Commentary: Political epidemiology, Republican presidents and dog food

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Political partisans in the USA, as elsewhere, regularly remind their less motivated compatriots that elections have consequences. Much law, moreover, protects the right to say or print virtually anything to motivate political participation. This combination often leads to lurid claims by advocates that their opposition embraces policies that kill the most innocent and vulnerable among us. Republican politicians, for example, regularly stiffen the backs of their supporters by pledging, if elected, to protect the innocent by curtailing a woman’s right to abortion. Democrats, on the other hand, whip up energy among their faithful by claiming that Republican refusal to implement universal health insurance leads to needless death of vulnerable populations. Following in this tradition, Rodriguez et al. make clear that they did not intend their analyses to discriminate between competing explanations of temporal variation in infant mortality. The authors, rather, engage in what they call ‘political epidemiology’—an enterprise whose products we should apparently buy not because we confirm our biases, but because they have been vetted by epidemiologists. I, even as a Democrat who has served in political office, am not buying and I doubt many serious epidemiologists will. To paraphrase an American political adage, if epidemiologists ‘bought’ claims of health effects for the same reasons we buy dog food, the grocer would have to pull this brand from the shelf.

I could criticize Rodriguez et al. for providing no theory and few specifics as to how and why Republican presidents presumably increase infant mortality. The political imagination, however, knows few bounds so a list of plausible mechanisms, including reduced funding for health care, impediments to receiving income transfers, and bad or unfair fiscal and economic policies would ensue. The facts that the population suffered many such stressors while Democrat served as President and that Democratic as well as Republican state governors pursue such policies with relish would not constrain the litany.

I could also puzzle over whether Rodriguez et al. should have used period or cohort infant mortality as their outcome; but choosing between these options requires knowing the theory that motivated the test and the authors provide none. ‘Political epidemiology’ apparently does not
need theory. A more useful line of criticism, therefore, would probably focus on the authors’ claim that they have discovered a ‘robust, quantitatively important association’.

All correlational tests of association essentially measure the degree to which observed variables differ, as predicted by theory, from the values expected under the assumption of no association (i.e. the ‘counterfactual’). These tests typically assume that the expected value of observed data is their mean. If, in other words, I had to minimize error in guessing the next value produced by a process I had observed over time, I would guess the mean of past values. This approach makes sense when the values I observed exhibited no patterns. Time series, however, often exhibit ‘autocorrelation’ in the form of secular trends, cycles and the tendency to remain elevated or depressed, or to oscillate, after high or low values. The expected value, or counterfactual, of such a series is rarely its mean. No one would guess the mean of prior values for the next value of a times series that, for example, trends up or down.

Strategies to derive counterfactuals for autocorrelated time series fall generally into two categories. The first, or Campbell/Stanley approach, estimates the counterfactual from values in a comparison population presumed subject to the same phenomena that cause autocorrelation in the test population. The comparison population should not, of course, experience the circumstance that we suspect affects the test population. In our case, we could regress infant mortality over time in the USA on the infant mortality in, for example, Canada and use the coefficient to estimate values in a comparison population presumed subject to the same phenomena that cause autocorrelation in the test population. The comparison population should not, of course, experience the circumstance that we suspect affects the test population. In our case, we could regress infant mortality over time in the USA on the infant mortality in, for example, Canada and use the coefficient to estimate values in the USA. Better yet, we could use several or many comparison populations because researchers can acquire presumably good-quality infant mortality data with little effort. In Popperian logic, the suspicion that Republican presidents somehow induce infant mortality would pass a refutation or falsification test if the observed values for the USA during Republican presidencies exceeded (judged by whatever prior level of evidence the antagonists in the controversy set) those expected.

The benefits of the Campbell/Stanley approach include ‘control’ not only of shared autocorrelation but also of unmeasured, indeed unsuspected, shared confounders that do not induce autocorrelation. The costs include that the approach will not detect, and, therefore cannot control, autocorrelation unique to the test population.

The second or Granger/Weiner approach estimates the counterfactual from ‘history’. This involves, essentially, identifying a line with three qualities. First, it minimizes the sum of the squared distances between it and the observed values of the time series. Second, it can be described mathematically so that a reasonably efficient coefficient derived from all time periods in the series can predict a value for any period in the series. Third, the model predicts the value at time $t$ only from values at time $t-n$ in which $n$ can take any value up the number of prior time points available. In our case, such a model would predict infant mortality from coefficients derived from all test years regardless of whether Republicans or Democrats pre-sided as president. If Rodriguez were correct, the model would over-predict when Democrats preside and under-predict when Republicans preside. If this pattern of over- and under-prediction emerged, again at a level set a priori by the antagonists in the controversy, the suspicion that Republican presidents somehow kill infants would have passed a falsification test. The benefits of the Granger/Weiner approach include that it will control all autocorrelation in US infant mortality whether or not shared with a comparison population. The costs include that it will not control the effect of unmeasured confounders that do not induce autocorrelation in the US infant mortality rate.

These approaches can, of course, be combined to realize the benefit of both. We could predict the US infant mortality rate from that in Canada and inspect the residuals of the model for autocorrelation. If we find any, we could add past values of US infant mortality to the equation and arrive at the counterfactual from both history and comparison.

Rodriguez et al. choose a Granger/Weiner approach. They, however, implemented the approach in a way I believe inappropriate for the data. They used a form of ‘spline regression’ typically explained with a ‘hinge’ metaphor. Simply, perhaps too simply, put, the method assumes that a time series exhibits a series of segments each of which has its own slope that defines a best fitting line for the points in that segment. ‘Hinges’ connect these segments and indicate when one sloping line ends and another begins. Properly setting the hinges allows fitting the correct line to each segment. A model that includes the correct hinges can predict a counterfactual for any point from the slope of the line segment in which the point resides. Researchers often resort to empirical searching for the number of hinges that most reduce the sum of squared errors (or some other metric of ‘fit’) from resulting line segments—a process that, by axiom, yields line segments.

I believe that the hinge metaphor does not apply to infant mortality over time. Concluding that these series exhibit line segments requires a very active imagination—one perhaps stimulated by the belief that presidencies induce segments. I suggest that if Rodriguez et al. were willing to implement Granger/Weiner logic by searching for and modelling autocorrelation, they should have not constrained the search to line segments. They should have used the Box-Jenkins approach that identifies and fits a large family of models including segmented lines as well as autoregression, moving averages and integration.
that they colour their argument with the authority of epidemiology, they should have applied a combination of Campbell/Stanley and Granger/Wiener approaches. Indeed, the epidemiological literature has included descriptions and applications of such combinations for decades.\(^5\)

I retrieved cohort infant mortality rates for the USA and Canada from the Human Mortality Database for the years 1950 through 2009.\(^3\) I regressed the US rate on that for Canada and used Box-Jenkins methods to identify and express autocorrelation in the residual series. I then used the methods of Chang, Tiao and Chen to determine whether the differences between observed and expected series showed hinges such as those implied by spline regression.\(^7\) Using a 95% (2-tailed) criterion, only one shift appeared in the 1965 to 2009 window—infant mortality shifted downward in 1971. Increasing sensitivity to detect shifts exceeding the 90% criterion yielded two more changes—a downward shift in 1981 and another in 1989.

I understand that I have entered into ‘political epidemiology’ and that readers, therefore, may not ‘take my word’ that the data show no association between presidential party affiliation and infant mortality in the USA. I, however, urge similar scepticism regarding Rodriguez et al.’s claim of a ‘robust, quantitatively important association’. As I note above, epidemiologists can easily access the methods and data needed to come to their own conclusions.

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References


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**Letter to the Editor**

**Rejoinder: Time series analysis and US infant mortality: de-trending the empirical from the polemical in political epidemiology**

**From Javier M Rodriguez,\(^1\) John Bound\(^1,2\) and Arline T Geronimus\(^1,3\)*

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In *US Infant Mortality and the President’s Party*,\(^1\) we analysed US infant mortality data from 1965 through 2010, and found that once one ‘filters’ out a smooth trend from the series, a persistent pattern emerges, with infant mortality rates being above trend during Republican Administrations and below trend during Democratic ones. As we noted in the article and detailed in its online appendix, this pattern is robust to whether we ‘filtered’ the trend using cubic splines, linear polynomials or fractional polynomials. The deviations from trend are visually evident and are easily detectable in the second stage regressions we estimated. Any claims about causality would be