Reviewer's report

Title: Obesity-related health impacts of fuel excise taxation- an evidence review and cost effectiveness study

Version: 0 Date: 20 Feb 2017

Reviewer: Philippe Giabbanelli

Reviewer's report:

The authors have performed a review and developed a model about the potential health benefits and cost savings that could be triggered by increasing taxes on fuel in Australia. The manuscript is well written, methodologically sound, and presents a useful research contribution. I commend the authors for having thought about most of the limitations; as I wrote my notes about limitations when reading the text, I was pleased to see them addressed in at least some form in the discussion. My suggestions are minor, and I believe that they can straightforwardly be addressed by the authors. I am hesitant to seeing them as 'discretionary' because some are simple yet essential. I have listed them below, in no particular order of priority.

1. I understand the focus on conservative results, and on relaxing some of that through sensitivity analysis. It's a good methodology. I am surprised by one assumption though: any uptake in physical activity was assumed "to have occurred in those already moderately or highly physically active". The logic would seem fine, which is that you're not modelling a big behavioural switch, but rather than those who do 'some' will do a little extra. Still, isn't a lot of the population sedentary? That is, isn't this assumption rather massive? I also find it odd to have such a strong assumption for the sake of being conservative, immediately followed by one that takes the other direction: assuming that people who're making an increased use of public transport will maintain it over the long-term.

2. The results under the (mostly conservative) base assumptions are small. HALYs are small, healthcare cost savings are small (particularly at the scale of the healthcare budget). However, it's politically not an easy intervention to take. The authors have acknowledged all that. They also found that results are highly sensitive to cross price elasticity and varying it to something still 'feasible' yields very different results: one order of magnitude above. I think this is important and should be conveyed in the abstract. As some may be tempted to skim through studies (particularly as they get as long as this one), the message should not be lost.
3. The authors acknowledged as a limitation that socio-economic groups were not considered. They rightly stated that such broad levies are strongly felt by low- and medium-socio economic groups. I am not an expert about the numbers in Australia, so I can only tell that in the countries I often work on, at least low SES groups are disproportionately affected by overweight and obesity. Given that there is a potentially strong association between SES and the health outcomes under consideration, I would flag the inclusion of SES as being a priority for future models.

4. The authors did consider two distances, 400 and 800m. They are ad-hoc but that's what we use by lack of anything better. I may have lost track of one aspect, so the authors are welcome to just clarify it in their response letter. In order to switch to active travel, it must be 'feasible'. That is, there should be access to public transportation (using that 400 / 800m) or direct access to the travel destinations. How realistic is that in Australia? Is there data that could be referred to showing a histogram of distance per inhabitants to public transportation for example? I work on countries where infrastructure "could be better" and in such a context, a population switch to AT is tricky.

5. Please clarify how the 2 reviewers worked. Did they both decide on which papers were eligible? Did they both extract the information? Were there conflicts between them, and some resolution process?

6. Why is 2010 the reference year? Seven years ago is starting to feel a bit borderline. I don't fully understand where the difficulty would lie in moving it to a more recent year, e.g. the national fuel excise tax can be brought closer to now, and then data is extrapolated to 2010 it may as well be pushed closer.

7. Please move Table 3 earlier in the manuscript. I was already looking for it on page 10, because that's when you start listing the model components and data sources.

8. The idea that data is available by making a "reasonable request" to the author has already been inadequate for several years when it comes to Modelling & Simulation (M&S). The standards for replicability are to upload all relevant information on a third-party repository (e.g., OSF.io) where it will be properly archived. I would expect the upload to include the Excel model with a brief overview on how to use it, for instance. Enforcing this requirement is important to have 'real science' where we can double check the calculation, and re-use the work to move the field forward.
9. The one-way sensitivity analysis is 'alright' but I think the authors can make a nice improvement here, with not that much more work (given that they can just change the input in the model). I'd suggest creating a heatmap with cross price elasticity as the x-axis, distance as the y-axis, and total HALYs as the colour; the same can be done with healthcare cost savings as colour. There are two reasons why this would be useful. First it will give us a lot more information about the response curve, which we really don't know much about currently. Second, it can make an important communication tool. If the authors wish to support the sort of intervention suggested in their paper, thinking about effective visuals would be an asset. This is a discretionary change as the paper is also valid without it, but can be stronger with it.

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