Can We Assess the Effects of Revolution? A third look at the Cuban Evidence

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One is always pleased to have his or her work read carefully. My pleasure is doubled when the readers are economists and statisticians. But the evaluation by Pilarski and Snyder has a number of uses beyond providing these personal satisfactions. For one, it updates the Cuban findings to 1976. More broadly, however, it draws attention to two critical questions about doing research on revolution. First, is it possible to get good data in a revolutionary situation? Second, granting good data, to what extent are the observed changes caused by the revolution? I will address each of these questions in turn.

It goes without saying that reliable indicators of social and economic phenomena are harder to come by in a revolutionary situation. Certain common data-gathering procedures are difficult for researchers to apply. Further, data gathered routinely by government statistical bureaus may be spotty, inconsistent, or distorted for political purposes. A prime example here is annual Cuban GNP figures across the Batista and Castro eras. There are gaps in the series, the method of computation changes from the capitalist to the socialist government, and the figures beg to be altered for political ends. Hence, if one is interested in tracking an aggregate indicator of Cuban economic growth, a substitute should be found for GNP. As discussed in detail in my article (pp. 1139–41), strong reasons exist for using energy consumption as a proxy for GNP.

When a simple interrupted time-series (ITS) analysis is applied to energy consumption observations, the results show that the Cuban Revolution was followed by significant, positive economic growth in the short run but significant negative economic growth in the long run. Pilarski and Snyder question the finding of a significant short-run effect, arguing that it is probably caused by Castro's distorting key observations for political ends. They show that, when the Batista figures are substituted for these key years (1957 and 1958), this significant, positive short-run effect disappears. Thus, they conclude, the energy consumption measure is vulnerable to politically motivated manipulation and cannot serve as a reliable national economic indicator in a revolutionary situation. I disagree.

The Castro and Batista figures on energy consumption for 1957 and 1958 do in fact differ widely. The question is, why this difference? A definitive answer is of course impossible, but I will put forward a few guesses. The explanations can be grouped into two broad categories: "fudging" and measurement error. I would first like to consider fudging, defined as manipulation of a statistic for political purposes. With regard to the Cuban case, there are two basic possibilities: Castro fudged the 1957 and 1958
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figures, or Batista did. Pilarski and Snyder entertain only the first possibility. However, inspection of the data suggests that the Batista team may actually be the culprits. Look at the scatterplot provided by Pilarski and Snyder. The Batista figures for 1957 and 1958 deviate seriously from the straight line formed by the observations on previous years (fit a ruler to the circled dots, 1949–56). In fact, if we accept these Batista figures, we are forced to conclude that, during his last two years in power, the Cuban economy was doing better than ever! Given that this outcome is so unlikely, the implication is strong that Batista altered the data to achieve it.

These energy consumption data, then, can be made to appear compatible with the fudging hypothesis, although the fudging may have been committed by Batista rather than Castro. However, I reject this interpretation in favor of a measurement error hypothesis. There might be at least two kinds of measurement error operating: random measurement error and "revolutionary" measurement error. The deviations (from the 1949–56 line) represented by the 1957 and 1958 observations can be explained by the notion of revolutionary measurement error. That is, the revolutionary situation prevailing in 1957 and 1958 made it unusually difficult to get complete, accurate data. Therefore, the Batista energy consumption estimates of those years were very preliminary. Before Batista's statisticians could revise these estimates, Castro came to power. Castro's statisticians, benefiting from the more complete data finally in on 1957 and 1958, were able to make more precise estimates. Empirical evidence that the Castro estimates are more accurate comes from the fact that they are much closer to the projection of a straight line from the 1949–56 points. In other words, the Castro revisions merely reflect the more complete data which eventually became available. Milder instances of this revision process occur throughout the entire series; regardless of whether the year is during the Batista or Castro eras, the figures tend to be revised in later volumes of the statistical yearbooks, in keeping with the more complete information that becomes available.

Thus, with regard to energy consumption, Castro's statisticians did not fudge; they merely had better data. Such a conclusion is not as controversial as it may sound. We are not talking about measuring politically sensitive variables like GNP or sugar production. Neither officials, academics, nor the public regarded energy consumption as a central economic indicator. Undoubtedly, unheralded government bureaucrats went along quietly gathering data on energy consumption, much as they had always done. After all, if sugar production data were left unaltered by Castro's statisticians, it seems unlikely that they would alter the obscure energy consumption figures (even critic Mesa-Lago concedes there was no sugar data manipulation until 1965; my analysis suggests Castro never manipulated the sugar data; see p. 1133).
Because of the foregoing reasons, I must accept the energy consumption series as I originally compiled it (including the "Castro" data on 1957 and 1958). An ITS analysis of this series indicates that the revolution had a positive, significant, short-run economic effect. I might add that this conclusion squares with the general impression of many observers of the Cuban economic scene immediately after the revolution.

Granting the reliability of these energy data brings us to the second question, To what extent are the observed changes caused by the revolution? In seeking an answer, I estimated the ITS model discussed above. From a methodological point of view, a revolution seems an ideal "interruption" to assess, because it so resists the confounding influence of third variables. That is, revolution represents such a massive manipulation that any significant changes afterward would almost undoubtedly be attributable to it. However, as with any quasi-experimental design, the confounding potential of third variables must be considered. A prime candidate here is the U.S. embargo, which began at almost the same time. One way to handle the embargo is simply to regard it as part of the revolutionary interruption, arguing that a revolution involves internal and external changes, for example, a shift in trade patterns. Then, any observed effect is assignable to the revolution, but with the understanding that the revolution encompasses external as well as internal events.

Another way to handle the issue is to separate the internal aspects of revolutionary change from the external ones. A precise partitioning into internal and external revolutionary effects is problematic in the Cuban case, because the U.S. embargo began in 1960. Still, this means there is one year, 1959, when the Cuban revolution was operating free of an embargo. For that year, the first year of the revolution, energy consumption increased significantly from 1958, as my ITS analysis shows. This suggests that, at least in the short run, a domestic revolution produces a positive economic effect.

It is more difficult to separate unambiguously the long-run effect of the domestic revolution from that of the U.S. embargo, as I am at pains to make clear in my discussion (p. 1146). I nowhere imply that "the United States must shoulder all the blame for Cuba's lower postrevolutionary growth rate," as Pilarski and Snyder charge. Instead, I indicate that the U.S. embargo is one of the "external influences" operating and "may share responsibility for the drop in economic output" (p. 1146, italics added). I go on to state, "But, at about the same time the embargo was taking hold, important internal changes in Cuba were occurring, for example, the transition to a socialist state was being completed, and the Soviet Union was assuming a major role. Therefore, by the 1960-61 period, major variables potentially influencing growth are once again confounded" (p. 1146).

This confounding of internal and external variables is not overcome by
the procedure Pilarski and Snyder propose. They insert into the original ITS model a dichotomous variable, $X_4$, scored 0 before 1961 and 1 thereafter, in an attempt to separate the effects of the domestic revolution and the embargo (see their eqq. [5] and [6]). This attempt fails on several counts: (1) Given that the principal interest is in the effect of revolution free of the contaminating influence of the embargo, the more proper test is the change from 1958 to 1959, for there was no embargo at all during this period (this effect, of course, is what is estimated by $b_2$ in my original equations; see also, Pilarski and Snyder’s eqq. [1] and [2]). (2) This dichotomous variable assesses only a short-run effect, from 1960 to 1961, whereas the research question concerns the long-run effect of the embargo. (3) Inclusion of the $X_4$ variable poses formidable multicollinearity problems (recall that $X_2$ and $X_4$ are scored exactly alike except for two years, 1959 and 1960). Although Pilarski and Snyder do not present a correlation of these variables, I suspect it is high enough to make their estimates quite unreliable. Certainly, the appearance of large coefficients for $b_2$ and $b_4$ and the opposite direction of their signs hint at a multicollinearity problem.

In the end, Pilarski and Snyder are terribly pessimistic, concluding that my analysis of revolution has limited utility because of “the problems inherent in applying regression methods to small, nonexperimental data sets.” How, then, are we to study revolution? Survey research? Field experiments? Participation-observation? While these methods have their strengths, they also have obvious weaknesses in this context. Admittedly, the regression analysis of aggregate time-series data on revolution is not problem free, as this exchange of comments demonstrates. Nevertheless, the problems are those we routinely grapple with in nonexperimental social science research, for example, measurement error, spuriousness, multicollinearity. The occasional presence of such difficulties does not cause me to give up on regression analysis of nonexperimental data. I am sure that Pilarski and Snyder, as economists and statisticians, have not really given up on it either.

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