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CAPITAL PUNISHMENT AND CAPITAL MURDER: MARKET SHARE AND THE DETERRENT EFFECTS OF THE DEATH PENALTY

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Capital Punishment and Capital Murder: Market Share and the Deterrent Effects of the Death Penalty

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I. Introduction

A. The New Deterrence

The modern debate on deterrence and capital punishment, now in its fourth decade, was launched by two closely timed events. The first was the 1976 United States Supreme Court decision in *Gregg v. Georgia*,[1] which restored capital punishment after its brief constitutional ban following *Furman v. Georgia*[2] in 1972.[3] In 1975, Professor Isaac Ehrlich published an influential article saying that during the 1950s and 1960s, each execution averted eight murders.[4] Although Ehrlich's article was a highly technical study prepared for an audience of economists, its influence went well beyond the economics profession. Ehrlich's work was cited favorably in *Gregg* and later was cited in an amicus brief filed by the U.S. Solicitor General in *Fowler v. North Carolina*.[5] No matter how carefully Ehrlich qualified his conclusions, his article had the popular and political appeal of a headline, a sound bite,

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1. 428 U.S. 153 (1976).
2. 408 U.S. 238 (1972).
3. *Gregg*, 428 U.S. at 169.
4. *See* Isaac Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life and Death*, 65 AM. ECON. REV. 397, 398 (1975); *see also* Isaac Ehrlich, *Capital Punishment and Deterrence: Some Further Thoughts and Additional Evidence*, 85 J. POL. ECON. 741 (1977) (continuing the examination of the deterrent effect of capital punishment using cross-sectional data from several states).
5. Brief for the United States as Amicus Curiae at 35, Fowler v. North Carolina, 428 U.S. 904 (1976) (No. 73-7031).

and a bumper sticker all rolled into one: "every execution deters eight killings."

Reaction was immediate: Ehrlich's findings were sharply disputed in academic forums such as the *Yale Law Journal*,[6] launching an era of contentious arguments in the press and in professional journals.[7] In 1978, an expert panel appointed by the National Academy of Sciences issued strong criticisms of Ehrlich's work.[8] Over the next two decades, economists and other social scientists attempted (mostly without success) to replicate Ehrlich's results using different data, alternative statistical methods, and other design modifications that tried to address glaring errors in Ehrlich's techniques and data. The accumulated scientific evidence from the NAS report and these later studies weighed heavily against the claim that executions deter murders.[9]

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6. *See* David C. Baldus & James W.L. Cole, *A Comparison of the Work of Thorsten Sellin and Isaac Ehrlich on the Deterrent Effect of Capital Punishment*, 85 YALE L.J. 170, 171 (1975) (arguing that a statistical study cannot prove that executions deter murders); William J. Bowers & Glenn L. Pierce, *The Illusion of Deterrence in Isaac Ehrlich's Research on Capital Punishment*, 85 YALE L.J. 187, 205 (1975) (concluding that Ehrlich failed to provide any reliable evidence that the death penalty deters murder); Isaac Ehrlich, *Deterrence: Evidence and Inference*, 85 YALE L.J. 209, 209–10 (1975) (arguing that certain critiques of his work have "selectively deleted observations, utilized an inferior regression specification, considered irrelevant variables and correlations, and revealed in the process misunderstanding of elementary statistical concepts"); Editors' Introduction, *Statistical Evidence on the Deterrent Effect of Capital Punishment*, 85 YALE L.J. 164, 169 (1975) (discussing the debate surrounding statistical studies regarding deterrence effects of the death penalty and the inherent vulnerability of complex statistical techniques).

7. There are numerous critiques of Ehrlich's work. *See, e.g.*, Jeffrey Grogger, *The Deterrent Effect of Capital Punishment: An Analysis of Daily Homicide Counts*, 85 J. AM. STAT. ASS'N 295, 295 (1990) (arguing that studies focusing on the relationship between homicide rates and executions, such as Ehrlich's, tend to yield ambiguous results); Edward E. Leamer, *Let's Take the Con Out of Econometrics*, 73 AM. ECON. REV. 31, 41–42 (1983) (criticizing the methodology used in analyzing the deterrent effect of capital punishment); 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, 102 (1989) (discussing Ehrlich's data set); Walter S. McManus, *Estimates of the Deterrent Effect of Capital Punishment: The Importance of the Researcher's Prior Beliefs*, 93 J. POL. ECON. 417, 425 (1985) (concluding that researchers' prior beliefs influence their conclusions).

Support and extensions of Ehrlich's work exist as well. *See, e.g.*, George A. Chressanthis, *Capital Punishment and the Deterrent Effect Revisited: Recent Time-Series Econometric Evidence*, 18 J. BEHAV. ECON. 81, 94 (1989) (upgrading Ehrlich's initial controversial results); James Peery Cover & Paul D. Thistle, *Time Series, Homicide, and the Deterrent Effect of Capital Punishment*, 54 S. ECON. J. 615, 621 (1988) (arguing that the deterrence effect cannot be analyzed properly without explicitly considering how the probabilities of punishment are defined); Stephen K. Layson, *Homicide and Deterrence: A Reexamination of the United States Time-Series Evidence*, 52 S. ECON. J. 68, 73–86 (1985) (updating Ehrlich's work with new data sets).

8. *See* Lawrence R. Klein et al., *The Deterrent Effect of Capital Punishment: An Assessment of the Estimates*, *in* DETERRENCE AND INCAPACITATION: ESTIMATING THE EFFECTS OF CRIMINAL SANCTIONS ON CRIME RATES 336, 336–60 (Alfred Blumstein et al. eds., 1978).

9. *See* William C. Bailey, *Deterrence, Brutalization, and the Death Penalty: Another Examination of Oklahoma's Return to Capital Punishment*, 36 CRIMINOLOGY 711, 729–32 (1998) (concluding that media coverage of executions has no significant deterrent effect on homicide); Jon Sorensen et al., *Capital Punishment and Deterrence: Examining the Effect of Executions on Murder*

The debate both revived and shifted within the past decade. Since 1996, more than a dozen studies have been published claiming that each execution can prevent anywhere from three to thirty-two homicides.[10] The new deterrence studies analyze data that span a twenty-year period since the resumption of executions following the United States Supreme Court's decisions in *Furman*[11] and *Gregg*.[12] The claims of these new studies are far bolder than the original wave of studies by Professor Ehrlich and his students.[13] Some claim that pardons, commutations, and exonerations cause murders to increase.[14] One says that even murders of passion, among the most irrational of lethal acts, can be deterred.[15] In short, these studies suggest that the deterrent effects of capital punishment apparently are

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in Texas, 45 CRIME & DELINQ. 481, 481–91 (1999) (finding no evidence of deterrence resulting from capital punishment using Texas execution and murder rate data from 1984 through 1997).

10. See Appendix A for a list of recent studies that claim deterrent effects from execution in the post-*Gregg* era. Three papers contest the claim that capital punishment deters murder. Lawrence Katz and his colleagues report no significant deterrent effects of executions on murder rates after controlling for prison conditions and other indicia of the overall performance of the criminal justice system. *See* Lawrence Katz et al., *Prison Conditions, Capital Punishment, and Deterrence*, 5 AM. L. & ECON. REV. 318, 339–40 (2003). John Donohue and Justin Wolfers examined evidence of deterrent effects and found that "the existing evidence for deterrence is surprisingly fragile." John J. Donohue & Justin Wolfers, *Uses and Abuses of Empirical Evidence in the Death Penalty Debate*, 58 STAN. L. REV. 791, 794 (2005). Jon Sorensen and his colleagues report no counter-deterrent effects on murder from a moratorium on capital punishment in Texas, once a rich set of control variables on the causes and correlates of murder was included in the analysis. *See* Jon Sorensen et al., *supra* note 9, at 481–91; *see also* JON SORENSEN & ROCKY LEANN PILGRIM, LETHAL INJECTION: CAPITAL PUNISHMENT IN TEXAS DURING THE MODERN ERA 39–47 (2006) (re-analyzing Cloninger and Marchesini's claim that the moratorium resulted in an increase in homicides); Dale O. Cloninger & Roberto Marchesini, *Execution and Deterrence: A Quasi-Controlled Group Experiment*, 33 APPLIED ECON. 569 (2001).

11. Furman v. Georgia, 408 U.S. 238 (1972).

12. Gregg v. Georgia, 428 U.S. 153 (1976).

13. Joanna Shepherd, an author of several studies finding a deterrent effect, has recently argued before Congress that recent research has created a "strong consensus among economists that capital punishment deters crime," going so far as to claim that "[t]he studies are unanimous." *Terrorist Penalties Enhancement Act of 2003: Hearing on H.R. 2934 Before the Subcomm. on Crime, Terrorism, and Homeland Security of the House Comm. on the Judiciary*, 108th Cong. 10–11 (2004) (statement of Joanna M. Shepherd) [hereinafter *Hearing*], *available at* http://judiciary.house.gov/media/pdfs/printers/108th/93224.pdf. These conclusions were repeated verbatim in recent testimony by Professor Paul Rubin, co-author on several recent studies also reporting deterrent effects from executions. *An Examination of the Death Penalty in the United States: Hearing before the Senate Subcomm. on the Constitution, Civil Rights, and Property Rights of the Senate Comm. on the Judiciary*, 108th Cong. (2006) (statement of Paul H. Rubin), *available at* http://judiciary.senate.gov/testimony.cfm?id=1745&wit_id=4991.

14. *See, e.g.*, H. Naci Mocan & R. Kaj Gittings, *Getting Off Death Row: Commuted Sentences and the Deterrent Effect of Capital Punishment*, 46 J.L. & ECON. 453, 474 (2003) (finding that "[e]ach additional execution decreases homicides by about five, and each additional commutation increases homicides by the same amount, while one additional removal from death row generates one additional homicide").

15. Joanna M. Shepherd, *Murders of Passion, Execution Delays, and the Deterrence of Capital Punishment*, 33 J. LEGAL STUD. 283, 318 (2004).

limitless, leading some proponents to offer execution as a cure-all both for murder and several other types of crime.[16]

Both legal scholars and social scientists have transformed this new social science evidence into calls for more executions that they claim will save lives,[17] and new rules that will remove procedural roadblocks and hasten executions.[18] Others challenge the scientific credibility of these new studies,[19] and warn about the moral hazards and practical risks of capital punishment.[20]

Obviously, the stakes are high in this latest round of the recurring debate on deterrence. We think the new results are wrong, for a simple reason. The measures of homicide used in the new deterrence studies are overly broad: by studying whether punishments affect all homicides, these studies fail to identify a more plausible target of deterrence—namely, those homicides that are punishable by death. By broadening the target of the search for deterrent effects, these studies have overestimated not just the number of lives saved by deterrence, but whether any murders are averted by the threat of execution.[21] In this study, we find no evidence of deterrence when the effects of execution are estimated for the subset of homicides that are most directly affected by execution.

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16. Zhiqiang Liu, *Capital Punishment and the Deterrence Hypothesis: Some New Insights and Empirical Evidence*, 30 E. ECON. J. 237, 254 (2004).

17. Cass R. Sunstein & Adrian Vermeule, *Is Capital Punishment Morally Required? Acts, Omissions, and Life–Life Tradeoffs*, 58 STAN. L. REV. 703, 748–50 (2005); *see also* Posting of Gary Becker to the Becker–Posner Blog, http://www.becker-posner-blog.com/archives/2005/12/ (Dec. 18, 2005, 06:02 PM) (citing the deterrence argument as more than convincing to support capital punishment); Posting of Richard Posner to the Becker–Posner Blog, http://www.becker-posner-blog.com/archives/2005/12/ (Dec. 18, 2005, 07:53 PM) (synthesizing arguments based on recent research into the powerful deterrent effect of the death penalty and noting that such values outweigh factors opposing capital punishment).

18. *See, e.g.*, Streamlined Procedures Act of 2005, S. 1088, 109th Cong. (2005); Criminal Alien Deportation Improvements Act of 1995, H.R. 668, 104th Cong. (1995).

19. Donohue & Wolfers, *supra* note 10, at 794 (2005) (reviewing the main study cited by Sunstein and Vermeule and finding that the evidence supporting deterrence is surprisingly tenuous); *see also* Richard Berk, *New Claims About Executions and General Deterrence: Déjà Vu All Over Again?*, 2 J. EMPIRICAL L. STUD. 303, 304 (2005) (contending that much empirical research is necessarily based on observational data and that there are therefore "a host of problems in trying to make credible causal inferences"); Jeffrey Fagan, *Death and Deterrence Redux: Science, Law and Causal Reasoning on Capital Punishment*, 4 OHIO ST. J. CRIM. L. (forthcoming 2007).

20. Carol S. Steiker, *The Ethics and Empirics of Capital Punishment: No, Capital Punishment Is Not Morally Required: Deterrence, Deontology, and the Death Penalty*, 58 STAN. L. REV. 751, 789 (2005) (responding to the claim of the "moral requirement" of Sunstein and Vermeule by stating that "neither those who have categorical moral objections to the death penalty nor even those who fully embrace consequentialism should be willing to make" the life–life tradeoff "that on closer inspection reveals itself as the most Faustian of bargains").

21. Others find the results too unstable to be deemed reliable. *See, e.g.*, Berk, *supra* note 19, at 328 (noting that the study data regarding deterrence is "highly skewed," with only small portions of the data influencing the final results); Donohue & Wolfers, *supra* note 10, at 794 (finding that the evidence supporting deterrence "cannot be reliably disentangled from the year-to-year changes in the homicide rate").

B. Errors in Aggregation

The question of whether the threat or actuality of execution adds to the deterrent effect on homicide produced by lengthy imprisonment alone has been the subject of statistical debate for more than a century.[22] The vast majority of statistical studies that try to address this issue have used a variety of punishment variables as independent variables (whether the death penalty is authorized, or used, or its frequency) and the total rate of intentional homicides[23] as the dependent variable.[24]

The use of total intentional homicide has always been an aggregation error in the deterrence debate in the United States.[25] Under common law, only the top grade of murder was ever eligible for the death penalty, but the traditional legal framework of the criteria that made criminal homicide potentially capital was far from clear until the United States Supreme Court imposed minimum constitutional standards for death eligibility in *Gregg v. Georgia*[26] and its companion 1976 cases.[27] The Supreme Court required the specific definition of murders that are death-eligible and the states responded with a series of death eligibility standards (usually drawn from section 210.6 of the Model Penal Code).[28]

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22. Ehrlich, *supra* note 4, at 397 (noting that debates over the "justness and efficacy of capital punishment" have involved some kind of statistical analysis from the time of Beccaria in the eighteenth century).

23. These studies most commonly compute the total rate of intentional homicides by using the counts of murders and non-negligent homicides supplied by either local law enforcement agencies or by the Department of Justice through the FBI's Uniform Crime Reporting Program. *See, e.g.*, Liu, *supra* note 16, at 244 (employing intentional homicide data supplied by the Department of Justice, which compiles Uniform Crime Reports with the information provided by local law enforcement agencies); Shepherd, *supra* note 15, at 304 (same). For an assessment of the Department of Justice data, see MICHAEL D. MALTZ, BUREAU OF JUSTICE STATISTICS, BRIDGING GAPS IN POLICE CRIME DATA 1 (1999), *available at* http://www.ojb.usdoj.gov/bjs/pub/pdf/bgpcd.pdf.

24. *See Hearing*, *supra* note 13, at 12 (noting that modern studies of the deterrent effect of capital punishment have used multivariate regression analysis, which separates the effects of different factors on a set number of murders).

25. *See* THORSTEN SELLIN, THE DEATH PENALTY: A REPORT FOR THE MODEL PENAL CODE PROJECT OF THE AMERICAN LAW INSTITUTE 52–59 (1959) (testing execution effects by counting separately particularly high-risk categories of homicides, such as killings of police officers and prison guards).

26. 428 U.S. 153 (1976).

27. Roberts v. Louisiana, 428 U.S. 325 (1976); Woodson v. North Carolina, 428 U.S. 280 (1976); Jurek v. Texas, 428 U.S. 262 (1976); Proffitt v. Florida, 428 U.S. 242 (1976).

28. Under *Gregg* and its companion cases, this definition can occur in one of two ways. A state may either narrowly define a class of death-eligible murders for a jury finding during the guilt–innocence phase of trial or a state may broadly define a class of death-eligible murders and provide for the narrowing of the class by jury findings of aggravating factors during the sentencing phase of trial. *See Jurek*, 428 U.S. at 276–77 (approving the Texas statute that embodies the narrow definition alternative); *Gregg*, 428 U.S. at 206–07 (approving the Georgia statute that embodies the broad definition alternative). For examples of the state statutes that were at issue at the time of *Gregg*, see GA. CODE ANN. § 27-2534.1 (Supp. 1974); TEX. CODE CRIM. PROC. ANN. art. 37.071 (Vernon Supp. 1974–1975); and FLA. STAT. § 921.141 (1973 & Supp. 1975). The modern version of the Georgia statute is codified at GA. CODE ANN. §§ 16-5-1, 17-10-30, 17-10-31, 17-10-35

Yet most of the new deterrence studies have estimated the effects of executions on total homicides. This makes little sense, either jurisprudentially or as a matter of behavioral science. Since *Gregg*, the statutory description of death-eligible murders has been a constitutional requirement for state and federal criminal codes. State statutes recognize that there are grades of willfulness or premeditation, and these will impact the likelihood of a homicide resulting in the death penalty.[29] Similarly, there are some homicides—such as killings of police or children—that evoke strong normative responses from legislatures which in turn are expressed in particular sections of capital statutes creating eligibility for the death penalty for such crimes.[30] Jurisprudentially, the idea that "death is different" has guided states to craft death penalty statutes that reserve execution for offenders who not only meet capital eligibility requirements but whose culpability rises to a threshold that matches the severity of a death sentence.[31]

Social science research on homicide also has distinguished among types of murders and murderers.[32] These studies suggest that the capacity for

(2005). The modern version of the Texas and Florida statutes can be found at the same citations as above.

29. Hans Zeisel, *The Deterrent Effect of the Death Penalty: Facts v. Faith*, 1976 SUP. CT. REV. 317, 326.

30. For example, several states include killings of children below statutorily defined ages as an aggravating circumstance that creates eligibility for capital punishment. *See, e.g.*, ARIZ. REV. STAT. ANN. § 13-703(F)(9) (2001); 720 ILL. COMP. STAT. 5/9-1(b)(7) (2002); LA. REV. STAT. ANN. § 14:30(A)(5) (1997); NEV. REV. STAT. § 200.033(10) (2005); OHIO REV. CODE ANN. § 2903.01(C) (West 1997); 42 PA. CONS. STAT. § 9711(d)(16) (1998); VA. CODE ANN. § 18.2-31(12) (2004).

31. Jeffrey Abramson, *Death-is-Different Jurisprudence and the Role of the Capital Jury*, 2 OHIO ST. J. CRIM. L. 117 *passim* (2004); *see, e.g.*, Ring v. Arizona, 536 U.S. 584, 605–06 (2002) ("[T]here is no doubt that '[d]eath is different.'") (alteration in original); *id.* at 614 (Breyer, J., concurring in the judgment) ("[T]he Eighth Amendment requires States to apply special procedural safeguards when they seek the death penalty."); Atkins v. Virginia, 536 U.S. 304, 337 (2002) (Scalia, J., dissenting) (suggesting that the majority opinion holding it cruel and unusual to punish persons with mental retardation with death is the "pinnacle of . . . death-is-different jurisprudence"); McCleskey v. Kemp, 481 U.S. 279, 340 (1987) (Brennan, J., dissenting) ("It hardly needs reiteration that this Court has consistently acknowledged the uniqueness of the punishment of death."); Wainwright v. Witt, 469 U.S. 412, 463 (1985) (Brennan, J., dissenting) (citing "previously unquestioned principle" that unique safeguards are necessary because death penalty is "qualitatively different"); Spaziano v. Florida, 468 U.S. 447, 459 (1984) (citing the Court's prior recognition of the "qualitative difference of the death penalty") (citation omitted); *id.* at 468 (Stevens, J., concurring in part and dissenting in part) ("[T]he death penalty is qualitatively different . . . and hence must be accompanied by unique safeguards . . . ."); Lockett v. Ohio, 438 U.S. 586, 604 (1978) (holding death to be "qualitatively different"); Woodson v. North Carolina, 428 U.S. 280, 305 (1976) (joint opinion of Stewart, Powell, & Stevens, JJ.) ("[T]he penalty of death is qualitatively different from a sentence of imprisonment, however long."); Gregg v. Georgia, 428 U.S. 153, 188 (1976) (joint opinion of Stewart, Powell, & Stevens, JJ.) ("[T]he penalty of death is different in kind from any other punishment . . . ."); Furman v. Georgia, 408 U.S. 238, 286–89 (1972) (Brennan, J., concurring) ("Death is a unique punishment . . . ."); *id.* at 306 (Stewart, J., concurring) ("The penalty of death differs from all other forms of criminal punishment, not in degree but in kind.").

32. *See, e.g.*, JACK KATZ, THE SEDUCTIONS OF CRIME 32–34 (1988) (differentiating homicides based on motivations for their commission); JAMES O'KANE, WICKED DEEDS: MURDER IN AMERICA 19–34 (2005) (examining and explaining the differences between homicide, murder, and

rational action among offenders often is doubtful, as they are prone to hyperdiscounting of risk and inflation of the immediate value of their actions.[33] Accordingly, to lump all homicides into a singular category that assumes that all murders are equally deterrable runs afoul of both law and facts.

Despite these legal boundaries on which homicides are death-eligible, nearly all studies have examined the effects of capital punishment on total homicides. Only three studies have examined the effects of execution on an index of murders that are eligible for the death penalty, and none have identified a deterrent effect on capital murders. Professor Robert H. Dann examined capital homicides—those eligible for the death penalty—in Philadelphia in the sixty days before and after each of five highly publicized executions that took place between 1929 and 1932.[34] He found no evidence of a change in capital homicide rates, nor in other homicide rates. Professor Leonard Savitz replicated Dann's research design for the period 1944–1947, examining capital-eligible homicides in the eight weeks before and after four highly publicized death sentences.[35] Like Dann, Savitz found no evidence of deterrence.[36]

More recently, Professors Ruth Peterson and William Bailey analyzed the effects of executions on rates of "felony murder," defined as killings committed in the course of six specific felony crimes: rape, robbery, burglary, larceny, vehicle theft, and arson.[37] They added another composite category that included murders committed in the course of nonfelony crimes including prostitution, narcotics violations, gambling, and a wide range of other felonies.[38] Using time-series analyses, they found no deterrent effects of executions on felony murders.[39] This was an improvement over earlier tests that lumped together capital and other homicides. After all, felony murder carries strict liability, a consequence of the intent-based retributivism that guides most of the capital murder statutes in effect in thirty-eight states

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manslaughter); KENNETH POLK, WHEN MEN KILL: SCENARIOS OF MASCULINE VIOLENCE 175–84 (1994) (distinguishing homicides based on relationships between victim and offender); MARVIN WOLFGANG, PATTERNS IN CRIMINAL HOMICIDE 20–27 (1958) (same).

33. *See, e.g*, Francisco Parisi & Vernon Smith, *Introduction* to THE LAW AND ECONOMICS OF IRRATIONAL BEHAVIOR 1, 1–2 (Francisco Parisi & Vernon Smith eds., 2005) (arguing that doubts over human rationality arise from people's varying degrees of "skills, endowments, and a variety of psychological and physical constraints"); *see also infra* notes 159–160 and accompanying text.

34. Robert H. Dann, *The Deterrent Effect of Capital Punishment*, FRIENDS' SOC. SERVICE SERIES , Mar. 1935, at 3, 5–6.

35. Leonard D. Savitz, *A Study of Capital Punishment*, 49 J. CRIM. L. CRIMINOLOGY & POL. SCI. 338, 340 (1958).

36. *Id.* at 341.

37. Ruth D. Peterson & William C. Bailey, *Felony Murder and Capital Punishment: An Examination of the Deterrence Question*, 29 CRIMINOLOGY 367, 372 (1991).

38. *Id.*

39. *Id.* at 379–80.

today.[40] However, this narrowing exercise produced no evidence of a deterrent effect, using homicide data from 1976 to 1987.

The most recent effort to disaggregate homicides was published by Professor Joanna Shepherd,[41] who used information about homicide circumstances and situations that is provided in police descriptions of homicides and made available through a public-use data archive.[42] Shepherd reported that executions deter all types of murder, including "crimes of passion" that so often are considered to be irrational and spontaneous acts that are beyond the rational reach of execution threats.[43] However, Shepherd's partitioning of the data was not indexed to statute, but to a set of categories descriptive of "different types of murders"[44] that were defined neither by statute nor, with the exception of "crimes of passion," by theory. More important, none of these categories was narrowed according to statutory criteria that bound the circumstances and conditions that qualify a murder as "capital."[45]

With this one exception, the majority of the current portfolio of deterrence studies, conducted principally by economists, have ignored these limited attempts to isolate the effects of capital punishment on the crimes to which it is targeted, and instead assume that the threat of execution will deter all manner of homicides. In this Article, we set out to correct this error.

C. The Research Enterprise

We shift the argument on deterrence by focusing not on general homicide trends and rates, but on the subset of homicides that have been defined as eligible for the death penalty by statute. These types of homicides should provide a more sensitive indicator than the overall homicide rate index for detecting a deterrent effect from execution.[46] We use the public-use

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40. Kevin Cole, *Killings During Crimes: Toward a Discriminating Theory of Strict Liability*, 28 AM. CRIM. L. REV. 73, 74–75 & n.6 (1990).

41. Shepherd, *supra* note 15, at 285.

42. *Id.* Information about the circumstances of events is provided by the Federal Bureau of Investigation in the *Supplementary Homicide Reports*, a data file of homicide records that includes information on victims, offenders (where known via arrest), and the circumstances of the homicide event. *See* MALTZ, *supra* note 23, at 31–39; *see also infra* note 71.

43. Shepherd, *supra* note 15, at 308.

44. *Id.* at 292 (discussing murders of intimates, acquaintances, and strangers, as well as crime-of-passion murders, murders committed during felonies, and murders of African Americans and whites).

45. *Id.* For a discussion of the aggravating and mitigating factors that a jury takes into account in the penalty phase of a capital murder case, see 40A AM. JUR. 2D *HOMICIDE* § 551 (1999).

46. *See, e.g.*, Franklin Zimring & Gordon Hawkins, *Deterrence and Marginal Groups*, 5 J. RES. CRIME & DELINQ. 100, 100–05 (1968) (advocating "marginal group" analysis, which aims to identify the persons whom deterrent measures are thought to control and to provide a more precise account of the deterrent effect of criminal laws). Some might consider this distinction nothing more than an "acoustic separation" that has little meaning in the reality of homicide commission. But as we show later on, the breadth and heterogeneity of homicide make this distinction meaningful. Moreover, one might argue that if there is no special deterrent effect for "capital homicides," then

data archive based on police descriptions of homicides from 1976–2003 to construct rates of potentially death-eligible killings. While death-eligible cases are a much larger fraction of total homicides than cases that produce death sentences, the types of killing that are eligible for the capital sanction are less than 25% of total criminal homicides, as will be seen in Table 1 and Figure 1.

Once potentially capital killings have been isolated, our research strategy is to probe for distinctive movements in death-eligible killings in death penalty states to show whether the prospect of execution is influencing homicide, rather than the many other factors that vary over time to produce fluctuations in homicide rates. The study uses the variation in nondeath-eligible killings as a natural control for temporal influences. We hypothesize that variations in the administration of the death penalty should produce increases and decreases in death-eligible killings that are distinct from changes in nondeath-eligible killings—such specific patterns are the distinctive fingerprint of the death penalty effect.

This strategy was first used in a study of "three strikes and you're out" laws which greatly increased the penalties for two classes of persons with prior records who had previously been responsible for about one-eighth of California felony arrests.[47] The test of deterrent impact in that study was to see if the proportionate share of persons in the two special penalty categories declined after the effective date of the new legislation.[48] The study found that the proportion of defendants eligible for third strikes in the post-law arrest pool declined 19% (indicating marginal deterrence) but that the proportion of second strike eligible defendants did not change (indicating no additional deterrence for this group).[49]

In the current study, any increase in execution risk should reduce the proportion of killings that are potentially death-eligible *if* it is the change in death risk that is operating net of other factors that may be influencing rates of both capital-eligible and other homicides. It is only for capital-eligible

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the threat of death could just as easily affect not only homicide but all nonlethal crimes. We constrain the analysis to the types of homicides most likely to produce death-eligible cases by controlling for robberies, the paradigm of a felony murder. *See infra* note 56 and accompanying text. We also conduct robustness tests by estimating the effects of death sentences and executions on felony murders, those murders committed during the course of commission of another homicide. *See infra* Part IV.

47. FRANKLIN E. ZIMRING ET AL., PUNISHMENT AND DEMOCRACY: THREE STRIKES AND YOU'RE OUT IN CALIFORNIA 85 (2001) [hereinafter ZIMRING ET AL., THREE STRIKES]; *see also* FRANKLIN E. ZIMRING ET AL., CRIME AND PUNISHMENT IN CALIFORNIA: THE IMPACT OF THREE STRIKES AND YOU'RE OUT 1–2 (2001) [hereinafter ZIMRING ET AL., IMPACT OF THREE STRIKES].

48. ZIMRING ET AL., THREE STRIKES, *supra* note 47, at 85.

49. *See id.* at 98 (noting that the proportion of third strike offenders dropped from 4.3% to 3.5% after the passage of the law while there was no significant change in the proportion of second strike offenders for the same time period).

killings that the threat has any reality.[50] If the risk of execution goes down, then the proportion of death-eligible killings in death penalty states should increase because the force of the death threat has weakened. The shorthand method for measuring these specific effects is to determine the "market share" of cases that would be death-eligible both over time and between states at various time points.

There are three different "market share" comparisons that test deterrence. In death penalty states, the market share of death-eligible cases should go down when execution risk increases and should go up when execution risk decreases. Cross-sectionally, the market share of death-eligible cases should be larger in states without a death penalty (no marginal deterrent of capital threat) than in states with a death penalty. And as execution risk increases, the market share of death-eligible cases should shrink in death penalty states but not in states without the death penalty. In sum, the comparison of death-eligible versus non-eligible homicides becomes the preferred method of choosing between execution effects and other temporal factors.

We begin with an analysis of the market share of capital homicides in the context of homicide trends from 1976 to 2003 in all death penalty states and compare patterns in those states with nondeath penalty jurisdictions. Then, we add a detailed analysis of trends in the state of Texas and in Houston, its largest city. We place special emphasis on Texas (and Houston) for two reasons. That state and that city have been the dominant users of executions in the modern era, with Texas accounting for more than one-third of all executions in the first quarter century after executions resumed (through 2006).[51] The second reason for a special Texas focus is that recent

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50. *See* Shepherd, *supra* note 15, at 292. Shepherd maintains that executions deter all types of murder by allowing all would-be murderers to update their expectations of punishment risk, compensating for the uncertainty about whether the murder they are about to commit would be charged and prosecuted capitally. *Id.* Such uncertainty, she claims, has less to do with the putative murder than with exogenous factors such as prosecutorial discretion, quality of defense counsel, and juror preferences. *Id.* These assumptions of cognition, risk analysis, cost measuring, and premeditation in homicide are rarely observed in research on murder and murderers, except perhaps among the very small percentage of murder-for-hire and premeditated killings. *See* Gregg v. Georgia, 428 U.S. 153, 186 (1976) ("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."). Rather, murderers are more likely to discount punishment risks and inflate the present value of whatever gains the crime may offer. *See* Yair Listokin, *Efficient Time Bars: A New Rationale for the Existence of Statutes of Limitations in Criminal Law*, 31 J. LEGAL STUD. 99, 100 (2002) (noting as "commonly accepted within . . . criminology" the view that "criminals discount the future at a higher rate than society"). Recognizing this, state legislatures have historically enacted murder laws that focus on intent as a metric to identify and isolate a set of murders for the most serious punishments available in that state. *See* Cole, *supra* note 40, at 74 (stating that a killing will be classified as a criminal homicide only if the killer possessed a certain mental state).

51. Death Penalty Info. Ctr., Executions in the United States, 1608–1976, By State, http://www.deathpenaltyinfo.org/article.php?scid=8&did=1110. The Death Penalty Information Center's website displays a number of statistical tables about the death penalty, which are periodically updated to take account of new death sentences, executions, and exonerations. This

social science analysis of general homicide patterns has shown that the evidence of execution impact on total homicide can be dismissed for U.S. death states other than Texas.[52] Indeed, much of the deterrent effect observed in the new deterrence studies is leveraged by the influence of Texas, and within Texas, the effects are concentrated in and leveraged by the patterns in Harris County.[53] If executions show a distinctive impact on death-eligible killings anywhere, Texas should be the place. Given the high rate of executions in Texas, the case for the impact of the death penalty on capital-eligible homicide over time cannot be so easily dismissed for Texas.

Finally, we use panel data methods to estimate a series of regression models to identify the effects of capital punishment on the rate of capital-eligible homicides. We adjust the estimates for the level of noncapital homicides in each state over time to control for variations from state to state in the base rates of homicide. This strategy allows us to estimate whether the changes in the noncapital homicide rate are simultaneously influencing the rate of capital homicides. We include two measures of capital punishment: the existence of a death penalty statute in each state for each year in the panel and then the number of death sentences and executions that took place in the state in the preceding year and the preceding two years. We scale both the number of capital-eligible homicides and other homicides to each state's population to ensure that any deterrent effects from execution are weighted proportionately to the state's population. We include a rich set of socioeconomic and criminal justice system variables that are robust correlates of the murder rate within and between states over time; these correlates and predictors of homicide have been validated extensively in research across cities and states over the past three decades.[54] We are particularly interested in the effects of incarceration rates in assessing whether punishment risks compete with other social and legal factors,

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Article's citations to the DPIC reflect the statistics on its webpage as of July 17, 2006, and archived copies of those statistics as of that date are on file with the Texas Law Review.

52. *See* Berk, *supra* note 19, at 320–24 (proving that the deterrent effect of executions disappears when Texas execution statistics are eliminated from statistical observations).

53. *See id.* at 328 (concluding that the inclusion of "Texas data can give the false impression that a deterrence relationship exists" and "distributional problems that characterize the number of executions remain when counties are the spatial units").

54. *See* Lauren J. Krivo & Ruth D. Peterson, *The Structural Context of Homicide: Accounting for Racial Differences in Process*, 65 AM. SOC. REV. 547, 558 (2000) (finding that "crime-generating processes" are correlated with structural and socioeconomic variables and not necessarily with race); Kenneth C. Land et al., *Structural Covariates of Homicides Rates: Are There Any Invariances Across Time and Social Space?*, 95 AM. J. SOC. 922, 951 (1990) ("By far, the strongest and most invariant effect is due to the resource-deprivation/affluence index; consistently across the four decennial census periods, cities, metropolitan areas, or states that are more deprived have higher homicide rates, and those that are more affluent have lower rates."); Robert J. Sampson & Janet L. Lauritsen, *Violent Victimization and Offending: Individual-, Situational-, and Community-Level Risk Factors*, *in* 3 UNDERSTANDING AND PREVENTING VIOLENCE: SOCIAL INFLUENCES 1, 48 (Albert J. Reiss, Jr. & Jeffrey A. Roth eds., 1994) ("Not surprisingly, a large proportion of recent neighborhood-based studies of violence have emphasized dimensions of poverty and economic inequality.").

including capital punishment, in predicting changes in homicide rates over time.[55] We include an index for the robbery rate to control for the supply of events that produce a large share of capital-eligible homicides.[56] We use alternate analytic methods that consider time trends in different ways, including procedures that account for the strong autocorrelation or stationarity of homicide rates over time.

II. Capital Homicides

A. The Rules and Grammar of Capital Murder

One of *Furman*'s legacies is the development within death penalty states of statutory language defining which homicides are eligible for capital punishment. As Professors Jonathan Simon and Christina Spaulding comment, these elements of homicides provide a "currency through which states seek to recognize various concerns and valorize certain kinds of subjects and situations."[57] Designed to eliminate the arbitrariness in death sentencing that underscored death penalty statutes and prosecutorial practices before *Furman*, the new statutes were designed to tighten and rationalize the justification for the execution of certain murderers and the exemption of others from death. The elements that informed most states were derived from the Model Penal Code[58] factors plus a few additional factors that legislators included at the time that each state drafted its initial post-*Furman* law.[59] Simon and Spaulding characterize the ritual addition each year of new aggravating factors to capital statutes as akin to state legislatures "hanging Christmas ornaments."[60]

Simon and Spaulding list fourteen aggravating factors that characterize capital statutes in the post-*Gregg* era, including the eight in the Model Penal Code plus six others that are common to the current era of death penalty legislation.[61] Some of these aggravators list special victims based on their

55. *See, e.g.*, Katz et al., *supra* note 10, at 339–40 (reporting a negative correlation between prison death rates—a proxy for poor prison conditions—and crime rates, but finding little deterrent effect of capital punishment); *see also* Steven D. Levitt, *Understanding Why Crime Fell in the 1990s: Four Factors that Explain the Decline and Six that Do Not*, J. ECON. PERSP., Winter 2004, at 163, 170–83 (finding four factors that explain the nationwide decrease in crime: the increased number of police, the rising prison population, the receding crack cocaine epidemic, and the legalization of abortion).

56. *See infra* text accompanying note 86 (indicating that 80% of forcible felony killings are robbery–homicides).

57. Jonathan Simon & Christina Spaulding, *Tokens of Our Esteem: Aggravating Factors in the Era of Deregulated Death Penalties*, *in* THE KILLING STATE: CAPITAL PUNISHMENT IN LAW, POLITICS, AND CULTURE 81, 81 (Austin Sarat ed., 1999).

58. MODEL PENAL CODE § 210.6 cmt. 12 (Revised Commentary 1980).

59. *See id.* at cmt. 13 (discussing the addition of "the knowing killing of a police officer, fireman, or prison" officer as an aggravating factor as the "most common departure" from the Model Penal Code by state legislatures).

60. Simon & Spaulding, *supra* note 57, at 82.

61. *See id.* at 84 tbl.4.1.

vulnerability: the very young and the very old.[62] Still others mention killings committed by persons serving prison sentences, multiple-victim shootings, killings committed in the course of crimes for monetary gain, crimes committed while fleeing a lawful arrest, and killings of police officers, correctional staff, or public officials.[63] Between 1972 and 1980, nearly all death penalty states adopted the eight aggravating factors from the Model Penal Code, and then added a core of other factors that today are commonly used in death penalty statutes: heinousness or atrociousness of the act, murders committed while lying in wait, and killings of witnesses in criminal or civil proceedings.[64] In a second wave of legislation, following the sharp rise in homicides nationally in the late 1980s, legislatures added another set of aggravating factors by expanding their felony murder laws.[65] These statutes listed special circumstances, such as drug deals, gang drive-by shootings, and murders or other crimes committed with automatic weapons. For over a decade beginning in the 1980s, these crimes captured the popular imagination and animated the political rhetoric and legislative response to the nation's worsening crime problems.[66]

These laws on the books provide one component of the logic that we used to define capital-eligible homicides. The contrasting component was the law in action. Beyond the current debate on proportionality is a larger question about who exactly is on death row, and the extent to which these persons are a mirror of the selection processes that create pools of death-eligible defendants from among persons arrested for murder. And until recently, there has been almost no systematic research on the types of aggravating factors that create death eligibility among persons either selected for capital prosecution or sentenced to death by judges or juries.[67]

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62. *See id.* at 91 tbl.4.2.

63. *Id.*

64. *Id.* at 84.

65. *See id.* at 91 tbl.4.2.

66. *See, e.g.*, 48 Hours on Crack Street (CBS television broadcast Feb. 19, 1988); *see also* WILLIAM J. BENNETT ET AL., BODY COUNT: MORAL POVERTY . . . AND HOW TO WIN AMERICA'S WAR AGAINST CRIME AND DRUGS 14 (1996) (advancing a prediction of a generation of "superpredators" who would kill wantonly and randomly and whose crimes would demand punishment by death).

67. One effort to identify which cases among the statutorily capital-eligible were selected for prosecution was recently completed in Maryland. *See* Raymond Paternoster et al., *Justice by Geography and Race: The Administration of the Death Penalty in Maryland, 1978–99*, 4 U. MD. L.J. RACE, RELIGION, GENDER & CLASS 1, 17 (2004). Examining 1,311 death-eligible cases from 1978 to 1999 based on the Maryland statute, MD. CODE ANN., CRIM. LAW § 2-303 (LexisNexis 2002), Paternoster and his colleagues identified a set of cases that were death-eligible and where capital charges were filed. Maryland's statute includes a total of fourteen aggravating factors that qualify a case as death-eligible. *Id.* The factors that were most common among the death-eligible cases are similar to the list compiled by Simon and Spaulding, though with some minor differences. *See* Paternoster et al., *supra*, at 59 tbl.1. Although the Maryland study addressed racial disparity, it generated statistical information on which statutory aggravating factors were most often present among cases selected for capital prosecution: murders committed during other crimes, murders with multiple victims, murders committed while the perpetrator was in a correctional institution, contract

We turned to Texas as an example of a law in action that produces a large set of capital cases. As most observers of the death penalty know, Texas's total of 369 post-*Gregg* executions is the highest in the United States, accounting for more than one execution in three since 1976, and nearly four times more than the 95 executions in Virginia, the next most frequent execution state.[68] Texas's murder statute lists nine aggravating factors that create eligibility for the death penalty.[69] These factors are similar to the Model Penal Code aggravating factors, but are somewhat narrower than the longer list of aggravators common in the states today.[70] Evidently, both in its categorical structure and its implementation, the Texas statute is sufficiently broad and flexible—elastic, in effect—as to generate a large number of capital-eligible homicides. The combination of the high rate of executions in Texas, the state's prominent role in the new deterrence literature, and its statutory framework provide an ideal setting to identify a set of capital-eligible cases and to test whether execution has a deterrent effect on that subset of cases. Accordingly, we adopted and operationalized the Texas statute as a second framework to identify a set of capital-eligible cases from across both death penalty states and nondeath penalty states in the post-*Gregg* era.

B. Applying the Rules

To identify which homicides were capital-eligible, we turned to the *Supplementary Homicide Reports*, a data archive created and maintained by the Federal Bureau of Investigation of the U.S. Department of Justice. Known as the SHRs, these case-level records are created by participating police departments across the country and compiled by the FBI.[71] Data are available from 1976 to 2003, and include records of 494,729 homicide cases.[72] The SHR has the unique advantage of providing detailed, case-level information about the context and circumstances of each homicide event known to the police.[73] This allows us to identify the presence of factors that map onto the statutory framework of the Texas murder statutes and more broadly onto the Model Penal Code aggravating factors.

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killings, and murders committed while fleeing capture by police. *Id.* The Maryland study was designed to identify racial disparity, so the odds ratio associated with each of the statutory factors was not computed.

68. Death Penalty Info. Ctr., *supra* note 51.

69. TEX. PEN. CODE ANN. § 19.03 (Vernon 2006).

70. *See* Simon & Spaulding, *supra* note 57, app. 4A at 102–09 (listing aggravating factors by jurisdiction).

71. Bureau of Justice Statistics, U.S. Dep't of Justice, Homicide Trends in the U.S., http://www.ojp.usdoj.gov/bjs/homicide/homtrnd.htm#contents. The source data are published as Uniform Crime Reports [United States]: Supplementary Homicide Reports, 1976–2003 (ICPSR Study No. 4351, 2005) [hereinafter SHR], *available at* http://webapp.icpsr.umich.edu/cocoon/ICPSR-STUDY/04351.xml.

72. SHR, *supra* note 71.

73. *Id.*

Much has been written about the *Supplementary Homicide Reports*, and the limitations of the archive are well known.[74] Nonreporting by some law enforcement agencies is probably the most significant concern, and efforts to overcome this limitation have generated the most attention among researchers.[75] Much of this attention has focused on developing ways to revise population estimates of the demographic distribution of homicide victims and offenders within specific years.[76] But for our purposes, missing data is a less serious limitation, because we have no reason to suspect that the ratio of capital-eligible to other homicides varies systematically in years when SHR observations are missing.[77] Our concern is with the observed patterns of circumstances and situations, and there is no theoretical or empirical reason to suspect that any particular circumstance, especially felony murder, would be more or less prevalent in those states where police agencies have failed to compile these records.

To generate estimates of the prevalence of capital homicides, we coded each homicide record in the SHR as a capital-eligible homicide if the circumstances included any of the following elements that are part of the recurrent language of capital-eligible homicides across the states: (a) killings during the commission of robbery, burglary, rape or sexual assault, arson, and kidnapping; (b) killings of children below age six;[78] (c) multiple-victim killings; (d) "gangland" killings involving organized crime or street gangs; (e) "institution" killings where the offender was confined in a correctional or other governmental institution; (f) sniper killings; and (g) killings in the course of drug business. We excluded killings by persons below age sixteen, whose eligibility for the death penalty was removed by the United States

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74. *See, e.g.*, MALTZ, *supra* note 23, at 33−39.

75. *See, e.g.*, Robert L. Flewelling, *A Nonparametric Imputation Approach for Dealing With Missing Variables in SHR Data*, 8 HOMICIDE STUD. 255 (2004) (discussing the nature and extent of the non-reporting problem and exploring possible solutions); James Alan Fox, *Missing Data Problems in the SHR: Imputing Offender and Relationship Characteristics*, 8 HOMICIDE STUD. 214 (2004) (same).

76. *E.g.*, Flewelling, *supra* note 75 (developing an imputation method to adjust demographic estimates for victims and offenders to more accurately reflect actual populations); Fox, *supra* note 75 (same).

77. *See* Betsey Stevenson & Justin Wolfers, *Bargaining in the Shadow of the Law: Divorce Laws and Family Distress*, 121 Q.J. ECON 267, 275 n.15 (2006) (estimating that nonparticipation in the SHR produced 37 of 2,754 state-year observations for the period 1964–1996 where missing data required interpolation to estimate gender-specific homicide rates).

78. We included killings of children that are found in the death statutes of states with high death sentencing or execution rates (Texas, Maryland, Pennsylvania, Virginia, and Alabama), but are not present in several other states with populous death rows or high execution counts (California, Florida, and Georgia). To illustrate, the following states include child killings in their capital statutes: ARIZ. REV. STAT. ANN. § 13-703 (F)(9) (2001 & Supp. 2005); 720 ILL. COMP. STAT. ANN. 5/9-1(b)(7) (West 2002 & Supp. 2005); LA. REV. STAT. ANN. § 14:30(A)(5) (1997 & Supp. 2006); NEV. REV. STAT. § 200.033(10) (2005); OHIO REV. CODE ANN. § 2903.01(C) (West 1997); 42 PA. CONS. STAT. ANN. § 9711(d)(16) (West 1998 & Supp. 2005); VA. CODE ANN. § 18.2-31(12) (2004). Several other states do not mention child killings. *See, e.g.*, CAL. PENAL CODE § 189 (West 2006); FLA. STAT. ANN. § 782.04 (West 2000 & Supp. 2006); GA. CODE ANN. § 17-10-30 (2004).

Supreme Court in *Thompson v. Oklahoma* in 1988.[79] The ban was extended in 2005 to all persons below the age of 18 in *Roper v Simmons*.[80] We also included a separate count of the killings of police officers. The annual data files, Law Enforcement Officers Killed and Assaulted (LEOKA) also are compiled by the U.S. Department of Justice through the FBI.[81] A separate count for this prominent category of capital-eligible homicides was needed because the SHR data do not permit classification of this group of homicides. These totals were compiled for both death penalty and nondeath penalty states from 1976 to 2003.[82] Figure 1 and Table 1 show the types of killings and their relative frequency.

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79. 487 U.S. 815, 838 (1988). For a review of the jurisprudence on immaturity and the diminished culpability of adolescents in capital trials, see Jeffrey Fagan, Atkins*, Adolescence, and the Maturity Heuristic: Rationales for a Categorical Exemption for Juveniles from Capital Punishment*, 33 N.M. L. REV. 207, 234–52 (2003), which discusses evidence of juveniles' immaturity, the risk of false confessions, and the risk of error in attempts to assess individual juveniles' culpability, and Victor L. Streib, *Prosecutorial Discretion in Juvenile Homicide Cases*, 109 PENN ST. L. REV. 1071, 1085 (2005), which discusses the importance of limiting the scope of prosecutorial discretion in juvenile homicide cases due to the special circumstances in these cases.

80. 543 U.S. 551 (2005).

81. LEOKA compiles data from the FBI's Uniform Crime Reports to create a data archive on law enforcement officers killed in the line of duty. In addition to maintaining a machine-readable data file, an annual report is published by the FBI. *See, e.g.*, Fed. Bureau of Investigation, Law Enforcement Officers Killed and Assaulted 2004, http://www.fbi.gov/ucr/killed/2004/openpage.htm.

82. Each state was classified in each year as a death penalty or nondeath penalty state according to the presence of a valid death penalty statute in that year.

Table 1: Capital-eligible homicides, all states[83]

Capital-eligible homicides by category

Category	N	% of all homicides	% of capital-eligible homicides
Homicides during crimes	59,459	11.8	48.2
Institution killings	816	0.2	0.7
Gangland killings	2,138	0.4	1.7
Youth gang killings	14,298	2.8	11.6
Sniper killings	489	0.1	0.4
Murders of children 6 and younger	17,187	3.4	13.9
Killings of police officers	1,410	0.3	1.1
Multiple victims	39,168	7.8	31.7
Total capital-eligible	123,485	24.5	100.0
Total noncapital-eligible	380,990	75.5	
Total	504,475	100.0	

Capital-eligible homicides during crimes by crime type

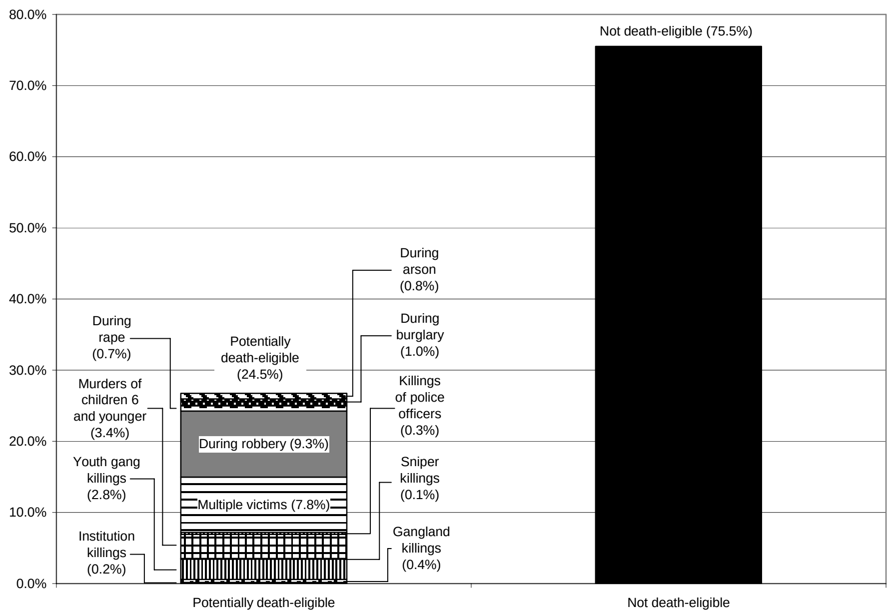
Category	N	% of all homicides	% of capital-eligible homicides
Robbery	46,861	9.3	37.9
Rape	3,732	0.7	3.0
Burglary	4,940	1.0	4.0
Arson	3,926	0.8	3.2
Total	59,459	11.8	48.2

83. SHR, *supra* note 71. For the LEOKA data, see Appendix B. The *Supplementary Homicide Reports* are filtered to exclude the deaths in New York associated with the attacks of September 11, 2001, but include those associated with the Oklahoma City bombing of April 19, 1995.

Capital-eligible homicides are limited to those committed by offenders ages 16 or above and those with unknown offender ages. Homicides committed by offenders younger than 16 are not considered capital-eligible. (It is likely that some of the "unknown offender" homicides were also committed by offenders under 16). Overall, 2.6% of homicides with offenders of known age were committed by juveniles. Whether homicides with offenders of unknown ages are similarly distributed is uncertain.

Total capital-eligible homicides is less than the sum of the individual categories, due to overlaps in the categories. For example, among homicides not committed by juveniles, 6,798 committed in the course of other crimes also had multiple victims; and, 880 homicides committed in the course of other crimes also had child victims. Also, killings of police officers was added in separately since these cases were not identifiable in the SHR records.

Figure 1: Murder and non-negligent homicide: Potentially death-eligible and other killings, 1976–2003[84]



84. *See supra* note 83.

Table 2: Capital-eligible homicides, Texas, 1977–2003[85]

Capital-eligible homicides by category

Category	N	% of all homicides	% of capital-eligible homicides
Homicides during crimes	5,723	11.6	54.6
Institution killings	117	0.2	1.1
Gangland killings	259	0.5	2.5
Youth gang killings	155	0.3	1.5
Sniper killings	18	0.0	0.2
Murders of children 6 and younger	1,520	3.1	14.5
Killings of police officers	148	0.3	1.4
Multiple victims	3,725	7.5	35.6
Total capital-eligible	10,476	21.1	100.0
Total noncapital-eligible	39,060	78.9	
Total	49,536	100.0	

Capital-eligible homicides during crimes by crime type

Category	N	% of all homicides	% of capital-eligible homicides
Robbery	4,583	9.3	43.7
Rape	354	0.7	3.4
Burglary	606	1.2	5.8
Arson	180	0.4	1.7
Total	5,723	11.6	54.6

Across all states, a total of 24.5% of all reported killings were potentially death-eligible types of cases, with the lion's share of these being forcible felony killings (11.8%) and killings with multiple victims (7.8%).[86] A small number of capital-eligible homicides were killings of children (3.4%). Among the forcible felony killings, nearly eight in ten (46,861/59,459, or 78.8%) were robbery killings.

85. *See supra* note 83.

86. We excluded from the probable capital cases FBI-classified drug cases (4.3%) and auto-theft killings (0.7%) noted by police. The drug category includes some cases that may be death-eligible under federal law and the auto cases that involve robberies would also count as robberies. For state criminal law purposes, these cases are not death eligible without forcible felony involvement. *See generally* Death Penalty Info. Ctr., Crimes Punishable by the Death Penalty, http://www.deathpenaltyinfo.org/article.php?did=144&scid=10 (listing crimes punishable by death state-by-state).

There were small differences in these distributions for death penalty and nondeath penalty states. In death penalty states, felony killings comprised 11.6% of all homicides, multiple-victim homicides were 7.7%, and homicides with child victims were 3.3%. In nondeath penalty states, felony killings were 12.5% of all homicides, multiple-victim homicides were 8.0%, and homicides with child victims were 3.6%.[87]

We repeated this analysis for homicides in Texas during the same period. The portion of homicides in Texas in this period that were potentially capital-eligible cases was slightly lower than the rate reported in Table 1: 21.1%. Most of these were forcible felony killings (11.6%) and killings with multiple victims (7.5%). As in the national estimate, a small number of capital-eligible homicides were killings of children (3.1%). Among the felony murders, the plurality again were robbery–homicides (43.7%, or 80% of all felony murders).

There are two major problems with trying to measure the extent of additional deterrence from a capital threat by the study of variation in a crime category where three-fourths of the offenses are not eligible for death. First, if there is any marginal deterrence from variations in execution risk, including so many cases where there was no risk of execution might dilute the apparent deterrence from those cases where the risk of execution was real. Any deterrent threat should be clustered in death-eligible cases, so including masses of ineligible cases reduces the apparent impact of the threat. Why not simply test the impact of execution risk on some aggregate crime category, like index crime as a whole or on all violent felonies? A fair test of deterrence should restrict the presumed dependent variable to those cases where the law intends to threaten death—on the 25% of cases where death is a possibility and not on the 75% of cases where it is not. The inclusion of so many cases where death is not a threatened sanction also risks falsely concluding that changes over time in homicide rates are caused by variations in threatened or administered rates of execution. The inclusion of all homicides assumes that the deterrent effect of execution is highly inelastic across a very heterogeneous set of circumstances and individuals of varying capacities. Adding in so many noncapital cases risks creating an ocean of artificial deterrence.

1. Testing the Accuracy of the Classifications.—The number and variety of death-eligible cases in Figure 1 and Table 1 were derived from the *Supplementary Homicide Reports* (SHR), an archive produced by the FBI as part of its Uniform Crime Report series. The SHR archive provides information about the circumstances in the majority of death-eligible killings, but not in all categories of death-eligible killings that are identified in the

87. Data are available from the authors at http://www2.law.columbia.edu/fagan/researchdata/caphom/.

majority of state statutes. For example, the SHR records provide sufficient information to identify homicides committed during the commission of other crimes (felony murders), institution killings, multiple-victim killings, sniper killings, and killings of very young children or the very old. But the SHR records do not provide information to identify cases of murder-for-hire, some of the murders that are capital-eligible because of the heinous or atrocious nature of the act, or murders of police officers.

To test the accuracy and comprehensiveness of the SHR categories that we used to segregate killings that carry a risk of a capital sentence, we identified the 100 most recent consecutive executions reported in U.S. court records as of March 1, 2006, listed in the execution database maintained by the Death Penalty Information Center (DPIC).[88] The DPIC database has the capacity to generate lists of executions according to user-selected identifiers such as state, defendant characteristics, and time period. To estimate the coverage of the SHR records among the universe of executed persons, we identified the 100 most recent executions and then obtained court records that stated the circumstances of the murder for which the defendants were executed. We then coded these cases to determine the specific statutory aggravators that these cases reflected. This procedure generated an index of the proportion of actual executions which were identified as death-eligible in our classification system.

The sample of executions covers the period from June 8, 2004 to February 15, 2006, beginning with William Zuern in the state of Ohio and concluding with Clyde Smith Jr. in Texas. In our analysis, all but five of the 100 cases would appear as death-eligible based on our definition. The exceptions were three homicides that we classified as "contract killings" or "murder-for-hire," and two characterized by the court and classified by us as "exceptional cruelty" ("atrocious" or "heinous").

These results validate the accuracy of the classification system that we used to identify capital-eligible homicides. Extrapolating this 95% accuracy, we estimate that the true proportion of death-eligible cases is 25% of total killings, and that 95% of these are in our death-eligible class. We also estimate that the proportion of cases misclassified as not death-eligible is five for every 300 cases classified as nondeath-eligible (the ratio of non-eligible to eligible is greater than three to 1). That is, we estimate that 1.67% of the group that we classified as nondeath-eligible is likely to be death-eligible. The cases in our death-eligible category are about 60 times more likely to end up with death sentences as cases in the non-eligible category.

We also checked the extent to which our categories of potentially death-eligible killing in Texas covered cases that generated actual executions. We examined the most recent fifty cases that led to executions in Texas as of

88. Death Penalty Info. Ctr., Searchable Database of Executions, http://www.deathpenaltyinfo.org/executions.php [hereinafter DPIC, Searchable Database].

May 10, 2006, beginning with Kenneth Bruce on January 14, 2004, and ending with Jackie Wilson on May 4, 2006.[89] All but two of these fifty cases fit exactly our statutory criteria and definition. The two that were not included in our definition were "contract killings," an aggravating factor that is frequently unknown when a case is listed by local police agencies when compiling their SHR reports. Accordingly, we estimate the definition of capital-eligible homicides in this study captures 96% of all Texas homicides that result in death sentences and executions, a figure comparable to the 95% accuracy estimate for all the death penalty states.

C. Execution and the Market Share of Capital Homicides

The study of trends in only death-eligible cases should solve both of the problems associated with aggregating capital and noncapital killings. If execution risk is driving homicide levels, then this should be a specific effect observed in death-eligible cases but not in other types of homicide. If, however, temporal influences independent of the death penalty are producing false inferences about deterrence, then we would expect to see similar trends in capital and noncapital-eligible homicides. That is why what we call the "market share" of death-eligible homicides is critical to our study.

Figures 2a–2c show the trends for the nation, and then separately for death penalty and nondeath penalty states. Recall that a state is a death penalty state in any year only if there was a valid death penalty statute in effect in that state during that specific year. To frame these trends, note that executions were a relatively rare event in the United States before 1984: executions rose from 5 nationally in 1983 to 21 in 1984, declined to 11 in 1988, and then rose steadily for over a decade—peaking in 1999 with 98 executions nationally before declining again to 59 in 2004.[90]

89. *Id.*

90. Death Penalty Info. Ctr., Executions by Year, http://www.deathpenaltyinfo.org/article.php?scid=8&did=146.

Figure 2a: Capital and noncapital homicide rate per 100,000 persons and percent capital, all states, 1976–2003[91]

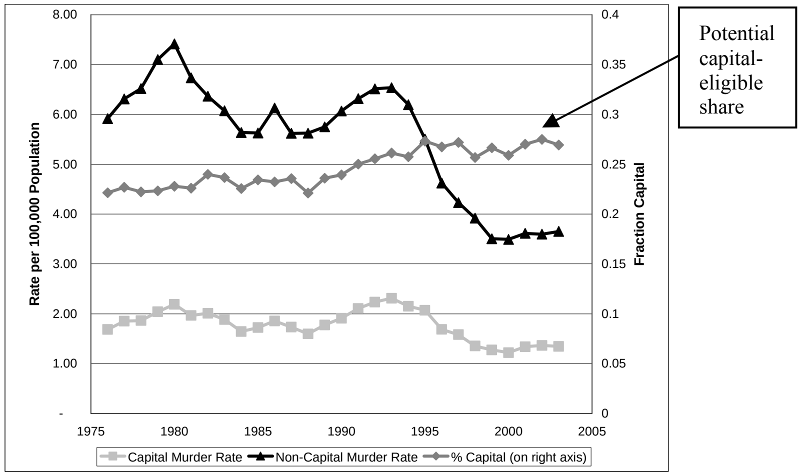
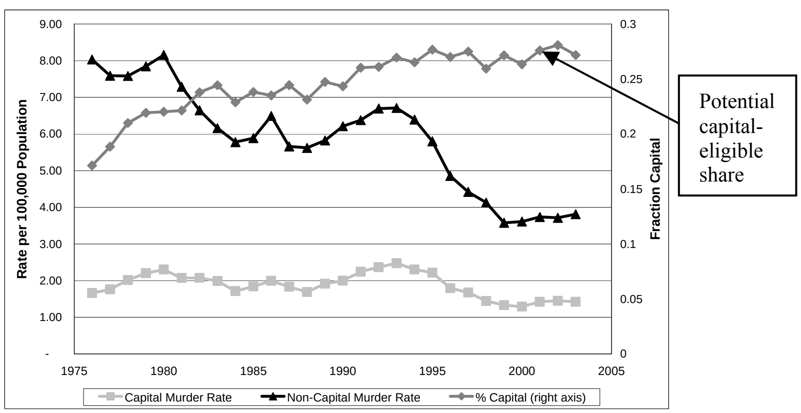


Figure 2b: Capital and noncapital homicide rate per 100,000 persons and percent capital, death penalty states, 1976–2003[92]



91. *See* SHR, *supra* note 71.
92. *See id.*

Figure 2c: Capital and noncapital homicide rate per 100,000 persons and percent capital, nondeath penalty states, 1976–2003[93]

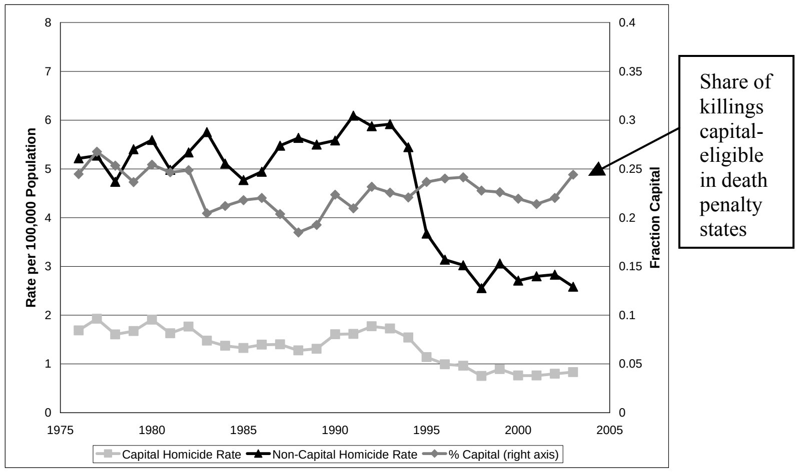


Figure 2a shows the national trend for all states from 1976 to 2003. The index of capital-eligible murders varies within a narrow range over nearly three decades, from a low of 1 per 100,000 persons in 2000 to a high of 2 per 100,000 persons in 1993. The long-term trend in noncapital murders shows a large decline over the same period, with a decline of nearly 50% from 1980 to 2000. Most important for our analysis is the long-term rise in the market share of homicides that are capital-eligible. The market share rises from a low of approximately 22% in 1975 to a peak of nearly 28% in 1995, and then varies by one percent each year above or below the 28% level through 2003.

This pattern also is evident in death penalty states. Figure 2b shows the same roller coaster pattern of capital-eligible homicides and a similar secular decline of more than 50% in noncapital homicides. The market share of capital-eligible crimes rises substantially in the death penalty states, from approximately 18% in 1975 to 27% in 1995. The market share fluctuates in a narrow range for the next nine years before returning to its previous high in 2004. The rise in market share of capital-eligible homicides was concurrent with a rise in executions (21 in 1984 to 98 in 1999).

Figure 2c identifies similar trends over the same period in states without the death penalty. Homicide rates are lower in these states over time, and the partitioned rates reflect the general base rate differences between death penalty and nondeath penalty states. The pattern of capital-eligible homicides fluctuates over time in a manner similar to the death penalty states. The market share of capital-eligible homicides in the nondeath penalty states varies erratically, between a high of 26% in both the early and later years of

93. *See id.*

the time series to a low of 19% in 1988, a year when executions were rare. The secular decline in noncapital homicides is sharpest beginning in 1995, when New York State passed a death penalty statute and its capital-eligible and other homicides were removed from this count.[94]

Every indication in the pattern over time of trends in death-eligible homicides is inconsistent with the anticipated influence of either a death penalty law or variations in rates of execution specifically on those types of homicide that these laws target. First, there is little variation in the rates of capital-eligible homicides over time. Second, the shape of the temporal trends in capital-eligible homicides in death penalty states and nondeath penalty states is nearly identical. That is, there is no visible influence of the death penalty on those cases where its impact should be concentrated. The fluctuations are timed nearly identically, and the range is also identical, both in timing and magnitude. Rates of death-eligible killings do not go down any faster than non-eligible killings when execution rates go up, and the death-eligible types of killings are no greater a share of the total in states with no death penalty. The trends in these death-eligible types of killings over time are no different in active execution states than in nondeath penalty states.

There appears to be no difference in capital-eligible homicide rates that can be attributed either to the presence of the death penalty or the frequency of its use. One of the staples of the death penalty debate in the United States is the interpretation of the base rate differences in homicides between death penalty and nondeath penalty states. Critics of the death penalty point to this differential as evidence of its weak deterrent effects.[95] Our analysis provides some confirmation of this claim, but for a very different reason: there is no difference in the magnitude or temporal change in the subset of homicides that should be most sensitive to the threat of execution.

D. Texas as a Natural Experiment

Several studies in the new deterrence literature point to Texas as the place where the deterrent effects of execution may be the strongest.[96] Among states, Texas is the most frequent user of capital punishment in the post-*Gregg* era, accounting for 369 of the 1,032 executions in the United

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94. 1995 N.Y. Laws 2 (codified as amended at N.Y. PENAL LAW § 60.06 (McKinney 2005)).

95. SELLIN, *supra* note 25, at 34; *see* Donohue & Wolfers, *supra* note 10, at 800–04 (discussing studies of the differences of homicide rates in death penalty and nondeath penalty states); *see also* John Lamperti, Does Capital Punishment Deter Murder? (2001) (unpublished manuscript), *available at* http://math.dartmouth.edu/~lamperti/capitalpunishment.pdf (analyzing studies to conclude that the death penalty does not deter or decrease the frequency of homicide).

96. *See, e.g.*, Cloninger & Marchesini, *supra* note 10, at 571–76 (reporting empirical findings in Texas consistent with the deterrent hypothesis); Joanna Shepherd, *Deterrence Versus Brutalization: Capital Punishment's Differing Impacts Among States*, 104 MICH. L. REV. 203, 233 (2005) (finding a strong deterrent effect in Texas). *But see* Berk, *supra* note 19, at 324, 328 (asserting that data give a "false impression" of deterrence in Texas due to three outlier years).

States since 1976.[97] This gives Texas unusual leverage on the relationship between executions and homicides in comparative analyses across states.[98] Indeed, recent social science analyses of general homicide patterns have shown that the evidence of execution impact on total homicide can be dismissed for U.S. death states other than Texas.[99] And within Texas, both death sentences and executions are concentrated in Harris County, which includes the city of Houston.[100] Since 1976, Harris County has accounted for 90 of the 369 executions in Texas in the time since *Gregg*, more than twice the number in Dallas County, the state's second highest contributor to Texas's death row.[101]

In addition, 282 persons from Harris County have been sentenced to death since *Gregg*,[102] and there are currently 137 on death row.[103] The county's high execution rate affords it statistical influence on the deterrence patterns that have been attributed to Texas. Accordingly, if executions show a distinctive impact on death-eligible killings anywhere, Texas should be the place. Given the high rate of executions in Texas, the case for the impact of the death penalty on total homicide over time cannot be so easily dismissed for Texas. Figures 3 and 4 show the trends in capital-eligible and noncapital homicide rates for Texas and Harris County, and the market share of capital-eligible homicides in each.

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97. According to DPIC, there have been 1,032 executions in the U.S. from *Gregg* through July 17, 2006. *See* Death Penalty Info. Ctr., Executions by State, http://www.deathpenaltyinfo.org/article.php?scid=8&did=186.

98. *See* Berk, *supra* note 19, at 305 (explaining how the large number of executions in Texas can skew statistical results).

99. *Id.* at 320–23.

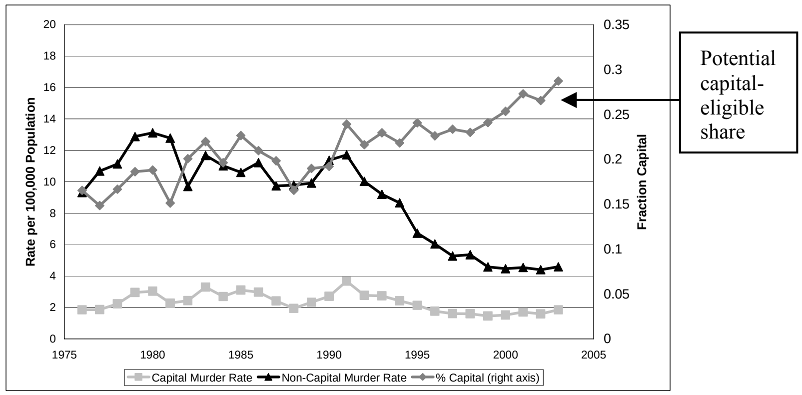
100. Tex. Dep't of Criminal Justice, County of Conviction for Executed Offenders, http://www.tdcj.state.tx.us/stat/countyexecuted.htm. The Texas Department of Criminal Justice's website displays a number of statistical tables about the death penalty, which are periodically updated to take account of new death sentences, executions, and exonerations. This Article's citations to the Texas Department of Criminal Justice reflect the statistics on its webpage as of July 17, 2006, and archived copies of those statistics as of that date are on file with the Texas Law Review.

101. *Id.*

102. Tex. Dep't of Criminal Justice, Total Number of Offenders Sentenced to Death from Each County, http://www.tdcj.state.tx.us/stat/countysentenced.htm.

103. Tex. Dep't of Criminal Justice, County of Conviction for Offenders on Death Row, http://www.tdcj.state.tx.us/stat/countyconviction.htm.

Figure 3: Capital and noncapital homicide rate per 100,000 persons and percent capital, Texas, 1976–2003[104]



The patterns in Texas closely resemble the patterns for all the death penalty states shown in Figure 2b. Capital-eligible homicides rise and fall over time, varying from a rate of 2 per 100,000 persons in 1976 to a peak of 4 before declining to a low rate of 1.8 in 1999 and beginning a shallow rise in the next four years through 2003. The rates fell by nearly half, from 4 per 100,000 persons, to less than 2 in 1996. The market share of capital-eligible homicides rises across the entire interval, and nearly doubles from 15% in 1988 to 29% in 2003. Similar to other states, noncapital homicides dropped sharply from 1990 to 1998 and have remained stable since. Since Texas resumed executions in 1982, its execution activity was consistently well above the national average for death penalty states.[105] But executions were extraordinarily high between 1996 and 2003. More than two-thirds of the post-*Gregg* executions took place in those years, with a peak of 40 executions in 2000 and another peak of 33 executions in 2002.[106] During this time, the rate of capital-eligible homicides was virtually unchanged, from 1.8 per 100,000 persons in 1996 to 2.0 in 2003.

One would expect the rate of capital-eligible homicides to decline steadily during years when there is very high execution activity. Assuming that would-be offenders who might be sensitive to execution risk are updating their information frequently, these updates based on high execution risk seem to have had little effect on the commission of capital-eligible murders. Executions in Texas were proceeding at a very high rate during this time, averaging almost three per month during the four-year period from 1997 to

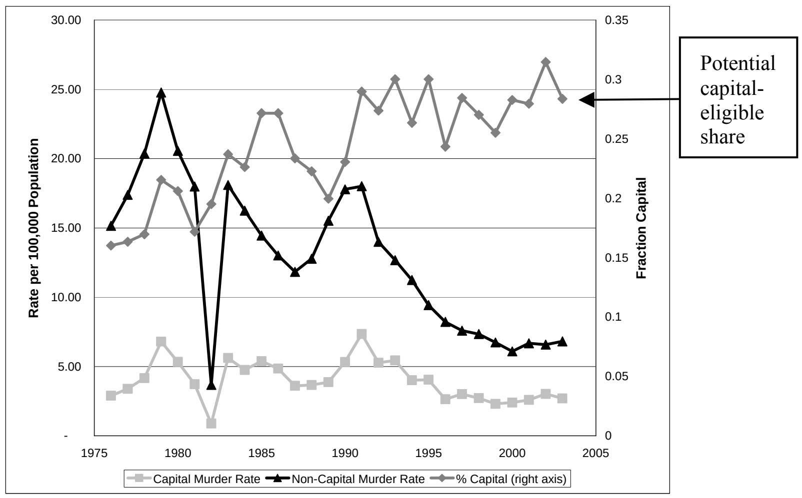
104. *See* SHR, *supra* note 71.

105. *See* Death Penalty Info. Ctr., *supra* note 97 (showing that Texas has executed 369 inmates since 1972, far outstripping second-place Virginia, with 95).

106. Tex. Dep't of Criminal Justice, Executions by Year, http://www.tdcj.state.tx.us/stat/annual.htm.

2000 inclusive.[107] Even allowing for a lag of a year or more, capital-eligible homicide rates in the succeeding years seemed unresponsive to the increase in executions in the late 1990s.[108]

Figure 4: Capital and noncapital homicide rate per 100,000 persons and fraction capital, Harris County, 1976–2003[109]



The second natural experiment is Harris County. A single county case study has strong internal validity, due to the stability over time in legal contexts that surround the decision to seek and apply the death penalty, and the absence of noise from variations in legal contexts and the factors that may drive murder rates over time in other parts of the state and the country.[110] Consistent with statewide trends in Texas and national trends in the death penalty states, the market share of capital-eligible homicides rose in Harris County from the onset of post-*Gregg* executions through 2003. Figure 4 also shows that the temporal fluctuation in the rate of capital-eligible homicides in Harris County is nearly identical to the statewide and national trends.[111] Rates remained stable from 1996 through 2001, the period

107. *Id.*

108. The period when such updates take place is a matter of theoretical speculation. At least one proponent of the deterrent effects of execution has suggested that updates may be as frequent as monthly. *See, e.g.*, Shepherd, *supra* note 15, at 309 (suggesting that "capital punishment's deterrent effect is captured in the monthly data regardless of the particulars of the model").

109. *See* SHR, *supra* note 71.

110. *See, e.g.*, JAMES EISENSTEIN & HERBERT JACOB, FELONY JUSTICE: AN ORGANIZATIONAL ANALYSIS OF CRIMINAL COURTS (1977) (discussing court processes in Baltimore, Chicago, and Detroit and showing how stable working groups of court officers function in courts to establish shared guidelines and rules for the evaluation and disposition of criminal cases).

111. We retained data from 1982, when rates were sharply lower than other years, despite the indication of problems in data compilation and reporting for Harris County in that year.

when execution activity in the state was at its peak. Noncapital homicides declined sharply from 1991 through 2000, in the same periods, capital-eligible homicides fell before rising after 2000. There was little change in capital-eligible homicides in Texas following the surge in executions in the late 1990s, and rates remained stable as executions declined in Texas after 1999.

Together, the Texas and Harris County exercises confirm the trends across death penalty states: the market share of homicides that are capital-eligible continued to rise in the face of higher execution rates.

E. The Opposite of Economics?

The most logical test of "price effect" deterrence, that is whether the threat of death is driving homicide fluctuations in death penalty states, is whether the subset of killings threatened with death decline more sharply than in states where an execution will not happen. As executions go up, the percentage of homicides where a death sentence is possible should go down in the death penalty states, and particularly in Texas, the only state with any apparent deterrence in the aggregate homicide data.[112] But there should be no such fluctuation in nondeath penalty states because there is no death threat for this class of cases.

This distinctive pattern does not happen. The patterns are visible to the naked eye. The fingerprint for execution influence is missing from Harris County, from Texas as a whole, and from all death penalty states. Instead, the market share is *rising* everywhere except the nondeath penalty states. Offenders faced with the threat of execution are not substituting less risky varieties of crime for crimes that lead to murder and capital risk, nor are they abandoning the types of crimes that might lead to a capital offense. But they do seem to be rejecting the types of murders that do not carry execution risk.[113] Evidently, secular trends or risk factors other than executions are animating the aggregate homicide totals in Texas and elsewhere. This is the opposite of recent price effect economic theories of death penalty deterrence.

The insensitivity of capital-eligible homicides to execution trends is especially surprising when considered in the context of the sharply declining rates of other homicides. As these noncapital-eligible homicides decrease in number, it would be logical that police and prosecutors would devote more attention to the smaller number of capital-eligible cases. Greater resources would be available for police investigations and clearance rates should improve. Prosecutors also would have more time and greater resources to devote to these cases, increasing the likelihood of lengthy prison sentences if not capital sentences. Yet even this concentration of criminal justice re-

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112. *See* Berk, *supra* note 19, at 320–24 (finding that all of the generalized deterrent effects in studies are attributable to Texas).

113. Figure 2b shows an overall declining trend of noncapital homicide rates in all death penalty states. Even when only examining Texas, Figure 3 confirms that trend.

sources on capital-eligible cases has not leveraged a decline in the rate of capital-eligible homicides.

The trends argue not against deterrence, but against the marginal deterrent effects of execution threats. Prison sentences and prison populations have been increasing dramatically since 1978,[114] and the largest segment of the prison population is inmates convicted of violent crimes.[115] At year end in 2002, there were 2.1 million persons incarcerated in state prisons, including an estimated 624,900 prisoners for a violent offense.[116] Overall, the 2004 incarceration rate in state prisons was 486 per 100,000 population.[117] The high rate of incarceration and the increasingly lengthy sentences imposed for violent offenses[118] may leave little margin for additional deterrent effects from the threat of execution.

In fact, the marginal punishment cost from the threat of execution may be discounted in the modal category of capital-eligible crimes: felony murders—homicides committed in the course of other crimes, especially robbery. The logic of criminal careers and the composition of the pool of capital-eligible homicides combine to argue against a marginal deterrent effect from the threat of execution. Robbery is not a crime that is committed casually, nor are robbers a random sample of the criminal population. Most have prior arrest records and many have completed spells in prison.[119] Most acknowledge the risk of punishment as intrinsic to their work yet tend to discount the cost of punishment or overvalue present benefits of the robbery, or both.[120] Moreover, the situational dynamics of robbery are volatile and unpredictable, and there is a very weak prospect that a risk heuristic of punishment will enter into the intense street dynamics of robbery interactions

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114. Alfred Blumstein & Allen J. Beck, *Population Growth in U.S. Prisons, 1980–1996*, 26 CRIME & JUST. 17, 18 (1999) ("Beginning in the early 1970s, the incarceration rate began a period of continuous growth of approximately 6.3 percent per year that has continued largely unabated to the present.").

115. Paige M. Harrison & Allen J. Beck, *Prisoners in 2004*, BUREAU JUST. STAT. BULL., Oct. 2005, at 3 tbl.3, *available at* http://www.ojp.usdoj.gov/bjs/pub/pdf/p04.pdf.

116. *Id.* at 9.

117. *Id.* at 3, 4 tbl.4.

118. In 2002, the average state prison sentence for a violent offense was eighty-four months, a rate that excludes life sentences. Matthew R. Durose & Patrick A. Langan, *Felony Sentences in State Courts, 2002*, BUREAU JUST. STAT. BULL., Dec. 2004, at 4, *available at* http://www.ojp.usdoj.gov/bjs/pub/pdf/fssc02.pdf.

119. *See, e.g.*, RICHARD T. WRIGHT & SCOTT H. DECKER, ARMED ROBBERS IN ACTION: STICKUPS AND STREET CULTURE 14 (1997). Wright and Decker interviewed men whose criminal careers included repeated robberies. Robbers were committed to a street culture that emphasized the material rewards and social status attendant to being successful "stick candy men," while minimizing or heavily discounting punishment risk. *Id.* at 16; *see also* KATZ, *supra* note 32, at 165 (stating that robbers who "persist in robbery for several years . . . must anticipate a break in their career for a long term of incarceration"); MERCER L. SULLIVAN, "GETTING PAID": YOUTH CRIME AND WORK IN THE INNER CITY (1989) (examining the lives of three Brooklyn-area youths who had prior arrest records before committing robberies).

120. WRIGHT & DECKER, *supra* note 119, at 118.

to reduce the risk of lethality,[121] especially when a gun is present. The presence of a gun in a robbery further increases not just the risk of lethality but the decision by the robber to use it.[122] In other words, there is a strong risk of cognitive errors in situations of intense arousal—errors that are likely to mitigate the deterrent effects of punishment risk.[123]

Felony murder offenders should be deterred both by the threat of prison *and* the threat of execution. But there seems to be no visible marginal threat from execution because both long prison sentences and execution are punishment costs, not risks. Perhaps present-oriented offenders discount such costs, reducing the salience of the threat of execution, leaving the margin for deterrence very thin.

III. Estimating the Deterrent Effects of Execution on Capital Homicides

A. Design

Next, we use panel methods to estimate a series of regression models to identify the effects of capital punishment on the rate of capital-eligible homicides from the resumption of capital punishment in the United States in 1976 following *Gregg* through 2002. We estimate models both for the nation and for Texas. The panel structure of the data lends itself to a class of statistical models that explicitly examine how time-varying factors—including capital punishment and other social and legal conditions—influence homicide trajectories that vary through time in an autoregressive structure.

Consistent with Sellin's strategy for estimating the effects of capital punishment,[124] we include estimators for states that do not have the death penalty. In the logic of experiments, an effect of execution on the homicide rate should be observed only in the states that have or use the death penalty.

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121. *Id.*; *see also* Jack Katz, *The Motivation of the Persistent Robber*, 14 CRIME & JUST. 277, 283–290 (1991) (arguing that robbers do not engage in rational behavior); Franklin Zimring & James Zuehl, *Victim Injury and Death in Urban Robbery: A Chicago Study*, 15 J. LEGAL STUD. 1, 33 (1986) (arguing that the malice rule, or any variation, would likely have a small effect on robbery behavior).

122. *See* Jeffrey Fagan & Deanna L. Wilkinson, *Social Contexts and Functions of Adolescent Violence*, *in* VIOLENCE IN AMERICAN SCHOOLS: A NEW PERSPECTIVE 55, 62 (D.S. Elliott et al. eds., 1999) ("The availability of a firearm may encourage a robber to . . . rely on a threat of force which may or may not need to be followed through."); Deanna L. Williams & Jeffery Fagan, *The Role of Firearms in Violence "Scripts": The Dynamics of Gun Events Among Adolescent Males*, LAW & CONTEMP. PROBS., Winter 1996, at 55, 71 (noting that "the availability and lethal nature of firearms has resulted in offenders taking on 'risky or harder' targets, anticipating little or no resistance when using a lethal weapon"); Zimring & Zuehl, *supra* note 121, at 14–16 (showing, statistically, that robberies involving guns are more likely to be lethal).

123. *See* Daniel Kahneman & Amos Tversky, *Choices, Values and Frames*, 39 AM. PSYCHOLOGIST 341, 349 (1984) (finding that "an individual's subjective state can be improved by framing negative outcomes as costs rather than as losses").

124. Thorsten Sellin, *Homicides in Retentionist and Abolitionist States*, *in* CAPITAL PUNISHMENT 135, 135 (Thorsten Sellin ed., 1967); *see also* Thorsten Sellin, *Experiments with Abolition*, *in* CAPITAL PUNISHMENT, *supra*, at 122, 122.

In states with the death penalty, the logic of medical experiments suggests that we also investigate how responsive each state is to varying dosages of a "treatment" like capital punishment. Accordingly, we include two measures of capital punishment: the existence of a death penalty statute in each state for each year in the panel and then the number of executions and death sentences in the state for each of the preceding three years.

We adjust the estimates of deterrence to control for variations from state to state in the base rates of homicide by including the noncapital homicide rate in each state for each year (lagged by one year). This strategy allows us to estimate whether the changes in the noncapital homicide rate are influencing the rate of capital homicides. Using population-averaged models, we scale the number of executions to each state's population to ensure that any deterrent effects from execution are weighted proportionately to the state's population.

We include a rich set of socioeconomic and criminal justice system variables that are robust correlates of the murder rate within and between states over time; these correlates and predictors of homicide have been validated extensively in research across cities and states over the past three decades.[125] However, some of these factors also may be spuriously correlated with the adoption of capital punishment and its use,[126] and statistical methods are needed to sort out these multiple and overlapping factors and to better isolate the causal effect of executions above and beyond the endogenous reasons why it is used.

For example, the rate at which prosecutors may seek the death penalty, the rate at which judges and juries might impose it, and the rate at which states may carry out death sentences, all may be correlated with the onset of other criminal justice measures, such as tough sentencing laws or expanded death penalty eligibility, that are designed to "get tough on crime." Estimating the effects of capital punishment is further complicated by contemporaneous increases in the likelihood of incarceration, longer prison sentences including "natural" life sentences (or life without parole) that may compete with the threat of execution to deter homicides. And, these contingencies also may deter other crimes as well.

We are particularly interested in the effects of incarceration rates in assessing whether punishment risks compete with other social and legal

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125. *See* Krivo & Peterson, *supra* note 54, at 558; Land et al., *supra* note 54, at 951; Sampson & Lauritsen, *supra* note 54, at 48.

126. *See* JAMES LIEBMAN ET AL., A BROKEN SYSTEM PART II: WHY THERE IS SO MUCH ERROR IN CAPITAL CASES, AND WHAT CAN BE DONE ABOUT IT 425 (2002), *available at* http://www2.law.columbia.edu/brokensystem2/report.pdf (showing that factors including the poverty rate, the percent of the population that is African American, and indices of each state's punitiveness (or use of incarceration) predict the use of the death penalty and the number and rate of reversals of death sentences).

factors, including capital punishment, and in predicting changes in homicide rates over time.[127] Since robbery–homicide is the paradigm crime among the subset of felony murders that are capital-eligible homicides,[128] we also include an index for the robbery rate to control for the supply of events that might produce capital-eligible homicides. We use alternate analytic methods that consider time trends in different ways, including procedures that account for the strong autocorrelation or stationarity of homicide rates over time, and we develop parameters to address selection biases inherent in the decisions of states to adopt the death penalty.

B. Model Estimation

Several new studies claim strong deterrent effects of capital punishment.[129] They share a common econometric language and preferences for particular analytic strategies. Typically, these studies use panel data on murder rates within states or counties over a number of years. We use that form to begin the analysis. The general analytic form is a regression equation where the murder rate in each state and year in the time series (or panel) is the dependent variable, and the predictors are a linear combination of fixed effects including the presence of a death penalty law in a given state and the predictability of execution given a death sentence in some previous era. Covariates include state effects that account for differences between the states and year effects that account for national time trends that affect the states. The general model form is:

$$Y_{ij} = \beta_{ij-1}\text{DETERRENCE} + \gamma_{ij}\text{CONTROLS} + \delta_{ij}\text{NONCAP} + \mu_i + \eta_j + \varepsilon_{ij}$$

where Y_{ij} is the rate of capital-eligible murders in state i and year j, DETERRENCE is a combination of execution and death sentence measures lagged for different periods, and CONTROLS is a combination of state social and economic characteristics that are well known predictors of both criminal activity and the use of the death penalty. We include each state's robbery rate in each year in this set of covariates, since robberies are a measure both of the general level of criminal activity and also of the potential supply of robbery–homicide incidents that comprise a significant portion of capital-

127. *See, e.g.*, Katz et al., *supra* note 10, at 339–40 (reporting a negative correlation between prison death rates—a proxy for poor prison conditions—and crime rates, but finding little deterrent effect of capital punishment); *see also* Fagan, *supra* note 19 (critiquing recent research on the deterrent effects of the death penalty for inadequately measuring and estimating the effects of incarceration and other criminal justice policy measures on changes in the homicide rate); Levitt, *supra* note 55, at 170–83 (finding four factors that explain the nationwide decrease in crime: the expansion in the number of police, the increasing prison population, the retreating epidemic of crack cocaine, and the legalization of abortion).

128. *See supra* note 86 and accompanying text.

129. *See Hearing*, *supra* note 13, at 10–11, 14–16 (discussing the results of thirteen studies that found deterrent effects); *see also* Donohue & Wolfers, *supra* note 10 (surveying the data from several studies that found deterrent effects); Fagan, *supra* note 19 (same).

eligible crimes.[130] In this study, where the market share of homicides that are capital-eligible is a central question, we also include the rate of noncapital-eligible homicides. (Details on these measures are discussed in the next section.) State and year fixed effects ($\mu_i + \eta_j$) also are included in the estimation.[131] In this study, we estimate first a state-level model and next a model with counties in Texas.

We begin the analysis by estimating this model form to determine how the death penalty influences the rate of capital-eligible homicides, with the presence of a death penalty statute in each state and year as the deterrence measure. Next, we estimate similar models but this time using measures of executions and death sentences as the deterrence variables. For each of these versions of deterrence, we first estimate a model with predictors limited to the rate of noncapital homicides and the covariates. We then re-estimate the models with the deterrence variables included.

In this general model form, using fixed effects for states (or counties) and years treats each area as having inherent unobservable characteristics that are consistent over time and independent of other areas. Likewise, the use of fixed effects treats each year as a separate experimental period, with its own characteristics that are independent of the previous year's. Such models are common in the recent deterrence literature, and we thus begin with a fixed effects model predicting capital homicide rates based on these state and year characteristics and controlling for noncapital homicide rates.

However, the reality of panel data suggests that this method will produce biased estimates: the strategy ignores the fact that murder rates within states vary *through* time, and that murder rates, whether within states or counties, are serially correlated over time.[132] This is the problem of autoregression, or serial correlation: the tendency of trends in longitudinal or time series data to be heavily influenced by the trends in preceding years. Statistically and conceptually, it is unlikely that effects of extremely rare events such as executions can influence trends that are so heavily influenced by their own history.[133]

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130. *See supra* Tables 1 & 2.

131. Most studies estimate models with states as the unit of analysis, while others include models where county murder rates are predicted from a combination of state- and county-level predictors. *See, e.g.*, Hashem Dezhbakhsh et al., *Does Capital Punishment Have a Deterrent Effect? New Evidence from Postmoratorium Panel Data*, 5 AM. L. & ECON. REV. 344 (2003).

132. *See, e.g.*, Marianne Bertrand et al., *How Much Should We Trust Differences-in-Differences Estimates?*, 119 Q.J. ECON. 249 (2004) (highlighting the problem of serial correlation in differences-in-differences estimations of causal relationships); *see also* Alberto Abadie, *Semiparametric Difference-in-Differences Estimators*, 72 REV. ECON. STUD. 1 (2005) (addressing some of the difficulties in the use of the difference-in-differences estimator to exclude variations created by serial correlation).

133. *See* Berk, *supra* note 19, at 311 (finding that "most of the variation in homicides is simply a function of the average number of homicides in each state" and that the number of executions adds "virtually nothing" to the analysis); *see also* BADI H. BALTAGI, ECONOMETRIC ANALYSIS OF

One class of models designed for these circumstances is hierarchical regression models that generate growth curves or trajectories of change over time. These regression models can identify the parameters that shape a pattern or sequence of behaviors over time, also known as a *trajectory* or *growth curve*, and estimate the effects of interventions or treatments that might influence these patterns.[134] The trajectories can be modeled using hierarchical or mixed effects estimation, in which some variables are considered fixed and others random. Variables are analyzed as fixed effects when we assume that they are measured without error, or that they are constant across studies. So, for example, variables such as population, the number of executions or death sentences, or the incarcerated population are fixed effects. In the second set of models, then, variables are analyzed as random effects when we assume they have measurement error, or when we are making inferences or generalizations to some probability distribution.

In this class of mixed effects growth curve models, the independent variables are modeled as fixed effects. Time is modeled both as a fixed effect to control for the effects of specific years in the time series, and a random effect, to estimate the rate of change over time in the dependent variable. Of particular interest in this class of models is the interaction of time with each of the fixed effects. This interaction allows the influence of a fixed effect to vary over time as the fixed effect itself changes. Accordingly, the interactions show whether and how the rate of change in the dependent variable over time is affected by the values of the predictor or independent variable at different points in time.[135]

PANEL DATA 84 (2001); Badi H. Baltagi & Qi Li, *Testing AR(1) Against MA(1) Disturbances in an Error Component Model*, 68 J. ECONOMETRICS 133 (1995); Robert C. Jung & A.R. Tremayne, *Testing for Serial Dependence in Time Series Models of Counts*, 24 J. TIME SERIES ANALYSIS 65 (2003).

134. *See, e.g.*, STEPHEN W. RAUDENBUSH & ANTHONY S. BRYK, HIERARCHICAL LINEAR MODELS 163–202 (2d ed. 2002); JUDITH SINGER & JOHN B. WILLETT, APPLIED LONGITUDINAL DATA ANALYSIS: MODELING CHANGE AND EVENT OCCURRENCE 4 (2003); Sophia Rabe-Hesketh et al., *Maximum Likelihood Estimates of Limited and Discrete Dependent Variable Models with Nested Random Effects*, 128 J. ECONOMETRICS 301 (2005) (conducting simulation studies indicating that adaptive quadrature presents "unbiased estimates for random component probit models").

135. Panel data often are troubled by correlated error terms over time in the relationships between the dependent variables and the predictors. To adjust for this problem, the models are estimated using AR(1) covariance structures. *See generally* SINGER & WILLETT, *supra* note 134.

The general model follows the form:

$$\begin{aligned} Y_{ij} = {} & \gamma_{00} + \gamma_{10}\text{TIME} + \gamma_{01}\text{DETERRENCE} + \gamma_{11}(\text{DETERRENCE* TIME}) + \\ & \gamma_{02}\text{PUNISHMENT} + \gamma_{12}(\text{PUNISHMENT * TIME})] + \\ & \gamma_{03}\text{OTHER\_CRIME} + \gamma_{13}(\text{OTHER\_CRIME * TIME})] + \\ & \gamma_{04}\text{DEMO\_ECON} + \gamma_{14}(\text{DEMO\_ECON * TIME})] + \\ & [\zeta_{i0} + \zeta_{i1}\text{TIME} + \varepsilon_{ij}] \end{aligned}$$

where Y_{ij} is the rate of capital-eligible homicides (per 100,000 population) in state *i* and year *j*, and DETERRENCE is a vector of variables including death sentences and executions.[136] We use the natural log of the capital homicide rate. The deterrence measures include separate contributions of executions lagged one year (year *j-1*), and a three-year moving average of death sentences prior to the current year in state *i* (years *j-1*, *j-2*, and *j-3*).[137] We also test an alternate and simplified model with a binary measure of whether there is a valid death penalty statute in effect in the prior year. PUNISHMENT is an alternate deterrence measure that indexes state prison incarceration to the felony crime rate.[138] The cross-level interactions of each predictor with TIME identify whether the effects of TIME differ by levels of the theoretical predictors—i.e., whether executions, death sentences, or punishment variables are, in fact, associated over time with a decrease in homicide rates. This is the critical test.[139] We use two alternate measures of time, a linear time function and a quadratic time function that reflects the non-linear trends in homicides over time.

The models include fixed effects for two crime patterns in the previous year. First, we control for the natural log of the rate of noncapital-eligible homicides as an index of the general level of lethal violence. Second, we include the natural log of the robbery complaint rate to control for the supply of events that might increase the supply of capital-eligible homicides. Both the state and county models include covariates to control for socioeconomic factors[140] that may influence both crime rates and the preferences of the

136. We also included measures of homicide trends for 1968–1976 in state *i* and year *j* to control for longer term homicide trends within states. However, Supplementary Homicide Report coding in this era did not include information on situations and circumstances to allow for classification of homicides as capital-eligible or noncapital-eligible.

137. We assume that a three-year period for recall of death sentences is a reasonable reflection given the present orientation of criminal offenders, and specifically of homicide offenders. *See* Fagan, *supra* note 19.

138. In state-level models, PUNISHMENT is the lagged natural logarithm of the punishment index. In the county-level models for Texas, this variable represents the state prison population measure in the given year.

139. *See* SINGER & WILLETT, *supra* note 134, at 3 ("Today we know it is possible to measure change, and to do it well, *if you have longitudinal data* . . . .").

140. In the state-level model, these factors include: the percent of the population in poverty, the Gini index of inequality, the percent of the population in the "peak crime age range" of 15–24, the percent of the population aged 65 or over, the logged population size, the percent of the population

criminal justice system for more punitive criminal justice policies, especially incarceration,[141] death sentences,[142] and also the overproduction of death sentences that lead to high rates of reversible errors.[143]

Prior studies using counties to estimate deterrent effects of the death penalty also ignore a second problem common to smaller spatial units: spatial autocorrelation, or spatial lag,[144] where the murder rates in a particular county may also reflect processes that are taking place in the adjacent counties and may create noise in the estimates for a particular county. The Texas county-level models thus also included a measure of spatial autocorrelation of time-lagged murder rates. This strategy controls for general crime trends in neighboring counties, as well as over time.[145]

We include two types of random effects: ζ_{0i}, a random intercept, and ζ_{1i}, a random effect of time. The random intercept reflects the fact that while capital-eligible homicide rates are estimated to vary based on the effects of deterrence variables and other predictors, states and counties also differ on unobservable characteristics which might affect the starting points of each trajectory at the outset of the time series. The random intercept provides the flexibility with which these differences can be modeled. Likewise, a random effect for time generates estimates of the variance components attributable to

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living in urban areas, and the percent of the population that is black. In the county model, the demographic and economic factors include the poverty rate and Gini index, the logged population size, and the percent of the population aged 15–24.

141. *See, e.g.*, David Jacobs & Jason T. Carmichael, *The Politics of Punishment across Time and Space: A Pooled Time-Series Analysis of Imprisonment Rates*, 80 SOC. FORCES 61 (2001) (showing that over a 60 year period, net of poverty and social disorganization, religious fundamentalism, political conservatism, and the rate of violent crimes, Republican strength and minority threat lead to higher imprisonment rates); David Jacobs & Ronald E. Helms, *Toward a Political Model of Incarceration: A Time-Series Examination of Multiple Explanations For Prison Admission Rates*, 102 AM. J. SOC. 323 (1996) (examining different economic and political factors to explain shifts in prison admissions since 1950).

142. David Jacobs & Jason T. Carmichael, *The Political Sociology of the Death Penalty: A Pooled Time-Series Analysis*, 67 AM. SOC. REV. 109, 126–27 (2002) (showing that racial tipping points in the population and economic inequality are significant predictors of the adoption of death statutes in states following *Furman* and *Gregg*).

143. *See, e.g.*, LIEBMAN ET AL., *supra* note 126.

144. Estimating the murder rate in a county without acknowledging what is going on next door may overstate the effect of a causal factor that is unique to that county. In other words, the estimates of deterrence or any other causal factor may simply be picking up the effects of causal factors operating nearby but not necessarily within the county itself. For a general discussion of spatial autocorrelation, see Edward Bullmore et al., *In Praise of Tedious Permutation*, *in* SPATIAL STATISTICS: METHODOLOGICAL ASPECTS AND APPLICATIONS 183, 190 (Marc Moore ed., 2001).

145. Spatial lag measures were available only through 1999, and the models with the spatial lag parameter were artifactually truncated at 1997 due to the inclusion of measures that were lagged as much as three years. The early termination of the time series could produce biased results that would change if later years were included. Since the effects of spatial lag were not significant in this first set of models, we re-estimated the models excluding the spatial lag measures and report those results. This strategy allowed us to include a larger number of years in the panel, including two years following the 1999 spike in executions in Texas.

differences in the state-specific or county-specific slopes of capital-eligible homicide trajectories. All estimates are population-averaged, and an autoregressive (AR[1]) covariance structure is assumed.[146]

While the state models use a linear estimation, the distribution of capital homicides in the county-level rates in Texas requires a different estimation method. The capital homicide rates by county are skewed: 4,763 county-year observations out of 6,240 county-years have no capital-eligible homicides, the range is from zero to 213 capital homicides, and the standard deviation (8.9) is far higher than the mean (1.5). Even with population-averaging, we still observe a nonlinear skewed distribution. In circumstances such as this where the distribution is nonlinear and right-skewed, a Poisson distribution provides a more efficient and accurate method to estimate the mixed effects regression.

Poisson techniques are appropriate to identify factors that predict the number of occurrences of an event within a specific observation period.[147] The Poisson distribution is a discrete distribution which takes on the values y = 0, 1, 2, 3, ... Poisson distributions typically assume that events are inevitable, and that they follow some known distribution or frequency pattern. It is often used as a model for the number of events (such as the number of telephone calls at a business or the number of accidents at an intersection) in a specific time period. It is useful in studies of law and crime to model phenomena such as the number of crimes or the number of prison sentences. The probability distribution for a Poisson process is defined as:

$$\hat{\Pr}(y) = N^{-1}\sum_{i=1}^{N}\hat{\Pr}(y|\hat{\mu}[X_i]).$$

The exact distribution depends on the expected rate of occurrence of the event of interest (y), and X is a vector of explanatory variables over time periods *i*. When y is low, the distribution is skewed to the left. When y is high, the distribution more closely resembles a normal distribution.

The estimations for Texas, then, follow the same analytic plan, with county-year fixed effects estimations first followed by trajectory models using mixed effects regressions to address time trends and autoregression in homicide rates. The Texas models differ in that we use the Poisson distribution to model the count of capital-eligible homicides instead of the linear

146. *See* Baltagi & Li, *supra* note 133, at 139–43 (comparing MA(1) and AR(1) in an error component model).

147. *See, e.g.*, William Gardner et al., *Regression Analyses of Counts and Rates: Poisson, Overdispersed Poisson, and Negative Binomial Models*, 118 PSYCHOL. BULL. 392, 396 (1995) (explaining why Poisson regression is a more reasonable model for count data than a normal-errors linear regression model); Kenneth C. Land et al., *A Comparison of Poisson, Negative Binomial, and Semiparametric Mixed Poisson Regression Models with Empirical Applications to Criminal Careers Data*, 24 SOC. METHODS & RES. 387 (1996).

form that estimates the rates.[148] We use an overdispersion correction[149] to adjust the standard errors for the large number of zeros observations, and use the log of the county population as the exposure measure.

C. Data and Measures[150]

Specific variables and their data sources are described in Appendix B. Appendix C reports means and standard deviations for all variables. Homicide data were obtained from the *Supplementary Homicide Reports*, part of the FBI Uniform Crime Reports (UCR) archives.[151] The UCR is a voluntary reporting system; data are compiled from police-agency reports submitted annually.[152] Data on specific homicide events from 1976–2003 were obtained from the *Supplementary Homicide Reports* (SHR), which includes incident-level data on offense, offenders, and victims.[153] We used the situation and circumstance information in each record to categorize homicides as capital-eligible or noncapital-eligible murders. The classification method was described earlier in Part II, and was vetted against two databases of actual executions.[154] While the SHR has varying patterns of missing data patterns that could produce nonclassical measurement error,[155] the market-

148. We considered using a Zero-Inflated Poisson (ZIP) model, an alternate form of poisson regressions that are often used when there are excessive numbers of zero values. *See generally* Diane Lambert, *Zero-Inflated Poisson Regression, with an Application to Defects in Manufacturing*, 34 TECHNOMETRICS 1 (1992); Christopher J.W. Zorn, *An Analytic and Empirical Examination of Zero-Inflated and Hurdle Poisson Specifications*, 26 SOC. METHODS & RES. 368 (1998) (comparing the zero-inflated Poisson model to the hurdle event count model). However, ZIP models may produce uninterpretable standard errors when the data are nested, as is the case here. *See* Quang H. Vuong, *Likelihood Ratio Tests for Model Selection and Non-nested Hypotheses*, 57 ECONOMETRICA 307 (1989).

149. *See* Gardner et al., *supra* note 147, at 397–99. Overdispersion occurs when the observed variance of the data is larger than the predicted variance. *Id.* at 396. A parameter, called the dispersion parameter, ϕ, is introduced to the model to lower this overdispersion effect, and is estimated as:

$$\hat{\phi} = (N-J)^{-1} \sum_{i=1}^{N} \frac{(y_i - \hat{\mu}[X_i, d_i])^2}{\hat{\mu}(X_i, d_i)}.$$

Id. at 397.

150. Data and statistical code for this study are from the authors at http://www2.law.columbia.edu/fagan/researchdata/caphom/.

151. SHR, *supra* note 71.

152. MALTZ, *supra* note 23, at 1.

153. SHR, *supra* note 71. Additional SHR data were obtained for the period 1968–1976, but these records did not include the types of detailed event information that would permit classification of each homicide as capital-eligible or noncapital-eligible. Marc Riedel & Margaret Zahn, Trends in American Homicide, 1968–1978: Victim-Level Supplementary Homicide Reports (ICPSR Study No. 8676, 1994), *available at* http://webapp.icpsr.umich.edu/cocoon/ICPSR-STUDY/08676.xml.

154. *See supra* subpart II(B).

155. The participation of agencies within states in the UCR reporting system is not inconsistent. As a result, it is difficult to tell whether failure to report any homicides in any month

share and disaggregation indicia are unlikely to be affected by random patterns of counties within states failing to report their data.[156]

Data for the deterrence measures were obtained from records of state trends in death sentences and executions maintained by the Death Penalty Information Center (DPIC)[157] and the Bureau of Justice Statistics of the U.S. Department of Justice.[158] The DPIC database includes all death sentences and executions from 1976 to 2003; no sampling was used or needed. The deterrence vector includes measures of executions lagged one and two years, and death sentences lagged one, two, and three years. We assume that rationality among would-be murderers is limited by their present orientation in estimating risk;[159] accordingly, we limit the recall periods in which they identify events that might influence their subjective perceptions of execution

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means that the agency did not comply or that there were no murders that year to report. *See* MALTZ, *supra* note 23, at 5; *see also* Fagan, *supra* note 19. In both the state and county analyses, the annual counts of homicides in the SHR were compared to homicide reports in the UCR. Observations in which the SHR underestimated the UCR by more than 25% were designated as outliers and dropped from the analysis. This results in the elimination of 185 of 1,300 observations in the state analysis and 105 of 6,076 in the Texas analysis. In the Texas analysis, where 1985 data were missing from the county UCR files and there was no basis for comparison with SHR files, a chi-squared test found no significant differences in the distribution of 1985 SHR county homicides compared to 1983–1987 counts. Accordingly, the 1985 SHR observations were retained in the analysis.

156. The stable and nearly flat distribution of the capital-eligible homicide rates suggest that the addition of missing values within states would be unlikely to alter the observed rates of capital-eligible homicides. *See, e.g.*, Stevenson & Wolfers, *supra* note 77, at 275 n.15.

157. DPIC, Searchable Database, *supra* note 88.

158. *See* Thomas P. Bonczar & Tracy L. Snell, *Capital Punishment, 2002*, BUREAU JUST. STAT. BULL., Nov. 2003, at 1–17, *available at* http://www.ojp.usdoj.gov/bjs/pub/pdf/cp02.pdf. The data are publicly available at the National Archive of Criminal Justice Data, Institute for Social Research, University of Michigan. *See* Bureau of Justice Statistics, U.S. Dep't of Justice, Capital Punishment in the United States, 1973–2002 (ICPSR Study No. 3958, 2004), *available at* http://webapp.icpsr.umich.edu/cocoon/ICPSR-STUDY/03958.xml.

159. *See, e.g.*, Charles Dean et al., *Criminal Propensities, Discrete Groups of Offenders, and Persistence in Crime*, 34 CRIMINOLOGY 547 (1996) (discussing various factors that effect a criminal's low self-control in assessing present orientation, such as neuropsychological deficit, upbringing, moral beliefs, and geographical location); Sarah Lichtenstein et al., *Judged Frequency of Lethal Events*, 4 J. EXPERIMENTAL PSYCHOL.: HUM. LEARNING & MEMORY 551, 574–77 (1978) (finding biases in the estimation of the frequency of lethal events due to "overestimation of [events with] low frequencies and underestimation of . . . [events with] high frequencies"); Yair Listokin, *Future-Oriented Gang Members? Gang Finances and the Theory of Present-Oriented Criminals*, 64 AM. J. ECON. & SOC. 1073 (2005) (noting that many individual crime-propensity theories stem from the notion that "[a]s long as the gains from crime are immediate while the costs of crime are delayed, present-oriented individuals will commit crimes"); Daniel S. Nagin & Greg Pogarsky, *Integrating Celerity, Impulsivity, and Extralegal Sanction Threats Into a Model of General Deterrence: Theory and Evidence*, 39 CRIMINOLOGY 865 (2001) (formulating a metric that utilizes discounting to assess the effect of the celerity, severity, and certainty of punishment on a criminal individual's decision-making process). For a review on risk perception and deterrence, see Paul H. Robinson & John M. Darley, *Does Criminal Law Deter? A Behavioral Scientist Investigation*, 24 OXFORD J. LEGAL STUD. 173 (2004), which reviews evidence from criminology and other behavioral sciences and concludes that the deterrent effects of the criminal law are quite limited.

risk.[160] However, the lagged death sentence variables are highly correlated ($r = .891$), so we use instead a moving average of death sentences in the prior three years. This variable is highly skewed, so we use the natural log of the moving average.

Data on robbery complaints were obtained from the UCR archives that are maintained and published by the U.S. Department of Justice.[161] County-level data for Texas were obtained from the same sources.[162]

The covariates include measures that are correlated with both murder and also with the use of the death penalty. Following the measurement strategy of Professor David Jacobs and Jason Carmichael,[163] and a similar strategy used by Professor James Liebman and his colleagues,[164] we include measures of the percentage of the population that is African American and the percentage of the population with incomes below the poverty line. To further identify inequality, we use a Gini coefficient to measure inequality within each state and year.[165] Other demographic controls include measures of the percentage of the population located in urban areas, since murder rates are higher in cities and other areas with higher population density.[166] We also include a measure of the percentage of the population that is between 15 and 24 years of age, because homicide rates were most volatile for this age group, especially during cyclical spikes in homicide rates.[167] We use a measure of the ratio of persons aged 35 and older to persons under 15 as an index of supervision or social control.[168] To control for the tendency of states to incarcerate noncapital homicide offenders, thereby deterring some murderers

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160. *See, e.g.*, Chris Guthrie, *Prospect Theory, Risk Preference, and the Law*, 97 NW. U. L. REV. 1115, 1116–19 (2003) (explaining four different mechanisms by which people make risky decisions).

161. *See* SHR, *supra* note 71.

162. County-level crime reports were missing from the UCR in 1985. Since our models use a one-year lagged measure of robbery reports as a predictor of capital homicide, the missing UCR data caused all 1986 data to be dropped from the county analysis.

163. Jacobs & Carmichael, *supra* note 142, at 117.

164. LIEBMAN ET AL., *supra* note 126, at 136–37, 144.

165. The Gini coefficient is a measure of inequality of a distribution. It ranges between 0 and 1, where 0 corresponds to perfect equality (e.g. everyone has the same income) and 1 corresponds to perfect inequality (e.g. one person has all the income, and everyone else has zero income). The Gini coefficient (G) is computed as:

$G=1-\Sigma f_i(p_i+p_{i-1})$

where:

f_i is the proportion of households in interval i

p_i is the proportion of total income received by recipients in interval i and all lower intervals.

See, e.g., PAUL RYSCAVAGE, INCOME INEQUALITY IN AMERICA 38 (1999); Philip M. Dixon et al., *Bootstrapping the Gini Coefficient of Inequality*, 68 ECOLOGY 1548 (1987).

166. Sampson & Lauritsen, *supra* note 54, at 54–55, 67–69.

167. *See* Philip J. Cook & John H. Laub, *The Unprecedented Epidemic of Youth Violence*, 24 CRIME & JUST. 27 (1998) (discussing the role of youth in violent crimes, particularly homicides).

168. Sampson & Lauritsen, *supra* note 54, at 58.

from further acts of lethal violence, we include an interaction of the punishment index with the noncapital homicide rate. Finally, a dummy variable is included to control for the effects of the mass killing of 168 persons in the Oklahoma City federal building bombing in 1995.

To estimate the punitive orientation of state criminal justice policies we include measures of punishment risk—defined as the number of state prison inmates per felony crime. We lag this measure by one year and use its natural log due to skew across the states.

Some studies use instrumental variables to resolve potential endogeneity in these relationships. For example, Hashem Dezhbakhsh and his colleagues use indicators of partisan political influence and police and judicial expenditures to sort out the relationship between population characteristics and crime rates, criminal justice policies, and use of the death penalty.[169] However, these instruments are correlated not only with the existence of the death penalty in a state,[170] but also with death sentences and executions,[171] incarceration rates,[172] and because these indicia are more salient in death penalty states where murder rates are higher,[173] with homicide rates. The potential for biases are not insignificant and require attention.[174]

The strategy for this analysis—random intercepts for capital homicide rates, fixed effects for states, lagged deterrence measures, and an autoregressive covariance structure—addresses a portion of the potential bias. To further identify the selection biases in state preferences for the death penalty and its endogeneity with predictors of murder, we estimate selection effects by the presence of a death statute. This parameter captures differences between death penalty and nondeath penalty states, controlling for factors correlated both with homicide and with the presence of a death statute. To derive it, we estimate a logistic model using the overall murder rate, incarceration per felony crime, and the set of socioeconomic variables discussed earlier to predict the presence of a death penalty statute in each state-year.[175] The selection model assigns a predicted "statute level" as each

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169. Hashem Dezhbakhsh et al., *supra* note 131, at 356–59. Partisan political influence was measured as the Republican presidential candidate's percentage of the vote in the most recent presidential election. *Id.* at 357.

170. *See generally* Jacobs & Carmichael, *supra* note 142.

171. *Id.* at 121 tbl.2, 122 tbl.3; *see also* John Blume et al., *Explaining Death Row's Population and Racial Composition*, 1 J. EMPIRICAL LEGAL STUD. 165, 168 (2004).

172. David Jacobs & Ronald Helms, *Toward a Political Sociology of Punishment, Politics and Changes in the Incarcerated Population*, 30 SOC. SCI. RES. 171, 182 (2001) (showing that each additional year of a Republican presidency increased the acceleration in the number of prisoners, in state prisons).

173. Jacobs & Carmichael, *supra* note 142, at 119 tbl.1.

174. *See, e.g.*, Richard A. Berk, *Knowing When to Fold 'Em: An Essay on Evaluating the Impact of* CEASEFIRE, COMPSTAT, *and* EXILE, 4 CRIMINOLOGY & PUB. POL'Y 451 (2005).

175. The variables used to predict "statute" are the overall murder rate, robbery rate, punishment index, percent aged 15–24, percent black, percent urban, poverty rate, and Gini

state's propensity to have a death penalty statute in each year. We use this parameter to estimate the effects of the presence of a death statute on capital-eligible homicides, in effect controlling for a state's decision to impose it. Next, we use this parameter in conjunction with the other deterrence variables to estimate the effects of the application of the death penalty.

IV. Results

A. State Analyses

Tables 3 and 4 tell similar stories about the effects of death statutes, death sentences, and executions on capital-eligible homicides. With different specifications and different functional forms in each table, there appears to be no evidence of deterrent effects of any component of capital punishment on the rates of capital-eligible homicides.

Table 3 examines the effects of the presence of a death penalty statute, apart from its implementation, in each state-year. Model 1 in Table 3 is a baseline model with only the punishment index and the rate of noncapital homicides (i.e., noncapital-eligible homicides) as predictors, with state and year fixed effects and a rich set of covariates relevant to state murder rates. In this functional form, there are no interactions with time; the coefficients instead show the average effect across states and years, controlling for time-varying conditions within the states. As expected, noncapital homicides exert a strong positive effect on the rate of capital-eligible homicides. Incarceration, as an alternate source of deterrence, is not a statistically significant predictor of capital homicides.

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coefficient—all measured contemporaneously in the outcome year. The logistic model was estimated with fixed year effects reflecting nationwide punishment trends over our time horizon.

Table 3: Regressions of felony homicide rate by death penalty statute, 1978–2002[176]

	Fixed effects		Trajectory		
Predictor	*Model 1*	*Model 2*	*Model 3*	*Model 4*	*Model 5*
Noncapital homicide rate	$.260^{***}$	$.281^{***}$	$.376^{***}$	$.371^{***}$	$.377^{***}$
	(.035)	(.037)	(.045)	(.051)	(.038)
Statute		-.041		-.097	-.056
		(.076)		(.088)	(.060)
Punishment index (lagged, logged)	.001	-.005	$-.243^{***}$	-.225	$-.201^{***}$
	(.026)	(.027)	(.038)	(.043)	(.029)
Interactions with Time					
Noncapital homicide rate			$-.012^{***}$	$-.011^{***}$	-.001
			(.004)	(.004)	(.0002)
Statute				.012	.0003
				(.009)	(.0005)
Punishment index (lagged, logged)			$.010^{***}$	$.009^{**}$	.0004
			(.003)	(.003)	(.0002)
State fixed effects	⊠	⊠	☐	☐	☐
Year fixed effects	⊠	⊠	⊠	⊠	⊠
Covariates	⊠	⊠	⊠	⊠	⊠
Random intercepts	☐	☐	⊠	⊠	⊠
Time*Time	☐	☐	☐	☐	⊠
BIC	14519	13904	-764.6	-710.9	-527.7
N	1,017	973	1,017	973	973

Significance: *** = $p < .001$, ** = $p < .01$, * = $p < .05$.

The presence of a death penalty statute is assessed in Model 2, and the effect is not statistically significant. Recall that we use a "predicted statute level" in lieu of the presence of the statute itself.[177] This indicator reflects the propensity of states to have a death statute, based on differences between states in factors that are correlated with the presence of a death statute: the homicide and robbery rates, population composition, and inequality. In

176. In Models 3–5, predictors for each year were nested within states, thereby controlling for each state's unique effects over time.

177. *See supra* note 175 and accompanying text.

effect, it is an indicator of whether a state has the death penalty in a given year, controlling for its propensity to enact it.

The results in Model 2 show that presence of a death statute does not predict differences in state-years in the rate of capital-eligible homicides. The parameter estimate is small, not statistically significant, and induces no changes in the other estimates. Its only effect is to slightly reduce the standard errors of the other parameters in the model. Noncapital homicide rates remain the strongest predictor of capital homicides. Comparing the fit estimates (BIC) in Models 1 and 2, Model 2 improves the fit slightly compared to Model 1 without the statute effect.

In Model 3, we introduce the effects of time, and estimate the changes in the effects of each predictor over time. This model analyzes capital-eligible homicide rates as a trajectory in each state and identifies the effects of the predictors in explaining the differences in trajectories in states with and without the death penalty. Again, we use year fixed effects to account for national trends in homicides. In lieu of state fixed effects, however, here we use random intercepts to account for different starting points in each state and random effects for time that effectively nests each time trend within a specific state trajectory. Predictors are nested within states in this model form, creating a specification that addresses each state's unique effects beyond what the covariates can capture. We also include the same set of covariates as in Models 1 and 2. Readers unfamiliar with these estimation techniques should read the upper portion of Models 3–5 in the same way as the state-year fixed effects results in Models 1 and 2: these are the average effects over time, but before considering the effects of trajectories *through* time.[178] To identify whether the over-time trajectories of capital-eligible homicide rates differ in death and nondeath penalty states and to identify whether statutes explain the differences, readers should focus on the lower portion of the table: the interactions of each predictor with time show the effects of each predictor on the rate of capital homicides over time. In this model form, changes in the rate of change or trajectory of a dependent variable are estimated from the interaction of time with each independent variable.[179]

Similar to Model 1, Model 3 shows baseline estimates without the deterrence predictor. The lower portion of Model 3 shows that noncapital homicide rates are significant negative predictors of the capital homicide rate. Punishment is not significant through time. The effect for noncapital homicides on capital homicides illustrates the market share phenomenon: as

178. The coefficients for noncapital homicides are greater in the trajectory models in the upper portion of the table than in the fixed effects analysis in Models 1 and 2, in part because of the explicit treatment of time in the covariance matrix.

179. SINGER & WILLETT, *supra* note 134, at 4.

the rate of noncapital homicides declines, it takes an increasingly large share of the homicide "market."

Model 4 shows that the effects of death penalty statutes are not significant, but even with this putative deterrent effect included in the estimate, the market share of capital homicides still increases over time as noncapital rates fall. The size and significance of the coefficient for noncapital homicide rates are unchanged. Again, there is a modest improvement in model fit. Punishment again is significant.

Model 5 repeats Model 4, adding a quadratic time trend to the linear time trend. This nonlinear time trend generates more restrictive tests of trajectories that include even small, temporary nonlinear trends within longer time trends that appear invariant. The results are virtually unchanged from Model 4. Again, the significant negative coefficient for the noncapital homicide rate suggests that the market share of homicides that are capital-eligible increases over time, even in the presence of a death penalty statute.

Next, we turn to the effects of the components of deterrence that are specific to the death penalty: executions and homicides. In Models 2, 4, and 5 in Table 4, we again include the predicted statute measure, an indicator of each state's propensity to have the death penalty. We add measures of the specific components of deterrence as indicators of the "dosage" of capital punishment in each state. Obviously, these values are set to zero for non-death states. By including the predicted statute indicator together with the deterrence components, we include a measure of the deterrent threat from having the death penalty "on the books" even in states where it is rarely or never used.

Table 4: Regressions of felony homicide rate by deterrence and punishment, all states, 1978–2002[180]

	Fixed effects		Trajectory		
Predictor	*Model 1*	*Model 2*	*Model 3*	*Model 4*	*Model 5*
Noncapital homicide rate	.260***	.290***	.376***	.356***	.377***
	(.035)	(.039)	(.045)	(.050)	(.039)
Statute		-.040		.048	-.093
		(.077)		(.083)	(.063)
Executions (lag 1)		.003***		-.002	-.001
		(.001)		(.009)	(.006)
Executions (lag 2)		-.001		-.008	-.012
		(.002)		(.010)	(.006)
Death sentence (3 yr moving average)		-.001		-.018	.007
		(.001)		(.010)	(.008)
Punishment index (lagged, logged)	.002	-.011	-.242***	-.289***	-.193***
	(.026)	(.028)	(.038)	(.042)	(.031)
Interactions with time					
Noncapital homicide rate			-.012***	-.008***	-.0006***
			(.004)	(.004)	(.0002)
Statute				.002	.0004
				(.008)	(.0004)
Executions (lag 1)				.0002	.000
				(.0005)	(.000)
Executions (lag 2)				.0003	.000
				(.0005)	(.000)
Death sentence (3 yr moving average)				-.001	.000
				(.001)	(.000)
Punishment index (lagged, logged)			.010***	.012***	.0004***
			(.003)	(.003)	(.0002)
State fixed effects	☒	☒	☐	☐	☐
Year fixed effects	☒	☒	☒	☒	☒
Covariates	☒	☒	☒	☒	☒
Random intercepts	☐	☐	☒	☒	☒
Time*Time	☐	☐	☐	☐	☒
BIC	14520	13933	-764.6	-670.6	-454.2
N	1,017	973	1,017	973	973

Significance: *** = p < .001, ** = p < .01, * = p < .05.

180. In Models 3–5, predictors for each year were nested within states, thereby controlling for each state's unique effects over time.

The first two models in Table 4 show results of fixed effects regressions, and the last three models show results of trajectory analyses. Model 1 repeats Model 1 from Table 3—with state and year fixed effects, a rich set of covariates, and a fixed effect for incarceration risks (punishment).

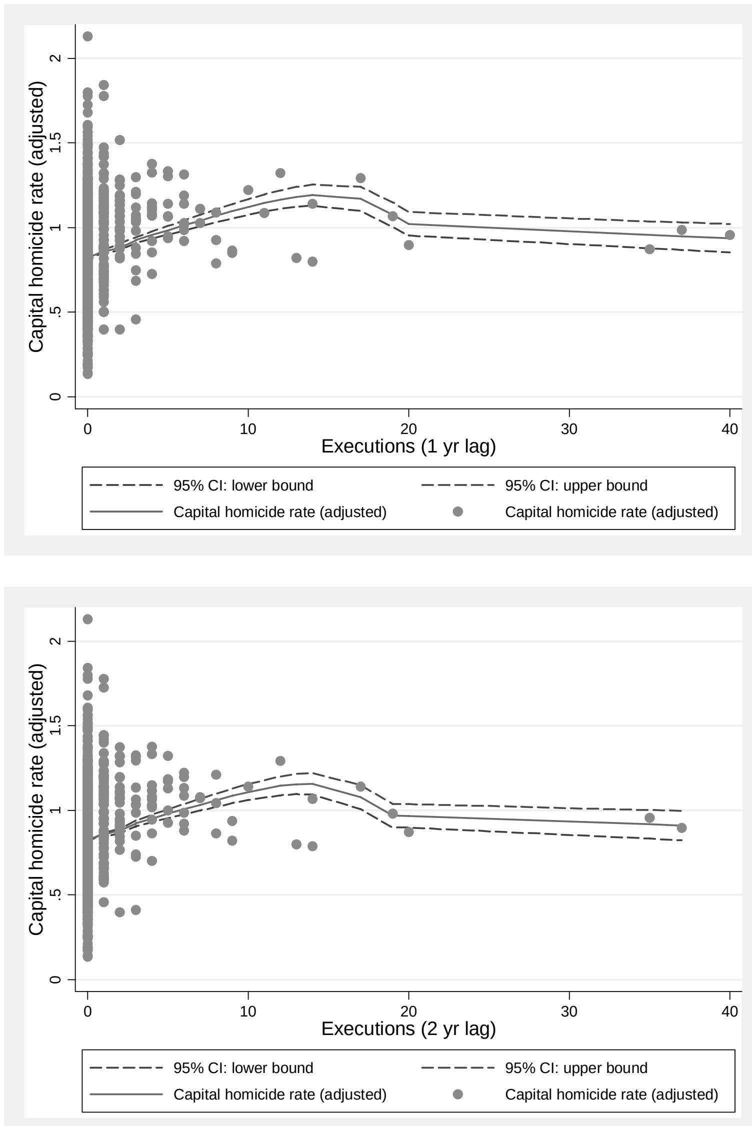
In Model 2, we add three components of deterrence: executions in the state lagged one and two years, and the log of the three-year moving average of death sentences in the state. We use moving averages because of the strong collinearity among the three separate lagged death sentence counts for the three prior years. The results are virtually unchanged from Model 2 in Table 3: there are no deterrent effects from any of the components of deterrence. The coefficients for the deterrence components are small and not statistically significant. The rate of noncapital homicides remains the strongest predictor, averaged over time, of the capital homicide rate.

The trajectory analyses in Models 3–5 lead to the same conclusions as the fixed effects analyses. Model 3 is a baseline trajectory model with only the punishment index and the noncapital homicide rate included as predictors. This model repeats Model 3 from Table 3, illustrating the strong influence of noncapital homicide rates on capital homicide rates averaged across time and the increasing "market share" of homicides that are capital homicides. Model 4 introduces the deterrence components, with time as a linear function. Model 5 again specifies time as a quadratic term.

The results replicate the pattern for the trajectory analyses shown in Table 3. We see once again the absence of deterrent effects from either the presence of a death statute or any of the components of deterrence.[181] The parameter estimates in the lower portion of Table 4 for each of the components of the death penalty are low, and they are not statistically significant. Similar to the results in Table 3, the significant negative estimate for noncapital homicides suggests that "market share" of homicides that are capital-eligible is growing even as the noncapital rate falls.

181. There is one weak positive effect in Model 5, but in the overall pattern of nonsignificance, this may be a chance result lacking validation in other specifications.

Figure 5: Capital-eligible homicide rates, adjusted for deterrence components, noncapital homicide rates, punishment risk and covariates, by executions lagged 1 and 2 years



We illustrate the results for Model 4 in Table 4 graphically in Figure 5. The graph shows a Lowess-smoothed function of the relationship between capital-eligible homicide rates and executions lagged one and two years, along with upper and lower bounds of the 95% confidence interval. The upper portion of Figure 5 shows the relationship of executions per state-year lagged by one year to the rate of capital-eligible homicides in death penalty states only, adjusted for the effects of the death penalty components, incarceration risks, and the socioeconomic and criminal justice covariates that were included in each of the regression models in Table 4. The lower portion of Figure 5 shows the relationship of the same adjusted rates of capital-eligible homicides to executions lagged by two years. The figures also include upper and lower 95% confidence intervals.

Both figures show the clustering of observations near zero for executions, a reflection of the scarcity of executions. The overall flatness of the curves—even at the extremes of execution frequency—is striking. Any variation from year to year occurs by chance, and the overall picture is one of no effect of the components of the death penalty on capital homicide rates. As executions increase to approximately 18 in any year, the homicide rate rises slightly. (Such high levels are exclusive to Texas.) The homicide rate declines slightly for the next two observations before flattening out for the remainder, which are widely spaced. In both figures, the lines are flat, and the results in Table 4 suggest that any deviation from a zero slope is simply chance within the confidence intervals.

Robustness checks are embedded in Tables 3 and 4, and include estimations with alternate functional forms and with alternate measures of the death penalty. First and most important, we estimated models using conventional fixed effects regression methods for panel data, with state and year factors modeled as fixed effects in a pooled, cross-sectional analysis. The alternative, a trajectory or growth curve analysis, estimates changes in slopes through time between groups and identifies factors that contribute to slope differences after controlling for random intercepts that represent different starting points for each state observation.[182] Each set of models was estimated with an alternate explanation for deterrence through incarceration risk. The models include a rich set of covariates that assesses the effects of factors that also may influence the homicide rate. Second, the results were stable using two operational definitions of the death penalty and two conceptualizations of deterrence. The convergence of results in Tables 3 and 4, with models that use different functional forms and specifications, provides strong evidence of the robustness of the empirical estimates.

We also estimated the same sets of models using only the rate of felony murders (logged) as the dependent variable. Felony murder is the most prevalent form of capital-eligible homicide, accounting for more than half the capital-eligible homicides nationwide and also in Texas. The results were unchanged: there were no significant effects for the deterrence variables.[183] Once again, the noncapital homicide rate is the strongest predictor of felony

182. *See supra* note 134 and accompanying text.

183. Data and tables are available from the authors at http://www2.law.columbia.edu/fagan/researchdata/caphom/.

murders, showing the growing market share of homicides that are capital-eligible.

While these internal comparisons are important tests, the apparent inelasticity of capital homicides over time and across states suggests that there is simply not all that much variance to explain in these multivariate models. Certainly, the overall homicide rate varies extensively over time and between states, as well as between death penalty states. This is the basis of nearly all deterrence tests over the past decade.[184] But the rate of capital-eligible homicides varies little over time or between states. And, the scarcity of executions in most states except Texas, also leaves little variance left to be explained by the "right hand side" of these equations. So, while robustness tests are critical in the face of difficult empirical estimations, the task here is simpler and less vulnerable to misspecification.

B. *Capital Homicide and Deterrence in Texas*

There are three differences in the analysis for Texas compared to the state-analyses. First, the models are estimated with a Poisson distribution of the count of capital-eligible homicides, with the counts scaled to the population of each of the 254 counties in the state. The sparseness of capital homicides in most counties in most years required this approach. The result was a high incidence of rates of zero or very low rates in most counties. The simple fact of low rates and near invariance over time complicated the regression analyses that were based on linear models that assumed normal distributions. Even log transformations, which would impose a less skewed structure on the data, did not create conditions amenable to the same type of analysis that we used for the state models. Accordingly, we use overdispersed Poisson regression models with fixed effects for the deterrence measures and random intercepts and random slopes to more efficiently estimate trends of murders through time.

Second, only statewide measures of death sentences and punishment risks (state prison populations) were available. County-level information was not available for the entire time period of interest in this analysis. We include the state-level predictor; however, as a statewide constant, it varies by year but not by county within years.[185] Execution data were available by

184. *See supra* notes 9–16, 129 and accompanying text.

185. None of the options to address this limitation were acceptable. All solutions required an estimation method to allocate inmates to counties as a function of population and crime rates. However, the empirical literature on criminal sentencing suggests that there are unobservable factors in states and local courts that shape sentencing practices and produce disparities by crime type, race, and other population characteristics. Any allocation formula would be unable to measure, much less identify statistically, the sources of these disparities, many of which lie in local politics and local legal cultures. *See, e.g.*, EISENSTEIN & JACOB, *supra* note 110; MARTHA A. MYERS & SUSETTE M. TALARICO, THE SOCIAL CONTEXTS OF CRIMINAL SENTENCING 1 (1987)

county and year. Since the analysis focuses on a single state with the death penalty, we include estimates only of the deterrent effects of the components of capital punishment. Texas had its first death sentence in 1977 and its first execution in 1982. Because of limitations in county-level data in Texas, the study period is 1978–2001.[186] Third, Models 3-5 include a dummy variable for Harris County, to capture the unique effects of the concentration of Texas executions in, that county.[187]

The models in Table 5 show no evidence of deterrent effects of capital punishment on the incidence of capital-eligible homicides in Texas counties. Neither death sentences nor executions are significant in any of the three models that test these effects in conjunction with other county- or state-level factors. The signs for the parameter estimates of executions at times are positive and other times are negative, a sign of instability in the estimates given the tight temporal spacing of the time lags. The parameter estimates for the statewide death sentence rates also are small and not statistically significant.

(exploring the "linkages between the social order and criminal sentencing" by "[f]ocusing on the county, court, and temporal contexts").

186. Models were estimated with and without Moran's I statistic for spatial autocorrelation in total homicide rate with adjacent Texas counties. The results with and without the spatial measures were identical, and the measure of spatial lag was not significant in any of the models. Texas counties are large areas, and it is not surprising that the parameters for spatial lag were not statistically significant. Accordingly, we report here the results without the spatial measures.

187. The dummy variable is included only in Tables 3–5. In the fixed effects regressions in Models 1 and 2, Harris County effects are captured as part of the fixed effects estimation.

Table 5: Poisson regressions of capital-eligible homicide rate by deterrence and punishment, Texas, 1978–2001[188]

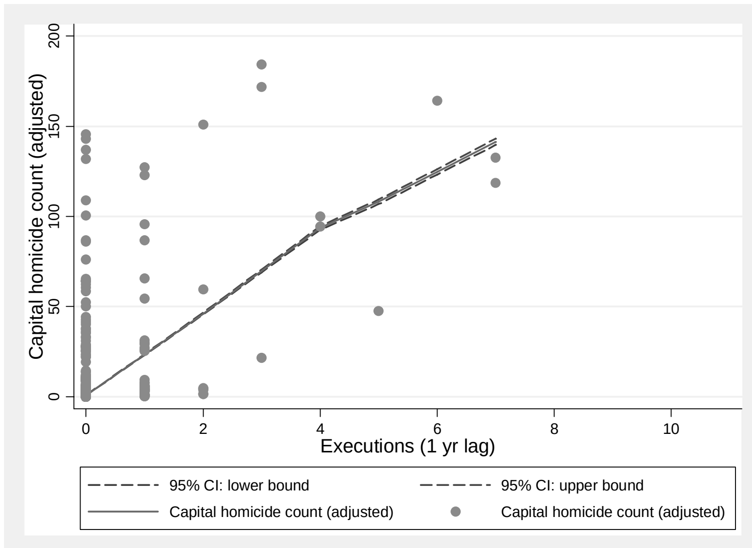
	Fixed effects		Trajectory		
Predictor	*Model 1*	*Model 2*	*Model 3*	*Model 4*	*Model 5*
Noncapital homicide rate	.0007* (.0003)	.001** (.0004)	.002*** (.0004)	.002*** (.0005)	.002*** (.0004)
Executions (lag 1)		.013 (.009)		.054 (.051)	.044 (.033)
Executions (lag 2)		.002 (.009)		-.013 (.053)	-.005 (.032)
Death sentence (3 yr moving average)		-.015 (.024)		-.016 (.011)	-.018 (.007)
Punishment index (lagged, logged)	-.099 (.112)	-.021 (.186)	.149 (.077)	.049 (.086)	-.0002 (.049)
Interactions with time					
Noncapital homicide rate			.000 (.000)	.000 (.000)	.000 (.000)
Executions (lag 1)				-.002 (.003)	.000 (.000)
Executions (lag 2)				.001 (.003)	.000 (.000)
Death sentence (3 yr moving average)				-.0003 (.001)	.000 (.000)
Punishment index (lagged, logged)			-.014** (.005)	-.006 (.006)	-.0002 (.0002)
County fixed effects	☒	☒	☒	☒	☒
Year fixed effects	☒	☒	☒	☒	☒
Covariates	☒	☒	☒	☒	☒
Random intercepts	☐	☐	☒	☒	☒
Time*Time	☐	☐	☐	☐	☒
BIC	10929	10951	26878	26995	27050
N	5,458	5,456	5,458	5,456	5,456

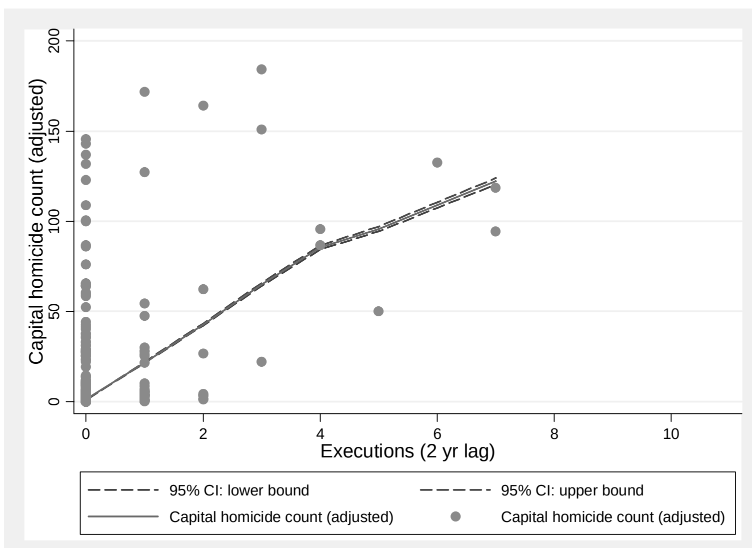
Significance: *** = p < .001, ** = p < .01, * = p < .05.

188. In Models 3–5, measures for each year were nested within counties, thereby controlling for each county's unique effects over time.

Figure 6 illustrates graphically the results of Table 5, Model 4. Figure 6 plots the predicted number of capital homicides in each Texas county-year as a function of executions in the county in the previous year and the previous two years. The graph shows a Lowess-smoothed function of the bivariate relationship, along with upper and lower bounds of the 95% confidence interval. As in Figure 5, the upper portion of Figure 6 shows the relationship of executions per county-year lagged by one year to the incidence of capital-eligible homicides, adjusted for the effects of the death penalty components, incarceration risks, and the socioeconomic covariates that were included in each of the regression models in Table 5. The lower portion of Figure 6 shows the relationship of the same adjusted incidence of capital-eligible homicides to executions lagged by two years. The figures also include upper and lower 95% confidence intervals.

Figure 6: Capital-eligible homicide counts in Texas counties, adjusted for deterrence components, noncapital homicide counts, punishment risk and covariates, by executions lagged 1 and 2 years





As before, both figures show the clustering of observations near zero for executions, a reflection of the scarcity of executions. The sparseness of executions and capital homicides produces a set of data points that are heavily clustered around zero. The rise in the slope over time is simply by chance, and is not a sign of a "brutalization" effect of capital punishment.

The narrow confidence intervals result from the relative sparseness of executions and the extreme right-skew of the distribution of capital-eligible homicides across a large dataset with nearly 5,000 observations. There were 0 capital-eligible homicides in 3,346 county-years, and another 422 have only 1. A few (3%) have more than 7 capital homicides, ranging widely from 7 to 213 executions. Predicted homicide counts based on our models are similarly skewed. Most (75%) observations are predicted to have less than 0.563 capital homicides, and 95% are predicted to have fewer than four. This distribution, heavily concentrated around low homicide counties, provides low standard errors of prediction, leading to narrow confidence intervals. The standard errors for the predicted capital homicide counts are quite narrow under these conditions, as seen in the tight confidence intervals around the predicted homicide counts.

Similar to the state-level analyses, we also estimated the same set of models using only the rate of felony murders (logged) as the dependent variable. The pattern of results were unchanged compared to Table 5: there were no significant effects for the deterrence variables.[189] As before, the noncapital homicide rate is the strongest predictor of felony murders, again showing the growing market share of homicides that are capital-eligible.

As in the national data, the inelasticity of capital homicides in Texas leaves little variance to explain, and also little room for leverage or influence by capital punishment. The relatively low incidence of capital homicides is unaffected by sparse executions when disaggregated across 254 counties, each with its own murder rate and unique conditions that shape the differences between places and changes in homicide trajectories over time. Expectations of capital punishment to influence capital homicides under these conditions simply are unrealistic.[190]

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189. Data and tables are available from the authors at http://www2.law.columbia.edu/fagan/researchdata/caphom/.

190. One important influence omitted from this analysis is the effect of drug markets on homicides. *See, e.g.*, Eric Baumer et al., *The Influence of Crack Cocaine on Robbery, Burglary, and Homicide Rates: A Cross-City, Longitudinal Analysis*, 35 J. RES. CRIME & DELINQ. 316 (1998) (analyzing the effect of crack cocaine on trends in burglary, robbery, and homicide); Daniel Cork, *Examining Space–Time Interaction in City-Level Homicide Data: Crack Markets and the Diffusion of Guns Among Youth*, 15 J. QUANTITATIVE CRIMINOLOGY 379 (1999) (examining the connection between the expansion of the use of crack cocaine and the growth in homicide rates); *see also* Roland G. Fryer Jr. et al., *Measuring the Impact of Crack Cocaine* (Nat'l Bureau of Econ. Research, Working Paper No. W11318, 2005), *available at* http://ssrn.com/abstract=720405. Markets usually are measured, however imperfectly, by drug arrest rates. *See* Richard Rosenfeld & Scott H. Decker, *Are Arrest Statistics a Valid Measure of Illicit Drug Use? The Relationship Between Criminal Justice and Public Health Indicators of Cocaine, Heroin, and Marijuana Use*, 16 JUST. Q. 685 (1999) (examining "alternative indicators" of illicit drug use besides drug arrests). However, data on drug arrests for Texas and the nation were unavailable systematically before 1985, and the coverage in the UCR databases is inconsistent. Moreover, homicides in drug transactions—other than so-called "drug kingpin" murders—rarely qualify as death-eligible under the statutes either in Texas or across the nation. Although federal law explicitly authorizes the death penalty for some

V. Conclusion

All of the recent studies claiming a relationship between death penalty policy and homicide rates suffer from an important and avoidable aggregation error: they examine the relationship between death penalty variables and total non-negligent homicide rates, despite the fact that three-fourths of all such killings do not meet the statutory criteria to be eligible for the death penalty. This study isolated the quarter of all killings that might qualify for death and used trends in these killings to test for marginal deterrent impact of death penalty policy. By using the FBI's *Supplementary Homicide Reports*, we isolated the fewer than 25% of reported killings that include 95% of all cases that produce executions in the United States. Almost all of the cases in this group are potentially eligible for the death penalty if convicted. For the other 75% of all homicides, fewer than 2% of all killings have any potential exposure to capital punishment.

Once these two types of killings have been separated, a natural method of testing the influence of the death penalty is to look for distinctive variations in the death-eligible killings that are consistent with marginal execution risk deterrence. Since the risk of an execution is more than fifty times greater in a death penalty state for the "death-eligible" cases, the variations in these cases but not the others should produce the distinctive fingerprints of death penalty policy deterrence, both over time and cross-sectionally.

But none of the distinctive patterns one might expect from marginal death penalty deterrence can be found in the nearly three decades since *Gregg*. Where the risk of execution goes up in a death penalty state, the death-eligible cases where that risk should make a difference do not decline more than the non-eligible cases, nor is the proportion of all homicides that risk a capital sanction in death states any smaller in those states than it is in states without any death penalty. An effective death penalty would produce changes in this category of homicides: the market share of all homicide that are death-eligible should decline in the face of the threat of execution. But that is not the case.

In fact, the incidence of death-eligible cases in those states is remarkably stable over time, insensitive to variations in the incidence of

"drug kingpins" under the 1988 Drug Kingpin Act, Pub. L. No. 100-690, § 7001, 102 Stat. 4181, 4387 (1988) (codified as amended at 21 U.S.C. § 848 (2000 & Supp. 2006)), such laws are rarely used by the states. Prior to the expansion of the federal death penalty in 1994, six persons were sentenced to death in federal courts under this drug kingpin statute. Death Penalty Info. Ctr., The Federal Death Penalty, http://www.deathpenaltyinfo.org/article.php?scid=29&did=147. Our analysis of the 100 most recent executions in state courts and the 50 most recent cases in Texas did not include anyone convicted under drug kingpin statutes, nor were any lower-level drug dealers identified in this exercise. Accordingly, while drug markets are considerable in the overall homicide rate, their omission from this analysis is inconsequential.

executions or to the large swings from one decade to the next in the number or rate of nondeath-eligible killings. Even in Texas, the leading execution state by far in the nation, the proportion of death-eligible killings is no smaller than in other categories of states, and there is no differential decline in death-eligible killings as the execution rate increased in the 1980s and 1990s. The marginal deterrent threat of executions on trends in these homicides would be plainly visible if it existed. This lack of effect obtains when simple comparisons are made over time and cross-sectionally, and the same pattern of non-effect persists when models to account for other influences on homicide are added. There is simply no visible evidence of the marginal deterrent impact of the death penalty on death-eligible killings.

There is an odd and rather sad irony in the persistent failure of modern deterrence arguments to classify homicides by execution eligibility. In the earlier era of less complex statistical comparisons, Thorsten Sellin tested the impact of death penalty policy on specific types of killings like those of police officers.[191] At that time, the detailed classification by death eligibility of most reported killings was not possible. The legal changes that made the classifications used in this study possible were produced by the United States Supreme Court cases of *Furman*[192] and *Gregg*[193] and the pattern of state statute these cases required. So the capacity to control for death eligibility increased after the 1970s, but the modern studies that proclaimed their statistical sophistication in citing strong deterrent effects from the death penalty failed to distinguish between death-eligible and non-eligible cases.

Our search for death penalty deterrence where it should be a strong influence on homicide rates has produced consistent results: the marginal deterrent effect of the threat or example of execution on those cases at risk for such punishment is invisible.

191. *See* SELLIN, *supra* note 25, at 52–59. Sellin's classic studies of more than fifty years ago included particularly high risk categories of homicides, such as killings of police officers and prison guards. *See* Thorsten Sellin, *The Death Penalty and Police Safety*, *in* CAPITAL PUNISHMENT, *supra* note 124, at 138, 152 (finding little difference between the murder rates of police officers in death penalty states and abolition states); Thorsten Sellin, *Prison Homicides*, *in* CAPITAL PUNISHMENT, *supra* note 124, at 154, 159 (finding that the threat of the death penalty had no effect in deterring prison violence).

192. Furman v. Georgia, 408 U.S. 238 (1972).

193. Gregg v. Georgia, 428 U.S. 153 (1976).

Appendix A: A partial list of studies published after 1990 on the deterrent effects of the death penalty

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).

Dale O. Cloninger, *Capital Punishment and Deterrence: A Portfolio Approach*, 24 APPLIED ECON. 645 (1992)

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

Dale O. Cloninger & Roberto Marchesini, *Execution Moratoriums, Commutations and Deterrence: The Case of Illinois*, 38 APPLIED ECON. 967 (2006).

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

Hashem Dezhbakhsh & Joanna M. Shepherd, *The Deterrent Effect of Capital Punishment: Evidence from a "Judicial Experiment*,*"* 44 ECON. INQUIRY 512 (2006).

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

Zhiqiang Liu, *Capital Punishment and the Deterrence Hypothesis: Some New Insights and Empirical Evidence*, 30 E. ECON. J. 237 (2004).

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).

Joanna Shepherd, *Deterrence Versus Brutalization: Capital Punishment's Differing Impacts Among States*, 104 MICH. L. REV. 203 (2005).

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

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

James A. Yunker, *A New Statistical Analysis of Capital Punishment Incorporating U.S. Postmoratorium Data*, 82 SOC. SCI. Q. 297 (2002).

Paul R. Zimmerman, *Estimates of the Deterrent Effect of Alternative Execution Methods in the United States: 1978–2000*, 65 AM. J. ECON. & SOC. (forthcoming 2006).

Paul R. Zimmerman, *State Executions, Deterrence, and the Incidence of Murder*, 7 J. APPLIED ECON. 163 (2004).

Appendix B: Data domains and sources

Domain	Source
Homicide rates and characteristics	State and county homicide totals are taken from Uniform Crime Reports [United States]: Supplementary Homicide Reports, 1976–2003 (ICPSR Study No. 4351, 2005), *available at* http://webapp.icpsr.umich.edu/cocoon/ICPSR-STUDY/04351.xml. The *Supplementary Homicide Reports* were filtered to exclude the deaths in New York associated with the attacks of September 11, 2001, but include those associated with the Oklahoma City bombing of April 19, 1995. Homicides in the following SHR categories were designated potentially capital murders: killings committed during crimes (rape, robbery, burglary, larceny, arson, car theft), multiple-victims killings, killings of persons younger than 6 years old, "gangland" homicides, sniper killings, killings of police officers, and institution killings. Homicides committed by offenders less than sixteen years old were excluded from the "potentially capital" pool. *Supplementary Homicide Reports* were compared to the aggregated Uniform Crime Reports at both the state-year and county-year levels to identify undercounts in the SHR data. State-years and county-years when observations undercounted the UCR homicide totals by at least 25% were excluded from the analysis. The final sample included 1,115 state-years of data, and 5,991 county-years of data.
Execution counts and statute information	State execution data and dates of death penalty reinstatement were compiled from the execution database of the Death Penalty Information Center (DPIC), http://www.deathpenaltyinfo.org/executions.php. County execution data for Texas were obtained from the Texas Department of Criminal Justice, http://www.tdcj.state.tx.us/stat/executedoffenders.htm. Executions in Texas were assigned to counties based on the offender's county of conviction.
Death sentence counts	Bureau of Justice Statistics, U.S. Dep't of Justice, Capital Punishment in the United States, 1973–2002 (ICPSR Study No. 3958, 2004), *available at* http://webapp.icpsr.umich.edu/cocoon/ICPSR-STUDY/03958.xml.

Law Enforcement Officers Killed and Assaulted (LEOKA)	The LEOKA data is available from the Inter-University Consortium for Political and Social Research, in the following reports: Fed. Bureau of Investigation, U.S. Dep't of Justice, Uniform Crime Reporting Program Data [United States]: Police Employee (LEOKA) Data, 2003 (ICPSR Study No. 4269, 2005), *available at* http://webapp.icpsr.umich.edu/cocoon/NACJD-DAS/04269.xml; Fed. Bureau of Investigation, U.S. Dep't of Justice, Uniform Crime Reporting Program Data [United States]: Police Employee (LEOKA) Data, 2002 (ICPSR Study No. 3996, 2004), *available at* http://webapp.icpsr.umich.edu/cocoon/ICPSR-STUDY/03996.xml; Fed. Bureau of Investigation, U.S. Dep't of Justice, Uniform Crime Reporting Program Data [United States]: Police Employee (LEOKA) Data, 2001 (ICPSR Study No. 3749, 2002), *available at* http://webapp.icpsr.umich.edu/cocoon/ICPSR-STUDY/03749.xml; Fed. Bureau of Investigation, U.S. Dep't of Justice, Uniform Crime Reporting Program Data [United States]: Police Employee (LEOKA) Data, 2000 (ICPSR Study No. 3445, 2002), *available at* http://webapp.icpsr.umich.edu/cocoon/ICPSR-STUDY/03445.xml; Fed. Bureau of Investigation, U.S. Dep't of Justice, Uniform Crime Reporting Program Data [United States]: Police Employee (LEOKA) Data, 1999 (ICPSR Study No. 3165, 2001), *available at* http://webapp.icpsr.umich.edu/cocoon/ICPSR-STUDY/03165.xml; Fed. Bureau of Investigation, U.S. Dep't of Justice, Uniform Crime Reporting Program Data [United States]: Police Employee (LEOKA) Data, 1998 (ICPSR Study No. 2907, 2001), *available at* http://webapp.icpsr.umich.edu/cocoon/ICPSR-STUDY/02907.xml; Fed. Bureau of Investigation, U.S. Dep't of Justice, Uniform Crime Reporting Program Data [United States]: 1975–1997 (ICPSR Study No. 9028, 2005), *available at* http://webapp.icpsr.umich.edu/cocoon/ICPSR-STUDY/09028.xml.
Drug arrests	State data on drug arrests 1985–2003 are aggregated from annual files of county-level crimes and arrests, Uniform Crime Reporting Program Data [United States]: County-Level Detailed Arrest and Offense Data (various years), available from University of Michigan, Inter-University Consortium for Political and Social Research, http://www.icpsr.umich.edu/.

Social and economic characteristics	State population, socioeconomic, age structure and racial composition data from 1970 to 2000 were obtained from the U.S. Bureau of the Census, Population Estimate Archives, http://www.census.gov/popest/archives/. Age structure data for 2000–2003 were obtained from the United States Census Bureau, Population Estimates by State, http://www.census.gov/popest/states/asrh/SC-est2004-02.html. State population and racial composition data are taken from the United States Census Bureau, Population Estimate Archives, http://www.census.gov/popest/archives/. Projections for intra-census years in the 1970s, 1980s, and 2000s were obtained from by the Bureau of the Census. Bureau of the Census, Intercensal County Estimates by Age, Sex, Race: 1970–1979, http://www.census.gov/popest/archives/pre-1980/co-asr-7079.html; Bureau of the Census, Intercensal Estimates of the Resident Population of States and Counties 1980–1989, http://www.census.gov/popest/archives/1980s/e8089co.txt; Bureau of the Census, 1990 to 1999 Annual Time Series of County Population Estimates by Age, Sex, Race, and Hispanic Origin, http://www.census.gov/popest/archives/1990s/CO-99-12.html; Bureau of the Census, 2000s, http://www.census.gov/popest/archives/2000s/. Census indicia for the decade 1990–2000 census were interpolated. *Urbanization.* Annual estimates of the percentage of state population residing in urban areas were obtained from the U.S. Census Bureau, Census of the Population 1990 and 2000, http://factfinder.census.gov. Mid-census data points were linearly interpolated. For 1988 and 1989, the percentage of the population in urban areas was taken from the Statistical Abstracts of the United States. *Poverty.* The percentage of each state's population with incomes below the poverty line was taken from the Census Bureau's annual poverty estimates, available at http://factfinder.census.gov (last visited Jan. 11, 2005). *Inequality.* State-level Gini coefficients were taken from the Census Bureau's "families" estimates for 1969, 1979, 1989, and 1999, available at http://www.census.gov/hhes/www/income/histinc/state/state4.html. Intervening years were linearly interpolated. Gini family estimates were used instead of household estimates since the latter were unavailable for 1969. In comparisons of Gini family estimates with Gini household estimates for the 1979-99 period, the household estimates generally were higher than the family estimates by no more than five percentage points for any of the measurement points, and they had parallel trends over time. County-level Gini coefficients for Texas also were taken from the U.S. Census Bureau and linearly interpolated for intra-census years.

Punishment	State-level punishment indices are computed as the ratio of the number of inmates incarcerated in the state in the relevant year per 100 FBI Index Crimes committed in the state in that year. Annual state prison population data are taken from the National Corrections Reporting Program. Data are from electronic spreadsheets available from Bureau of Justice Statistics Spreadsheets—Crime & Justice Electronic Data Abstracts, http://www.ojp.usdoj.gov/bjs/dtdata.htm#prisoners. Data from 1999–2003 are taken from the *See* BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, SOURCEBOOK OF CRIMINAL JUSTICE STATISTICS—2003 (Ann L. Pastore & Kathleen Maguire eds., 2004), *available at* http://www.albany.edu/sourcebook.
Robbery rates	State-level robbery rates also were recorded from the county-level FBI Uniform Crime Reports, 1970–2005. County-level robbery complaints for Texas were recorded from the FBI Uniform Crime Reports, 1978–2001 (data for 1985 were missing).
Spatial statistics	Spatially lagged homicide rates were computed for Texas counties based on Uniform Crime Report county homicide counts for each year. Counties were designated as "neighboring" if they shared a border. We computed a Moran's I, a weighted correlation coefficient used to detect departures from spatial randomness. Departures from randomness indicate spatial patterns, such as clusters. The statistic may identify other kinds of pattern such as geographic trend, including a lagged effect. The spatial lag was computed as the correlation of county homicide rates with the average murder rate in surrounding counties. *See* LUC ANSELIN, GEODA 0.95I RELEASE NOTES (2004); Luc Anselin et al., *GeoDa: An Introduction to Spatial Data Analysis*, 38 GEOGRAPHICAL ANALYSIS 5 (2006).

Appendix C: Means and standard deviations of key outcomes and predictors, 1978–2003[194]

	All states	Death penalty states	Nondeath penalty states	Texas	Harris County
Capital-eligible homicide rate	1.47 (0.85)	1.62 (0.85)	1.16 (0.78)	2.31 (0.60)	4.36 (1.41)
Noncapital homicide rate	4.90 (2.76)	5.46 (2.70)	3.73 (2.50)	8.81 (2.97)	13.61 (5.25)
Executions	0.72 (2.97)	0.98 (3.52)	0.18 (1.00)	12.52 (12.81)	2.90 (3.02)
Population	5,299,200 (5,647,348)	5,981,996 (6,114,259)	3,884,099 (4,196,685)	17,585,917 (2,314,260)	2,874,008 (299,058)
Robbery complaint rate (state-level)	147.1 (104.5)	159.6 (92.3)	121.1 (122.3)	198.5 (40.0)	55.7 (13.3)[195]
Poverty rate	12.7% (3.8)	13.3% (3.9)	11.7% (3.2)	16.4% (1.3)	14.1% (1.9)
% black	9.5% (8.5)	11.7% (9.2)	4.9% (4.4)	11.7% (.2)	19.5% (.2)
Ln (punishment index) (lagged, state-level)	1.49 (0.64)	1.60 (0.59)	1.25 (0.66)	1.76 (0.67)	
Inmate count (TX counties only)					78,103 (51,030)
% age 15–24	15.8% (2.1)	15.7% (2.1)	16.0% (2.1)	16.3% (1.7)	16.1% (1.8)
Gini coefficient	.39 (.03)	.40 (.03)	.38 (.02)	.43 (.02)	.43 (.06)
N	1,017	686	331	5,991	20

194. "Outlier" states and counties, in which the SHR murder count undercounts the UCR by more than 25%, are omitted. Counties missing one or more of the predictors are also omitted, leaving N observations.

195. Robbery complaints for Harris County are county-specific reports. Texas county panel is 1978–1998.