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Capital Punishment and Homicide Rates: Sociological Realities and Econometric Distortions

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Abstract
Sociological methods have consistently succeeded while econometric methods have failed in research on capital punishment and homicide. But econometricians aggressively promote their findings in public policy venues, while sociologists are less assertive. This is due to cultural differences between the disciplines, and to a philosophy of science that values falsification of hypotheses over progress in answering research questions. This problem has occurred and is likely to reoccur in other policy areas where sociologists are insufficiently assertive in defending their accomplishments.

Keywords
capital punishment, causal analysis, death penalty, econometrics, sociological methodology

Introduction

Sociologists and econometricians have often differed about whether capital punishment deters homicide (Goertzel, 2004). Stephen Cole (2001: 18) observed that 'most of the sociologists who have studied this issue are liberals and have concluded on the basis of evidence that capital punishment doesn't work. Economists who have studied this problem are substantially more conservative than sociologists and have concluded on the basis of evidence that capital punishment does work.' He concludes that this is an 'undoable problem' and recommends that sociologists study other things.

Cole's recommendation is unfortunate for several reasons. First, and most important, the econometricians who argue that the available statistical data show that the death penalty deters murder are wrong. Their research is seriously flawed, and has been shown

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to be so by eminent statisticians as well as by other econometricians. Second, if sociologists and criminologists give up on this topic, the econometricians are not likely to return the favor. Policy makers and the public are likely to draw the wrong policy conclusions if the only research is done by econometricians. And, third, the same methodological dispute has occurred in other policy areas. There are no important social policy issues that are not susceptible to an assault by flawed econometric research.

Sophisticated econometric studies that have produced disputed results include:

- a widely publicized Urban Institute study predicting dire consequences from welfare reform (Zedlewski et al., 1996);
- several studies warning that raising the 55 mile speed limit would cause a drastic increase in auto accidents (Baruya, 1997; Wendell Cox Consultancy, n.d.);
- a very widely circulated article arguing that legalizing guns cuts homicide rates (Lott and Mustard, 1997; see also Black and Nagin, 1998; Goertzel, 2002; Zimring and Hawkins, 1997);
- a study arguing that legalized abortion cuts crime rates (Donohue and Levitt, 2001; see also Joyce, 2001; Lott and Whitley, 2001);
- the best-selling statistical monograph of all time, *The Bell Curve,* which depended on the prestige of multiple regression to defend its causal arguments about race and intelligence (Herrnstein and Murray, 1994; see also Bowen and Bok, 1998; Glymour, 1997).

We cannot examine all of these cases in this article. Instead, we will focus on the case of capital punishment and homicide, where the problem has been most prolonged. It is a case where comparative sociological methods have worked well while econometric methods have failed miserably. Yet the econometricians persist, and they are often successful in getting their work taken seriously, while sociologists have been less assertive. One reason for this may be cultural differences between the disciplines. Davis (2001: 106) has caricatured the differences between sociologists and economists as follows: 'Economists are competitive, individualistic, conservative in their politics, smart, and arrogant. Sociologists are cooperative, group-minded, leftish in politics, sincere, and docile. Except as master and dog, these two psyches are not a promising combination.'

Of course, this caricature exaggerates the differences between the disciplines. There is a continuum of individuals in both professions. But the clash of disciplinary cultures is real and has important methodological and philosophical implications. Multiple regression and related methods have considerable prestige in sociology, in part because of their use in the more prestigious discipline of economics. Articles using these methods are published in leading sociological journals, despite the fact that sociological data rarely meet the statistical assumptions of the methods. This causes difficulties when research is published using methods we admire and often use ourselves but which may reach erroneous and objectionable conclusions.

Refuting these studies is difficult, often requiring obtaining the dataset and doing additional calculations. This is more than can be expected of a journal editor or reviewer, so the articles are published and when the refutations finally come out they are published in additional journal articles. These may then be refuted by the original authors. No real progress is made in this exchange of technical arguments, but articles are published that advance researchers' careers.

Instead of celebrating the successes of comparative methods, many sociologists and other social scientists pursue the *will o' the wisp* of the definitive statistical test. They apply ever more sophisticated statistical models to data that do not meet the assumptions of the methods. This stream of unproductive research is encouraged by a philosophy of science that values falsification of hypotheses even though no lasting progress is made.

Popper, Lakatos and the Philosophy of Social Science

Karl Popper (1959) argued that scientific progress is made by advancing hypotheses and then seeing if they can be falsified by empirical research. Popper's ideas were based largely on the history of the physical sciences, and they do not apply well to the social sciences. With modern computers, it is easy to test dozens or hundreds of statistical models and then to publish only the ones that survive. As De Leeuw observed:

> In many quantitative disciplines, most typically econometrics, the appropriate method is to assume a statistical model, then collect the data, then test the model by comparing the statistics with the model. If the model does not fit it is rejected. This is supposedly 'sticking out one's neck', which is presumably the macho Popper thing to do. There are various things problematic with this prescription … if you follow the prescription, and your data are any good, your head gets chopped off … people know their head will get chopped off, nobody follows the prescription. They collect data, look at their data, modify their model, look again, stick out their neck a tiny bit, modify the model again, and finally look around with a proud look on their face and a non-rejected model in their hand, pretending to have followed the Popperian prescription. Thus the prescription leads to fraud. (De Leeuw, 1994: 142)

The focus of econometric research is often more on the techniques than on a substantive contribution to knowledge. In an article titled *Economics as a Hard Science: Realistic Goal or Wishful Thinking?* Thomas Mayer observed:

> Much of the published research consists of taking a new technique out for a walk rather than of really trying to solve a problem. And also economics has become much too isolated from the other social sciences, since being hard scientists we do not want to use either the results or the tools of those who cannot claim our exalted status. (Mayer, 1980: 177)

This process can go on indefinitely, with researchers falsifying each others' models and debating technical issues. There are always new statistical techniques to try, and everyone tries not to notice that no real progress is being made. Meanwhile, people outside the narrow group caught up in the technical arguments become disillusioned and skeptical.

Moving beyond this kind of fruitless controversy requires a philosophy of science that is more appropriate than Popper's. Popper's ideas apply best to fields where experiments are expensive and time consuming, so few are done and they are likely to be critical. In these fields, a hypothesis is formed based on theory and past observations and then tested with new data. Popper's approach does not work well in statistical research where dozens of hypotheses can be tested in a few hours, where there are often multiple models that are roughly equally good (or bad), and where the choice of a dataset and decisions about dataset filtering are often critical. The choice of a model may depend on the researcher's intuition as to which one makes sense or is most elegant. In this kind of statistical work, the modeling process is repeated on the same dataset over and over again using different techniques until the researcher finds one that works. Anything discovered in this way must be tested with new data, but new data are often not available for several years.

Imre Lakatos's (1978) theory of 'research programs' fits the social sciences better. Lakatos pointed out that even in the physical sciences researchers cannot and do not abandon their core ideas every time they get a negative finding. A successful research program consists of core assumptions that are defended by a protective belt of auxiliary techniques and theories. For example, Newton's three laws of motion are the core assumptions of classical physics, and the auxiliary techniques and theories are the numerous specialized methods used to apply these laws in different contexts, based on making special-case assumptions. General relativity theory is now used to explain phenomena such as the deviation of Mercury's orbit from ellipticity, but before Einstein, this same phenomenon would have been explained via classical physics, using a complex combination of auxiliary techniques and special assumptions such as the assumption of an irregular distribution of mass within the sun.

Often the transition from one theory to another has to do with the replacement of complex or awkward explanations of phenomena with simpler and more elegant ones, rather than the straightforward supplanting of a totally inadequate theory by an adequate one. This is because in real science, unlike the Popperian abstraction, objective adequacy is often difficult to measure for many reasons including the fact that true out-of-sample testing is often not practical. The peculiarity of Mercury's orbit was known before Einstein created relativity theory – and whether he used this fact in constructing his theory, in some remote and indirect way, is hard to establish. Solar system astronomy is similar to social science in that it depends on observation rather than active experimentation, and the number of observations that can be made of many phenomena is limited. In this kind of circumstance, Popper's analysis applies worse than it does in fields such as high-energy physics or molecular biology where it is easier to set up rigorous controlled experiments.

If we apply Lakatos's model to the social sciences, the standard causal model (Abbot, 2004) defines the core assumptions of a research program that has been highly influential in fields such as sociology, criminology and political science for several decades. Although there are many variations on the model, known by names such as path analysis

and structural equation modeling, they all have the same basic logic. A large number of cases are selected for analysis. A number of variables are measured for each case, and one is selected as the dependent variable. Mathematical equations are then computed to replace the scores on the dependent variable with a weighted sum of the scores on the other 'independent' variables. There are many ways to do this and the results vary depending on which way is selected.

The core assumptions of the model have been defended with ever more elaborate statistical adjustment techniques. Whenever the model fails, the practitioners set to work developing more and more sophisticated auxiliary techniques in the hope that it will work the next time. This is justified with a Popperian philosophy of science that values hypothesis testing but does not question the lack of longer-term progress.

In Lakatos's view, research programs become degenerative when they are no longer making progress in answering important questions or advancing interesting new findings. This is clearly the case with multiple regression studies of capital punishment and homicide, as reviewed below, and it seems to be generally true of multiple regression research on social problems. Textbooks on regression and causal modeling (McClendon, 2002; Berk, 2004a) are long on technique, but offer no examples of controversial causal issues about social problems that have been resolved with their techniques. In a widely cited article, statistician David Freedman (1991: 292) stated frankly that '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.'

An unusually frank recognition that a research program has degenerated can be found in the report by two distinguished criminologists (Marvell and Moody) of the reception of a multiple regression 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. (Marvell and Moody, 1997: 221)

Multiple Regression, Capital Punishment and Homicide Rates

Multiple regression studies of capital punishment and homicide are a good example of what Lakatos called a *degenerative research program*. This research program began in earnest in 1975 when Isaac Ehrlich (1975) published *The Deterrent Effect of Capital*

Punishment: A Question of Life and Death, in the *American Economic Review.* His finding that each execution deterred eight future murders was eagerly received by advocates for the death penalty. Even before it was published, his study was cited by the Solicitor General of the USA in an *amicus curiae* brief filed with the US Supreme Court in defense of the death penalty. 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. Other econometricians using the same very limited data sources included Yunker (1976) who found a stronger deterrent effect than Ehrlich, Cloninger (1977) who supported his findings, and Bowers and Pierce (1975), Passell and Taylor (1977) and Hoenack and Weiler (1980) who found no deterrence at all.

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 appropriateness of econometric methods for the problem. They 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 laying to rest all controversy about the behavioral effects of deterrence policies should not be expected. (Manski, 1978: 422)

Unfortunately, this was not the end of the matter. Ehrlich was not persuaded by his critics and found flaws in their work. He remained convinced of the validity of using econometric modeling with death penalty data. In a recent interview (Fessenden, 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.'

Other economists have continued with the same approach. In recent examples, Mocan and Gittings (2001) concluded that each execution decreases homicides by five to six while Dezhbaksh et al. (2002) argued that each execution deters 18 murders. But most criminologists (Fessenden, 2000) disagree, agreeing with mathematician John Lamperti (n.d.) who concluded that 'regression on nationally aggregated data can never yield reliable evidence on deterrence, pro or con. The signal, if any, is hopelessly buried in the noise.'

This research has important public policy implications. Advocates for capital punishment continue to cite econometric studies as proof of deterrence. In a Minority Report introduced in hearings on the Innocence Protection Act, Senator Orrin Hatch (2002: 51) stated that: 'All of the scientifically valid statistical studies – those that examine a period of years, and control for national trends – consistently show that capital punishment is a substantial deterrent.' Senator Hatch (2002: 52) conceded that 'to be sure, some studies – usually conducted by avowed death-penalty opponents – have concluded that the death penalty has no deterrent effect.' But he argued that these studies were flawed because they did not rely on rigorous methodologies that adequately controlled for other variables that affect the homicide rate.

In testimony before the House Judiciary Committee, Subcommittee on Crime, Terrorism and Homeland Security economist Joanna Shepherd (2004: 1) acknowledged that research in the 1970s and 1980s had reached 'conflicting results' but claimed that 'recent studies have exploited better data and more sophisticated statistical techniques' and that 'the modern studies have consistently shown that capital punishment has a strong deterrent effect, with each execution deterring between three and 18 murders.' Although there is no research at all on the topic, Shepherd went on to state that 'the pervasive consistency of capital punishment's deterrence of other kinds of murder suggests that capital punishment would deter at least some terrorist murders.' She conceded the difficulties in applying the death penalty to suicide bombers, but argued that the strength of the econometric findings outweighed this consideration.

The new studies to which Shepherd referred have been subjected to a devastating critique by statistician Richard Berk (2004b) in an article called *New Claims About Executions and General Deterrence: Deja Vu All Over Again*? Berk shows that the new findings are the result of 11 anomalous observations from a very few states that have large numbers of executions. He concludes that 'in short, by forcing a linear relationship and capitalizing on a very few extreme values, a negative relationship can be made to materialize. But the linear form is inconsistent with the data, and the 11 extreme values highly atypical.' (Berk, 2004b: 15)

Berk is the author of a definitive text on multiple regression (Berk, 2004a) and he examines all of the claims about lags, transformations and standardizations in excruciating detail. He concludes that:

> it would be bad statistics and bad social policy to generalize from the 11 observations to the remaining 989. So, for the vast majority of states for the vast majority of years, there is no evidence for deterrence in these analyses. And even for the remaining 11 observations, the credible evidence for deterrence is lacking. (Berk, 2004b: 23)

The only consistent thing about econometric studies of social problems is their consistent ability to produce models on all sides of an issue. Senator Hatch was misinformed when he called econometric methods 'scientifically valid' when applied to social problems data. A scientifically valid method is one that is generally recognized as such by scientists specializing in a field. But many prominent economists (McKim and Turner, 1997) and statisticians (Freedman 1991, 2003; Lieberson, 1985) have argued that econometric methods are not valid when applied to this kind of sociological data. In a widely reprinted essay called *Let's Take the Con out of Econometrics* published in the *American Economic Review,* Edward Leamer (1983: 42) shared his judgment that 'any inference from these data about the deterrence effects of capital punishment is too fragile to be believed'.

In a careful technical review of the studies of capital punishment and homicide rates, McManus found that:

> there is much uncertainty as to the 'correct' empirical model that should be used to draw inferences, and each researcher typically tries dozens, perhaps hundreds, of specifications before selecting one or a few to report. Usually, and understandably the ones selected for publication are those that make the strongest case for the researcher's prior hypothesis. (McManus, 1985: 417)

In still another rigorous and comprehensive review Cameron observed that:

> What emerges most strongly from this review is that obtaining a significant deterrent effect of executions seems to depend on adding a set of data with no executions to the time series and including an executing/non-executing dummy in the cross-section analysis … there is no clear justification for the latter practice. (Cameron, 1994: 214)

By systematically comparing models with different assumptions, McManus showed that researchers whose prior beliefs led them to structure their models in different ways would obtain predictable conclusions:

> The data analyzed are not sufficiently strong to lead researchers with different prior beliefs to reach a consensus regarding the deterrent effects of capital punishment. Right-winger, rational-maximizer, and eye-for-an-eye researchers will infer that punishment deters would-be murderers, but bleeding-heart and crime-of-passion researchers will infer that there is no significant deterrent effect. (McManus, 1985: 425)

Zimring and Hawkins (1997: 57) were not speaking metaphorically when they said that econometric research can 'facilitate statistical findings to warm the hearts of true believers of any stripe.' Thirty years of econometric research on capital punishment and homicide rates has produced no trustworthy knowledge on the topic. We know less now, from econometric studies, than we thought we knew in 1975. When a research tradition spins its wheels for three decades, it is time to use a different approach.

An Alternative: Comparative Sociological Analysis.

If multiple regression for causal analysis of social problems is a degenerative research program, failing to produce new insights or reliable findings, what can replace it? In the physical sciences, degenerative research programs are sometimes replaced when a new program appears that proves much more productive. This is a process that Kuhn (1996 [1962]) described as a scientific revolution or a shift in paradigm. This model of scientific revolution, however, is not a good fit for the social sciences. In the social sciences, alternative theories and methods coexist for long periods of time without one succeeding another. Of course this sometimes happens in physical science too. In chemistry, valence bond theory and molecular orbital theory coexisted for a long time, the former because

of its simplicity and the latter because of its accuracy. In molecular biology, shotgun sequencing and clone-by-clone sequencing were competing methods used to sequence the human genome; and now biologists are figuring out how to most profitably use them together in sequencing new genomes.

There have been two parallel traditions in research on capital punishment and homicide: the econometric tradition and one using comparative sociological methods. The classic comparative sociological study of capital punishment and deterrence of homicide in the USA was published by Thorsten Sellin in 1959. Sellin (Pinatel, 1968) was a distinguished sociology professor at the University of Pennsylvania and one of the pioneers of scientific criminology. He was a prime mover in setting up the government agencies that collect statistics on crime. The core idea of his methodology was to combine quantitative and qualitative data. This involved two steps (Tocafundi, 1996): 'First, a comprehensive view of the subject which incorporated historical, sociological, psychological, and legal factors into the analysis in addition to the development of analytical models; and second, the establishment and utilization of statistics in the evaluation of crime.'

Sellin applied this combination of quantitative and qualitative methods in an exhaustive study of capital punishment in American states. He used every scrap of data that was available, together with his knowledge of the history, economy and social structure of each state. He compared states to other states and examined changes in states over time. Every comparison he made led him to the 'inevitable conclusion … that executions have no discernable effect on homicide rates.' (Sellin, 1959: 34)

Sellin's work has been replicated many times, as new data have become available, and the replications have confirmed his finding that capital punishment has not deterred homicide (Bailey and Peterson, 1997; Zimring and Hawkins, 1986). These studies are an outstanding example of what Freedman (1991) calls 'shoe leather' social research. The hard work is collecting the best available data, both quantitative and qualitative. Once the statistical data are collected, the analysis consists largely in displaying them in tables, graphs and charts and interpreting them in light of qualitative knowledge of the states in question. When reports of this kind of research are written well, or presented to an audience with presentation software, the findings can be understood by readers or viewers with only modest statistical sophistication. This allows consumers of the research to make their own interpretations, drawing on their qualitative knowledge of the states in question.

In his insightful analysis of sociological methods, Abbott (2004: 21–3) refers to this kind of research as 'small-N comparison'. In studying capital punishment and homicide, small-N comparative researchers systematically contrast states that are similar to each other in respects other than their use of capital punishment. They study what happens when states or countries make changes in their use of capital punishment. Archer and Gartner (1984) examined 14 countries that abolished the death penalty and found that abolition did not cause an increase in homicide rates.

A major advantage of the small-N comparative method is that it fits the kind of data actually available on topics such as capital punishment and homicide rates. The number of executions in the USA is quite small and very unevenly distributed among states (85 in 2000, 40 of which were in Texas). Forcing this data into a statistical model that

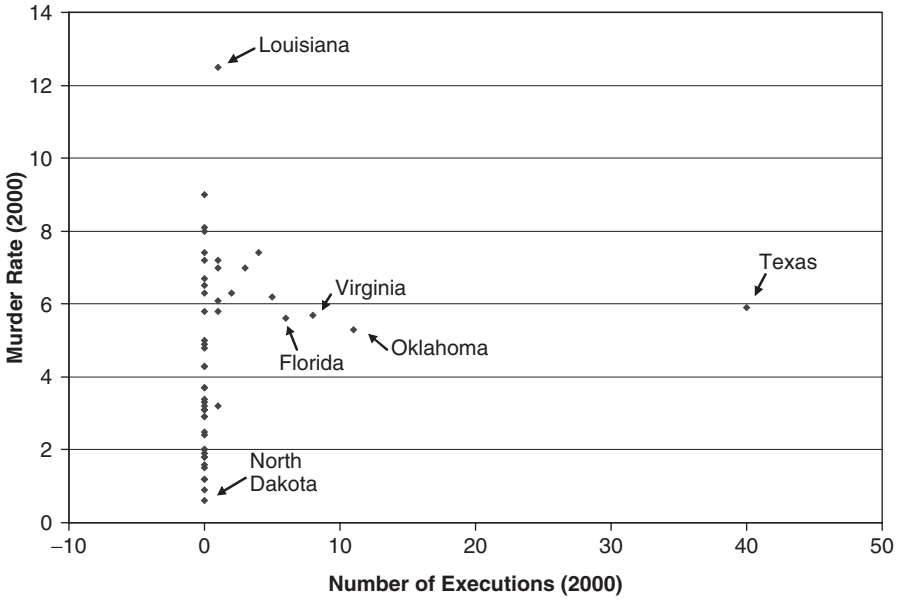

Figure 1 Executions and Homicide Rates in US States

assumes normally distributed data is simply inappropriate. The state of Texas is a striking outlier that dominates any regression analysis. Just looking at the scatter diagram in Figure 1, one can see that, despite its much higher execution rate, Texas's homicide rate is about average (Bureau of Justice Statistics, 2002).

Instead of over-generalizing from very limited data, making statements about 'capital punishment' and 'homicide' in the abstract, small-N comparative analysts carefully examine the data they actually have for specific cases. Instead of forcing their data into one large equation, or a set of equations, they look at trends in each state separately, then make comparisons between states that have similar or contrasting profiles. For example, Figure 2 shows trends in executions and homicide rates in Texas and New York. New York had no executions for the entire period, while Texas began executing prisoners in 1981. Just looking at the graph, one can see why linear regression analysis is inappropriate. The trends simply do not approximate a straight line.

There is a clear turning point in the Texas homicide rate in 1992, ten years after it began executing people. This was at the same time that homicide rates turned down nationally. The moratorium on executions in Texas in most of 1996 and part of 1997 did not have any discernable effect on the homicide rate, although Cloninger and Marchesini (2001) managed to detect one with an econometric analysis.

It is true that homicide was higher in Texas before 1992 than it was in New York, and now it is about the same. But no amount of econometric number crunching can tell us whether that change was due to the increase in executions or to other factors such as the

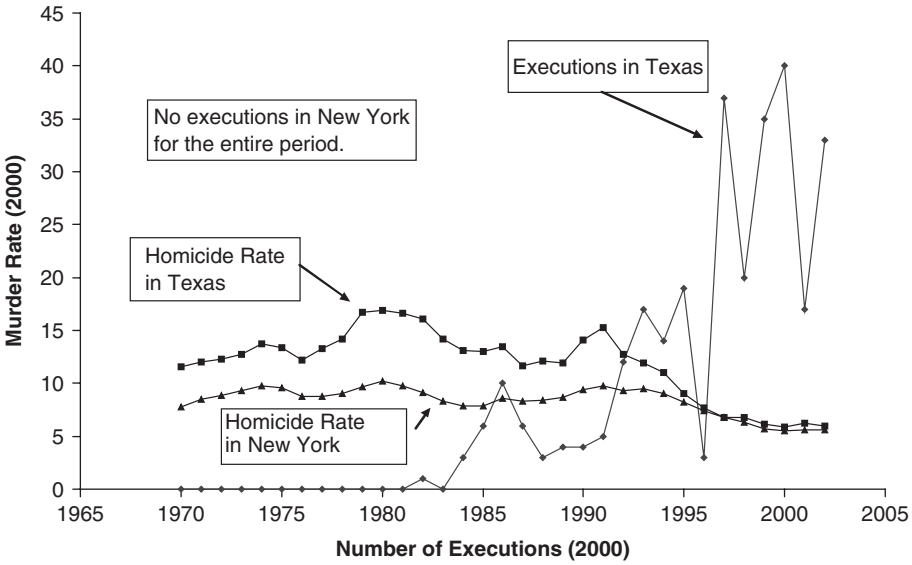

Figure 2 Executions and Homicide Rates in Texas and New York

increase in the prison population. The available data are simply not sufficient to control for all the relevant variables.

Comparative analysis provides data that can inform public debate about social problems. Instead of listening to arguments about esoteric statistical issues, readers can examine charts and graphs of meaningful data. Anyone who wants to check an analysis can look up the data, usually freely available from public sources, and do her or his own analysis. It takes only modest computer skills to enter the data into a spreadsheet program and prepare graphs. A recent example is the work of two *New York Times* reporters (Bonner and Fessenden, 2000) who compared states with the death penalty to those without and found that those without the death penalty often had lower homicide rates.

Instead of trying to falsify an abstract hypothesis, holding everything else constant, small-N comparative analysts go through their data with a fine tooth comb, trying to describe what actually happened. An outstanding example of this approach is a study of the sharp drop in crime rates in the 1990s by Blumstein (2000). By disaggregating his data, Blumstein provides a tremendous amount of useful information about trends in homicides by different age groups, racial groups, urban and rural groups, as well as about weapons used and trends in imprisonment. He draws on a rich body of qualitative information in interpreting the statistical trends, and provides an informative and persuasive description of what happened. Capital punishment does not enter into his analysis because there is no evidence that changes in execution rates correlated with any of the differences he sought to explain. This is a highly productive model for research on social problems.

Conclusions

Econometricians believe, or at least behave as if they believe, that objective truths about social reality can be described with precise, mathematical formulas. Comparative researchers are more sensitive to how people with different points of view and ways of thinking perceive a complex world. They are less likely to believe that objective truth about the social world can be proved with a set of complex equations. The ironic thing is that the econometricians' pursuit of objective truth has led them to create a fantasy world of mathematical certainties, while the comparative researchers have kept in closer touch with reality.

Econometricians inhabit the land of Ceteris Paribus, a place where everything is constant except when allowed to vary along precisely delimited parameters. Ceteris Paribus was first described in Edwin Abbot's (1884) classic fairy tale, *Flatland*. In *Flatland* everything moves along straight lines, flat plains or rectangular boxes. If you plot the heights and weights of a group of *Flatlanders* on a graph with height on one axis and weight on the other, all the points fall on a straight line.

Of course, econometricians know they do not live in *Flatland.* But the mathematics works much better when they pretend they do. So they adjust the data in one way or another to make it straighter. Then they qualify their remarks, saying 'capital punishment deters homicide, ceteris paribus.' When the real world data diverge greatly from the straight lines of *Flatland*, as they usually do, this can lead to distorted results.

Econometricians do not ask historically specific questions such as: 'does Texas with its high execution rate have a lower homicide rate than states without the death penalty' or 'did the homicide rate go down when Texas began executing people, compared to trends in other states that did not?' Instead, they try to answer the question: 'if we use the latest, most sophisticated statistical methods to control for extraneous variables, can we say that the death penalty deters homicide rates *other things being equal?*' After decades of effort, we know the answer to that question*: there are many ways to model things statistically, and the conclusions depend on which one is chosen*. We also know that of the many possible ways to specify a regression model, each researcher is likely to prefer one that will give results consistent with his or her predispositions.

True to their competitive nature, the econometricians favor a Popperian philosophy of science that values falsifying each other's hypotheses. With our cooperative values, sociologists are better served with a Lakatosian model that emphasizes collective progress in solving problems and adding to the store of knowledge. The best way to do this is to combine quantitative and qualitative methods because mathematical formulas cannot capture the complexity and richness of social life. As Conley (2004: 206) states in his book *The Pecking Order*: 'Numbers tell us a certain amount, but they never go the distance. In order to write this book (rather than just a couple of academic articles for obscure journals), it was necessary to talk to actual siblings about their childhoods, their family relationships and their socioeconomic trajectories.'

Sociologists sometimes think of qualitative interviews or observations as mere illustrations of their statistical findings. This is better than not using qualitative findings at

all. But qualitative data can provide a richness of understanding that can never be matched by statistical models. The most successful studies of social problems, such as Conley's *The Pecking Order* and Putnam's *Bowling Alone* (2000) make the most of both qualitative and quantitative data. These are not simply works of 'public sociology' presenting a watered down version of more sophisticated professional studies. They present a richer, more informative analysis than technical statistical articles. An analysis that is more worthy of the classical sociological tradition.

This is not to devalue statistical work when it is used appropriately. Quantitative data can provide a useful check on qualitative observations. Margaret Mead (1988), for example, did exclusively ethnographic work in her research on adolescent sexuality in Samoa. If she had supplemented her work by going down to the police station and collecting the statistics on rape, she might have asked better questions of her respondents and emerged with a more accurate picture of life on the island (Freeman, 1997). Ethnographers can gain by supplementing their work with statistics, just as survey researchers need to supplement their work with focus groups, qualitative interviews and ethnographic observations.

The attempt to convert sociology into a hard science by using statistical controls as a substitute for experimentation has failed. Correlation really is not causation, no matter how complex the controls. There is no way to create a ceteris paribus world where nothing varies except the independent and dependent variables in a particular study.

After hearing John Lott speak on concealed weapons and homicide rates, and checking with other experts who gave opposite opinions, journalist David Boldt (1999) lamented that 'trying to sort out the academic arguments is almost a fool's errand. You can drown in disputes over t-statistics, dummy variables and "Poisson" vs "least squares" data analysis methods.' Boldt was correct to suspect that he was being lured into a fool's errand. There are no important findings in sociology or criminology that cannot be fully understood by journalists and policy makers who lack graduate degrees in econometrics. Sociology at its best has more in common with history and journalism than it does with physics or chemistry. We have not been able to prove general laws about society. We have been able to produce rigorous, well documented descriptions of social reality, including the fact that capital punishment has not had a significant effect on homicide rates.

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