parameter values on uncertainty in results is not typically addressed.” If true, this would be in contravention of the standards, such as those described in the Drummond textbook on economic evaluation [1]. But is this ‘typically’ the case? The authors do not provide any evidence. They should either review modelling studies to investigate this, or remove this assertion from the paper. (Major compulsory revision)

The section ends strong with an eloquent reference to “a view of simulation studies of food taxes and subsides as preliminary forays in an incremental, phased research process, intermediate in kind between analytic theory and empirical testing.”

Next, the paper theorizes that modelling studies are not amenable to statistical pooling in the same way observational studies are (under conditions). The paper would gain in strength if it would then refer to examples where such problematic analyses were done. The Eyles et al review only partly qualifies. That review does mention that “Where there were three or more studies within each major category that were sufficiently alike in terms of pricing strategy and outcome, findings were quantitatively pooled to produce a mean PE estimate.” However the ‘pooling’ seems to have consisted of calculating a weighted average of the central estimates and the reporting of ranges, rather than formal meta-analysis, and therefore the degree of uncertainty was not understated. I am not sure this amounts to the alleged logical fallacy. Would that not depend on whether the methods were determined beforehand? Eyles et al do not make this explicit, and no protocol was published prior to the analyses. Nevertheless, it does seem that
if three or more studies independently came to similar conclusions, unless they used the same data and methods, amount to stronger (or less weak) evidence than two or less such studies, which might justify some more attention in a review than the hundreds of other, but diverse, outcomes. (Discretionary revision)

The section on vote counting suffers from the same problem: nobody seems to have used this method, and that may well be because other researchers made the same considerations as are made in this article. If this section were part of a systematic review of possible approaches to quantitatively summarize the results of studies that model the effects of fiscal policies on health, it would add value. (Major compulsory revision)

The article ends with a call to policy makers to implement food taxes and subsidies and make sure they are accompanied by pre-planned rigorous evaluation. That point is well taken. The article could gain strength by outlining how this could be accomplished. What design elements should be taken into account? (Discretionary revision)

Lastly, as a public health researcher and modeller, I would not necessarily present modelling and intervention evaluation as mutually exclusive options, but rather stress the potential synergism, and encourage researchers with modelling expertise to be part of the evaluation team. The evaluation can provide empirical evidence for parameters and mechanisms that models have shown (or not) to be influential but uncertain, and the models can make inferences to go beyond what can be measured, such as the expected changes in disease frequency that can be expected as a result of an observed change in diet. (But of course this is not my paper, and this is therefore an entirely discretionary revision.)

The ‘Authors’ contributions’ section refers to MS and RN who are not in the list of authors, and may therefore have to be moved to the section on acknowledgements. (Minor essential revision)

Reference


**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I have no competing interests, other than having done some of the modelling this paper discusses.