

This paper investigates the socio-economic determinants of suicide looks using a macro panel data set for Canada. As the authors point out is an important topic within the area of public health. From an applied side, the empirics of this paper goes in the right direction taking into account the time series properties of the variable often employed in suicide research. Nevertheless, I am seriously concerned about the originality of the manuscript. While the paper reads okay, the empirical results do not reveal any evidences that are significantly different than previous findings in this area.

Contribution:

The main contribution of this paper is that previous suicide studies have ignored the properties of the time series employed in the empirical analysis. That is very good starting point. It is quite clear that very close attention needs to be paid to the time series aspects of the data used in international, national, and regional panel regressions of suicide rates. Perhaps certain restrictions will be found to be acceptable. Whether this proves to be the case or not, it is abundantly clear that extreme care and extensive testing is warranted in pursuing this avenue of research and it could be that the available data is simply not up to giving clear-cut results. Finally, when working with different datasets from different countries at different levels, it is not difficult to expect that we might see either positive or negative relations between macroeconomic conditions and suicide. Therefore, I do not agree with the conclusions section. The paragraph... “All three innovations should increase...” (page 10)

There are several issues that the author still needs to address:

(1) The author did not carry out an in depth literature review of the variables that are commonly related to suicide: inequality, income, female labour participation rate, divorce rate, age, etc. The author can refer to the studies done by Sen (1981), Trovato and Vos (1992), Lin (2006), Leigh and Jencks (2007), Chuang and Huang (2007), among others

(2) Most of the empirical work in this field is ecological in nature. A few papers have explored the relationship between economic variables and individual suicides. One of the main problems is the use of aggregate data. The authors does not point out in the discussion that there is not much available data and that there may be problems with using this sort of data. What the authors do not tell us what the problems are, or try to address them in any way. The aggregation problem is exceptionally problematic if we include control variables as the economic inequality proxied by the Gini index. This variable has been included as control in recent empirical work as cited later. In fact this paper does not explain the problem of using aggregate data. Obviously individual level data of some sort is available, if there is not enough, or it does not cover the area the author would like then I think that the author would be better advised trying to collect that data.

(3) Another limitation of this paper is that does not include any discussion about the theoretical framework. The authors should present the main economic theories of

suicide. For instance, Hamermesh and Soss's (1974, *Journal of Political Economy*, cited in the text) economic theory of suicide predicts that income level, age, and unemployment are important predictors of suicide. Mental health is represented here by suicide rates. In my view, the authors are estimating a health production function. Furthermore, there are unexplored issues as the role of income uncertainty on suicidal behaviour. It is an interesting theoretical exercise, but may be also of potential for empirical study using individual level data.

(4) Endogeneity might be an issue here. For instance, let us examine the relationship between employment and suicide. Poor labor market performance such as low earnings might lead to stress, depression and then to suicidal behavior. But there might be the other way around. Suicidal behavior is related to substance abuse and depression which are likely to have negative effects on employment by making it more difficult to concentrate at work. This might be the case of other variables (for example, income) included in the empirical approach. Therefore, an instrumental variables (IV) approach should be employed.

(5) How about other social and behavioral variables (inequality, alcohol and anti-depressants) and variables related to social capital?. The paper does not include these factors. So, the authors should re-estimate all models adding these social-behavioral factors.

(6) The starting point of the time series analysis is testing for the (non-) stationarity of the individual variables. It is possible to check the order of integration for each variable country by country. Alternatively, one can use panel tests to jointly test for all countries for either non-stationarity (the Levin Lin Chu (1993), cited in the text, tests are probably the most common) or for stationarity (Hadri's (2000) LM test, not cited in the text).

(7) There are clear problems of functional form and omitted variables. The functional form is not tested. In fact, the nature of the relationship between the variables is not discussed at all. In my view, an interesting approach to explore the non-linear impact of X on Y is to use Kernel regression estimator following Robinson (1988), see also Blundell and Duncan (1998) for details of this method.

(8) Using as dependent variable the suicide rate, all regressions should be weighted by the corresponding population. The authors are using adjusted or crude suicide rates? Where is the correlation matrix? One might for example worry about the correlation between divorce rates and GDP

(9) The paper should include a section called sensitivity analysis or robustness checks. With a sample of different states, why do not explore the presence of potential outliers?. I would suggest you to identify outliers the Huber (1981) procedure. Hadi (1992) yields similar results, except that generates a large number of outliers. I would encourage to the authors to spend some time on this issue.

On the whole, there are serious flaws in the paper and in my opinion it is not up to the quality standards of Health Economics. Therefore, I suggest that the manuscript be rejected.

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