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Deterrence versus Brutalization: Capital Punishment's Differing Impacts among States

Joanna M. Shepherd
Emory University

DETERRENCE VERSUS BRUTALIZATION: CAPITAL PUNISHMENT'S DIFFERING IMPACTS AMONG STATES

*Joanna M. Shepherd**

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INTRODUCTION

Recent empirical studies by economists have shown, without exception, that capital punishment deters crime. Using large data sets that combine information from all fifty states over many years, the studies show that, on average, an additional execution deters many murders. The studies have received much publicity, and death penalty advocates often cite them to show that capital punishment is sound policy.

Indeed, deterrence is the central basis that many policymakers and courts cite for capital punishment. For example, President Bush believes that capital punishment deters crime and that deterrence is the only valid reason for capital punishment.[1] Likewise, the Supreme Court, when it held in its landmark 1976 decision that capital punishment was constitutional, cited deterrence as one of its main reasons.[2] Moreover, the Court confirmed that

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1. In the presidential debate with Al Gore on October 17, 2000, Bush was asked, "Do both of you believe that the death penalty actually deters crime?" He responded, "I do, it's the only reason to be for it . . . . I don't think you should support the death penalty to seek revenge. I don't think that's right. I think the reason to support the death penalty is because it saves other people's lives." Election 2000 Presidential Debate with Republican Candidate Governor George W. Bush and Democratic Candidate Vice President Al Gore (Oct. 17, 2000) (transcript available at http://www.debates.orf/pages/trans2000c.html). In the debate, Gore also agreed that capital punishment deterred crime. *Id.*

2. In *Gregg v. Georgia*, the Court provided as a main reason for upholding capital punishment:

> We may nevertheless assume safely that there are murderers, such as those who act in passion, for whom the threat of death has little or no deterrent effect. But for many others, the death penalty undoubtedly is a significant deterrent. There are carefully contemplated murders, such as murder for hire, where the possible penalty of death may well enter into the cold calculus that precedes the decision to act. And there are some categories of murder, such as murder by a life prisoner, where other sanctions may not be adequate.

428 U.S. 153, 185–86 (1976).

the main factor that motivated most state legislatures to prescribe capital punishment was deterrence.[3] Similarly, a central issue in debates on whether federal law should include capital punishment is deterrence.[4] We can also reasonably assume that juries and trial judges, in deciding whether to impose or overturn death sentences, will incorporate common understandings about deterrence. Governors may be similarly influenced in making decisions about clemency.

In contrast to the economic studies, recent studies by sociologists and law professors have reached an opposite conclusion. The studies are often restricted to a single state or small group of states rather than economists' examination of the average for the nation as a whole. They usually find no deterrence. Death penalty opponents cite these studies.

Each group tends to ignore the other's research. In this paper, I reconcile the results and show that both conclusions can be correct.

Using the same large data set of U.S. counties from 1977 to 1996 that many other crime studies use (and that I used in one of my earlier studies), I change the focus from *national* averages for deterrence.[5] Instead, I examine whether capital punishment's impacts on murder rates differ among *states*.

The results are striking. Consider the twenty-seven states where at least one execution occurred during the sample period. Executions deter murder in only six states. Capital punishment, however, actually *increases* murder in thirteen states, more than twice as many as experience deterrence. In eight states, capital punishment has no effect on the murder rate. That is, executions have a deterrent effect in only twenty-two percent of states. In contrast, executions induce additional murders in forty-eight percent of states. In seventy-eight percent of states, executions do not deter murder.

I then explore why these differences exist among states. After investigating various possible explanations, I identify an important factor (although other factors are also undoubtedly important): on average, the states where capital punishment deters murder execute many more people than do the states where capital punishment incites crime or has no effect. Using various statistical techniques, I show that a threshold number of executions for deterrence exists, which is approximately nine executions

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3. The Court noted:

> The value of capital punishment as a deterrent of crime is a complex factual issue the resolution of which properly rests with the legislatures, which can evaluate the results of statistical studies in terms of their own local conditions and with a flexibility of approach that is not available to the courts. Indeed, many of the post-*Furman* statutes reflect just such a responsible effort to define those crimes and those criminals for which capital punishment is most probably an effective deterrent.

Id. at 186 (citations omitted).

4. For example, when Congress was considering whether to extend the federal death penalty to terrorist acts, I was asked to testify before the House Judiciary Committee about deterrence. *See Terrorist Penalties Enhancement Act of 2003: Hearing on H.R. 2934 Before the Subcomm. on Crime, Terrorism, and Homeland Sec. of the House Comm. on the Judiciary*, 108th Cong. (Apr. 21, 2004) (written testimony of Joanna M. Shepard), *available at* http://judiciary.house.gov/media/pdfs/shepherd042104.pdf.

5. For a detailed discussion of the data, see *infra* text accompanying notes 31–33.

during the sample period. In states that conducted more executions than the threshold, executions, on average, deterred murder. In states that conducted fewer executions than the threshold, the average execution increased the murder rate or had no effect.

An intuitive explanation is that each execution has two opposing effects. First, the execution creates a brutalization effect: it contributes to creating a climate of brutal violence. The execution sets an example of killing to avenge grievances, an example that some private individuals then follow. Second, the execution creates some deterrence: potential criminals recognize that the state is willing to wield the ultimate penalty. For the first few executions, however, the deterrent effect is small. Only if a state executes many people does deterrence grow; only then do potential criminals become convinced that the state is serious about the punishment, so that the criminals start to reduce their criminal activity. When the number of executions exceeds the threshold, the deterrence effect begins to outweigh the brutalization effect. In the seventy-eight percent of states where executions either increase murders or have no effect, the brutalization effect either counterbalances or outweighs the deterrent effect. The deterrent effect outweighs the brutalization effect only in six states.

The results suggest that earlier economic papers' focus on national averages masked variation among states. Because the six states with deterrence, such as Texas, execute many people, the executions in these states deter many murders. In contrast, most of the states where executions increase murder execute few people. When the large number of executions in the deterrence states are averaged in with the small number of executions in all of the other states, the large deterrent effect in those states dominates the opposite brutalization effect in the other states. Thus the result from earlier economics papers: on average, an execution in the United States deters crime. This paper shows that these averages are powered by a handful of high-execution, high-deterrence states. In most states, capital punishment either increases murder or has no effect.

The results also explain the findings of no deterrence in papers that have focused on individual states, rather than on the nation as a whole. As the results here show, in seventy-eight percent of states, executions do not deter murder.

All of the primary models' general lessons are consistent across two other models that use data from other time periods and with different levels of aggregation.

This Article's results have two important policy implications. First, policymakers' false beliefs about capital punishment's universal deterrent effect may have caused many people to die needlessly. If deterrence is capital punishment's purpose, as is often stated by our president and others, then, in the majority of states where executions do not deter crime, executions kill convicts uselessly. Moreover, in the many states where the brutalization effect outweighs the deterrent effect, executions not only kill convicts needlessly but also induce the additional murders of many innocent people. A very rough estimate is that, all told from 1977 to 1996, executions in no-deterrence states have killed more than 5,000 innocent people, or 250 per year. Thus, in the many states that execute without a deterrent effect, policymakers should consider abandoning the death penalty. These states' executions do not deter crime. If deterrence is the goal, capital punishment in these states simply does not work. Instead, it needlessly kills both convicts and innocents.

Of course, if policymakers in the no-deterrence states have goals other than deterrence, such as retribution, then they might continue capital punishment, despite the absence of deterrence. In the many states, however, where executions not only fail to deter but also cause additional murders of innocent people, policymakers might think twice before permitting state-sponsored revenge that, in effect, kills innocent bystanders.

Second, suppose that a state was considering whether to start executing people. It could not focus only on deterrence, ignoring other important moral, legal, and economic issues. The state would need to recognize that deterrence cannot be achieved with a half-hearted execution program. Unless the state executed enough people to exceed the deterrence threshold, then a large risk would exist that the executions would increase murders. People in many states may be unwilling to establish such a large execution program.

The rest of the Article is organized as follows. After Part II discusses capital punishment's recent history in the United States, Part III reviews the conflict in recent studies on capital punishment and deterrence. Part IV explores differences in states' applications of capital punishment and tests the effect on murder of executions in individual states. In Part V, I examine possible causes of the different effects of executions on murder across states. Part VI then offers results from two other models and data sets. Finally, Part VII presents conclusions.

I. The Death Penalty's Recent History in the United States

During the first half of the twentieth century, executions were both frequent and popular. More executions occurred during the 1930s than in any other decade in U.S. history, an average of 167 executions each year. Although the use of capital punishment declined somewhat in the 1940s and 1950s, executions were still much more frequent than today: approximately 130 a year in the 1940s and seventy-five a year during the 1950s, compared to an average of forty-eight per year in the 1990s.[6] Over sixty-five percent of the U.S. public approved of the death penalty during these decades.[7]

In the late 1950s, however, public support increasingly turned away from the death penalty. Various social forces combined to reduce capital punishment's popularity and use. Examples include growing doubts about

6. *See* DEATH PENALTY INFORMATION CENTER, HISTORY OF THE DEATH PENALTY, PART I (2005), http://www.deathpenaltyinfo.org/article.php?scid=15&did=410#EarlyandMid-TwentiethCentury.

7. RAYMOND PATERNOSTER, CAPITAL PUNISHMENT IN AMERICA 20 (1991).

the morality of the death penalty, consciousness that most of western Europe had abandoned capital punishment, abatement of the crime wave of the 1930s, lack of evidence that executions had a deterrent effect, strengthened belief in the racially discriminatory use of the death penalty, and increasing concern over the arbitrariness of the death penalty's application.[8] Public approval of capital punishment reached its lowest point in 1966 when only forty-two percent of the public supported it.[9] Reflecting the public's growing disapproval of capital punishment, the number of executions steadily declined throughout the 1960s, and by 1968, they stopped altogether.

By the 1960s, all states with capital punishment laws had changed them from the mandatory statutes originally borrowed from English common law to discretionary statutes. Under the new statutes, juries had complete control over whether a defendant received a death sentence or not. This sentencing freedom often caused application of the death penalty to seem arbitrary and random. The U.S. Supreme Court began hearing cases involving the discretionary capital statutes in the late 1960s. While the constitutionality of capital punishment was being challenged, no states were willing to put people to death.

The Supreme Court finally resolved the constitutionality of discretionary capital statutes in three cases in 1972: *Furman v. Georgia*, *Jackson v. Georgia*, and *Branch v. Texas*, collectively referred to as the *Furman* decision.[10] In a five-to-four decision, the justices held that discretionary capital statutes resulted in arbitrary sentencing, violating the Eighth Amendment's Cruel and Unusual Punishment Clause. This decision effectively voided forty states' death penalty statutes and commuted the sentences of over 600 death row inmates.

After *Furman*, the states quickly began to draft new death penalty laws. Although some states passed mandatory capital statutes that the Supreme Court soon found unconstitutional, others enacted guided discretion statutes. These statutes provided juries with a set of factors they should consider when making their death penalty determination. The Supreme Court approved these guided discretion statutes in 1976 in *Gregg v. Georgia*,[11] *Jurek v. Texas*,[12] and *Proffitt v. Florida*,[13] collectively referred to as the *Gregg* decision. A major reason for the Court's holding that capital punishment was constitutional was its conclusion that capital punishment deterred crime.[14]

After the enactment of new, constitutional death penalty statutes, death rows quickly filled. The moratorium on executions that began in 1968 ended in January 1977, with the voluntary execution of Gary Gilmore in Utah. As

8. THE DEATH PENALTY IN AMERICA 25 (Hugo Adam Bedau ed., 3d ed. 1982).

9. PATERNOSTER, *supra* note 7, at 19.

10. 408 U.S. 238 (1972).

11. 428 U.S. 153 (1976).

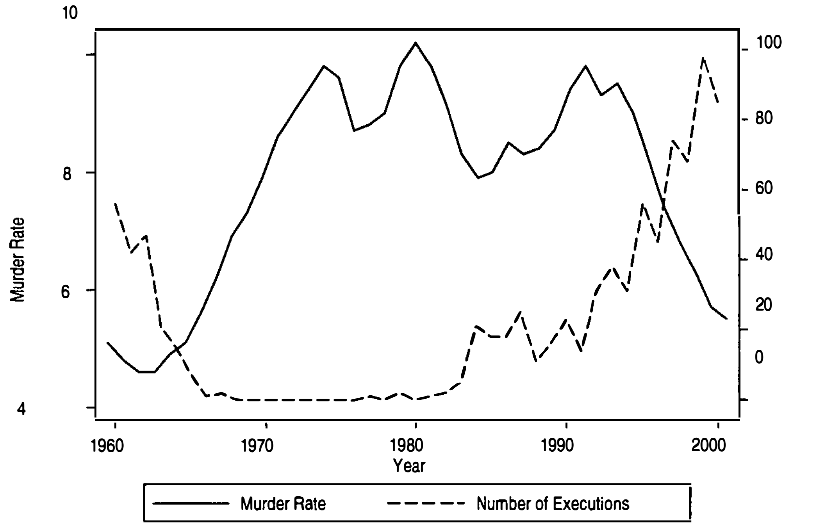
12. 428 U.S. 262 (1976).

13. 428 U.S. 242 (1976).

14. *See supra* note 2.

Figure 1 reveals, the number of annual executions has steadily increased since 1977, peaking in 1999 with ninety-eight executions. Since 1977, there have been 856 executions in thirty-two states. Today, the approval rating for the death penalty is over seventy-four percent, after reaching an all-time high of eighty percent in 1994.[15]

FIGURE I
U.S. MURDER RATE AND EXECUTIONS



Despite the recent resurgence in executions, the use of the death penalty varies widely across regions. Executions have been concentrated in the South for most of this century, and the concentration there has recently become even stronger. Southern states accounted for approximately half of the 3,859 executions between 1930 and 1968.[16] Of the executions since 1977, over seventy-five percent have occurred in the South. Southern states were also, in general, less likely to abolish the death penalty before 1972 and quicker to reinstate it and execute people after 1976.

In contrast, there has been considerable public disapproval of the death penalty in other regions for centuries. Twelve midwestern and northern states do not have capital punishment laws, and a number of these states legally abolished the death penalty as early as the mid-1800s. Although other states in these regions have capital punishment laws, several have performed no postmoratorium executions.

15. Jeffrey M. Jones, *Support for the Death Penalty Remains High at 74%; Slight Majority Prefers Death Penalty to Life Imprisonment as Punishment for Murder*, GALLUP NEWS SERVICE, May 19, 2003, *available at* http://www.deathpenaltyinfo.org/article.php?scid=23&did=592.

16. FRANKLIN E. ZIMRING & GORDON HAWKINS, CAPITAL PUNISHMENT AND THE AMERICAN AGENDA 30 (1986).

Historians and psychologists offer many potential explanations for the differences in capital punishment's popularity between the South and other regions: the South's tradition of lynching, Southern evangelical religions, the region's prolonged rural/frontier experience, a history of racial subjugation, the loss of the Civil War, and a siege mentality that places blame on others.[17]

II. The Conflict in Previous Studies on Capital Punishment and Deterrence

For decades, researchers have reached conflicting conclusions about whether capital punishment deters crime. In order to explore the conflict, I now describe both the earlier literature and recent research. In later Parts, I present results that resolve the conflict.

A. *Early Literature on Capital Punishment and Deterrence*

In the United States, whether capital punishment deters crime has been debated for decades. The initial participants in the debate were psychologists and criminologists. Their research was either theoretical or based on comparisons of crime patterns in states with and without capital punishment. However, because they did not use multiple-regression statistical techniques, the analyses were unable to distinguish the effect on murder of capital punishment from the effects of other factors.[18]

The debate in the economics literature began with Isaac Ehrlich's two papers in 1975 and 1977.[19] Since the 1960s, economists had been investigating whether potential criminals, at least on average, rationally responded to increased threats of punishment.[20] Economists theorized that potential criminals would reduce their criminal activity if the expected costs to them of their criminal behavior increased because of increased penalties. Ehrlich was the first to use multivariate regression analysis to explore this hypothesis empirically with respect to capital punishment, testing whether the number of murders would fall in response to increased imposition of the death penalty. In contrast to earlier methods, this approach allowed Ehrlich to separate the effects of many different factors on murder.

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17. *See, e.g.*, Dov Cohen & Richard E. Nisbett, *Self-Protection and the Culture of Honor: Explaining Southern Violence*, 20 PERSONALITY & SOC. PSYCHOL. BULL. 551 (1994); Richard E. Nisbett, *Violence and U.S. Regional Culture*, 48 AM. PSYCHOLOGIST 441 (1993).

18. *See, e.g.*, H.J. EYSENCK, CRIME AND PERSONALITY (Paladin 1970) (1964); J. THORSTEN SELLIN, THE DEATH PENALTY (1959).

19. Isaac Ehrlich, *Capital Punishment and Deterrence: Some Further Thoughts and Additional Evidence*, 85 J. POL. ECON. 741 (1977); Isaac Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life and Death*, 65 AM. ECON. REV. 397 (1975).

20. *See, e.g.*, Gary S. Becker, *Crime and Punishment: An Economic Approach*, 76 J. POL. ECON. 169 (1968).

Ehrlich's 1975 paper examined U.S. time-series data for the period 1933–1969. Time-series data are data for one unit (for Ehrlich, for the entire U.S.) over several time periods. He tested the effect on national murder rates of possible deterrent variables (the probabilities of arrest, conviction, and execution), demographic variables (population, fraction of nonwhites, and fraction of people age fourteen to twenty-four), economic variables (labor force participation, unemployment rate, real per capita permanent income, per capita government expenditures, and per capita expenditures on police), and a time variable. He found a statistically significant negative relationship between the murder rate and execution rate, indicating a deterrent effect: more executions meant less crime. Specifically, he estimated that each execution resulted in approximately seven or eight fewer murders.

Next, Ehrlich's 1977 paper studied cross-sectional data from the fifty states in 1940 and 1950. Cross-sectional data are data from several units (here, the fifty states) for one time period (1940 or 1950). That is, instead of his first paper's approach of testing how the total U.S. murder rate changed across time as the national execution rate changed, Ehrlich now explored the relationship during a single year between each of the states' execution rates and their murder rates.

Again, Ehrlich used multivariate regression analysis to separate the effect on murder of different factors. He included possible deterrent variables (probabilities of conviction and execution, median time spent in prison, and a "dummy" variable that distinguished executing states from nonexecuting states), demographic variables (state population, urban population, percent of nonwhites, and percent of people age fifteen to twenty-four and twenty-five to thirty-four), and economic variables (median family income and percent of families with income below half of the median income). Again, his findings indicated a substantial deterrent effect of capital punishment on murder.

Ehrlich's finding loosed a flood of interest in econometric analysis of capital punishment and deterrence. The papers that immediately followed Ehrlich used his original data (1933–1969 national time-series or 1940 and 1950 state-level cross-section) and variants of his econometric model.

The results were mixed. Many found a deterrent effect of capital punishment, but others did not. For example, using Ehrlich's data, studies by Yunker, Cloninger, and Ehrlich and Gibbons found a deterrent effect.[21] In contrast, Bowers and Pierce, Passel and Taylor, and Hoenack and Weiler found no deterrence when they used the same data with alternative specifications.[22] Similarly, McAleer and Veall, Leamer, and McManus found

21. Dale O. Cloninger, *Deterrence and the Death Penalty: A Cross-Sectional Analysis*, 6 J. BEHAV. ECON. 87, 98 (1977); Isaac Ehrlich & Joel Gibbons, *On the Measurement of the Deterrent Effect of Capital Punishment and the Theory of Deterrence*, 6 J. LEGAL STUD. 35 (1977); James A. Yunker, *Is the Death Penalty a Deterrent to Homicide? Some Time Series Evidence*, 5 J. BEHAV. ECON. 45 (1976).

22. William J. Bowers & Glenn L. Pierce, *The Illusion of Deterrence in Isaac Ehrlich's Research on Capital Punishment*, 85 YALE L.J. 187 (1975); Stephen A. Hoenack & William C. Weiler, *A Structural Model of Murder Behavior and the Criminal Justice System*, 70 AM. ECON.

no deterrent effect when different variables were included over the same sample period.[23] Finally, Black and Orsagh found mixed results depending on the cross-section year they used.[24]

In the late 1980s and 1990s, a second generation of econometric studies extended Ehrlich's national time-series data or used more recent cross-sectional data. As before, some papers found deterrence while others did not. For example, Cover and Thistle and Layson used an extension of Ehrlich's national time-series data, covering up to 1977.[25] Although Layson found a significant deterrent effect of executions, Cover and Thistle corrected for data flaws and found no deterrent effect. Chressanthis employed national time-series data covering 1966 through 1985 and found a deterrent effect.[26] In contrast, Grogger used daily data for California during 1960–1963 and found no deterrent effect.[27]

Most of the early studies—both the first wave and the second generation—suffered from basic flaws: they suffered important data limitations because they used either national time-series or cross-section data. Using national time-series data created a serious aggregation problem. Any deterrence from an execution should affect the crime rate only in the executing state; one state's high execution rate would not be expected to change the crime rate in nearby states, where the first state's laws and execution proclivity do not apply.

Aggregation—lumping all states together in a national time series—diluted such distinct effects, creating "aggregation bias." For example, suppose that the following happened concurrently: the murder rate in a state with no executions randomly increased at the same time that the murder rate dropped in a state with many executions. Aggregate data might incorrectly lead to an inference of no deterrence; the aggregate data, with the two states lumped together, would show an increase in executions leading to no change in the murder rate.

Cross-sectional studies also suffer serious problems. Most importantly, they prevent researchers from using so-called "fixed-effects estimation" to control for jurisdiction-specific characteristics that could be related to mur-

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REV. 327 (1980); Peter Passell & John B. Taylor, *The Deterrent Effect of Capital Punishment: Another View*, 67 AM. ECON. REV. 445 (1977).

23. Edward E. Leamer, *Let's Take the Con out of Econometrics*, 73 AM. ECON. REV. 31 (1983); Michael McAleer & Michael R. Veall, *How Fragile are Fragile Inferences? A Re-Evaluation of the Deterrent Effect of Capital Punishment*, 71 REV. ECON. & STAT. 99 (1989); Walter S. McManus, *Estimates of the Deterrent Effect of Capital Punishment: The Importance of the Researcher's Prior Beliefs*, 93 J. POL. ECON. 417 (1985).

24. Theodore Black & Thomas Orsagh, *New Evidence on the Efficacy of Sanctions as a Deterrent to Homicide*, 58 SOC. SCI. Q. 616 (1978).

25. James Peery Cover & Paul D. Thistle, *Time Series, Homicide, and the Deterrent Effect of Capital Punishment*, 54 S. ECON. J. 615 (1988); Stephen K. Layson, *Homicide and Deterrence: A Reexamination of the United States Time-Series Evidence*, 52 S. ECON. J. 68 (1985).

26. George A. Chressanthis, *Capital Punishment and the Deterrent Effect Revisited: Recent Time-Series Econometric Evidence*, 18 J. BEHAV. ECON. 81 (1989).

27. Jeffrey Grogger, *The Deterrent Effect of Capital Punishment: An Analysis of Daily Homicide Counts*, 85 J. AM. STAT. ASS'N 295 (1990).

der. For example, with cross-section data, a researcher cannot control for fundamental but immeasurable variables, such as a violent culture in certain states.[28] Cross-section data also preclude any consideration of what happens to crime, law enforcement, and judicial processes over time.

Moreover, both time-series and cross-section data shared the problem of having few observations. For example, for the year analyzed, Ehrlich's national time-series data had only thirty-seven observations and his cross-section data had only fifty observations. With so few observations, strong statistical conclusions are impossible.

Noting the inadequacy of time-series and cross-section data, several authors called for new research using panel data, an approach that I describe below.[29] In addition, a National Academy of Sciences panel convened to study the early deterrence literature. It concluded that new research should be conducted with disaggregated data that looked at smaller geographic units, such as counties or cities rather than the nation as a whole, and smaller time periods, such as months rather than years. The panel also suggested that new studies examine the impact of executions on different types of homicides.[30]

Researchers responded to the invitation. In addition to using panel data, several new studies employ disaggregated data of the sort recommended by the panel. Likewise, another study examined executions' impacts on different homicide types. I now discuss these modern studies of the past decade.

B. *Modern Studies of Capital Punishment's Deterrent Effect*

Most recent studies have overcome the fundamental problems associated with national time-series and cross-section data by using panel data techniques. "Panel data" are data from several units (the fifty states or all U.S. counties) over several different time periods. That is, panel data follow a cross-section over time. For example, a panel dataset might include data on each of the fifty states, or even on each U.S. county, for a series of years.

Panel data produce many more observations than cross-section or time-series data. For example, a state-level, panel data set of fifty states over ten years would have 500 observations. By contrast, a national, time-series data set over the same period would have only ten observations and a state–level, cross-section data set from one of the years would have only fifty

28. Technically, cross-sectional studies are affected by unobserved heterogeneity that cannot be controlled for in the absence of time variation. The heterogeneity is caused by jurisdiction-specific characteristics that may correlate with other variables of the model, resulting in biased, incorrect estimates.

29. *See, e.g.*, K.L. Avio, *Capital Punishment*, *in* 1 THE NEW PALGRAVE DICTIONARY OF ECONOMICS AND THE LAW 205 (Peter Newman ed., 1998); Samuel Cameron, *A Review of the Econometric Evidence on the Effects of Capital Punishment*, 23 J. SOCIO-ECON. 197 (1994).

30. *See* NAT'L. ACAD. SCI., *Report of the Panel on Research on Deterrent and Incapacitative Effects*, *in* DETERRENCE AND INCAPACITATION: ESTIMATING THE EFFECTS OF CRIMINAL SANCTIONS ON CRIME RATES 1 (Alfred Blumstein et al. eds., 1978).

observations. Through inexorable statistical laws, more observations permit more accurate measurement of the capital punishment's impacts.

Furthermore, panel data allow researchers to control for important jurisdictional differences among U.S. states or counties by using fixed-effects estimation (which cross-section data cannot do), while avoiding aggregation bias (a problem of time-series data). Several studies have analyzed data that are more disaggregated than in the early studies. This minimizes aggregation bias over geographic units or periods of time, enabling researchers to estimate any deterrent effect more precisely. In addition to enjoying the benefits of panel data, recent studies have access to more recent data that make conclusions more relevant for the current environment.

In the past decade, eight papers have been written in the economics literature that use improved panel data and more sophisticated regression techniques. Their conclusion is unanimous: all of the modern economics papers find evidence of deterrence. Four other papers in the past decade have not used panel data, but also find a deterrent effect. Several studies, however, in sociology journals and law reviews have produced mixed results; some find deterrence while others do not.

1. *Modern Economics Papers Using Panel-Data Techniques*

All of the modern papers that use panel-data analysis find a deterrent effect.

i. Hashem Dezhbakhsh, Paul H. Rubin, and I examined whether deterrence existed using county-level panel data from 3,054 U.S. counties over the period 1977–1996.[31] This is the only study to use county-level data, allowing us to estimate better the demographic, economic, and jurisdictional differences among U.S. counties that can affect murder rates. Moreover, the large number of county-level observations extended the empirical tests' reliability.[32] We found a substantial deterrent effect; both death row sentences and the executions themselves resulted in decreases in the murder rate. Our conservative estimate was that each execution results in, on average, eighteen fewer murders. Our main finding, that capital punishment has a deterrent effect, was consistent across many different ways of performing the statistical analysis.[33]

ii. In another paper, I used state-level, monthly panel data from 1977 to 1999 to examine two gaps in the capital punishment literature.[34] First, I in-

31. Hashem Dezhbakhsh et al., *Does Capital Punishment Have a Deterrent Effect? New Evidence from Postmoratorium Panel Data*, 5 AM. LAW & ECON. REV. 344 (2003).

32. Technically, it extends the analysis' degrees of freedom, increases variability, and reduces colinearity among variables.

33. The deterrent effect remains with different choices of functional form (double-log, semi-log, or linear), state-level vs. county-level analysis, sampling period, endogenous vs. exogenous probabilities, and level vs. ratio specification of the main variables.

34. Joanna M. Shepherd, *Murders of Passion, Execution Delays, and the Deterrence of Capital Punishment*, 33 J. LEGAL STUD. 283 (2004).

vestigated the types of murders deterred by capital punishment. Some people believe that certain types of murder are not deterrable.[35] To the contrary, I found that the combination of death row sentences and executions deterred all types of murders: murders between intimates, acquaintances, and strangers, crime-of-passion murders and murders committed during other felonies, and murders of African American and white people.[36] I estimated that each death row sentence deters approximately 4.5 murders and that each execution deterred approximately three murders.

The second issue that the paper addressed is the impact on deterrence of execution delays. In 1996, Congress passed the Anti-Terrorism and Effective Death Penalty Act of 1996 that limits federal habeas review in capital cases. If criminals prefer lengthy death row waits to short ones, as their numerous appeals and requests for stays suggest, then shortening the time until execution could increase the death penalty's deterrent impact. I found that shorter waits on death row increased deterrence. Specifically, one extra murder is deterred for every 2.75-years reduction in the death row wait before each execution.

iii. Hashem Dezhbakhsh and I used state-level panel data from 1960 to 2000 to examine capital punishment's deterrent effect.[37] This was the only study to use data from before, during, and after the 1972–1976 Supreme Court moratorium on executions. Our study advanced the deterrence literature by exploiting an important characteristic that other studies overlooked: the experimental nature of the Supreme Court moratorium.

First, we performed before-and-after moratorium comparisons. Specifically, we compared the murder rate for each state immediately before and after it suspended or reinstated the death penalty. These before-and-after comparisons were informative because many factors that affected crime—for example, law enforcement, judicial, demographic, and economic variables—changed only slightly over a short period of time. In addition, the moratorium began and ended in different years in different states. Considering the different start and end dates, the duration of the moratorium varied considerably across states, ranging from four to thirty years. Observing similar changes in murder rates immediately after the same legal change in different years and in various states provided compelling evidence of the moratorium's effect on murder. The before-and-after comparisons revealed that about ninety-one percent of states experienced an increase in murder

35. They claim that murders by intimates or crimes of passion are products of uncontrollable rage, and they are therefore nondeterrable. Others even argue executions could even increase the number of murders by strangers, as the brutality of executions incites criminals.

36. Intimates are defined as spouses, common-law spouses, parents, children, siblings, in-laws, step-relations, and other family. Crime-of-passion murders include lovers' triangles, murders by babysitters, brawls under alcohol, brawls under drugs, arguments over money, other arguments, and abortion-murders (abortions performed during the murder of the mother).

37. Hashem Dezhbakhsh & Joanna M. Shepherd, The Deterrent Effect of Capital Punishment: Evidence from a 'Judicial Experiment,' (Emory Univ., Law & Econ. Research Paper Series, Working Paper No. 04-04, 2004).

rates after they suspended the death penalty. In about seventy percent of the cases, the murder rate dropped after the state reinstated the death penalty.

We supplemented the before-and-after comparisons with time-series and panel-data regression analyses that, unlike many existing studies, used both pre- and postmoratorium data. The regressions disentangled the impact of the moratorium itself on murder from the effect of actual executions on murder; we found that the moratorium had a significant positive effect on murder and that executions had significant negative effects on murder. These estimates suggested that both adopting a capital statute and exercising it have strong deterrent effects.[38]

iv. John R. Lott, Jr. and William M. Landes used state-level panel data from 1977 to 1995 to examine whether right-to-carry concealed handgun laws deterred multiple-victim public shootings.[39] Included in their analysis were tests of the deterrent effect of executions on murder. The authors found that right-to-carry concealed handgun laws do result in fewer multiple victim public shootings. They also found that executions have a significant deterrent effect on the overall murder rate. Specifically, a one percent increase in the execution rate was associated with a seven percent decline in the overall murder rate.

v. and vi. Two papers by FCC economist Paul Zimmerman found a deterrent effect.[40] In his first paper, Zimmerman used state-level panel data from 1978 to 1997 to examine the relationship between state execution rates and murder rates. In his second paper, he employed state-level panel data from 1978 to 2000 to examine which execution methods had the strongest deterrent effects. In both papers, Zimmerman found a significant deterrent effect of capital punishment. He estimated that each execution deterred an average of fourteen murders and that executions by electrocution had the strongest impact.

vii. H. Naci Mocan and R. Kaj Gittings used state-level panel data from 1977 to 1997 to examine the relationship between executions, commutations, and murder.[41] Again, the authors found a significant deterrent effect; they estimated that each execution deterred an average of five murders.

38. We also confirm that our results hold up to changes in our choice of regressors, estimation method, and functional form. The deterrent variables' coefficients are remarkably consistent in sign and significance across eighty-four different regression models. In addition, we verify that the negative relationship between the death penalty and murder is not a spurious finding. Before-and-after moratorium comparisons and regressions reveal that the death penalty does not cause a decrease in property crimes, suggesting that the deterrent effect is not reflecting general trends in crime.

39. John R. Lott, Jr. & William M. Landes, Multiple Victim Public Shootings, Bombings, and Right-to-Carry Concealed Handgun Laws: Contrasting Private and Public Law Enforcement (Univ. of Chicago, John M. Olin Law & Econ., Working Paper No. 73, 2000).

40. Paul R. Zimmerman, *Estimates of the Deterrent Effect of Alternative Execution Methods in the United States: 1978–2000*, AM. J. ECON. & SOC. (forthcoming) [hereinafter Zimmerman, *Alternative Execution Methods*]; Paul R. Zimmerman, *State Executions, Deterrence, and the Incidence of Murder*, Mar. 3, 2003, *available at* http://ssrn.com/abstract=354680.

41. H. Naci Mocan & R. Kaj Gittings, *Getting Off Death Row: Commuted Sentences and the Deterrent Effect of Capital Punishment*, 46 J.L. & ECON. 453 (2003).

Their results also indicated that both commuting death row prisoners' sentences and removing them from death row increased in murder. Specifically, each commutation resulted in approximately five extra murders and each removal from death row generated one additional murder.

viii. A recent paper by Lawrence Katz, Steven D. Levitt, and Ellen Shustorovich used state-level panel data covering the period 1950–1990 to measure the relationship between prison conditions, capital punishment, and crime rates.[42] They found that the nonexecution death rate among prisoners (a proxy for prison conditions) had a significant, negative relationship with overall violent crime rates and property crime rates; worse prison conditions deterred crime. As expected, the execution rate had no statistically significant relationship with overall violent crime rates (which consist mainly of robbery and aggravated assault rates) and property crime rates; that is, executions had no effect on noncapital crimes.

The authors estimated several different models to test for a relationship between the execution rate and murder rates. Although some specifications showed no relationship, many models, especially those that controlled for the economic and demographic differences among states, did produce a deterrent effect.

2. *Modern Economics Papers Using Other Techniques*

All modern economics papers that used techniques other than panel data also found deterrence.

i. Instead of a panel data study, Dale O. Cloninger and Roberto Marchesini conducted a portfolio analysis that was, in effect, a controlled group experiment: the Texas unofficial moratorium on executions during most of 1996.[43] They found both that the moratorium appeared to have caused additional homicides and that murder rates significantly decreased after the moratorium was lifted.

ii. Harold J. Brumm and Dale O. Cloninger used cross-sectional data covering fifty-eight cities in 1985 to distinguish between criminals' perceived risk of punishment and the ex-post risk of punishment measured by arrest rates, conviction rates, or execution rates.[44] They found that the perceived risk of punishment, including the probability of execution, was negatively and significantly correlated with the homicide commission rate.

iii. and iv. Two other papers, one by Isaac Ehrlich and Zhiqiang Liu and the other by Zhiqiang Liu, used Ehrlich's original state-level, cross-section data.[45] Both found a strong deterrent effect.

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42. Lawrence Katz et al., *Prison Conditions, Capital Punishment, and Deterrence*, 5 AM. L. & ECON. REV. 318 (2003).

43. Dale O. Cloninger & Roberto Marchesini, *Execution and Deterrence: A Quasi-Controlled Group Experiment*, 33 APPLIED ECON. 569 (2001).

44. Harold J. Brumm & Dale O. Cloninger, *Perceived Risk of Punishment and the Commission of Homicides: A Covariance Structure Analysis*, 31 J. ECON. BEHAV. & ORG. 1 (1996).

45. Isaac Ehrlich & Zhiqiang Liu, *Sensitivity Analyses of the Deterrence Hypothesis: Lets Keep the Econ in Econometrics*, 42 J.L. & ECON. 455 (1999); Zhiqiang Liu, *Capital Punishment and*

3. *Modern Papers by Sociologists and Criminologists*

Sociologists have also studied the deterrent effect of capital punishment in several papers in sociology journals in the past decade. Although they employed empirical analysis, the methods they used are often very different from the methods used by economists. In contrast to the economics studies, most of the sociology studies find no deterrence.

i. John K. Cochran, Mitchell B. Chamlin, and Mark Seth examined the deterrence question using weekly, time-series data from Oklahoma from 1989 to 1991.[46] Although their weekly data was very disaggregated by time, the researchers severely restricted the number of observations in their study by limiting their analyses to the state of Oklahoma. Thus, they have only 156 observations. In fact, only one execution took place in Oklahoma during this period. Furthermore, the authors included no variables to control for demographic, economic, law enforcement, or other factors on murder rates. The researchers concluded that there was no deterrent effect because they found no evidence of deterrence after the one execution during their sample period.

ii. William Bailey used the same data as Cochran, Chamlin, and Seth to explore the deterrence issue and found no evidence of a deterrence effect.[47] Although his data suffered from having few observations and only one execution, Bailey extended the analyses to include control variables. Moreover, Bailey examined the effect of executions in other states on Oklahoma's murder rate. Although most capital punishment studies had assumed that deterrence was limited to the state where the execution occurs, Bailey measured whether there was a cross-state effect. He found no evidence of a deterrent effect within states or across states.

iii. A paper by Jon Sorensen, Robert Wrinkle, Victoria Brewer, and James Marquart tested the deterrence hypothesis in Texas.[48] The authors used monthly time-series data from the state of Texas from 1984 to 1997 and found no deterrent effect when including the appropriate control variables.[49]

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the Deterrence Hypothesis: Some New Insights and Empirical Evidence, 30 E. ECON. J. 237 (2004). The study by Ehrlich and Liu offers a theory-based sensitivity analysis of estimated deterrent effects. Liu's study uses switching regression techniques in estimations that take into account the endogenous nature of the status of the death penalty.

46. John K. Cochran et al., *Deterrence or Brutalization? An Impact Assessment of Oklahoma's Return to Capital Punishment*, 32 CRIMINOLOGY 107 (1994).

47. William C. Bailey, *Deterrence, Brutalization, and the Death Penalty: Another Examination of Oklahoma's Return to Capital Punishment*, 36 CRIMINOLOGY 711 (1998).

48. Jon Sorenson et al., *Capital Punishment and Deterrence: Examining the Effect of Executions on Murder in Texas*, 45 CRIME & DELINQ. 481 (1999).

49. The authors restricted their analysis to an ordinary least squares regression that assumed that the causality between murder and law enforcement variables ran in only one direction: conviction rates, incarceration rates, and executions affected crime rates, but crime rates did not affect conviction rates, incarceration rates, or executions. In contrast, almost all other capital punishment papers assumed that causality runs in both directions; for example, increasing murders may lead officials to direct more resources to fighting crime, increasing convictions, incarcerations, and exe-

iv. James A. Yunker tested the deterrence hypothesis using two sets of postmoratorium data: state cross-section data from 1976 to 1997, and national time-series data from 1930 to 1997.[50] These data were vulnerable to many of the same criticisms as early economic studies. National time-series data may cause aggregation bias; cross-section data could not consider trends in crime or law enforcement variables and failed to control for omitted jurisdiction-specific variables that may affect crime. He found a strong deterrent effect in the time-series data that disappeared when the data were limited to the 1930–1976 period. Therefore, he concluded that postmoratorium data was critical in testing of the deterrence hypothesis.

v. A paper by Richard Berk, a sociologist, found that eliminating a few specific states from the data caused estimates of capital punishment's average impact on murders across all states to show no deterrence.[51]

4. *Modern Papers in Law Reviews*

Two empirical papers testing whether capital punishment deters have been published in law reviews in the past decade. Both found no deterrence.

i. Craig J. Albert tested the deterrence hypothesis using state-level panel data from 1982 to 1994.[52] He includes many of the same control variables as Ehrlich did in his early studies, but does not include any time variables. Like Ehrlich, he also performed both ordinary least squares regressions and two-stage least squares regressions. Albert found no evidence of a deterrent effect.

ii. Lisa Stolzenberg and Stewart J. D'Alessio used monthly data and a different statistical procedure from other papers to examine the relationship between the frequency of executions, newspaper publicity, and the incidence of murder in Houston, Texas.[53] They examined the period from January 1990 to December 1994. The authors included no control variables to capture changes in economic, demographic, or other factors during the time period. The authors reported no deterrent effect.

5. *A Theory for Reconciling the Results*

Although the results of all of the articles in economics journals supported the deterrence hypothesis, this consensus did not cross disciplines.

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cutions. Ignoring the reverse causality could lead to biased results that underestimate, overestimate, or reverse the impact of law enforcement variables on crime.

50. James A. Yunker, *A New Statistical Analysis of Capital Punishment Incorporating U.S. Postmoratorium Data*, 82 Soc. Sci. Q. 297 (2001).

51. Richard Berk, *New Claims about Executions and General Deterrence: Déjà Vu All Over Again?*, Mar. 11, 2005, *available at* http://preprints.stat.ucla.edu/396/JELS.pap.pdf.

52. Craig J. Albert, *Challenging Deterrence: New Insights on Capital Punishment Derived from Panel Data*, 60 U. Pitt. L. Rev. 321 (1999).

53. They use "fully recursive vector ARMA [regressions] . . . ." Lisa Stolzenberg & Stewart J. D'Alessio, *Capital Punishment, Execution Publicity and Murder in Houston, Texas*, 94 J. Crim. L. & Criminology 351, 352 (2004).

Most of the articles in sociology journals and law reviews found no evidence of a deterrent effect.

The contrasting conclusions may all be correct if capital punishment's impact on the murder rate differs among jurisdictions. Because the studies examined different jurisdictions in different periods, some may examine jurisdictions that have an overall deterrent effect while others examine jurisdictions that experience no deterrence. The rest of this Article will explore both whether the deterrent effect differs across states and possible causes of the earlier studies' differing results.

III. Testing the Deterrence Hypothesis among States

After reviewing differences in the frequency and manner with which states apply capital punishment, I describe my empirical model for testing executions' impact on murders in each state. I then discuss the model's results: executions' impact varies widely among states, deterring murders in some states, but increasing them in others. Finally, I show how this Article's results reconcile results from earlier papers.

A. *Differences in the Application of Capital Punishment across States*

There are great differences in the application of the death penalty across states. For example, states vary widely in their definitions of capital crimes, their frequency of imposing capital sentences, their frequency of executions, their methods of execution, and the publicity their executions receive. These important differences might affect the deterrent impact of each states' executions.

Table 1 and Appendices 1 through 3 present some of the important differences between states' application of the death penalty. Appendix 1 discusses the crimes punishable by death as of 2001. It is difficult precisely to compare states' laws for capital punishment because states define first-degree murder and aggravating factors differently. But there are important differences in the crimes punishable by death. For example, in Georgia, any murder is technically a death-eligible crime, although, of course, the U.S. Constitution substantially limits the reach of Georgia's death penalty. In contrast, Alabama and Pennsylvania treat only first-degree murders with eighteen aggravating circumstances to be punishable by death.

Although the legislation listed in Appendix 1 tells us what crimes *could* be punished by death in each state, states vary tremendously in how often they *actually* sentence people to death. Table 1 reports the number of death row sentences imposed between 1977 and 1996; the table includes information only on states that have actually sentenced people to death during this period.[54] The numbers vary from an extreme high of 713 death row sentences in Florida to only one death row sentence in New York.

54. For this table, I limit my sample to 1977–1996 because my empirical estimations cover this period.

TABLE 1
EXECUTIONS AND DEATH ROW SENTENCES: 1977–1996

State	Number of Death Row Sentences	Number of Executions
Alabama	294	13
Arizona	207	6
Arkansas	90	12
California	560	4
Colorado	12	0
Connecticut	6	0
Delaware	22	8
Florida	713	38
Georgia	226	22
Idaho	34	1
Illinois	262	8
Indiana	82	4
Kentucky	60	0
Louisiana	128	23
Maryland	47	1
Mississippi	133	4
Missouri	142	23
Montana	10	1
Nebraska	19	2
Nevada	119	6
New Jersey	48	0
New Mexico	13	0
New York	1	0
North Carolina	308	8
Ohio	249	0
Oklahoma	238	8
Oregon	52	1
Pennsylvania	283	2
South Carolina	129	11
South Dakota	2	0
Tennessee	142	0
Texas	668	107
Utah	17	5
Virginia	104	37
Washington	28	2
Wyoming	5	1

As with death sentences, the number of executions that states perform varies substantially. The last column of Table 1 reports each state's number of executions performed between 1977 and 1996. Some states produce many death sentences, but few executions. For example, during the sample period, California condemned 560 people, but executed only four. Twelve states do not have laws that authorize capital punishment: Alaska, Hawaii, Iowa, Maine, Massachusetts, Michigan, Minnesota, North Dakota, Rhode Island, Vermont, West Virginia, and Wisconsin. Of the thirty-eight states that currently have capital punishment laws, eleven had performed no executions prior to 1997: Colorado, Connecticut, Kansas, Kentucky, New Hampshire, New Jersey, New Mexico, New York, Ohio, South Dakota, and Tennessee.[55] At the other extreme, Texas performed 107 executions between 1977 and 1997.

Appendix 2 lists the authorized methods of execution by state. All states except Nebraska allow executions by lethal injection; Nebraska still requires electrocution.[56] Some states allow prisoners to choose between lethal injection and electrocution. Utah allows prisoners to be executed by firing squad if the inmates chose this method before the passage of legislation in 2004 banning the practice.[57]

States also differ in how much publicity each execution receives. Appendix 3 reports each state's average number of newspaper articles and news transcripts found on LexisNexis that covered each execution between 1997 and 1999.[58] The numbers differ widely: Colorado and Ohio had averages of over 700 news reports (including both newspapers and transcripts) for each of their executions, probably because of their novelty, as these were the first executions in the states. At the other extreme, there was only one news transcript on LexisNexis reporting on Montana's 1998 execution.

The substantial differences in both the application of capital punishment and publicity about it might cause differences in whether each state's executions are a deterrent. The next Section explores whether there are differences in executions' impact among states.

To be sure, there are many other differences among states in the application of capital punishment. However, most of the differences, such as whether capital punishment is applied unfairly or in a racist manner, are impossible to measure. Standard statistics demonstrates that the absence from the analysis of these other factors will not harm the results except in

55. Colorado and Kentucky performed their first executions in 1997, New Mexico executed its first prisoner in 2001, Ohio performed its first execution in 1999, and Tennessee executed its first prisoner in 2000. New York's death penalty law was declared unconstitutional in 2004. People v. LaValle, 817 N.E.2d 341, 344 (N.Y. 2004).

56. Nebraska's last execution was in 1997, by electrocution.

57. Two of Utah's six executions since 1977 have been by firing squad. Four more executions by firing squad are scheduled for upcoming years.

58. I used LexisNexis to search for the name of each executed person in the month before the execution, the month of the execution, and the month after the execution. I searched both newspapers in the state where the execution took place and all news transcripts. Although the numbers are good approximations of the amount of publicity each execution receives, they are not perfect because LexisNexis does not cover all newspapers and started covering some newspapers in the mid to late 1990s. I searched for executions only after 1997 to minimize the problem of lack of or uneven coverage.

the rare situation where the omitted factors are closely correlated with the included variables.[59]

B. *Data and Empirical Model*

Because the data and techniques of my 2003 paper[60] were accepted in a leading peer-reviewed journal and have become well-known in the capital punishment debate,[61] I use the same data and similar analyses as before as my model, except that I now test the effect of executions in different states. The data are a panel-data set that covers 3,054 counties for the 1977–1996 period. It is a well-known data set that has been used not only in my 2003 capital-punishment paper, but in several other empirical studies of crime.[62] The county-level data allow me to include county-specific characteristics in my analysis; I discuss the county-level economic and demographic variables below. This reduces the aggregation problem from which much of the literature suffers. By controlling for these characteristics, I can better isolate the effect of punishment policy.[63]

To test capital punishment's effect in different states, I estimate a system of equations that represents the interaction between criminals and the

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59. *See* PETER KENNEDY, A GUIDE TO ECONOMETRICS 91 (3d ed. 1992).

60. Dezhbakhsh et al., *supra* note 31.

61. Publicity surrounding the original study included television interviews on CNN Sunday; National Fox News; The O'Reilly Factor on the National Fox News Network; and CBS, ABC, and FOX local affiliates. Print interviews included the *Chronicle of Higher Education* and *The Atlanta Business Chronicle*. Radio interviews included BBC; Five Alive; WJR in Detroit, MI; KRLD in Arlington, TX; WLW in Cincinnati, OH; KTSA in San Antonio, TX; CHED in Edmonton, Canada; WRVA in Richmond, VA; CJME in Saskatoon, Canada; NTR in Saskatoon, Canada; WMVZ in Detroit, MI; KXNT in Las Vegas, NV; and KRLA in Los Angeles, CA. The paper was also cited in the National Center for Policy Analysis; Executive Alert; The Weekly Standard; and The National Journal. The paper was also requested for use by the Senate Judiciary Committee; U.S. Naval Academy; House of Representatives (Rep. Bob Goodlatt); Attorney General of Alabama; New York State Assembly (Stephen Kaufman); and the Chief of Criminal Appeals Division of Chicago (Renee Goldfarb).

62. Dezhbakhsh et al., *supra* note 31; *see also* EARL L. GRINOLS ET AL., CASINOS, CRIME AND COMMUNITY COSTS (Univ. of Ill. & Univ. of Ga., Working Paper, 2000); LOTT, JR. & LANDES, *supra* note 39; Eric D. Gould et al., *Crime Rates and Local Labor Market Opportunities in the United States: 1979–1997*, 84 REV. ECON. & STAT. 45 (2002); John R. Lott, Jr. & David B. Mustard, *Crime, Deterrence, and Right-to-Carry Concealed Handguns*, 26 J. LEGAL STUD. 1, 39–48 (1997); David B. Mustard, *Reexamining Criminal Behavior: The Importance of Omitted Variable Bias*, 85 REV. ECON. & STAT. 205 (2003); Joanna M. Shepherd, *Fear of the First Strike: The Full Deterrent Effect of California's Two- and Three-Strikes Legislation*, 31 J. LEGAL STUD. 159 (2002); Joanna M. Shepherd, *Police, Prosecutors, Criminals, and Determinate Sentencing: The Truth about Truth-in-Sentencing Laws*, 45 J.L. & ECON. 509 (2002).

63. Moreover, panel data allow me to overcome the unobservable heterogeneity problem that affects cross-sectional studies. Neglecting heterogeneity can lead to biased estimates. I use the time dimension of the data to estimate county-fixed effects and condition my two-stage estimation on these effects. This is equivalent to using county dummies to control for unobservable variables that differ among counties. This way I control for the unobservable heterogeneity that arises from county specific attributes such as attitudes towards crime, or crime reporting practices. These attributes may be correlated with the justice-system variables (or other exogenous variables of the model) giving rise to endogeneity and biased estimation. An advantage of the data set is its resilience to common panel problems such as self-selectivity, nonresponse, attrition, or sampling design shortfalls.

criminal justice system. Such systems are commonly used in empirical studies of crime, and especially in empirical studies of capital punishment.[64] A system of equations, instead of a single equation, is required because of the relationship between murder rates and the behavior of the police and court system. Specifically, if there is a deterrent effect, then increases in what I call the "deterrent factors"—the probability of arrest, the probability of receiving a death row sentence, or the probability of execution—should cause murder rates to decrease. However, the causal relationship could also run in the other direction: increases in murder rates could pressure police, prosecutors, judges, and juries to increase arrest rates, death row sentencing rates, and execution rates. Because a single-equation model would be unable to capture, and correct for, the reverse causality, the model could produce biased, incorrect results. My system of equations addresses that problem.

1. *A Summary of the Model*

The model has four equations. The first equation examines the influences on the murder rate of various factors, including the deterrent variables; the second through fourth equations model the influence on each of the deterrent variables. In a nutshell:

- The first equation measures how murder rates respond to the deterrent variables and other demographic and economic factors;
- the second equation measures the effect on the first deterrent variable, the probability of arrest, of murder rates and police expenditures;
- the third equation measures the effect on the second deterrent variable, the probability of a capital sentence, of murder rates, expenditures on the judicial system, prison admissions, and a partisan influence variable that measures the political conservatism of a state's voters; and
- the fourth equation measures the effect on the final deterrent variable, the probability of execution, of murder rates, expenditures on the judicial system, and a partisan influence variable.

The system of equations is the same system used in my previous capital-punishment paper[65] with one exception: instead of using one execution variable that estimates the average deterrent effect across all executions in all states, I use fifty execution variables that estimate the deterrent effect separately for each state.

64. See all of the modern economics papers listed *supra* Section III.B.1.

65. Dezhbakhsh et al., *supra* note 31, at 352.

2. *The Model's Technical Structure*

For technically-inclined readers, I express the system symbolically:

$$M_{i,t} = \alpha_i + \beta_1 Pa_{i,t} + \beta_2 Ps \mid a_{i,t} + \beta_3 SD_i Pe \mid s_{i,t} + \gamma_1 Z_{i,t} + \gamma_2 TD_t + \varepsilon_{i,t}, \quad (1)$$

$$Pa_{i,t} = \phi_{1,i} + \phi_2 M_{i,t} + \phi_3 PE_{i,t} + \phi_4 TD_t + \varsigma_{i,t}, \quad (2)$$

$$Ps \mid a_{i,t} = \theta_{1,i} - \theta_2 M_{i,t} - \theta_3 JE_{i,t} - \theta_4 PI_{i,t} - \theta_5 PA_{i,t} - \theta_6 TD_t - \xi_{i,t}, \quad (3)$$

$$Pe \mid s_{i,t} = \psi_{1,i} + \psi_2 M_{i,t} + \psi_3 JE_{i,t} + \psi_4 PI_{i,t} + \psi_5 TD_t + \zeta_{i,t}, \quad (4)$$

where M is county murder rates, Pa is the arrest rate for murder in each county, $Ps|a$ is the conditional probability of receiving a death sentence if arrested, $Pe|s$ is the conditional probability of execution if sentenced to death row, Z is a series of economic and demographic variables, PE is police payroll expenditure, JE is public expenditure on all participants in the judicial system, PI is partisan influence as measured by the Republican presidential candidate's percentage of the statewide vote in the most recent election, PA is prison admissions, TD is a set of time dummies that capture national trends in these perceived probabilities, and ς, ξ and ζ are error terms.

3. *The Model's Details*

The first equation measures the response of the behavior of criminals to the deterrent factors while controlling for a series of other factors found in the series Z. To determine whether a change in the murder rate is really due to the deterrent variables, the equation permits us to make sure that the other factors are not really the cause of the change. First, Z includes the aggravated assault and robbery rates because some murders are the by-products of violent activities such as aggravated assault and robbery. Including these variables permits us to see whether a change in the murder rate is due to a change in the deterrent variables, or is instead due to a change in the number of aggravated assaults or robberies.

In addition, Z measures possible economic and demographic influences on crime. Economic variables are used as proxy for legitimate and illegitimate earning opportunities. An increase in legitimate earning opportunities increases the opportunity cost of committing crime, and should result in a decrease in the crime rate. For example, if more higher-paying jobs become available, then criminals may stop committing crimes and obtain these jobs instead. Likewise, an increase in illegitimate earning opportunities increases the expected benefits of committing crime, and should result in an increase in the crime rate.

The economic variables that I use are real per capita personal income, real per capita unemployment insurance payments, and real per capita income maintenance payments. The income variable measures both the labor market prospects of potential criminals and the amount of wealth available to steal. The unemployment payments variable is a proxy for overall labor market conditions and the availability of legitimate jobs for potential criminals. The transfer payments variable represents other nonmarket income earned by poor or unemployed people.

Demographic variables include population density, and six gender and race segments of the population ages ten to twenty-nine (male, female; black, white, other). Population density is included to capture any relationship between drug activities in inner cities, which are correlated with population density, and the murder rate. For example, an increase in crime in a county may not be due to changes in the deterrent variables, but instead to increasing population density.

The age, gender, and race variables represent the possible differential treatment of certain segments of the population by the justice system, changes in the opportunity cost of time through the life cycle, and gender/racially based differences in earning opportunities. For example, an increase in crime could be due to an increase in the number of young minorities, who, because of racial discrimination by employers, have no legitimate job opportunities, and must instead turn to crime.

The control variables also include the state level National Rifle Association (NRA) membership rate. It is possible that the level of gun ownership could affect the crime level, either up or down.[66]

The last three equations measure the influences on the level of effort of law enforcement agencies and the criminal justice system in apprehending, convicting, and punishing perpetrators. Police and judicial/legal expenditure, *PE* and *JE*, represent spending on enforcement. As more expenditures increase law enforcement's capabilities, the probabilities of both arrest and conviction given arrest should increase.

Partisan influence, indicating whether a jurisdiction is conservative, is used to capture any political pressure to get tough with criminals, a message popular with Republican candidates. The influence is exerted through changing the makeup of the court system, such as the appointment of new judges or prosecutors who are tough on crime. This affects the justice system and is, therefore, included in equations (3) and (4).

Prison admission, defined as the number of new court commitments admitted during each year,[67] is a proxy for the existing burden on the justice system. The burden may affect judicial outcomes. For example, judges may hesitate to impose long sentences if the jails have recently been filled with many new sentenced prisoners.

As is standard and appropriate in such analysis, all four equations also include a set of time dummy variables that capture national trends and influences affecting all counties but varying over time. The variables correct for the possibility that a change in murder rate may be due, not to the deterrent variables, but to national trends in murder rates. In addition, county dummies are included to control for unobservable variables that differ among counties, such as differences in crime, attitudes towards crime, or differences in the justice system. Two states may continually have different

66. Lott, Jr. & Mustard, *supra* note 62.

67. This does not include returns of parole violators, escapees, failed appeals, or transfers.

murder rates, not because of differences in the deterrent variables, but because of other unobservable differences between the two states. The county dummy variables will capture any factors that I have not otherwise included that are constant for a county over time.

As is normal and appropriate, I estimate the simultaneous system of equations (1)–(4) with a corrected[68] two-stage least squares regression.[69]

4. *The Model's Six Variations*

Following my earlier paper's analysis, I estimated six different versions of my model.[70] The models differ only in the way that the perceived probabilities of a death sentence and execution are measured. Estimating these probabilities in different ways is standard in empirical crime papers and even dates back to Ehrlich's first capital punishment paper.[71] I based the variant models on those most often seen in the literature.

For Model 1, the conditional execution probability—the probability that a person with a death sentence is actually executed—is measured by executions at year t divided by number of death sentences six years earlier, at year t-6. For Model 2 this probability is measured by number of executions six years in the future at t+6, divided by the number of death sentences at t. The two ratios reflect forward looking and backward looking expectations, respectively. The displacement lag of six years reflects the lengthy waiting time between sentencing and execution, which averages six years for the period I study.[72]

For the probability of a death sentence given that a person has been arrested, I use a two-year lag displacement, reflecting an estimated two-year lag between arrest and sentencing. Therefore, the conditional sentencing probability for Model 1 is measured by the number of death sentences in year t divided by the number of arrests for murder at year t-2. For Model 2 this probability is measured by number of death sentences at t+2 divided by number of arrests for murder at t. Because of the absence of an arrest lag—arrests usually occur soon after a murder—no lag displacement is used to

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68. The estimation is weighted to correct for the heteroskedasticity of the error term. These equations represent the aggregation of an individual's equations. In the individual equations, the error terms are stochastic with mean zero and variance c^2. Because of this, when the error terms are summed over n (the number of people in the county), the new error terms are heteroskedastic because their variances (c^2/n) are proportional to county population. Tests for heteroskedasticity indicate that the error term in the unweighted regression is indeed heteroskedastic. Tests indicate that the heteroskedasticity has been corrected after weighting by the square root of the county population. In addition, tests for overidentification indicate that the model is correctly specified and employs valid instruments.

69. I chose a single-equation method, two-stage least squares, over a systems method because in a systems method any specification error in one equation is propagated throughout the system, which can lead to inconsistency. WILLIAM H. GREENE, ECONOMETRIC ANALYSIS 616 (2d ed. 1993). Single-equation methods, such as two-stage least squares, confine the error to the particular equation in which it appears.

70. Dezhbakhsh, et al., *supra* note 31, at 361.

71. *See* Ehrlich, *supra* note 19.

72. THE DEATH PENALTY IN AMERICA, *supra* note 8.

measure the arrest probability. It is simply the number of murder-related arrests at *t* divided by the number of murders at *t*.

These measures are not the true probabilities of arrest, sentencing, or execution. Instead, they are averages. However, they are closer to the probabilities as viewed by potential murderers than would be the true measures. This formulation from our previous paper is consistent with a previous study that shows that criminals form perceptions based on observations of friends and acquaintances.[73]

For Model 3, I measure the conditional probability of execution given a death sentence by using a six-year moving average. The probability of execution at year *t* is defined as the sum of executions during (*t*+2, *t*+1, *t*, *t*-1, *t*-2, and *t*-3) divided by the sum of death sentences issued during (*t*-4, *t*-5, *t*-6, *t*-7, *t*-8, and *t*-9). The six-year window length and the six-year displacement lag capture the average time from sentence to execution for my sample. In a similar fashion, a two-year lag and a two-year window length is used to measure the conditional death sentencing probabilities. Given the absence of an arrest lag, no averaging or lag displacement is used when computing arrest probabilities.

Models 4, 5, and 6 are similar to Models 1, 2, and 3 except for the way they treat undefined probabilities. In several years some counties had no murders, and some states had no death sentences. This rendered some probabilities in Models 1 through 3 undefined because of a zero denominator. Estimates in Models 1 through 3 are obtained excluding these observations.

To avoid losing data points in Models 4 through 6, for any observation (county/year) where the probabilities of arrest or execution are undefined, I substituted the relevant probability from the most recent year when the probability was not undefined. I look back up to four years, because in most cases this eradicates the problem of undefined probabilities. The assumption underlying such substitution is that criminals will use the most recent information available in forming their expectations. So a person contemplating committing a crime at year *t* will not assume that he will not be arrested if no crime was committed, and hence no arrest was made, during this period. Rather, he will form an impression of the arrest odds based on arrests in recent years. This approach mirrors that in earlier published research.[74]

None of the models is necessarily more theoretically correct than the others. They each represent a different way that criminals may think about their probability of execution, for example, do criminals think about how many people were sentenced to death row six years ago and how many people were executed this year (Models 1 and 4), or do they consider how many people were sentenced to death row this year and how many people they expect to be executed six years from now (Models 2 and 5), or do they apply an averaging approach and consider the sum of how many people have been

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73. Raaj K. Sah, *Social Osmosis and Patterns of Crime*, 99 J. POL. ECON. 1272 (1991).

74. *See id.* For the states that have never had an execution, the conditional probability of execution takes a value of zero. For the states that have never sentenced anyone to death row, the conditional probability of a death row sentence takes a value of zero.

executed recently and will be in the next few years versus the sum of how many people were sent to death row over a six-year period in the past (Models 3 and 6)? Because no one is sure which model best describes how criminals perceive risks, I present results from all of them.

C. *Empirical Results*

The results are striking. Executions deter murder in a few states, have no impact in a few more, but *increase* murders in many more states than the number where there is deterrence.

The results of the two-stage least squares, weighted estimation with fixed effects, is reported in the table in Appendix 4. Although I estimate the entire simultaneous equation system (1)–(4) for each of the six variations of the model separately, I report in the table the results of only the murder equation (1); this is the equation that reveals the relationship between executions and the murder rate.[75] The table presents the results of all six variations of the model because none of the models is necessarily most correct.

The table reports the total effect of the execution probability on the murder rate in each state. For each state and model, the regression coefficient (top number) indicates the magnitude and direction of the effect. A negative coefficient indicates deterrence. A positive coefficient indicates that executions instead increase murders. In the capital punishment literature, an increase in murders because of executions is often referred to as a "brutalization effect." Executions create an atmosphere of brutality that spurs criminals to more violence.

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75. The effects of many other variables on murder are also consistent across models. As expected, the murder rate has a statistically significant, positive relationship with both the aggravated assault rate and the robbery rate in all six models. Many murders are committed during another crime. The arrest rate and probability of receiving a death row sentence are negatively related to the murder rate in most models, indicating a deterrent effect of these variables. However, the probability of a death row sentence is not statistically significant in some of the models.

Many of the demographic variables also have the expected relationships with murder rates. The percent of the county population that is African American has a statistically significant, positive relationship with murder in most of the models. Some minority groups have fewer legitimate earning opportunities, and thus a lower opportunity cost of criminal activities relative to their white counterparts. The percentage of the population that is male has a statistically significant, positive relationship with murder in all models; most murders are committed by men. The percentage of the population that is ten to nineteen years of age has a negative and significant relationship with murder in most of the models.

The murder rate is positively related to both per capita real income and per capita real welfare payments in all models. This suggests that overall income measurements for a county represent the amount of wealth available to steal in the county; as the amount of wealth available to steal increases, crime increases. The real per capita unemployment insurance payments have a statistically significant, negative relationship with murder rates: more aid to unemployed people lowers their need for criminal activity.

Population density has a statistically significant, negative relationship in all models. Although murder rates are higher in more densely populated cities, they are not higher in more densely populated counties—my unit of measurement. The majority of the most densely populated counties are suburban counties that tend to have lower crime rates than either urban or rural counties.

The coefficients for three variables are statistically insignificant in most models: the percentage of the population that is twenty to twenty-nine, the percentage of the population that belongs to a minority group other than African American, and the NRA membership rate.

Not all of the results are statistically significant. The table reports the t-statistics (bottom number) for each state and model. T-statistics equal to or greater than 1.645 are considered statistically significant at the 10% level and t-statistics equal to or greater than 1.96 are considered statistically significant at the 5% level. A t-statistic of 1.645 means that there is ninety percent certainty that the coefficient is different from zero. Empiricists typically require t-statistics of at least 1.645 to conclude that one variable affects another in the direction indicated by the coefficient.

The results reveal large variation across states in capital punishment's effect on murder. Among the twenty-seven states that had at least one execution during the sample period, there are states where executions deter murders. There are states where executions have no effect on murder. And there are many states where executions *increase* murders.

Despite the differences in the way the conditional probabilities are estimated across the six models, the results for each state are quite robust. Although the level of statistical significance differs for some models, the direction (that is, positive or negative effect) of the statistically significant coefficients within each state is the same, regardless of the model.

To permit interpretation of the results in Appendix 4, I transformed the statistically significant coefficients from each of the six models into each state's increase or decrease in number of murders after one execution.[76] From among these, I then selected each state's median change.

Each state's median increase or decrease in murders per execution is graphed in Figure 2. The figure shows that the executions in six states have a deterrent effect. For these states, the median decrease in the number of murders from each execution ranges from sixty-one in South Carolina to six in Nevada. Eight states experience no change in murders after executions.

76. The coefficients in Table 1 are the partial derivatives of murder per 100,000 population with respect to each model's measure of the probability of execution given sentencing. Given the measurement of these variables, the following transformations give the change in the number of murders as a result of one execution in 1996 (the most recent year of data):

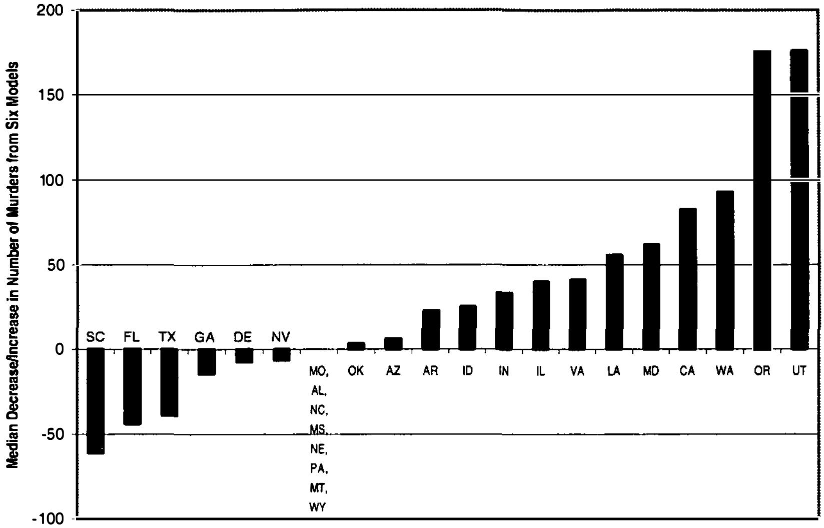
β_3 (Population$_{1996}$/100,000) ($1/S_{1990}$) for models 1 and 4,

β_3 (Population$_{1990}$/100,000) ($1/S_{1990}$) for models 2 and 5,

β_3 (Population$_{1996}$/100,000) ($1/[S_{1992}+ S_{1991}+ S_{1990}+ S_{1989}+S_{1988}+S_{1987}]$ for models 3 and 6,

where S is the number of individuals sentenced to death. I perform these transformations for every coefficient in Table 1 that is significant at the 10% level. Then, I find the median result of the transformations for each state to obtain the median increase or decrease in number of murders after one execution. I use medians instead of means so that the numbers will not be influenced by extreme outliers. Results using the mean value of the transformations are similar.

FIGURE 2
INDIVIDUAL STATE DETERRENT EFFECTS:
NUMBER OF MURDERS DETERRED OR INCITED
BY AN AVERAGE EXECUTION



In contrast, thirteen states have a median *increase* in murders after each execution, suggesting a brutalization effect. The magnitude of the increase ranges from three in Oklahoma to 175 in Utah and Oregon.[77] However, three of the states experiencing an increase—Idaho, Maryland, and Oregon—performed only one execution during my sample period. Much caution is in order before drawing conclusions based on the experience of one execution.

Figure 2 reports a very different picture from the previous empirical studies that found that executions deterred murders. In many states, executions have a brutalization effect, increasing the number of murders. In contrast, a deterrent effect exists in far fewer states. Likewise, more than three times as many states have a brutalization effect or no effect (twenty-one) than have a deterrent effect (six).

Some back-of-the-envelope calculations permit a very rough estimate of the sizes of the brutalization and deterrent effects for states that are above and below the threshold. We can also get some idea of the net effect. It should be recognized that, because of the calculation method, the estimates are very imprecise. Nonetheless, they provide some idea of orders of magnitude.

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77. The special characteristics of the executions in Utah and Oregon may cause the large increase in murders. Utah has executed people by firing squad, and Oregon executed only one person during the sample period.

There were 192 executions between 1977 and 1996 in deterrent states, fifty-four in no-effect states, and 112 in brutalization states. I multiplied the median number of murders deterred or incited in each state by the total number of executions in each state to compute the net lives saved or lost. In deterrent states over the twenty-year sample period, executions saved approximately 6,918 lives.

Subtracting the 192 executions yields a net lives saved of 6,726, or approximately 336 per year. In no-effect states, fifty-four lives were lost, all executions.

In brutalization states, the 112 executions caused approximately 5,246 murders—again, this estimate is very rough. Adding the 112 executions yields the total lives lost of 5,358. This was 268 per year.

Although these estimates are inexact, they do suggest that both the deterrent and brutalization effects can be substantial. Executions save many lives in deterrent states. But, in brutalization states, they lead to the deaths of almost as many innocent people.

The following rough comparison of the lives saved in deterrent states and lost in brutalization states suggests that, for the country as a whole, capital punishment saves lives. That is, considering the country as a whole and adding the effects in every state that conducts capital punishment, my results suggest, if with imprecision, that executions save lives. Said another way, if the country's only choices were either to continue with the present levels of executions in each state or end executions in all states, then continuing the executions saves lives.

Considering the lives only of innocent people—ignoring the deaths of the convicts whom executions kill directly—capital punishment saves lives. Subtracting the murders caused in the brutalization states (very roughly 5,246) from those saved in the deterrence states (6,918) shows that net lives saved nationwide from executions is 1,672, or eighty-four per year. Capital punishment even saves lives if we add in the lives of those who are executed. Subtracting both the 5,358 total lives lost in brutalization states, including the lives of those executed and the additional murders caused by the executions, and the fifty-four executions in no-effect states, from the 6,726 net lives saved in deterrent states yields a net saving of 1,314, or sixty-six per year.

The results also show that, if saving lives were the only goal, the present pattern of executions is bad policy. Although capital punishment's net effect is now to save lives, thousands more lives could be saved if states with either no-effect or net brutalization ceased executing people. If only the deterrence states continued with their executions, then 6,918 innocent lives would be saved, or 346 per year—far more than the present system's eighty-four per year. Because of the brutalization in many states, the present system causes approximately 262 innocent people each year to die unnecessarily.

If we also consider the lives of those executed, the present system looks even worse. If only deterrence states continued executions, then a net of 6,726, or 336 per year, would be saved, compared to only 1,314, or sixty-six per year, if the present system continues. That is, approximately 270 people per year now die unnecessarily because of capital punishment in states where it does not deter.

D. *Reconciliation with Other Papers*

This Article's results are consistent with the findings of deterrence in previous economics papers. The weighted average of all of the increases or decreases shown in Figure 1, where the weights are each state's total number of executions between 1977 and 1996, is a negative 4.5; each execution deters, on average, 4.5 murders. Because states with a deterrent effect have large numbers of executions, the average nationwide effect *per execution* is deterrence, even though the effect in most states is brutalization or no effect. That is, when estimating the average effect on murders across all states, instead of estimating separate effects for each state, the results indicate a deterrent effect.[78] When all states are lumped together, the deterrent effect in six states conceals both the brutalization effect in thirteen states and the complete absence of effect in the rest.

Moreover, the results help us to understand the results of the non-economics papers that find no deterrence. These papers tend to focus on individual jurisdictions, rather than on the United States as a whole. Like those papers, my present research shows that, in many states, executions do not deter.

The results also are consistent with Richard Berk's recent paper, which suggested that findings of a deterrent effect in nationwide estimates disappear if certain states such as Texas are eliminated from the analysis.[79] My results show that Texas is one of the states with a large deterrent effect. If Texas' deterrent effect and large number of executions are excluded from national averages, then it is not surprising that the nationwide average deterrent effect would become smaller; there would be fewer executions with a deterrent effect to outweigh the executions with a brutalization effect.

IV. A Threshold Effect Helps to Explain Capital Punishment's Differing Impacts across States

I now examine possible causes of the different effects of executions in different states. First, I examine summary statistics of the characteristics of states with a deterrent effect, states with no effect, and states with a brutalization effect. Then, I perform additional regressions on the characteristics that differ significantly among the three groups of states. Finally, I discuss the results' implications.

To summarize, the analysis suggests a threshold effect. In states with fewer than a threshold of approximately nine executions during the sample

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78. My results are consistent with a recent study that shows that, in analyses that estimate capital punishment's average effect on murders across all states, dropping certain states from the analyses makes the overall deterrent effect disappear. Berk, *supra* note 51.

79. *See id.*

period, each execution increases the number of murders. In states that exceed the threshold, executions deter murder. Deterrence and nondeterrence states are not different in a statistically significant way in the other factors that I examine, such as how much publicity executions receive, the characteristics of the executed people, and the method of execution.

A. *Summary Statistics*

I group the states that had at least one execution into three groups: states with a deterrent effect (Delaware, Florida, Georgia, Nevada, South Carolina, and Texas), states with no effect (Alabama, Mississippi, Missouri, Montana, Nebraska, North Carolina, Pennsylvania, and Wyoming), and states with a brutalization effect (Arizona, Arkansas, California, Idaho, Illinois, Indiana, Louisiana, Maryland, Oklahoma, Oregon, Utah, Virginia, and Washington).

Table 2 reports the mean and median values of seven characteristics for each group of states, including the average amount of capital punishment, the amount of publicity that executions receive in the states, the characteristics of the executed people, and the execution method. For each characteristic, I also perform a mean comparison test between the states with a deterrent effect and all other states. The results of this test indicate whether the difference between the means is statistically significant.

TABLE 2
CHARACTERISTICS OF STATES WHERE EXECUTIONS DETER MURDERS, EXECUTIONS HAVE NO EFFECT ON MURDERS, AND EXECUTIONS INCREASE MURDERS

	States with Deterrent Effect	States with No Effect	States with Brutalization Effect	T-statistic from Mean Comparison Test Between Deterrent States and Other States
Total Number of Executions	32 (mean) 16.5 (median)	6.7 3	8.6 5	2.71*
Total Number of Death Row Sentences	312.8 177.5	149.3 137.5	142.2 90	1.97+
Average Publicity per Execution	63.8 54.8	36.6 37	116.2 80.5	.76
% of Executions That Were Single-Victim Offenders	70.7 81.1	80.1 89.1	50.37 62.5	.58

	States with Deterrent Effect	States with No Effect	States with Brutalization Effect	T-statistic from Mean Comparison Test Between Deterrent States and Other States
% of Executions That Were Offenders with No Prior Felony Record	24.6 22.5	41.2 27.3	18.2 16.7	.17
% of Executions That Were Offenders not on Probation, Parole, Escape, or Imprisoned	50.4 47.0	87.4 100	42.6 50	.62
% of Executions by Electrocution	34.67 10	25 0	12.02 0	1.04
Notes: The mean (top number) and median (bottom number) of each variable are reported. "*" indicates that the test is significant at the 5% level; "+" indicates that the test is significant at the 10% level.				

1. *Amount of Capital Punishment*

First, I examine differences between the groups in their total number of executions and total number of death row sentences to determine if the frequency of executions or death row sentences is related to a state's deterrent or brutalization effect. The execution numbers differ substantially. States with a deterrent effect performed an average of thirty-two executions between 1977 and 1996. States with either no effect or a brutalization effect conducted far fewer executions: approximately one-quarter as many, on average. States with no effect performed an average of 6.7 executions, while states with a brutalization effect performed an average of 8.6 executions during this period.

The pattern is similar for death row sentences. Again, deterrent states have far more death row sentences than states with no effect or a brutalization effect: more than twice as many, on average. However, brutalization states have the fewest death row sentences, with no-effect states averaging slightly more than brutalization states.

Mean comparison tests indicate that the average total number of executions are statistically different between deterrent states and other states at the 5% level. Likewise, the average values of the total number of death row sentences are statistically different between deterrence states and other states at the 10% level.

2. *Publicity*

If executions are to have any effect on murders, the publicity surrounding each execution should influence the magnitude of the effect.[80] Mean and median publicity per execution is somewhat higher for brutalization states, perhaps because the executions in the states were often the states' first execution after the moratorium. In contrast, in states where executions are frequent, each execution receives relatively little attention, however, mean-comparison tests indicate that there is no statistically significant difference between deterrent states and other states in the average publicity per execution.[81]

3. *Characteristics of Executed Persons*

I explore differences in the types of people executed to determine whether this influences the different effect of capital punishment in the states. Regression results from a separate project confirm that the deterrent effect is larger for executions of people who have killed multiple victims (instead of one victim), executions of people with no prior felony record, and executions of people who were not on probation or parole or had escaped from prison.[82] However, the mean and median values of these characteristics are similar across the deterrent states, brutalization states, and the no-effect states. Moreover, mean-comparison tests indicate that there is no significant difference in the types of people executed among the three groups of states.

4. *Method of Execution*

Finally, I compare the average method of execution between the groups. During this period, most executions were performed by electrocution or lethal injection. Other studies have found that electrocution deters more people than lethal injection.[83] Although deterrent states appear to have used electrocution more frequently than other states, the difference between the means is not statistically significant.

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80. Regression results from another project I am working on suggest that the more publicity each execution receives, the greater the deterrent effect.

81. See *supra* note 58 and accompanying text for a description of the publicity measure.

82. *See* Joanna M. Shepherd, *Executions, Deterrence, and the Characteristics of the Person Executed* (2005) (unpublished regressions) (on file with author). The full theoretical explanation of these results is beyond the scope of this paper. The varying deterrent effects for executions of different types of criminals is probably caused by both the publicity surrounding the different types of criminals and by how similar potential criminals think they are to the executed criminal.

83. Zimmerman, *Alternative Execution Methods*, *supra* note 40.

5. *The Threshold Effect*

Of the four factors, the only characteristics that vary significantly between deterrent states and nondeterrent states are the total number of executions performed in the state and the total number of death row sentences imposed in the state. Not only is the *overall* deterrent effect larger with a greater number of executions or death row sentences, the deterrent effect *per execution* is also larger.

The summary statistics are our initial indication that deterrence is subject to a threshold effect. On average, executions begin to deter murders only after some threshold number of executions has been performed. Until a state executes that number, its executions either have no effect on the number of murders or are counterproductive, causing a brutalization effect that increases murders.

Other differences among states may also be important in determining whether a state experiences a deterrent effect. Most of these, however, are impossible to measure. Possible important differences include, among others, racist application of the death penalty, how prosecutors' charging decisions are made, and the manner in which authorities determine when or if to execute a condemned prisoner.

B. *Regression Results*

I perform additional regressions to explore the threshold effect in more detail. First, I perform a spline regression to examine how the relationship between murders and executions changes as a state's total number of executions increases. Then, I perform a dummy-variable regression to examine the effect on murder rates of conducting executions when states are below the threshold compared to above the threshold.[84]

1. *Spline Regression*

A spline regression is a statistical method for determining whether there is a structural change, or threshold, in the relationship between two variables.[85] My summary statistics have suggested that states may experience a brutalization effect as they begin to perform executions, but at some threshold level of executions, a deterrent effect emerges. Spline regressions can test for such knots or thresholds in the murder rate as a state's total number of executions increases. The regressions can explore whether the direction of capital punishment's effect on murder depends on how many executions a state has performed.

The system of equations is similar to the system I used to estimate separate deterrent effects for individual states. Indeed, all variables are defined

84. Unreported regressions exploring the relationship between murder and the number of executions per prisoner yield similar results to those reported below.

85. *See* GREENE, *supra* note 69, at 237.

as before, except that the death row sentence and execution variables are replaced with a variable, $_{i,t}$, that measures the number of executions when states are below the threshold number and the number when they are above the threshold. The system of equations I estimate is:

$$M_{i,t} = \alpha_i + \beta_1 Pa_{i,t} + \beta_2 \psi_{i,t} + \gamma_1 Z_{i,t} + \gamma_2 TD_t + \varepsilon_{i,t}, \qquad (5)$$

$$Pa_{i,t} = \phi_{1,i} + \phi_2 M_{i,t} + \phi_3 PE_{i,t} + \phi_4 TD_t + \varsigma_{i,t}. \qquad (6)$$

I test for a threshold at nine total executions since 1977. The summary statistics suggest that the threshold may occur at approximately this number; a threshold of nine executions is above the mean total number for brutalization states but below the median total number for deterrent states. My regression will test for the existence of this threshold by measuring the effect on murder rates of additional executions when states have performed less than nine executions since 1977 versus when they have performed nine or more executions since 1977.

The results of the spline regression are reported in the first column of Table 3. I report both the coefficients and the t-statistics for the variable measuring the number of below-threshold executions and for the variable measuring the number of above-threshold executions. The coefficients represent the slope of the relationship between the total number of executions performed before that date and murder rate.

TABLE 3
RELATIONSHIP BETWEEN THE NUMBER OF EXECUTIONS SINCE 1977 AND MURDER RATES

Variables	Coefficients/T-statistics	
	Spline Regression	Dummy-Variable Regression
Below-the-Threshold States: States with 1–8 Executions	.05 1.99*	.28 3.68*
Above-the-Threshold States: States with 9 or More Executions	-.04 10.52*	-1.23 10.51*

The results suggest that below-threshold executions have a brutalization effect and above-threshold executions have a deterrent effect. The statistically significant, positive coefficient for the below-threshold variable indicates that, when states have conducted fewer than nine executions, each execution increases the murder rate. The statistically significant, negative coefficient for the above-threshold variable indicates that, when states have performed nine or more executions, each execution decreases the murder rate.

I also test other numbers, instead of nine, as the possible threshold level. Although I do not report the coefficients from all of these regressions, the results indicate that the threshold number is somewhere between six and eleven executions. Thresholds between six and eleven produce statistically significant coefficients that are similar in magnitude to the coefficients when nine is treated the threshold level; thresholds below six and above eleven produce statistically insignificant results. Thus, the exact threshold is likely state-specific; it will vary between six and eleven depending on the state's characteristics.

2. *Dummy-Variable Regressions*

I also perform dummy-variable regressions to test the presence of a threshold effect. Whereas the spline regression estimates the change in murder rates with each *additional* execution when the state's total number of executions is below versus above the threshold, a dummy-variable regression estimates the effect on murder rates of simply being a below-threshold state versus an above-threshold state. That is, a spline regression answers the question: What is the effect on murder rates of performing *one more execution* if a state has performed fewer than nine (or other possible threshold) executions? A dummy-variable regression answers the question: What is the effect on murder rates of having performed fewer than nine executions?

The dummy-variable regression will estimate a system of equations similar to the spline regression's system of equations:

$$M_{i,t} = \alpha_i + \beta_1 Pa_{i,t} + \beta_2 BT_{i,t} + \beta_3 AT_{i,t} + \gamma_1 Z_{i,t} + \gamma_2 TD_t + \varepsilon_{i,t}, \quad (7)$$

$$Pa_{i,t} = \phi_{1,i} + \phi_2 M_{i,t} + \phi_3 PE_{i,t} + \phi_4 TD_t + \varsigma_{i,t}. \quad (8)$$

where BT stands for "below the threshold" and takes values of one if a state has performed from one to eight executions between 1977 and the year in question, and zero otherwise. The variable AT stands for "above the threshold" and takes values of one if a state has performed nine or more executions between 1977 and the year in question, and zero otherwise. The coefficient on BT is the difference in murder rates between states that have performed between one and nine executions and states that either have performed no executions or nine or more executions. The coefficient on AT is the difference in murder rates between states that have performed between one and nine executions and states that have performed either no executions or fewer than nine executions.

The results of the dummy-variable regression are reported in the second column of Table 3. The statistically significant, positive coefficient on the below-the-threshold variable indicates that executions have a brutalization effect when states have performed fewer than the nine executions. The statistically significant, negative coefficient on the above-the-threshold variable indicates that executions have a deterrent effect when states have performed nine or more executions.

C. *Explaining Brutalization, Deterrence, and the Threshold Effect*

The results of the summary statistics, spline regression, and dummy-variable regressions are consistent in finding a threshold effect. As states begin to perform executions, the first executions do not deter crime. Instead, they either have no statistically significant effect on the murder rate, or the executions have a brutalization effect, increasing murders. However, when the number of executions reaches a threshold level of nine or so over the past twenty years, additional executions begin to have a deterrent effect and murder rates decrease.

My finding of a brutalization effect of executions in states with few executions confirms empirically what some scholars have predicted theoretically. It has been theorized that executions might increase murder, not deter them, and that the brutalization effect is the consequence of the beastly example that executions present.[86] Executions devalue human life and "demonstrate that it is correct and appropriate to kill those who have gravely offended us."[87] Thus, the lesson taught by capital punishment may be "the legitimacy of lethal vengeance, not of deterrence."[88]

My results suggest that a substantial brutalization effect is generally present after an execution, regardless how many executions the state has already conducted recently.

Figure 3 provides a way of understanding this. As shown in Figure 3's marginal brutalization curve, regardless of how many executions a state has already conducted, an additional execution has a tendency to increase murders substantially.

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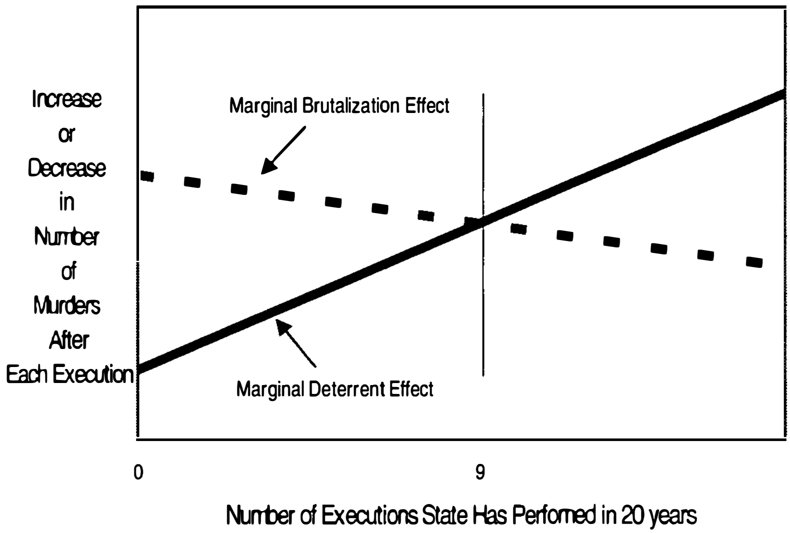
86. CESARE BECCARIA, OF CRIMES AND PUNISHMENTS 50 (H. Paolucci trans., 1964) (1764).

87. William J. Bowers & Glenn Pierce, *Deterrence or Brutalization: What is the Effect of Executions?*, 26 CRIME & DELINQ. 453, 456 (1980).

88. Cochran et al., *supra* note 46, at 110.

FIGURE 3
MARGINAL DETERRENT AND BRUTALIZATION EFFECTS FROM ADDITIONAL EXECUTIONS



My results suggest that the additional, marginal brutalization effect of an additional execution decreases as a state commits more executions. According to the results, the brutalization effect of a state's first execution can be large. For example, the results show that the single execution that Oregon conducted induced approximately 175 murders. So it appears that the first state-sponsored killing can induce many private copycat killings. Additional executions, however, appear not to add as much as the first execution to the state's environment of violence. Thus, Figure 3's marginal brutalization curve slopes down from left to right.[89]

Executions also create a countervailing effect. Each additional execution increases the projected probability, in potential criminal minds, that murder will result in execution. Thus, in Figure 3, the curve for the deterrent effect slopes up from left to right: the more executions that a state has already conducted, the more that each additional execution deters. When a state conducts more executions, potential criminals begin to realize that execution could possibly be imposed on them. With increasing numbers of executions, criminals begin to change their behavior; they commit fewer murders to avoid the risk of execution. For example, in Texas, each execution deters many murders because the state's many other executions have demonstrated to potential criminals that it will execute people who murder. In contrast, in

89. The brutalization curve may also be flat in some jurisdictions. The marginal brutalization effect would still be eventually outweighed by the marginal deterrent effect.

a state that has executed only a single person in the last twenty years, criminals may remain unconvinced that the state has the fortitude to execute more people.

My results suggest that the brutalization effect initially outweighs the deterrent effect. As Figure 3 shows, until a state conducts approximately nine executions, each execution's tendency to breed brutality and violence outweighs the execution's tendency to deter it; until nine executions, the brutalization curve is above the deterrence curve. However, in a state with more than nine executions, each additional execution's growing deterrent effect finally exceeds the brutalization effect. After a state conducts a number of executions that exceeds the threshold, some people may still be induced to kill by an execution. The number of murders eliminated through deterrence, however, now exceeds the number caused by brutalization.

There could be other, unmeasured factors that determine whether states experience deterrence, no effect, or brutalization. This is suggested by the fact that not all states that have performed many executions experience deterrence and not all states that have performed few executions experience brutalization. My statistically significant empirical results, however, indicate that the number of executions a state has performed is a fundamental determinant of capital punishment's effect in the state.

V. Other Models

To confirm the different impacts across states, I test the individual effects of capital punishment among states using two other data sets. The data sets include a monthly, state-level data set from 1977 to 1999, and an annual, state-level data set from 1960 to 2000. I have used these exact data sets in my other studies of capital punishment's deterrent effect and other capital punishment researchers have used similar data sets. I am now, for the first time, using the data to estimate separate deterrent effects for individual states.

We should expect some differences among the data sets in the states that fall into each group: deterrent, no effect, and brutalization. In some states where the increase or decrease in murders after an execution is short-lived, the monthly data may pick up a statistically significant deterrent or brutalization effect that the annual data did not. Conversely, in some states where there are important demographic, economic, or jurisdictional differences among counties, the county-level data may pick up a statistically significant effect that state-level data did not. The varying time periods of the data sets may also result in differences if states experienced deterrence or brutalization during some years, but not others. Nevertheless, the results from the other data sets can support the primary data set's evidence that capital punishment has different impacts in different states.

A. *State-Level Monthly Data: 1977–1999*

First, I estimate different effects of capital punishment among states using monthly data on homicides, executions, and other variables at the state level over the period 1977–1999. I used this data set in another recently published study in *The Journal of Legal Studies.* Because the data and models were peer reviewed for the journal, I will use the same data, variables, and models augmented to measure individual state effects.

Analyzing monthly data has a potential advantage over annual data because it allows me to observe brief fluctuations in murder rates after executions; however, the monthly data is at the state level, so I cannot control for the demographic, economic, and jurisdictional differences among U.S. counties that can affect murder rates.

My state-level, monthly data set includes variables similar to the primary data set already discussed. It includes data on murders, executions, death penalty sentences, per capita income, unemployment rates, and several demographic characteristics. The only important difference is that the monthly data set does not include arrests for murder because this variable is not collected monthly.

$$\frac{m_{i,t}}{n_{i,t}} = \beta_1 DETER_{i,t} + \beta_2 ECON_{i,t} + \beta_3 DEMO_{i,t} + \beta_4 s_i + \beta_5 y_t + \beta_6 m_t + \varepsilon_{i,t} \qquad (9)$$

where m/n is the murder rate (murders/100,000 population) in state i in month t.

The variable *DETER* stands for the vector of deterrence variables: the probability of a death row sentence and the probability of execution. The probability of a death row sentence in a given month is defined as a moving average of the number of death row sentences in the current and previous eleven months divided by a similar twelve-month moving average of the number of murders. The probability of execution is defined as a twelve-month moving average of the number of executions divided by a twelve-month moving average of the number of people on death row. Although in the original publication I used one execution variable to estimate the average deterrent effect across all executions in all states, I now use fifty execution variables that estimate the deterrent effect separately for each state.

The variable *ECON* is a vector of economic variables: the real per capita monthly income in the state and the monthly unemployment rate in the state. The variable *DEMO* is a vector of demographic variables: the percentage of the county population that is between ten and twenty-nine years of age, the percentage of the county population that is male, the percentage of the county population that is African American, and the percentage of the county population that is some minority group other than African American. The variable y is a series of year dummies, the variable m is a series of monthly dummies, and the variable s is a series of state dummy variables.

I estimate equation (9) using a weighted least-squares regression with fixed effects.[90] Fixed effects estimation can control for the unobservable heterogeneity arising from state-specific attributes that could otherwise result in biased estimation.

The results of the regression suggest that capital punishment's effect varies across states. In the monthly, state-level data, capital punishment has a statistically significant deterrent effect in six states, no effect in fifteen states, and a statistically significant brutalization effect in eight states (see Table 4).[91] Although there are some differences in the states in each category, thirteen states remain in the same category as in the primary, county-level data.

TABLE 4
STATE-LEVEL MONTHLY DATA: 1977–1999:
DIFFERING IMPACTS AMONG STATES AND TOTAL
NUMBER OF EXECUTIONS

	States with Deterrent Effect	States with No Effect	States with Brutalization Effect	T-statistic from Mean Comparison Test Between Deterrent States and Other States
States in Each Category	Alabama, Colorado, Kentucky, Louisiana, South Carolina, Texas	Delaware, Florida, Idaho, Illinois, Mississippi, Missouri, Montana, Nebraska, North Carolina, Ohio, Oklahoma, Oregon, Utah, Virginia, Washington, Wyoming	Arizona, Arkansas, California, Georgia, Indiana, Maryland, Nevada, Pennsylvania	
Total Number of Executions	45 (Mean) 21.5 (Median)	17.9 10	11.4 7.5	1.92+

Moreover, the differences between the categories' total number of executions are similar to the primary, county-level results. As Table 4 reveals, the states with deterrent effects have a substantially higher average and me-

90. The weight is the square root of the state population to correct the heteroskedasticity of the error term.

91. These states were significant at the 90% confidence level.

dian total number of executions. Mean comparison tests indicate that the average total number of executions is statistically different among the groups at the six percent level. This evidence supports my threshold effect hypothesis.

B. *State-Level Annual Data: 1960–2000*

I also estimate capital punishment's differing impacts across states using a state-level, annual data set from 1960 to 2000. State-level, annual data has been used in numerous capital punishment studies. I have used the exact data used here in a recent paper, *The Deterrent Effect of Capital Punishment: Evidence from a Judicial Experiment.*[92]

This state-level, annual data set is more aggregated in both the time and geographic dimension than monthly or county data. Thus, some effects that were apparent in data that could either measure short-term, monthly changes in murder rates or control for county-level jurisdictional differences may not be statistically significant in more aggregated data. However, this data covers a much longer time period than the other data sets. In fact, mine is the only data set in the capital punishment literature that has data from before, during, and after the Supreme Court moratorium in the 1970s. Thus, this data set may be more likely to pick up effects that lasted for only a few years, instead of the entire time period.

Once again, I estimate one of the primary models from my original study using this data. However, instead of estimating the average deterrent effect across all executions in all states, I now use fifty execution variables that estimate the deterrent effect separately for each state. The regression equation is:

$$\frac{m_{i,t}}{n_{i,t}} = \beta_1 EXEC_{i,t} + \beta_2 ECON_{i,t} + \beta_3 DEMO_{i,t} + \beta_4 POLICE + \beta_5 s_t + \beta_6 y_t + \varepsilon_{i,t} \qquad (10)$$

where m/n is the murder rate (murders/100,000 population) in state i in year t.

The deterrent variable is the number of executions in each state. The economic variables (*ECON*) include real per capita personal income and the unemployment rate. The demographic variables (*DEMO*) are the percentages of population age fifteen to nineteen, age twenty to twenty-four, and belonging to a minority group.

Once again, the only important difference in the included variables between this model and the primary model on county-level data is the arrest rate; state-level murder arrests are not available for many years in this longer time period. This model, however, does include full-time state police employees (*POLICE*) as a nonpunishment deterrent factor; enhanced police presence may increase detection and apprehension, deterring some criminal activities.

92. Dezhbakhsh & Shepherd, *supra* note 37, at 13–20.

Other controls include state indicators (s) that capture unobservable differences among states that are constant through time. Year indicators (y) capture long-term national trends in crime.

I estimate equation (10) using a least-squares regression with state-fixed effects that is weighted to correct the heteroskedasticity of the error term. The results for the different states are in Table 5. In the annual, state-level data, capital punishment has a statistically significant deterrent effect in five states, a statistically significant brutalization effect in six states, and no effect in twenty-five states.[93] Ten of the states remain in the same category as in the primary, county-level regressions.

TABLE 5
STATE-LEVEL ANNUAL DATA: 1960–2000:
DIFFERING IMPACTS AMONG STATES AND
TOTAL NUMBER OF EXECUTIONS

	States with Deterrent Effect	States with No Effect	States with Brutalization Effect	T-statistic from Mean Comparison Test Between Deterrent States and Other States
State-Level, Annual Data from 1960–2000	Alabama, Florida, Georgia, Mississippi, Texas	Arkansas, California, Connecticut, Delaware, Iowa, Idaho, Kansas, Kentucky, Louisiana, Maryland, Montana, North Carolina, Nebraska, New Jersey, New Mexico, Nevada, Ohio, Oklahoma, Oregon, South Carolina, Tennessee, Utah, Virginia, Washington, Wyoming	Arizona, Colorado, Illinois, Indiana, Missouri, Pennsylvania	
Total Number of Executions	81.8 (Mean) 37 (Median)	14.3 5.5	18.5 11	3.52*

93. These states were significant at the 90% confidence level.

Again, there is strong evidence for a threshold effect. Both the mean and median total number of executions is substantially higher for the states with a deterrent effect. Mean comparison tests confirm this; the average total number of executions is statistically different among the groups at the five percent level.

As we expected because of the different characteristics of the data, there are some differences between the states in each category among the three data sets. Nevertheless, all three data sets confirm that the impact of capital punishment differs significantly among states. Moreover, there is strong evidence of a threshold effect in all three data sets.

CONCLUSION

Using a large data set of all U.S. counties from 1977 to 1996, I have examined whether capital punishment's impact on murder rates differs among states. The results are striking. Of the twenty-seven states in which at least one execution occurred during the sample period, capital punishment deters murder in only six states. In contrast, in thirteen states, or more than twice as many, capital punishment actually *increases* murder. In eight states, capital punishment has no effect on the murder rate. Equivalently, in only twenty-two percent of states did executions have a deterrent effect. In contrast, executions induced additional murders in forty-eight percent of states. Executions created no deterrence in seventy-eight percent of states. These results are generally robust in models using data from other time periods and state-level data.

The paper then explored the threshold effect that explains why a few states have deterrence but many more others have just the opposite. On average, the states where capital punishment deters murder execute many more people than do the states where capital punishment does not deter murder. I show that a threshold number of executions exists, which is approximately nine executions during the sample period. In states that conducted more executions than the threshold, each execution, on average, deterred murder. In states that conducted fewer executions than the threshold, the executions, on average, increased the murder rate.

Perhaps each execution contributes to brutalizing the society and increasing murder. However, if a state executes many people, then criminals become convinced that the state is serious about the punishment, and the criminals start to reduce their criminal activity. When the number of executions exceeds the threshold, the deterrence effect begins to outweigh the brutalization effect.

The results suggest that earlier economic papers' focus on national averages masked variation among states. When the large number of executions in the deterrence states are averaged in with the small number of executions in all of the other states, the large deterrent effect in the deterrence states dominates the opposite brutalization effect in the other states. Thus the result from earlier economics papers: on average, an execution in the United States deters crime. However, this Article shows that these averages are powered by a handful of high-execution, high-deterrence states. In most states, capital punishment either increases murder or has no effect. The results also explain the findings of no deterrence in papers that have focused on individual states, rather than on the nation as a whole. As the results here show, in a large majority of states, executions do not deter murder.

My results have three important policy implications. First, if deterrence is the objective, then capital punishment generally succeeds in the few states with many executions. Second, the many states with numbers of executions below the threshold may be executing people needlessly. Indeed, instead of deterring crime, the executions may be inducing additional murders: a rough total estimate is that, in the many states where executions induce murders rather than deter them, executions cause an additional 250 murders per year. Third, to achieve deterrence, states must generally execute many people. If a state is unwilling to establish such a large execution program, it should consider abandoning capital punishment.

A final word of caution is appropriate. This Article's central results are consistent across many different models and data sets, showing conclusively that capital punishment's impact differs widely among the states; however, the results cannot yet offer definitive conclusions about the degree to which capital punishment deters or induces murders in a specific state. That awaits further work.

APPENDIX

APPENDIX I
CRIMES PUNISHABLE BY THE DEATH PENALTY, BY STATE

Alabama	Intentional murder with 18 aggravating factors.
Arizona	First-degree murder accompanied by at least 1 of 10 aggravating factors.
Arkansas	Capital murder with a finding of at least 1 of 10 aggravating circumstances; treason.
California	First-degree murder with special circumstances; train wrecking; treason; perjury causing execution.
Colorado	First-degree murder with at least 1 of 15 aggravating factors; treason.
Connecticut	Capital felony with 8 forms of aggravated homicide.
Delaware	First-degree murder with aggravating circumstances.
Florida	First-degree murder; felony murder; capital drug trafficking; capital sexual battery.
Georgia	Murder; kidnapping with bodily injury or ransom when the victim dies; aircraft hijacking; treason.
Idaho	First-degree murder with aggravating factors; aggravated kidnapping.
Illinois	First-degree murder with 1 of 15 aggravating circumstances.
Indiana	Murder with 16 aggravating circumstances.
Kansas	Capital murder with 7 aggravating circumstances.
Kentucky	Murder with aggravating factors; kidnapping with aggravating factors.
Louisiana	First-degree murder; aggravated rape of victim under age 12; treason.
Maryland	First-degree murder, either premeditated or during the commission of a felony, provided that certain death eligibility requirements are satisfied.
Mississippi	Capital murder; aircraft piracy.
Missouri	First-degree murder.
Montana	Capital murder with 1 of 9 aggravating circumstances; capital sexual assault.
Nebraska	First-degree murder with a finding of at least 1 statutorily-defined aggravating circumstance.
Nevada	First-degree murder with at least 1 of 14 aggravating circumstances.
New Hampshire	Six categories of capital murder.
New Jersey	Knowing/purposeful murder by one's own conduct; contract murder; solicitation by command or threat in furtherance of a narcotics conspiracy.
New Mexico	First-degree murder with at least 1 of 7 statutorily-defined aggravating circumstances.
New York	First-degree murder with 1 of 12 aggravating factors. (Note: On June 24, 2004, the New York death penalty statute was ruled unconstitutional.)
North Carolina	First-degree murder.
Ohio	Aggravated murder with at least 1 of 9 aggravating circumstances.
Oklahoma	First-degree murder in conjunction with a finding of at least 1 of 8 statutorily defined aggravating circumstances.
Oregon	Aggravated murder.
Pennsylvania	First-degree murder with 18 aggravating circumstances.
South Carolina	Murder with 1 of 10 aggravating circumstances.
South Dakota	First-degree murder with 1 of 10 aggravating circumstances; aggravated

	kidnapping.
Tennessee	First-degree murder with 1 of 14 aggravating circumstances.
Texas	Criminal homicide with 1 of 8 aggravating circumstances.
Utah	Aggravated murder.
Virginia	First-degree murder with 1 of 12 aggravating circumstances.
Washington	Aggravated first-degree murder.
Wyoming	First-degree murder.
This table was taken verbatim from TRACY L. SNELL, CAPITAL PUNISHMENT 2000, at 2 tbl.1 (2001) (citations omitted), *available at* http://www.ojp.usdoj.gov/bjs/pub/pdf/cp00.pdf.	

APPENDIX 2
METHODS OF EXECUTION BY STATE

Alabama	Effective 7/1/02, lethal injection will be administered unless the inmate requests electrocution.
Arizona	Authorizes lethal injection for persons sentenced after 11/15/92; those sentenced before that date may select lethal injection or lethal gas.
Arkansas	Authorizes lethal injection for persons committing a capital offense after 7/4/83; those who committed the offense before that date may select lethal injection or electrocution.
California	Provides that lethal injection be administered unless the inmate requests lethal gas.
Colorado	Lethal injection is the sole method.
Connecticut	Lethal injection is the sole method.
Delaware	Lethal Injection is the sole method. Hanging was an alternative for those whose offense occurred prior to 6/13/86, but as of July 2003 no inmates on death row were eligible to choose this alternative and Delaware dismantled its gallows.
Florida	Allows prisoners to choose between lethal injection and electrocution
Georgia	Lethal injection is the sole method. (On October 5, 2001, the Georgia Supreme Court held that the electric chair was cruel and unusual punishment and struck down the state's use of the method)
Idaho	Authorizes firing squad only if lethal injection is "impractical".
Illinois	Lethal injection is the state's method. However, it authorizes electrocution if lethal injection is ever held to be unconstitutional.
Indiana	Lethal injection is the sole method.
Kansas	Lethal injection is the sole method.
Kentucky	Authorizes lethal injection for those convicted after March 31, 1998; those who committed the offense before that date may select lethal injection or electrocution
Louisiana	Lethal injection is the sole method.
Maryland	Authorizes lethal injection for those whose capital offenses occurred on or after 3/25/94; those who committed the offense before that date may select lethal injection or lethal gas.
Mississippi	Lethal injection is the sole method.
Missouri	Authorizes lethal injection or lethal gas; the statute leaves unclear who decides what method to use, the inmate or the Director of the Missouri Department of Corrections.
Montana	Lethal injection is the sole method.
Nebraska	Electrocution is the sole method.
Nevada	Lethal injection is the sole method.
New Hampshire	Authorizes hanging only if lethal injection cannot be given.
New Jersey	Lethal injection is the sole method.
New Mexico	Lethal injection is the sole method.
New York	Lethal injection is the sole method.

North Carolina	Lethal injection is the sole method.
Ohio	Lethal injection is the sole method.
Oklahoma	Authorizes electrocution if lethal injection is ever held to be unconstitutional and firing squad if both lethal injection and electrocution are held unconstitutional.
Oregon	Lethal injection is the sole method.
Pennsylvania	Lethal injection is the sole method.
South Carolina	Allows prisoners to choose between lethal injection and electrocution
South Dakota	Lethal injection is the sole method.
Tennessee	Authorizes lethal injection for those sentenced after Jan. 1, 1999; others choose between the electric chair and lethal injection.
Texas	Lethal injection is the sole method.
Utah	Lethal Injection is the sole method of execution. Firing squad was chosen by some inmates prior to the passage of legislation banning the practice, and is only available for those inmates.
Virginia	Allows prisoners to choose between lethal injection and electrocution
Washington	Provides that lethal injection be administered unless the inmate requests hanging.
Wyoming	Authorizes lethal gas if lethal injection is ever held to be unconstitutional.
This table was taken verbatim from DEATH PENALTY INFO. CTR., METHODS OF EXECUTION, http://www.deathpenaltyinfo.org/article.php?scid=8&did=245 (last visited Aug. 26, 2005) (updating TRACY L. SNELL, CAPITAL PUNISHMENT 1996, at 5 tbl.3 (1997), *available at* http://www.ojp.usdoj.gov/bjs/pub/pdf/cp96.pdf).	

APPENDIX 3
AVERAGE PUBLICITY PER EXECUTION, BY STATE: 1997–1999

State Name	Average Newspaper Coverage	Average News Transcripts Coverage
Alabama	5	17
Arizona	17	41
Arkansas	21	8
California	17	184
Colorado	365	424
Delaware	5	21
Florida	37	143
Georgia	7	0
Illinois	43	47
Indiana	50	50
Kentucky	18	60
Louisiana	12	24
Maryland	80	201
Missouri	22	35
Montana	0	1
Nebraska	32	15
Nevada	23	21
North Carolina	11	31
Ohio	123	592
Oklahoma	10	26
Oregon	77	81
Pennsylvania	0	15
South Carolina	7	6
Texas	10	28
Utah	44	89
Virginia	14	31
Washington	71	16

APPENDIX 4
INDIVIDUAL STATE EFFECTS OF THE PROBABILITY OF EXECUTION ON THE MURDER RATE

STATE	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Alabama	0.05	-2.50	1.44	1.80	-3.22	2.53
	0.02	-2.47	0.66	1.12	-4.91	1.42
Arizona	6.92	2.82	8.50	9.43	1.29	11.03
	2.97	1.56	3.42	4.70	1.01	4.90
Arkansas	10.76	7.60	14.74	13.26	3.64	20.40
	2.60	2.92	2.42	4.10	1.29	4.43
California	12.55	20.32	27.54	12.45	-2.12	22.95
	4.46	7.73	6.26	4.90	-1.36	6.50
Delaware	-16.23	-8.72	-14.67	-13.46	-9.31	-15.94
	-1.43	-1.62	-1.50	-2.29	-2.74	-1.89
Florida	-32.96	-7.66	-39.68	-33.96	-16.18	-38.93
	-15.96	-6.13	-15.90	-19.10	-21.35	-18.24
Georgia	-10.64	-1.39	-9.08	-11.81	-4.38	-9.56
	-5.76	-1.26	-4.29	-7.61	-5.70	-5.43
Idaho	1.95	5.34	1.66	9.13	3.78	11.32
	0.45	1.67	0.43	3.22	1.97	4.05
Illinois	11.63	-0.01	27.28	15.20	2.72	27.41
	3.71	0.00	7.61	6.29	1.49	9.11
Indiana	9.01	2.12	11.26	7.35	0.25	8.54
	4.09	1.71	4.53	4.49	0.32	4.68
Louisiana	22.51	-0.36	39.33	18.10	-1.09	27.28
	8.38	-0.22	10.14	8.49	-0.98	10.07
Maryland	7.14	4.07	11.22	12.03	2.76	17.75
	2.49	2.05	3.33	5.74	2.14	6.41
Mississippi	0.12	2.82	-0.75	2.57	1.40	3.36
	0.05	1.96	-0.23	1.19	1.46	1.36
Missouri	-0.81	26.88	3.41	1.03	41.13	4.09
	-0.31	10.37	1.10	0.49	24.25	1.59
Montana	30.62	1.24	24.08	14.64	3.40	16.28
	1.62	0.11	1.56	1.90	0.90	1.81
Nebraska	11.03	3.37	-2.93	2.33	-0.84	1.93
	1.09	0.97	-0.92	0.98	-0.73	0.81
Nevada	-1.56	-7.76	4.58	-5.49	-11.56	6.03
	-0.41	-2.45	1.19	-1.81	-5.97	1.65
North Carolina	-2.80	1.66	-2.32	0.70	-0.93	1.26
	-1.69	1.26	-1.24	0.50	-1.38	0.82
Oklahoma	8.95	-0.02	9.43	6.67	-1.03	6.91
	4.25	-0.01	4.01	3.98	-1.12	3.91

STATE	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Oregon	10.76	7.09	12.85	11.49	5.26	15.25
	2.81	1.84	3.37	4.34	2.48	4.70
Pennsylvania	-2.62	1.22	-0.53	4.62	0.99	4.97
	-1.02	0.81	-0.16	2.33	0.89	1.96
South Carolina	-12.10	-4.27	-13.49	-10.16	-6.24	-10.73
	-5.89	-3.48	-6.09	-6.04	-9.59	-5.80
Texas	-17.68	-2.41	-16.00	-17.54	-9.95	-15.28
	-16.65	-2.97	-14.87	-19.84	-19.13	-17.18
Utah	9.59	12.03	4.24	9.54	9.33	9.23
	2.68	4.55	1.48	4.28	4.90	3.66
Virginia	6.84	8.03	7.38	6.52	2.25	8.98
	3.94	8.16	3.81	4.63	3.71	5.91
Washington	5.56	4.64	11.35	11.90	4.98	16.64
	1.38	1.76	3.46	5.54	2.81	6.02
Wyoming	4.86	-14.87	1.84	2.84	-3.38	6.79
	0.39	-1.01	0.30	0.71	-1.24	1.73