Reviewer's report

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

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Reviewer: Chi-kin Law

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Bando and colleagues investigated the influence of income on suicide rate of Brazil, State of Sao Paulo and City of Sao Paulo with the use of temporospatial analyses. They detected there has been a direct association between income and risk of suicide, but the two factors were inversely associated at the state level.

General comment

As suggested by Erwin Shneidman, suicide is a multifaceted event and that biological, cultural, sociological, interpersonal, intrapsychic, logical conscious and unconscious, and philosophical elements are present. I am not agreed with the use of income, as the sole indicator, to explain the variation of suicidal risk throughout the whole country. That would oversimplify the nature of suicide and, more importantly, present a wrong message to the stakeholders of suicide prevention. If the risk of suicide is always positively associated with income, does it imply the Brazilian government should take all possible measures in lowering economic development to keep every member in poverty as a universal approach for suicide prevention? And for selective approach, the government should devote more effort in providing social, medical and family services among the high income group in lowering their suicidal risk. In contrast, they can draw less attention on the poor, as their risk of suicide is always lower?!

In principle, all of these are politically unsound that would further widen the social inequality. The authors should rethink whether a higher income would heighten the risk of suicide or the increased suicidal risk was contributed by an increasing income disparity within the territory. Unless living in a Marxist regime, we can't expect every resident in a higher income district is rich person. For a poor person living in a higher income district, his or her psychological distress for suicidal behavior should be much greater than another person living in a low income region.

So, there is a need for the authors to re-clarify their research question in the manuscript. Making more money itself should never be considered as a risk factor of suicide, before the authors can provide more evidence in substantiating their argument.

Major compulsory revision
1. In page 5, it mentions the HDI is based on life expectancy, education, GDP per capita, and a development index (ranging from 0 to 1). What does it represent for? How to calculate HDI from these 4 factors?

2. The authors should clarify why it is necessary to calculate the risk ratio at the 3 levels: national, state and city. According to the last paragraph in page 5, it mentioned source of suicide data of Sao Paulo state and city were different from the national statistics. Are they comparable in terms of data completeness and reliability? There may be a problem, if the 2 data sources are not comparable with each other.

3. The study classified all geographical districts into 3 areas according to the income level. What are the criteria in defining these areas? Are they comparable at the national, state and city level?

4. Following the last question, the authors should also clarify whether there have been any changes in terms of zoning over the whole study period. I assume that, due to economic developing in Sao Paulo city, some districts should get richer or less socially deprived at the end of 12-year period.

**Level of interest:** An article of limited interest

**Quality of written English:** Not suitable for publication unless extensively edited

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests