Reviewer's report

Title: Suicide rates and income in Sao Paulo and Brazil: a temporal and spatial epidemiologic analysis from 1996 to 2008.

Version: 1 Date: 19 February 2012

Reviewer: Ying-Yeh Chen

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Review for “Suicide rates and income in Sao Paulo and Brazil; a temporal and spatial epidemiologic analysis from 1996 to 2008

Major compulsory revisions --

1. One of the main concerns is the change in data quality across time and places. For the whole Brazil analysis, the upward trend in suicide over the study period was an artifact due to improvement in data quality, as the authors indicated. In figure 3C low suicide rates in some northeast regions (MA, BA and PE) may be also an artifact, as these low suicide rate regions showed clear boundaries corresponding to the boundaries of macro-regions. It’s very intriguing that findings were in opposite directions in different analyses in different regions. This may also in part due to poor data quality (poor data, low suicide rate, poor areas). Analyses may be better based on more recent, more reliable data, e.g. 2003 onwards. The alternative approach is to conduct sensitivity analyses using combined data of certified suicides and possible suicides (e.g. undetermined deaths).

2. Regional suicide rates did not seem to be sex and age-adjusted. Population structure may vary substantially across area and lead to bias in ecological analysis - poor areas, older population, higher rates.

3. Why did the authors restrict their analyses to compare suicide rates between three categories of areas, as they have income data for each area (figures 3B, 4B, 5C - confusing here, 5C but not 5B!)? They can carry out ecological analyses based on area-specific data, which would show higher variability than the three categories. But these ecological analyses should account for spatial autocorrelation (rates in areas that are closer to each other are more related), using models such as Bayesian hierarchical modelling.

4. The authors also had data for Gini and education and why they did not include these in their analyses? Their results suggest interactions between income level and income inequality.

5. An important limitation of the spatial analysis using SaTScan for area data is the assumption that all suicides ’concentrate’ in the centroids of each area unit - this is very unlikely in areas where the distribution of population is substantially unequal. This should be pointed out. In such case I feel SatScan should be better used in point data (i.e. the exact location of each suicide is known) or ‘very small’ area data. It’s also very strange that areas in the high risk cluster in Figure 4D
scattered across a large region with some areas geographically located within this cluster not classified as part of the spatial cluster - this should not happen as the 'circles' of SaTScan should cover all these areas.

6. In fact it's difficult to conclude on any spatial patterning of suicide based on figures 3C, 4C and 5B - estimates of suicide rates in small areas/populations are likely to be very unstable and some 'smoothing' approaches should be considered in small-area analyses. It is also crucial to report the mean/median population and suicide numbers (and perhaps their ranges) for the area units used in analyses.

6. page 12, paragraph 2, line 2,-- seem to mixed up Taiwan and China, the per capita GDP for Taiwan is quite high (around US20,000), categorize it as a developing country is not correct.

7. One minor issue - the year of census was not indicated.

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

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.