Econometric Modeling as Junk Science

By

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Originally printed in condensed form in the Spring 2002 edition of the *Skeptical Inquirer* as *Myths of Multiple Regression & Murder: Econometric Modeling as Junk Science.* Reprinted by the author's permission.

Do you believe that every time a prisoner is executed in the United States, eight future murders are deterred? Do you believe that a 1% increase in the percentage of a state's citizens carrying concealed weapons causes a 3.3% *decrease* in the state's murder rate? Do you believe that 10 to 20% of the decline in crime in the 1990s was caused by an increase in abortions in the 1970s? Or that the murder rate would have increased by 250% since 1974 if the United States had not built so many new prisons? Did you believe predictions that the welfare reform of the 1990s would force 1,100,000 children into poverty?

If you were misled by any of these studies, you may have fallen for a pernicious form of junk science: the use of mathematical modeling to evaluate the impact of social policies. These studies are superficially impressive. Produced by reputable social scientists from prestigious institutions, they are often published in peer reviewed scientific journals. They are filled with statistical calculations too complex for anyone but another specialist to untangle. They give precise numerical "facts" that are often quoted in policy debates. But these "facts" turn out to be will o' the wisps. Often before the ink is dry on one apparently definitive study, another appears with equally precise and imposing, but completely different, "facts." Despite their numerical precision, these "facts" have no more validity than the visions of soothsayers.

These predictions are based on a statistical technique called multiple regression that uses correlational analysis to make causal arguments. Although economists are the leading practitioners of this arcane art, sociologists, criminologists and other social scientists have versions of it as well. It is known by various names, including "econometric modeling," "structural equation modeling," "path analysis" and simply "multivariate analysis." All of these are all ways of using correlational data to make causal arguments.

The problem with this, as anyone who has studied statistics knows, is that correlation is not causation. A correlation between two variables may be "spurious" if it is caused by some third variable. Multiple regression researchers try to overcome the spuriousness problem by including all the variables in analysis. The data available for this purpose simply is not up to this task, however, and the studies have consistently failed. But many social scientists have devoted years to learning and teaching regression modeling. They continue to use regression to make causal arguments that are not justified by their data, but that get repeated over and over in policy arguments. I call these arguments the myths of multiple regression.

Five Myths of Multiple Regression

Myth One: More Guns, Less Crime.

John Lott, an economist at Yale University, used an econometric model to argue that "allowing citizens to carry concealed weapons deters violent crimes, without increasing accidental deaths." Lott estimated that each one percent increase in gun ownership in a population causes a 3.3% *decrease* in homicide rates. Lott and his co-author, David Mustard released the first version of their study on the Internet in 1997, and tens of thousands of people downloaded it. It was the subject of policy forums, newspaper columns, and often quite sophisticated debates on the World Wide Web. The debate followed predictable ideological lines, with one prominent critic denouncing the study as methodologically flawed before she had even received a copy. In a book with the catchy title *More Guns, Less Crime,* Lott taunted his critics, accusing them of putting ideology ahead of science.

Lott's work is an example of statistical one-upmanship. He has more data and a more complex analysis than anyone else studying the topic. He demands that anyone who wants to challenge his arguments become immersed in a very complex statistical argument, based on a data set that is so large that it cannot even be manipulated with the desktop computers most social scientists use. He is glad to share his data with any researcher who wants to use it, but most social scientists have tired of this game. How much time should researchers spend replicating and criticizing studies using methods that have repeatedly failed? Most gun control researchers simply brushed off Lott and Mustard's claims and went on with their work. Two highly respected criminal justice researchers, Frank Zimring and Gordon Hawkins (1997: 57) wrote an article explaining that:

> *"just as Messrs. Lott and Mustard can, with one model of the determinants of homicide, produce statistical residuals suggesting that 'shall issue' laws reduce homicide, we expect that a determined econometrician can produce a treatment of the same historical periods with different models and opposite effects. Econometric modeling is a double-edged sword in its capacity to facilitate statistical findings to warm the hearts of true believers of any stripe."*

Zimring and Hawkins were right. Within a year, two determined econometricians, Dan Black and Daniel Nagin (1998) published a study showing that if they changed the statistical model a little bit, or applied it to different segments of the data, Lott and Mustard's findings disappeared. Black and Nagin found that when Florida was removed from the sample there was "no detectable impact of the right-to-carry laws on the rate of murder and rape." They concluded that "inference based on the Lott and Mustard model is inappropriate, and their results cannot be used responsibly to formulate public policy."

Myth Two: Imprisoning More People Cuts Crime

The Lott and Mustard case was exceptional only in the amount of public attention it received. It is quite common, even typical, for rival studies to be published using econometric methods to reach opposite conclusions about the same set of data. In one exceptionally frank statement of frustration with this state of affairs, two highly respected criminologists, Thomas Marvell and Carlisle Moody (1997: 221), reported on the reception of a study they did of the effect of imprisonment on homicide rates. They reported that they:

> *"widely circulated [their] findings, along with the data used, to colleagues who specialize in quantitative analysis. The most frequent response is that they refuse to believe the results no matter how good the statistical analysis. Behind that contention is the notion, often discussed informally but seldom published, that social scientists can obtain any result desired by manipulating the procedures used. In fact, the wide variety of estimates concerning the impact of prison populations is taken as good evidence of the malleability of research. The implication, even among many who regularly publish quantitative studies, is that no matter how thorough the analysis, results are not credible unless they conform with prior expectations. A research discipline cannot succeed in such a framework."*

To their great merit, Marvell and Moody frankly acknowledged the problems with multiple regression, and made some suggestions for improvement. This, however, is more the exception than the rule with econometricians, who often become so immersed in their models that they lose track of how arbitrary they are. Many of them come to believe that their models are more real, more valid, than the messy, recalcitrant, "uncontrolled" reality they purport to explain.

Myth Three: Executing People Cuts Crime.

In 1975 *The American Economic Review* published an article by a leading economist, Isaac Ehrlich of the University of Michigan, who estimated that each execution deterred eight homicides. Before Ehrlich, the best known specialist on the effectiveness of capital punishment was Thorsten Sellen, who had used a much simpler method of analysis. Sellen prepared graphs comparing trends in different states. He found little or no difference between states with or without the death penalty, so he concluded that the death penalty made no difference. Ehrlich, in an act of statistical one-upmanship, claimed that his analysis was more valid because it controlled for all the factors that influence homicide rates.

Even before it was published, Ehrlich's work was cited by the Solicitor General of the United States in an *amicus curiae* brief filed with the United States Supreme Court in defense of the death penalty. Fortunately, the Court decided not to rely upon Ehrlich's evidence because it had not been confirmed by other researchers. This was wise, because within a year or two other researchers published equally sophisticated econometric analyses showing that the death penalty had no deterrent effect.

The controversy over Ehrlich's work was so important that the National Research Council convened a blue ribbon panel of experts to review it. After a very thorough review, the panel decided that the problem was not just with Ehrlich's model, but with the use of econometric methods to resolve controversies over criminal justice policies. They (Manski, 1978: 422) concluded that:

> *"because the data likely to be available for such analysis have limitations and because criminal behavior can be so complex, the emergence of a definitive behavioral study lying to rest all controversy about the behavioral effects of deterrence policies should not be expected."*

Ehrlich was not persuaded by these critics, and found flaws in their work. He remains a lonely true believer in the validity of his model. In a recent interview (Bonner and Fessendren, 2000) he insisted that "if variations like unemployment, income inequality, likelihood of apprehension and willingness to use the death penalty are accounted for, the death penalty shows a significant deterring effect."

Myth Four: Legalized Abortion Caused the Crime Drop in the 1990s.

In 1999, John Donohue and Steven Levitt released a study with a novel explanation of the sharp decline in murder rates in the 1990s. They argued that the legalization of abortion by the U.S. Supreme Court in 1973 caused a decrease in the birth of unwanted children, a disproportionate number of whom would have grown up to be criminals. The problem with this is that the legalization of abortion was a one-time historical event and there are too little data for a valid regression analysis. The results are likely to vary depending on how data are selected for analysis. In this case, as James Fox (2000: 303) pointed out: "by employing a single statistic summarizing change over this twelve-year span, [Donohue and Levitt] miss most of the shifts in crime during this period - the upward trend during the late 1980s crack era and the downward correction in the post-crack years. This is something like studying the effects of moon phases on ocean tides but only recording data for periods of low tide."

When I was writing this article, I included a sentence stating "soon another regression analyst will probably reanalyze the same data and reach different conclusions." A few days later, my wife handed me a newspaper story about just such a study. The author was none other than John Lott of Yale, together with John Whitley of the University of Adelaide. They crunched the same numbers and concluded that "legalizing abortion increased murder rates by around about 0.5 to 7 percent" (Lott and Whitely, 2001).

Why such markedly different results? Each set of authors simply selected a different way to analyze an inadequate body of data. Econometrics cannot make a valid general law out of the historical fact that abortion was legalized in the 1970s and crime went down in the 1990s. We would need at least a few dozen such historical experiences for a meaningful statistical test.

Myth Five: Welfare Reform Will Throw a Million Children into Poverty.

On August 1, 1996, as the United States Senate considered an epochal change in welfare policies, the Urban Institute issued a widely publicized report claiming to demonstrate that: *the proposed welfare reform changes would increase poverty and reduce incomes of families in the lowest income group... We estimate that 2.6 million more persons would fall below the poverty line as a result, including 1.1 million children*. (Urban Institute, 1996, p. 1)

Welfare advocates rallied around this prediction, but policy makers were not persuaded. Senators who supported the reform simply did not believe that social scientists could make valid predictions of that sort. And they were right. The Urban Institute could not even predict the direction of change, let alone its magnitude. Child poverty went down, not up, after the welfare reform.

The Urban Institute's model was much more complex than the other models we have examined in this paper, but the added complexity seems only to have compounded the problem. Using sophisticated "microsimulation" techniques, they took correlations that existed in the past, fed them into complex equations, then treated these equations as general laws. All their mathematics was based on the assumption that nothing fundamental would change, in which case, of course, welfare reform would fail. All the model did was produce numbers to illustrate their arguments and make them appear scientific. But the point of the reform was to change things, and it did.

Why Regression Fails

Although they seem complicated, regression models are actually simplifications of the real world. To simplify the mathematics, regression uses linear equations. This means it assumes that if you plot the relationship between any two variables on a graph, the trend will look like a straight line. Regression models also assume that variables are distributed according to a classic bell-shaped normal curve. And it assumes analysts know which variables are causes and which are effects. Of course, regression analysts know that the real world does not fit their assumptions, and they make various adjustments to the data to compensate. But the adjustments create other problems. The only valid way to test a model after all these adjustments is to show that it works to predict future trends. Regression models that have not been demonstrated to work with fresh data, other than the data used to create them, are junk science.

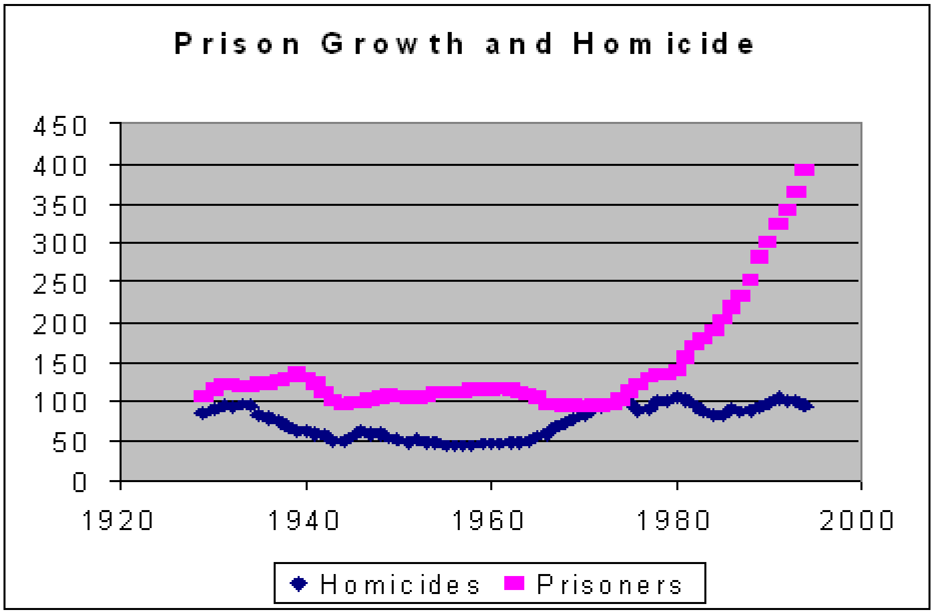
Why Regression Fails: Linear Models of a Nonlinear World.

When faced with the nonlinearity of the real world, the first instinct of the regression modeler is to standardize and control the data. In doing this, they minimize or eliminate the most interesting and important historical events. They end up analyzing a standardized and idealized world that bears little relationship to reality. For example, consider the trends in prison growth and homicide that Marvell and Moody (1997) sought to explain. Their paper begins with a graph showing trends in prisoners per 100,000 people and homicides per 1,000,000 people in the United States. This very interesting and useful graph is reproduced below from their data.

The interesting things about the trends the graph portrays are the turning points, the places where the trends diverge from linearity. Homicide rates increased sharply from the mid 1960s to the early 1970s, then leveled off. The number of prisoners shot up markedly beginning in the 1970s, as the United States built more prisons in response to the increasing crime rate. The homicide rate leveled off in the 1980s and remained stable thereafter.

Instead of trying to explain these important turning points, Marvell and Moody used multiple regression techniques to "control" for it. They introduced controls for every measurable variable they could think of, including (Marvell and Moody, 1997: 209) "age structure, economic factors, public relief, race, and variables marking World War II and the crack epidemic."

All these controls purged the striking historical changes from their data. This led them to the conclusion that a 10% increase in prison populations leads to roughly 13% fewer homicides. But a simple inspection of their graph shows that the promised 13% decline in the homicide rate for each 10% increase in imprisonment since 1975 simply did not occur.



Marvell and Moody were troubled that the expected reduction did not take place, but it was not enough to cause them to abandon their econometric methods. They, after all, were not discussing the real world but a world simplified and purified by a long series of mathematical adjustments. Confronted with the historical facts, they argued that, had imprisonment not increased, homicide would have gotten a lot worse. They went on, however, to observe that this would never really have happened because the government would have taken other actions to prevent it.

But what is the value of an analysis that leads to implications that the authors realize could never actually take place? How valid can the theory underling the multiple regression analysis be if it leaves out key variables, such as political constraints, simply because they cannot be quantitatively measured? How much do the results depend on arbitrary decisions about which control variables to introduce and how to measure them? Marvell and Moody were left with statistics that purported to tell us what might have happened if nothing that actually happened had happened.

Why Regression Fails: The World is Not a Bell-Shaped Curve.

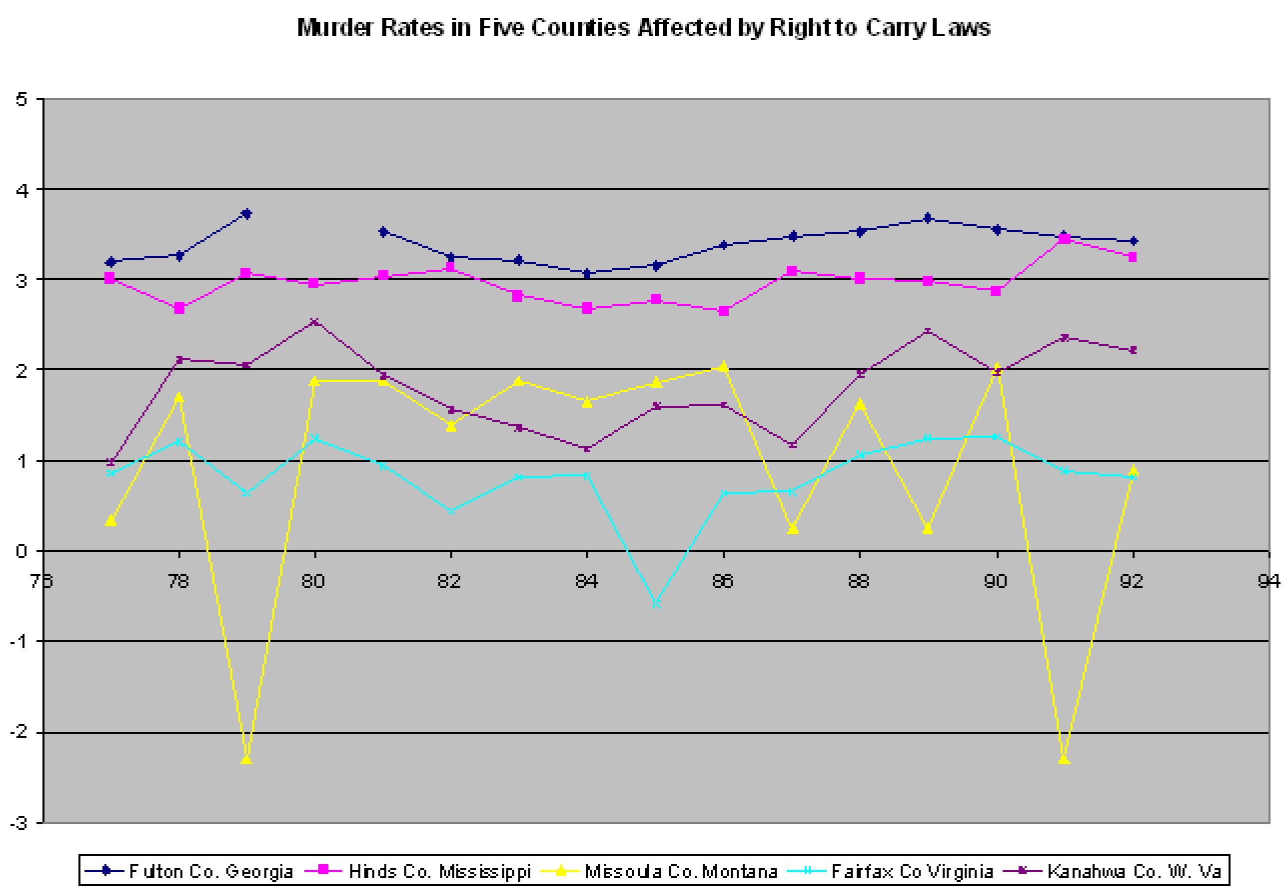
In addition to linearity, multiple regression assumes that each variable is "normally" distributed about all the others in a classic bell-curve pattern. This means that most cases should be clustered around the average within each category, with few at the extremes. Often the data violates this assumption in major ways that lead to completely erroneous results. A good example is John Lott's data on gun control.

Lott collected a massive data set that he generously made available to other researchers. Unfortunately, he did not begin by graphing his data, perhaps because he had so much of it. But it is always a good idea to begin an analysis with graphs so as to see the trends before they are obscured by all the statistical adjustments. If one cannot graph everything, it is still worthwhile to graph some representative cases. So, using Lott's data, I plotted trends in murder rates for a number of counties where "shall issue" laws had gone into effect during the period covered by his study. Before "shall issue" laws were passed, local officials had discretion in granting permits to carry concealed weapons.

After they were passed, they had to issue a permit to any law abiding adult who wanted one. If Lott's hypothesis were correct, we would expect to see the murder rate go down once the laws were passed.

The following graph shows trends in murder rates for the largest counties in several states that adopted "shall issue" laws between 1977 and 1992. The date at which the laws went into effect varied from state to state. Before reading further, the reader may find it interesting to examine the graph and try to infer when the law took effect in each county.

Examining the graph, we see that the pattern in Missoula County, Montana, appears to be quite erratic, with very sharp declines in the murder rate in 1979 and 1991. This, however, is not a real phenomenon, but the result of one of the adjustments Lott made to compensate for the non-normality of his data. Instead of using the actual numbers, he converted his numbers to natural logarithms. This is a common practice, since natural logarithms often fit the assumptions of multiple regression better than the actual data. The number in John Lott's data file for Missoula County in those years is -2.30. This is odd, since a County's murder rate cannot go below zero, unless previously murdered people are brought back to life. No such luck, however. To get the actual murder rate in each county, one has to invert the logarithms in Lott's data set with the formula *true rate* = $e^{logarithmic\ rate}$, where e = *2.71828.* This can be done easily with the e^x button on a scientific calculator. Entering -2.3 in such a calculator and pushing the e^x button yields .100, or one tenth of a murder per 100,000 population. Actually, the true figure for murders in Missoula County in 1979 and 1991 was zero. Lott used .1 instead of zero because the natural logarithm of zero is mathematically undefined, so leaving it at zero would have created missing data. There are a great many -2.3's in his data files on murder, because many of the counties are quite small, much smaller than Missoula with 81,904 people in 1992.



The distribution of murder rates in American counties is not at all close to the bell-shaped normal curve. There are great many small counties with few or no murders, and a few quite large ones with a great many. Converting the data to natural logarithms is one way of minimizing the statistical effects of non-normal distributions, but it can introduce other distortions as we see in this case.

Leaving aside the distortions in Missoula County caused by the conversion of the data to natural logarithms, the trends in these counties are quite smooth. There is no apparent effect from the introduction of "shall issue" laws in Missoula County in 1991, in Fulton County (Atlanta, Georgia) in 1990, in Hinds County (Jackson, Mississippi) in 1990, in Fairfax County (Fairfax, Virginia) in 1988 and Kanawha County (Charleston, West Virginia) in 1989.

One might ask, why are we dealing with these medium sized counties instead of major population centers? This was my first clue to the fundamental flaw in Lott's argument. My first inclination was to graph the trends in America's largest cities, because that's where the homicide problem is most severe. I immediately discovered that *none of these cities had a "shall issue" law.* The "shall issue" laws were put into effect primarily in states with low population density. This meant that Lott's data did not meet the fundamental assumptions for a regression analysis. To work properly, multiple regression requires that the "shall issue" variable be normally distributed throughout the data set. The mathematical calculations used to "control" for spurious relationships can't work if there is not a sufficient range of variation in the key variables. This was the "smoking gun" hidden in Lott's mass of tables and sophisticated equations. At no point in the book did he acknowledge this fact. When I asked him about this, he shrugged it off. He didn't did not see it as a problem, since he "controlled" for population size.

These irregularities in the data completely invalidated Lott's analysis. It took two years before Ayres and Donohue (1999) verified this in an econometric analysis, but Zimring and Hawkins zeroed in immediately on the problem in 1997. Having studied gun control legislation, they knew that "shall issue" laws were instituted in states where the National Rifle Association was powerful, largely in the South, the West and in rural regions. These were states that already had few restrictions on guns. They observed that this legislative history frustrates (Zimring and Hawkins 1997: 50) "our capacity to compare trends in 'shall issue' states with trends in other states. Because the states that changed legislation are different in location and constitution from the states that did not, comparisons across legislative categories will always risk confusing demographic and regional influences with the behavioral impact of different legal regimes." Zimring and Hawkins (1977: 51) further observed that:

> *"Lott and Mustard are, of course, aware of this problem. Their solution, a standard econometric technique, is to build a statistical model that will control for all the differences between Idaho and New York City that influence homicide and crime rates, other than the "shall issue" laws. If one can "specify" the major influences on homicide, rape, burglary, and auto theft in our model, then we can eliminate the influence of these factors on the different trends. Lott and Mustard build models that estimate the effects of demographic data, economic data, and criminal punishment on various offenses. These models are the ultimate in statistical home cooking in that they are created for this data set by these authors and only tested on the data that will be used in the evaluation of the right-to-carry impacts."*

What Lott and Mustard were doing was comparing trends in Idaho and West Virginia and Mississippi with trends in Washington, D.C. and New York City. What actually happened was that there was an explosion of crack-related homicides in major eastern cities in the 1980s and early 1990s, most of them among people who were quite well armed despite the lack of gun permits. Lott's whole argument came down to a claim that the largely rural and western "shall issue" states were spared the crack-related homicide epidemic because of their "shall issue" laws. This would never have been taken seriously if it had not been obscured by a maze of equations.

Why Regression Fails: Lack of Predictive Testing.

The acid test in statistical modeling is prediction. A useful model should make predictions that are better than random guessing. Only in this way can cause and effect be distinguished and causal predictions be tested. Regression modelers often do this with historical data, in effect using data from the more distant past to predict the more recent past. The problem with this is that, when the outcome is already known, it is too easy to adjust the model to fit the known outcome. This is like using the day before yesterday's weather to predict yesterday's weather, or the day before yesterday's stock prices to predict yesterday's prices. This may be useful as a way of learning, but the only really persuasive test is to predict tomorrow's weather or stock prices. This criterion, success in prediction, is used to evaluate models of financial markets, the weather, medical outcomes, population trends and many other phenomena. These models all work imperfectly, and regression gives us a good measure of just how imperfectly.

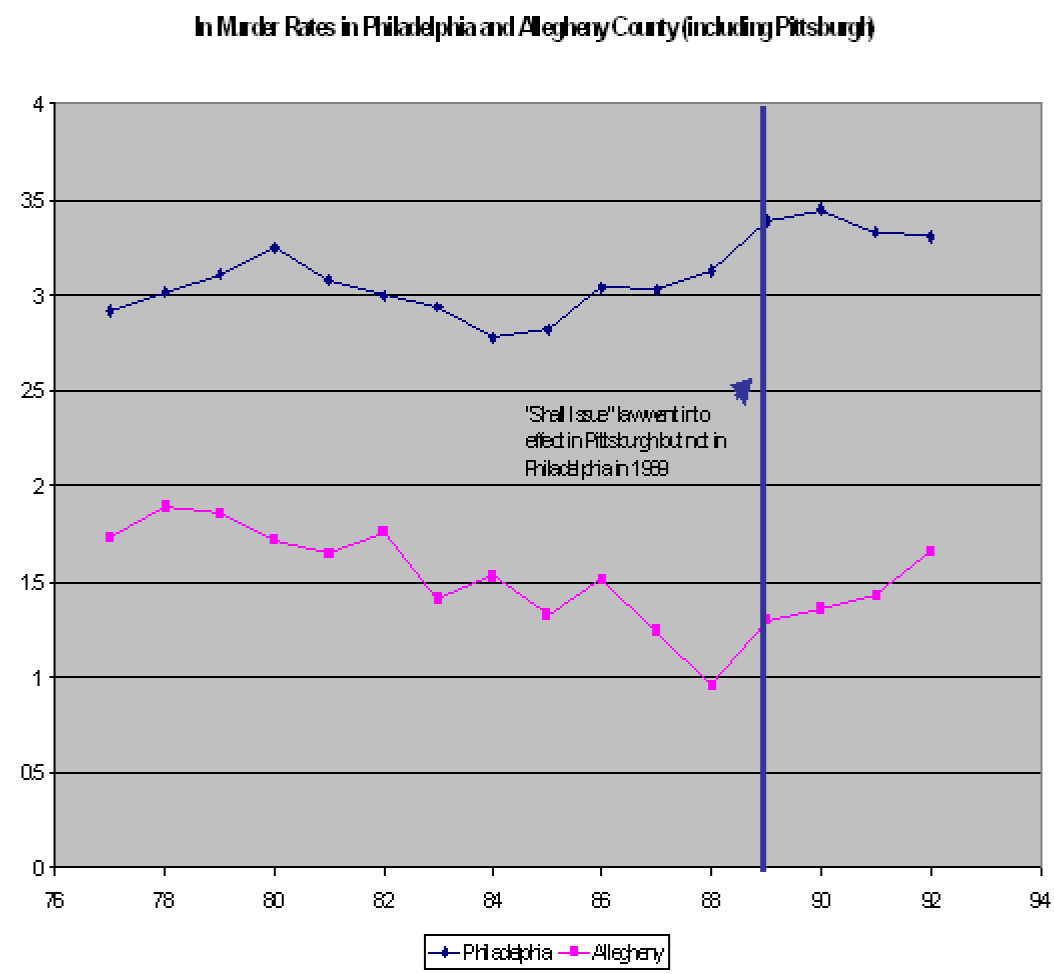
Unfortunately, researchers who use econometric techniques to evaluate social policies generally do not subject their models to predictive tests. They could do so, either by making future predictions and waiting to see what happens, or, if that would take too long, by developing their model with data from one population and then using it to predict results with another population. But most researchers simply do not do this, or if they do the models do not work so the results are never published. (The Urban Institute did make a prediction, but they did not wait for the outcome data to publicize their conclusions. When the data showed that their model did not work, they simply took it down from their WEB site.)

The journals that publish these studies do not require predictive testing, which suggests that the editors and reviewers have low aspirations for their fields. Instead, researchers take data for a fixed period of time and keep fine tuning and adjusting their model it until they can "explain" the trends *that have already happened*. There are always a number of ways to do this, and with modern computers it is not terribly hard to keep trying until you find something that fits. At that point, the researcher stops, writes up the findings, and sends the paper off for publication. Later, another researcher may adjust the model to obtain a different result. This fills the pages of social science journals and helps young professors get tenure. Everybody pretends not to notice that little or no progress is being made.

The Alternative: Insist on Intelligible Graphs and Tables

When junk science is released to the media by scholars at prestigious universities, and published in reputable refereed journals, people become understandably skeptical about the value of social science research. A few years ago *The Economist* (May 13, 1995) published a tongue-in-cheek editorial announcing that "74.6% of sociology is bunk." Cynics wondered if the estimate might not have been low. But it is important not to throw out the baby with the bath water. There is good solid work being done in sociology, criminology and other social sciences, although it may not make it into the journals that value statistical complexity over reliable findings. The most reliable work uses simpler statistical techniques that do not require so much adjustment and standardizing of the data. This has the great advantage that it the work can be read and used by people who have not devoted years of their lives to learning obscure econometric techniques.

Studies that make extensive use of graphics, such as those of Sellin (1959) and Blumstein and Wallman (2000) have been much more successful and informative than regression studies. As an example of the power of simple graphical techniques, we can graph some of the data from John Lott's gun control data set. When a data set is so huge, it may be necessary to select a small part of it for graphing, but this can be quite informative if the selection is done well. In reviewing Lott's data, I found that in the state of Pennsylvania, a "shall issue" law was passed in 1989, but the city of Philadelphia was exempted from it. This provided an excellent opportunity for "natural experiment, " comparing trends in two metropolitan areas that differed on a key variable. The graph that follows compares trends in Philadelphia, which is a city and a county, with those in Allegheny County, which includes Pittsburgh. The graph shows that murder rates are generally higher in Philadelphia than in Allegheny County, but the passage of a law giving citizens the right to get permits to carry concealed weapons did not have the positive effect posited by John Lott. In fact, the Allegheny County murder rate was declining prior to the passage of the law, then increased slightly. In Philadelphia, the murder rate had been increasing, then it leveled off despite the fact that the new law did not apply in that city. The violent crime statistics for the same two counties show the same pattern. Disaggregating the data in this way allows us to draw on our qualitative, historical knowledge in interpreting statistical trends. To discredit this kind of finding, concealed weapons advocates would have to show how other factors somehow compensated for the failure of the shall issue law to have any apparent effect.



Conclusions

These cases may be enough to persuade most readers that multiple regression is not of much use in proving causal arguments, at least about the historical impact of social policies. In fact, the problem is broader than that, and many specialists doubt that multiple regression is a valid way of settling any kind of theoretical argument. In 1985, Stanley Lieberson (1985: ix), a distinguished professor at the University of California, wrote “I am fully sympathetic with the empirical research goals found in much of contemporary sociology, with its emphasis on rigor and quantification. However…I have reluctantly reached the conclusion that many of the procedures and assumptions in this enterprise are of no more merit than a quest for a perpetual motion machine.” In 1991, David Freedman, a distinguished sociologist at the University of California at Berkeley and the author of textbooks on quantitative research methods, shook the foundations of regression modeling in the social sciences when he frankly stated "I do not think that regression can carry much of the burden in a causal argument. Nor do regression equations, by themselves, give much help in controlling for confounding variables" (Freedman, 1991: 292).

Freedman's article provoked a number of strong reactions. Richard Berk (1991: 315) observed that Freedman's argument "will be very difficult for most quantitative sociologists to accept. It goes to the heart of their empirical enterprise and in so doing, puts entire professional careers in jeopardy.”

The social science community does not have good procedures for acknowledging the failure of a widely used research method. Methods that are entrenched in graduate programs at leading universities and published in prestigious journals tend to be perpetuated. Many laymen assume that if a study has been published in a good, peer reviewed journal, there is some assurance of its validity. The cases we have examined here show that this is not the case. Peer review assures that established practices have been followed, but it is of little help when those practices themselves are faulty.

Finding the flaws in regression studies is difficult. Usually, the only way to be sure of them is to obtain the data set and reanalyze the data. This is more than can be expected of a reviewer from a professional journal. It takes time, usually a year or two, for a multiple regression study to be replicated, and many studies never get replicated because they are not of sufficient interest to anyone. Even if a replication is done and does not confirm a study, journal editors may feel that this is simply a case of normal scientific debate. The problem, however, is that no real progress occurs. We are no closer to having a useful mathematical model for predicting homicide rates than we were when Ehrlich published his paper in 1975.

There are no important findings in sociology or criminology that are so complex that they cannot be communicated with graphs and tables that are intelligible to intelligent laymen and policy makers. It is time to admit that the emperor has no clothes. Multiple regression and other mathematical modeling techniques have simply not lived up to their promise as a method of evaluating the impact of public policies. Studies that claim otherwise are junk science.

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Top