Reviewer's report

Title: Assessing the Effects of Alcohol Tax Policy on Alcohol-attributed Disease Mortality in Taiwan: A Geographic Analysis

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Reviewer: Lillian Remer

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1. The purpose of this paper is well defined though very limited. However, in the background the authors anticipate changes in “mortality rate to vary with geographical area depending on specific life styles or social circumstances.” (emphasis is mine). Instead of modeling changes in ADM before and after the tax increase in terms of socio-demographic characteristics, they seem to have aggregated the data to township and then done descriptive thematic mapping of the outcome measure.

2. The description of methods is NOT adequate.
   a. They give no descriptives for the 359 townships other than the average population – not even a range or standard deviation so unless one is familiar with Taiwan (I am not), it is difficult to know what they were working with. Stating that “Appropriate geographic variations (sic) in population densities and socioeconomic conditions in Taiwan can be captured at this spatial scale.” is not adequate for a scientific paper. They should provide a table of basic demographics describing the townships.
   b. If they geocoded the death records, what was the geocoding rate? Was geocoding in mountainous regions as accurate as in urban areas? Were ungeocoded records lost to the analysis or was geocoding done to the township and not to the point location?
   c. Table I gives the tax before and after, but what is the real cost before (e.g., imported alcohol would include the tariff as part of the cost but we would need to know what portion of consumed alcoholic beverages were imported and subject to the tariff).
   d. How rapidly is population growing in Taiwan? Is it appropriate to use only data from 2002 for both the before tax change and after tax change datasets or will that artificially reduce the pre-tax change DSR (through an overestimated denominator) and overestimate the post tax change DSR due to low population estimates?
   e. The spatial analysis is based on an unidentified distance (d). Is it possible this is an adjacency (since it seems to be 1 or 0 and the size of townships appears to vary greatly).
   f. It appears they used the High/Low clustering (Getis-Ord General G) tool in Spatial Statistics toolbox of ArcGIS but if so they should have had z-score and
p-values so it is unclear what the “Monte Carlo significance test” was about. And they don’t describe what or how they did their MC testing. Are “administrative tracts” smaller than townships? Could this mean they were doing a within township analysis?

3. Are the data sound? Probably. Further, it appears they have point located data which is rarely obtainable and very valuable in geospatial analysis. I hope the authors will further analyze this data to determine how much of the change is due to socio-demographics and whether any is due to the actual geographic location.  

4. Yes, they explain the data source

5. The discussion and conclusion are adequately supported by the data but they did not do more than a descriptive analysis and did not adequately describe what they did do (see Q2 above).

6. Yes, they discussed limitations, but I see the admission that townships are not appropriate for defining communities or cultural boundaries as very problematic, as is the admission that township as the unit of analysis is not appropriate for detection of community level differences. If they have point located data why was it not aggregated to the village level and perhaps do a Bayesian Poisson analysis?

7. Yes, they provide an adequate background though

a. I find it humorous that they would say “there have been no studies assessing the effects of alcohol taxes in national-scale geographic and demographic settings.” When Taiwan is only 13,951 sq miles and is not a country any more (it is technically part of China), and they seem only to have used population based death rates not “demographic settings.” Many of their cited U.S.studies are state or multi-state level population based analyses where the states are large than Taiwan. If they had addressed demographic or socio-economic differences within the country this comment might have been reasonable.

b. Some of their references are not restricted to chronic Alcohol Disease mortality. For example in both the Finnish and US studies, alcohol related traffic deaths and alcohol toxicity account for a significant portion of the alcohol related deaths.

8. The title adequately reflects the content though it is a bit misleading and is clarified by the abstract. For example if they were indeed analyzing Tax Policy, they would need to compare the impact of changes in import tariffs to changes in alcohol beverage taxes. Instead they compare a “before” based on tariffs with an “after” based on beverage tax.

9. The writing is acceptable but there are a few places where English is clearly not their first language. Presumably the editors will fix those.

Quality of written English: Needs some language corrections before being published
Declaration of competing interests:

I declare that I have no competing interests.