THE DETERRENT EFFECT OF CAPITAL PUNISHMENT: EVIDENCE FROM A "JUDICIAL EXPERIMENT"

HASHEM DEZHBAKHSH and JOANNA M. SHEPHERD*

We use panel data for 50 states during the 1960–2000 period to examine the deterrent effect of capital punishment, using the moratorium as a "judicial experiment." We compare murder rates immediately before and after changes in states' death penalty laws, drawing on cross-state variations in the timing and duration of the moratorium. The regression analysis supplementing the before-and-after comparisons disentangles the effect of lifting the moratorium on murder from the effect of actual executions on murder. Results suggest that capital punishment has a deterrent effect, and that executions have a distinct effect which compounds the deterrent effect of merely (re)instating the death penalty. The finding is robust across 96 regression models. (*JEL* C1, K1)

I. INTRODUCTION

The contemporary debate over capital punishment involves a number of important arguments based on either moral principles or social welfare considerations. The primary social welfare issue, viewed as "the most important single consideration for both sides in the death penalty controversy," is whether capital punishment deters capital crimes; see, for example, Zimring and Hawkins (1986). This issue is also of great interest to several states that are currently considering a change in their death penalty laws.[1]

Psychologists and criminologists studied the death penalty initially and reported no deterrent effect; see, for example, Sellin (1959) and Eysenck (1970). Economists joined the debate later, with Ehrlich's 1975 and 1977 studies that report a significant deterrent effect applying regression analysis to U.S. aggregate data for 1933–69 and state-level data for 1940 and 1950. Ensuing studies use either Ehrlich's data with different regression specifications—different regressors, functional form, or endogenous variables—or postmoratorium data with a variant of Ehrlich's regression model. Results range from a substantial deterrent effect to no effect or a small adverse effect.[2]

This study uses a new approach to advance the deterrence literature. We exploit an important characteristic that other studies have not considered: the experimental nature of the moratorium that the Supreme Court imposed on executions during the 1970s. The moratorium can be viewed as an exogenously imposed "judicial experiment." The experiment's effect on murder rates provides evidence about the deterrent effect of capital punishment.

We compare the murder rate for each state immediately before and after it suspended or reinstated the death penalty. Many factors that affect crime—for example, law enforcement, judicial, demographic, and economic variables—change only slightly over a short period of time. Therefore, changes in a state's murder rate quickly following a change in its death penalty law can be attributed to the legal change. Also, there are considerable cross-state variations in the timing and duration of the moratorium that began and ended in different years in different

*We are thankful to George Shepherd, an anonymous referee, and the journal's editor for helpful comments and Christine Stemm for research assistance. The usual disclaimer applies.

Dezhbakhsh: Professor, Economics Department, Emory University, 1602 Fishburne Drive, Atlanta, GA 30322-2240. Phone 1-404-727-4679, Fax 404-727-3082, E-mail econhd@emory.edu

Shepherd: Assistant Professor, Emory University, School of Law, Gambrell Hall, Atlanta, GA 30322-2240. Phone 404-727-8957, Fax 404-727-6820, E-mail jshepherd@law.emery-edu

1. Illinois suspended executions in 2000, and moratorium bills have recently been introduced in Pennsylvania, Tennessee, Oklahoma, Ohio, Delaware, and Nebraska. Massachusetts, on the other hand, is considering reinstating death penalty.

2. A brief description of the deterrence studies appears in the next section. For literature summaries see Cameron (1994) and Avio (1998).

Economic Inquiry
(ISSN 0095-2583)
Vol. 44, No. 3, July 2006, 512–535

doi:10.1093/ei/cbj032
Advance Access publication January 11, 2006

TABLE 1
Executions and Death Penalty Laws, 1960–2000 (States Currently without the Death Penalty)

States	No. of Executions (1960–72)	Last Execution	State Abolished Death Penalty in
Alaska	0	April 1950	1957
Hawaii	0	No execution	1957
Iowa	3	September 1962	1965
Maine	0	1887	1887
Massachusetts	0	1947	1984
Michigan	0	Before 1837	1846
Minnesota	0	Before 1911	1911
North Dakota	0	Before 1930	1973
Rhode Island	0	Before 1930	1984
Vermont	0	1954	1965
West Virginia	0	Before 1955	1965
Wisconsin	0	1851	1853
Plus District of Columbia	0	April 1957	1972

Notes: On June 29, 1972, the Supreme Court effectively suspended the death penalty. The moratorium ended in July 1976, and the first postmoratorium execution took place in January 1977. Rhode Island reenacted its death penalty statute in 1977 and Massachusetts in 1982, before both states abolished it in 1984 (see Bowers 1981).

states, ranging from four to thirty years (see Tables 1 and 2).[3] These variations can strengthen our comparison-based inference, because observing similar changes in murder rates immediately after the same legal change in different years and in various states provides compelling evidence of the moratorium's effect on murder. Moreover, our analysis benefits from the experiences of the states that suspended and reinstated their death penalty statutes several times.[4] For states that experience similar murder rate changes after suspensions (or reinstatements) in different crime, economic, and demographic environments, the contribution of the legal change to murder is paramount.

We supplement the before-and-after comparisons with time-series and panel data regression analyses. Unlike most existing studies, we use both pre- and postmoratorium data. The regressions disentangle the effect of having death penalty laws on murder from the effect of actual executions on murder. Basing our inference on two different approaches allows us to corroborate our findings and also detract from the modeling concerns voiced at previous studies that use only regression analysis.

We also apply a battery of tests to examine the robustness of the findings. These include regression sensitivity checks with 89 additional regression equations that differ from our base models in regressors, functional form, data, and estimation method. In addition, we test whether the moratorium has an effect on property crimes that are not punishable by death. If the relationship between the moratorium and these crimes is similar to the relationship between the moratorium and murder, then we should suspect that murder rate changes are the result of broader trends in criminal behavior affecting all crimes. We confirm that the observed deterrent effect is not driven spuriously by common crime patterns.

3. For example, 12 states abolished their death penalty statutes before the 1972 Supreme Court decision. These states include Alaska, Hawaii, Iowa, Maine, Michigan, Minnesota, New Mexico, New York, Oregon, Vermont, West Virginia, and Wisconsin. Six of these states abolished their statutes during our sampling period. The death penalty statutes of Massachusetts and Rhode Island were abolished twice, once in 1972 and a second time in 1984. Note that Massachusetts reinstated the death penalty in 1982 before it abolished it altogether in 1984, and Rhode Island reinstated the death penalty in 1977 before it abolished it in 1984. In addition, for the states that rewrote their death penalty statutes during 1972–76, the moratorium officially ended with the 1976 *Gregg* decision. However, 12 other states did not reenact their death penalty statutes until years later, and as late as 1995 (New York, see Table 2). Delaware restored its death penalty statute twice (once in 1961); so it adds a thirteenth observation to the sample of non-1976 switches.

4. For example, Rhode Island suspended the death penalty in 1972, reinstated it in 1977, and abolished it in 1984. Although many factors that affect Rhode Island's murder rate differ in 1972 and 1984, the murder rate increased after both suspensions: by 13% after 1972 and by 25% after 1984 in one-year comparisons. The 1984 increase even reversed a declining trend in Rhode Island's murders. This single state experience, however, is only anecdotal evidence. Our method of inference draws on evidence that is pools across various states.

TABLE 2
Executions and Death Penalty Laws, 1960–2000 (States Currently with the Death Penalty)

	Before *Furman v. Georgia* (1960–June 1972)			Since *Furman v. Georgia* (July 1972–2000)			
States	**No. of Executions**	**Last Execution**	**Death Penalty Statute Change**	**No. of Executions**	**First Execution**	**Death Penalty Reenacted**	**Total**
Alabama	5	1965	—	23	1983	March 1976	28
Arizona	4	1963	—	22	1992	August 1973	26
Arkansas	9	1964	—	23	1990	March 1973	32
California	30	1967	Abolished 1972	8	1992	August 1977	38
Colorado	6	1967	—	1	1997	January 1975	7
Connecticut	1	1960	—	0	—	October 1973	1
Delaware	0	—	Restored 1961	11	1992	March 1974	11
Florida	12	1964	—	50	1979	Dec. 1972	62
Georgia	14	1964	—	23	1983	March 1973	37
Idaho	0	—	—	1	1994	July 1973	1
Illinois	2	1962	—	12	1990	July 1974	14
Indiana	0	—	—	7	1981	May 1973	7
Kansas	6	1965	—	0	—	April 1994	6
Kentucky	1	1962	—	2	1997	January 1975	3
Louisiana	1	1961	—	26	1983	July 1973	27
Maryland	1	1961	—	3	1994	July 1975	4
Mississippi	10	1964	—	4	1983	April 1974	14
Missouri	4	1965	—	46	1989	Sept. 1975	50
Montana	0	—	—	2	1995	March 1974	2
Nebraska	0	—	—	3	1994	April 1973	3
Nevada	1	1961	—	8	1979	July 1973	9
New Hampshire	0	—	—	0	—	January 1991	0
New Jersey	3	1963	Abolished 1972	0	—	August 1982	3
New Mexico	1	1960	Abolished 1969[a]	0	—	July 1979	1
New York	10	1962	Abolished 1965[a]	0	—	Sept. 1995	10
North Carolina	1	1961	—	16	1984	June 1977	17
Ohio	7	1963	—	1	1999	January 1974	8
Oklahoma	6	1966	—	30	1990	May 1973	36
Oregon	1	1962	Abolished 1964	2	1996	Dec. 1978	3
Pennsylvania	3	1962	—	3	1995	March 1974	6
S. Carolina	8	1962	—	25	1985	July 1974	33
S. Dakota	0	—	—	0	—	January 1979	0
Tennessee	1	1960	—	1	2000	February 1974	2
Texas	29	1964	—	239	1982	January 1974	268
Utah	1	1960	—	6	1977	July 1973	7
Virginia	6	1962	—	81	1982	October 1975	87
Washington	2	1963	—	3	1993	Nov. 1975	5
Wyoming	1	1965	—	1	1992	February 1977	2

Notes: On June 29, 1972, the Supreme Court effectively suspended the death penalty (*Furman v. Georgia*). Some states reenacted their capital statutes soon after *Furman,* but the death penalty was officially reinstated with the Supreme Court's decision on July 2, 1976 (*Gregg v. Georgia*).

[a]The abolition of the death penalty in New Mexico and New York was restrictive because it did not cover killing an on-duty police officer or a prison guard by inmates.

The article is organized as follows: Section II discusses the recent history of the death penalty in the United States and reviews the literature on the deterrent effect of capital punishment. Section III reports results for the United States during the 1960–2000 period using national data. The analysis includes regressions as well as comparisons of murder rates before and after the changes in the death penalty laws. Section IV discusses the results of before-and-after comparisons and regressions using state-level panel data. Section V demonstrates the robustness of our findings and provides evidence that our results are not driven by broader criminality and enforcement factors. Section VI concludes.

II. CAPITAL PUNISHMENT AND DETERRENCE

In this section we briefly discuss the recent history of the death penalty in the United States and review the literature on the deterrent effect of capital punishment.

History of the Death Penalty

During the first half of the twentieth century, executions were both frequent and popular. More executions occurred during the 1930s than in any other decade in American history: an average of 167 each year. Although the use of capital punishment declined somewhat in the 1940s and 1950s, executions were still much more frequent than today: approximately 130 a year in the 1940s and 75 a year during the 1950s, compared to an average of 48 per year in the 1990s. Over 65% of the American public approved of the death penalty during these decades; see. Paternoster (1991).

In the late 1950s, however, public support for the death penalty started to decline, reaching a low of 42% in 1966. Opposition to the death penalty increased because of growing doubts about the morality of the death penalty, awareness of Western Europe's abandonment of capital punishment, abatement of the 1930s crime wave, lack of deterrence evidence, widespread belief in the racially discriminatory use of the death penalty, and increasing concern about the arbitrariness of death penalty sentences; see Bedau (1982). Reflecting the public's growing disapproval, executions declined steadily throughout the 1960s.

By the 1960s, all executing states had changed their mandatory capital statutes, borrowed originally from English common law, to discretionary statutes. Under the new statutes, juries had complete control over whether a defendant received a death sentence. This sentencing freedom introduced arbitrariness into death penalty application. As a result, the U.S. Supreme Court began hearing cases involving the discretionary capital statutes in the late 1960s. While the constitutionality of capital punishment was being challenged, no states were willing to put people to death.

The Supreme Court finally resolved the constitutionality of discretionary capital statutes in three cases in 1972: *Furman v. Georgia*, *Jackson v. Georgia*, and *Branch v. Texas*, collectively referred to as the *Furman* decision (408 U.S. 238). In a five-to-four decision, the justices held that discretionary capital statutes resulted in arbitrary sentencing, violating the Eighth Amendment's cruel and unusual punishment clause. This decision effectively voided the death penalty statutes of all executing states and commuted the sentences of over 600 death row inmates.

After *Furman*, the states quickly began to draft new death penalty laws. Some states passed mandatory statutes that the Supreme Court soon found unconstitutional, whereas others enacted guided discretion statutes that provide guidelines for juries in death penalty cases. The Supreme Court approved these statutes in 1976 in *Gregg v. Georgia* (428 U.S. 153), *Jurek v. Texas* (428 U.S. 262), and *Proffitt v. Florida* (428 U.S. 242), known collectively as the *Gregg* decision. During the next 20 years many states enacted new constitutional death penalty statutes. After the enactment of these new statutes, death rows quickly filled up. Since 1977, there have been 856 executions in 32 states. Today, the approval rating for the death penalty is over 74%, down from an all-time high of 80% in 1994 (Jones 2003).

Despite the recent resurgence in executions, the use of the death penalty varies widely across states. As Table 1 shows, 12 states do not have capital punishment laws: Alaska, Hawaii, Iowa, Maine, Massachusetts, Michigan, Minnesota, North Dakota, Rhode Island, Vermont, West Virginia, and Wisconsin. Most of these states have not executed anyone since the early 1900s, and Wisconsin, Michigan, and Maine legally abolished the death penalty as early as the 1800s.

Of the 38 states that currently have capital punishment laws, 6 have performed no post-moratorium executions: Connecticut, Kansas, New Hampshire, New Jersey, New York, and South Dakota. As Table 2 shows, several of these states abolished their death penalty laws years before the 1972 *Furman* decision. Other states were prompt to reenact their capital statutes after *Furman*, and began postmoratorium executions as soon as the late 1970s and early 1980s. However, some of the states have performed only one or two postmoratorium executions.

Literature on the Deterrent Effect of the Death Penalty

In the United States, the deterrence issue has been a topic of acrimonious debate for decades. The initial participants in the debate were psychologists and criminologists. Their research was either theoretical or based on comparisons of crime patterns for matched regions with different rates of execution. The studies generally found no deterrent effect (Sellin 1959; Cameron 1994).

The debate in the economics literature began with Ehrlich's seminal work (1975, 1977) that first used regression analysis to study the deterrent effect of capital punishment. His strong deterrent finding contrasted sharply with the earlier findings. Since then, although many researchers have used Ehrlich's data and sample period, they have reached divergent conclusions by using different specifications and functional forms. For example, using the same data, Yunker 1976), Cloninger (1977), Ehrlich and Gibbons (1977), Ehrlich and Liu (1999), and Liu (2004) have found a deterrent effect of capital punishment. In contrast, Bowers and Pierce (1975), Passel and Taylor (1977), and Hoenack and Weiler (1980) find no deterrence when they use the same data with alternative specifications. Similarly, McAleer and Veall (1989), Leamer (1983), and McManus (1985) find no deterrent effect when different variables are included over the same sample period. Finally, Black and Orsagh (1978) find mixed results depending on the cross-section year they use.

Others have updated Ehrlich's time-series data or used more recent cross-section data. These studies also produce different results by using similar data with different econometric specifications. Layson (1985) and Cover and Thistle (1988), for example, use identical extensions of Ehrlich's time-series data: 1933–77. Although Layson finds a significant deterrent effect of executions, Cover and Thistle correct for data nonstationarity and find no support for the deterrent effect. Chressanthis (1989) employs time-series data covering 1966 through 1985, and Brumm and Cloninger (1996) use cross-sectional data covering 58 cities in 1985; both studies find a deterrent effect. In contrast, Grogger (1990) uses daily data for California during 1960–63 and finds no significant deterrent effect.

Recent studies have used panel data instead of time-series or cross-section data but examine only the postmoratorium evidence. Again, similar data with different techniques produce disparate results. Using state-level panel data, Lott and Landes (2000), Cloninger and Marchesini (2001), Mocan and Gittings (2003), Zimmerman (2004, forthcoming), and Shepherd (2004) find a deterrent effect of capital punishment. However, Albert (1999) finds no deterrent effect with state data. Dezhbakhsh et al. (2003) also find a deterrent effect using county-level panel data.

Most recent studies use postmoratorium data, and earlier studies used premoratorium data; no study has used data from both periods. Moreover, all of these studies have based their analyses on regressions, applying slight variations in specification to similar data sets. No study has used the 1970s death penalty moratorium in the context of a controlled group experiment.

III. ANALYSIS OF NATIONAL DATA

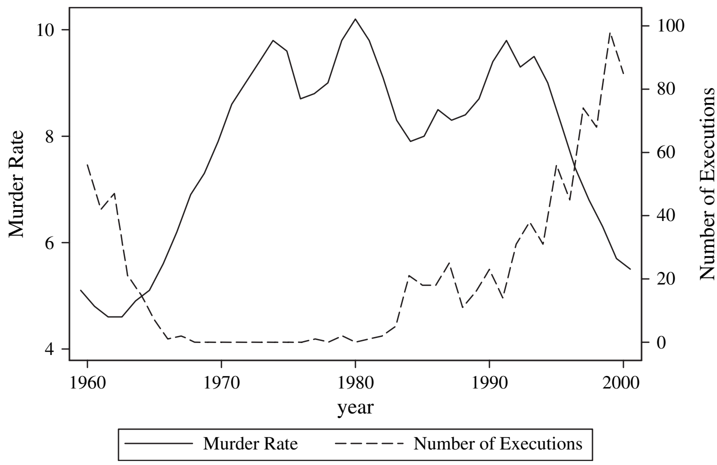
We begin by taking a simple "first cut" at the data as a way of motivating the more detailed state-level analysis that follows. We examine the aggregate crime and execution data for the United States for the period 1960–2000, discuss general trends in the data, present before-and-after moratorium comparisons of murder rates, and report preliminary regression results. The data and sources are described in the appendix.

General Trends

Figure 1 displays the murder rate and the number of executions in the United States for the period 1960–2000.[5] The two series appear to move in opposite directions. Executions declined precipitously during the 1960s, dropped to zero for most of the 1970s, and increased with some fluctuations during the 1980s and 1990s. The increase is particularly steep during

5. Following the common practice, the murder rate used in our analysis is the number of murders and nonnegligent manslaughters per 100,000 population.

FIGURE 1
U.S. Murder Rate and Executions



the 1990s. The murder rate, on the other hand, reversed its downward trend of the early 1960s and increased rapidly throughout the early 1970s, with some fluctuations in the late 1970s. The murder rate more than doubled from its 1963 low to its late 1970s high. The rate has declined since then, initially with brief fluctuations and later steadily, returning to its lows of the early 1960s.

We can identify three distinct periods in Figure 1: (1) the early 1960s with high (but falling) executions and falling murder rates, (2) 1964 to the mid- to late 1970s with very few executions and rapidly rising murder rates, and (3) 1990 to 2000 with soaring executions and sharply declining murder rates. The pattern during the remaining period (1980–89) is not as clear or persistent, although the two series still have opposite trends.

Before-and-After Comparisons

The death penalty moratorium that the Supreme Court imposed between 1972 and 1976 provides a judicial experiment for analyzing deterrence. The likelihood that a murder committed during this period would be punished by execution was 0 throughout the country. This was not the case before 1972 or after 1976. Thus, a comparison of murder rates immediately before and after 1972 can shed some light on how imposing a moratorium on the death penalty affects murders. Similarly, a comparison of murder rates immediately before and after 1976 can provide evidence on the effect of lifting the moratorium on murder. Before-and-after comparisons over short horizons provide evidence that is as close to ceteris paribus as possible absent a full econometric model.

We make these comparisons using one-year, two-year, and three-year windows. For example, for the one-year window, we compare murder rates for 1971 and 1973 and murder rates for 1975 and 1977.[6] For the two-year window the average murder rate for 1970 and 1971 is compared with the average murder rate for 1973 and 1974, and the average murder rate for 1974 and 1975 is compared with the average murder rate for 1977 and 1978. Three-year comparisons are similarly made using three-year averages.

The results indicate that the annual murder rate jumped by 0.8 or 9.3% when the moratorium was imposed and dropped by 0.8 or 8.3% when the moratorium was lifted. The two-year average murder rate also jumped by 1.35 or 16.3% when the moratorium was imposed and dropped by 0.8 or 8.2% when the moratorium was lifted. The larger effect for the two-year comparisons is caused by rising murder rates during the moratorium years. Three-year averages show a similar pattern with an increase of 1.66 or 20.9% and a drop of 0.4 or 4.1% for the respective effects.

6. The year of the change (1972 or 1976) is discarded because it covers two regimes.

TABLE 3
Murder Rate Regression Results; U.S. National Data (1960–2000)

		Entire Period (1960–2000)		Subperiod 1960–76		Subperiod 1977–2000	
Regressors		**Coefficients**	**Estimates**	**Coefficients**	**Estimates**	**Coefficients**	**Estimates**
Executions	−0.031 (−3.35***)	—	—	−0.073 (−3.57***)	—	−0.037 (−7.25***)	—
Executions lagged	—	−0.038 (−4.18***)	—	—	−0.046 (−3.11***)	—	−0.039 (−6.86***)
Moratorium binary variable	—	—	1.708 (2.16**)	—	—	—	—
Intercept	8.465 (27.05***)	8.636 (29.69***)	7.591 (27.46***)	7.737 (18.28***)	8.712 (26.88***)	9.472 (45.76***)	9.397 (44.92***)

Notes: The dependent variable in all equations is the murder rate. t-statistics are in parenthesis. ** and *** indicate significance at the 5% and 1% levels, respectively. The Supreme Court's effective moratorium on the death penalty ended in 1976 and the first postmoratorium execution took place in 1977.

Regression Results

To put the exploratory evidence into a proper statistical context, we examine the movements of murders and executions over various periods using regressions with and without control variables. Table 3 reports the results from the simple regressions with no control variables. We regress the murder rate on each deterrent variable—executions, lagged executions, and a moratorium dummy variable—over our entire sample period, 1960–2000. We also run regressions of executions and lagged executions on the murder rate for two sub-periods: 1960–76 (before and during the moratorium) and 1977–2000 (postmoratorium).

The estimated coefficients of execution and its one-period lag are negative and highly significant for all three time horizons, indicating that murders and executions move in opposite directions. The lagged results suggest that the effect of executions on murder may spread over time. The lag effect reflects the timing of executions within a year (January versus December, for instance) and the possibility that a community's memory of an execution may linger. The larger t-statistics for the postmoratorium estimates may reflect the strong opposite trends that murders and executions exhibit during the 1990s.[7]

The above results are highly robust to various modeling choices.[8] For example, the deterrent coefficient estimates maintain their sign and significance when we use log linear or differenced specifications.[9] The observed negative relationship also holds between the murder rate and higher lags of executions. Moreover, we repeated the two-equation estimation for two other subperiods: the 1980s and 1990–2000. Results confirm that the negative relationship is significant during both decades but much stronger during the 1990s; for example, p-values for the coefficient estimates are (0.003 and 0.004) and (0.000 and 0.000) for the 1980s and 1990–2000, respectively. Although the strength of the relationship seems to change over time, its direction and significance does not.

The relationship between murders and executions is not symmetrical. A regression of the number of executions on the murder rate produces a significant coefficient estimate, but regressions of the number of executions on both the murder rate lagged one year and the murder rate lagged two years produce insignificant estimates, suggesting that any causal

7. In the state-level analysis, we explore factors that might contribute to differences in the magnitude of the relationship between murders and executions.

8. Results of national data robustness checks are not reported but are available from the authors.

9. Cover and Thistle (1988) have shown that data nonstationarity can affect findings in this literature. The similarity of the regression estimates based on level data with those based on differenced data reported here, however, suggests that we do not need to be concerned about problems stemming from unit roots in our analysis. In fact, we applied to national murder rates and property crime rates the augmented Dickey-Fuller test for the null hypothesis of unit root with drift, with Schwartz criteria for lag selection, s. The results were only borderline insignificant with p-values of 16% and 12%, respectively. Given that these unit root test are notorious for low power (inability to reject an incorrect unit root null), a low p-value makes the unit root problem an unlikely scenario here. See Maddala and Kim (1998), chapters 3 and 4, for a discussion of unit root tests and their power inadequacies.

TABLE 4
Murder Rate Regression Results; U.S. National Data (1960–2000)

	Sampling Period (1960–2000)			
Regressors	**Coefficients**		**Estimates**	
Executions	−0.0542 (−7.06***)	−0.0456 (−5.45***)	—	—
Executions lagged	—	—	−0.0571 (−7.65***)	−0.0490 (−6.18**)
Moratorium binary variable	—	0.9592 (2.14**)	—	0.9491 (2.26**)
Unemployment rate	0.2840 (3.02****)	0.2835 (3.16***)	0.2578 (2.85***)	0.2576 (3.01***)
Per capita real income	0.0002 (0.72)	0.0004 (1.65)	0.0002 (0.85)	0.0005 (1.80*)
Expenditures on police protection	0.0001 (1.02)	−0.0000 (−0.04)	0.0001 (0.85)	−0.0000 (−0.20)
Intercept	2.7440 (1.55)	0.9174 (0.48)	3.0719 (1.78*)	1.2363 (0.68)
Adjusted R^2	0.77	0.79	0.78	0.80

Notes: The dependent variable in all equations is the murder rate. *t*-statistics are in parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

link is more likely to run from executions to murders.

The regression of the murder rate on the moratorium binary variable reflects the fact that murder rates were higher during the moratorium years. The moratorium binary variable, which takes a value of 1 for every year during the moratorium period and 0 otherwise, is intended to extract the gross effect of the moratorium on the probability distribution of the U.S. murder rate. The estimated coefficient is positive and significant at the 5% level, indicating that the mean of the distribution of the murder rate increased by 1.708 during the moratorium period. Comparing this estimate with the estimate of the constant term (7.591), which is the mean of the murder rate over the remaining period, we observe a 22.5% increase in the mean murder rate during the moratorium years.

We repeat the previous regressions adding several control variables as regressors; these include the unemployment rate, per capita real income, and real expenditures on police protection. The first two variables control for economic conditions that may influence the cost of committing crime. The third variable serves as a deterrent variable; additional expenditures on police protection may increase the likelihood of apprehension, which should deter crime.

Table 4 presents the regression estimates. The signs and significance of the death penalty variables are strikingly similar to those reported for the simple regressions in Table 3. The execution variable and its lag have negative and highly significant coefficients in all equations, indicating that more executions are associated with fewer murders. The coefficient of the binary variable that identifies the moratorium years is positive and significant, indicating that even after we control for other relevant variables, the moratorium still leads to an increase in murders.[10]

The unemployment rate is positive and highly significant in all equations, suggesting that more unemployment may lead to a higher murder rate, as expected from economic theory. The effects of income and police expenditure on murders turn out to be insignificant in all but one equation. Donohue and Levitt (2001) also report that unemployment has a positive effect and income has an insignificant effect on murders and other crimes.

10. It would be tempting to use these estimates to ascertain how many lives are spared as the result of each execution. The arithmetic is simple, but the interpretation is difficult. Given the relatively large sampling span over which both executions and murder rates vary greatly, a local interpolation will not be very informative. If the model was estimated over a much shorter span, such an exercise could then be of some use.

IV. ANALAYIS OF STATE DATA

National data masks cross-state variations in murder rates and the frequency of executions. Because capital statutes are passed by state legislatures and executions are usually carried out by states, the deterrent effect of an execution may be confined to the executing state.[11] We examine state-level evidence to make a more powerful inference about the deterrent effect of capital punishment. State-level analysis exploits cross-state dispersions in the timing and duration of the moratorium to isolate its effect on murders, therefore reducing the possibility of spurious (or incidental) results.

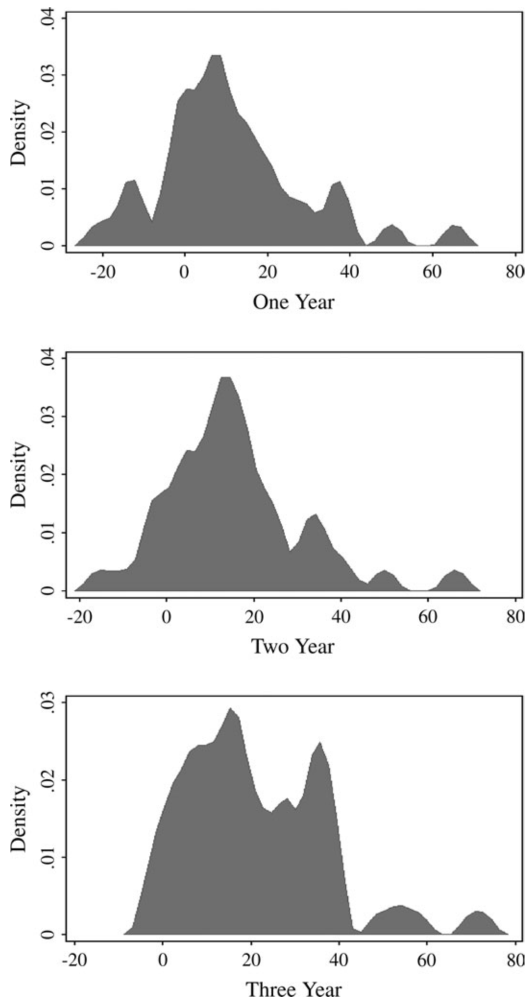
In the following sections, we first compare states murder rates before and after the moratorium. Then, we report the regression results from state-level panel data. We use data covering all 50 states and the District of Columbia for the period 1960–2000. The data and sources are described in the appendix.

Before-and-After Comparisons

In this section, we analyze the effect of both suspending and reinstating the death penalty on state murder rates. We compare the murder rates for the years immediately before and after the legal change.[12] We similarly compare two-year (three-year) murder rate averages for the two years (three years) preceding the switch and the two years (three years) following the switch. The resulting murder rate changes, referred to as one-year, two-year, and three-year changes, provide the basis for the analysis in this section.

We first examine the effect of suspending (or abolishing) the death penalty on murders. Figure 2 presents kernel density estimates of the cross-state distributions of the change in murder rates following the suspension (or abolition) of the death penalty.[13] The three

FIGURE 2
Frequency Distribution of the Percentage Change in Murder Rates before and after Suspending (or Abolishing) the Death Penalty



Notes: The frequency distributions for the murder rate changes are estimated using biweight kernel with optimal bandwidth. A positive value indicates an increase in the state's murder rate after the death penalty is suspended (abolished). For the one-year comparison, murder rates immediately before and after the change are compared. For two-year (three-year) comparisons, the average crime rate two years (three years) before and the average crime rate two years (three years) after the legal change are compared. All changes are in percents. Two outlier observations are excluded from one-year comparisons, one from two-year comparisons, and one from three-year comparisons. All four observations are positive and larger than 200.

11. Executions carried out by the federal government are quite rare; only one occurred in 1963 during our sample.

12. The year of the change is discarded because it covers two regimes.

13. Kernel density estimates have the advantage of being smooth and also independent of the choice of origin—unlike histograms, which are not independent of the location of the bins. We estimate densities using biweight kernel with optimal bandwidth. Biweight kernel provides density estimates that track the underlying histogram more closely than other kernels such as Epanecknikov or Gaussian, and yet it has a high efficiency (99%); see Silverman (1994). Also, using optimal bandwidth diminishes the arbitrariness inherent in choosing bandwidth—which is similar to choosing the number of bins in a histogram.

TABLE 5
Percentage Change in Murder Rates before and after Suspending (or Abolishing) the Death Penalty; State Panel Data (1960–2000)

Statistics	One-Year Comparison	Comparison of Two-Year Averages	Comparison of Three-Year Averages
No. of observations	45	45	45
No. of positive observations	33	39	41
Median	8.33	14.89	18.37
Mean	10.05	16.25	21.86
Sample SE	2.811	2.213	2.480
t-statistic	3.57***	7.34***	8.81***
p-value for the t-test	0.0004	0.0000	0.0000

Notes: t-statistics test the hypothesis of no change against the alternative of more murders. ** and *** indicate significance at the 5% and 1% levels, respectively. For the one-year comparison, murder rates in the year before and after the change are compared. For two-year (three-year) comparisons, the average crime rate two years (three years) before and the average crime rate two years (three years) after the legal change are compared. All changes are in percents. Two outlier observations are excluded from one-year comparisons, one from two-year comparisons, and one from three-year comparisons. All four observations are positive and larger than 200%.

charts correspond to the three comparison windows of one, two, and three years. Positive values indicate an increase in the murder rate. The concentration of the distributions in the positive range in Figure 2 is striking. In fact, all three measures suggest that suspending the death penalty is very likely to be followed by an increase in murders. This is more pronounced for the three-year change, perhaps reflection that as time passes, more criminals respond to a change in the punishment level.

The descriptive statistics characterizing the three probability distributions are reported in Table 5.[14] Each column contains statistics pertaining to a comparison window. The number of observations is slightly larger than the number of states with switches, because a few states had more than one switch, generating more than one observation.[15] A considerable number of observations are positive, indicating that many states experience an increase in the murder rate after they suspend (or abolish) the death penalty. This is as large as 41 observations or 91% of all cases (for three-year comparisons). The remaining observations are not all negative; there is no change in the murder rate in one two-year comparison, one three-year comparison, and two one-year comparisons.

Moreover, both the sample median and mean are positive, suggesting an overall increase in murder following the suspension of the death penalty. The sample standard errors are relatively small. The mean based t-statistics are positive and highly significant for all three measures. Overall, these statistics and the distribution estimates in Figure 2 suggest that states that suspend or abolish their capital punishment statutes experience a significant increase in their murders.

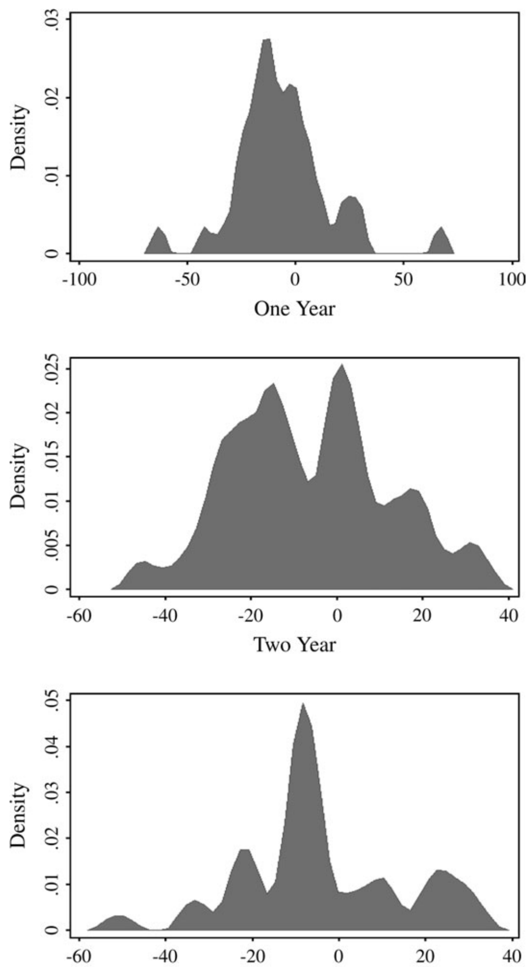
Next we examine the effect of reinstating the death penalty on murders. Figure 3 presents the cross-state distributions (probability density functions) of changes in the murder rate immediately after reinstating the death penalty. Each chart contains the kernel density estimate for one of the three comparison measures. In contrast to Figure 2, the distributions are more concentrated in the negative range, suggesting that there are more cases where the murder rate declines after the death penalty is reinstated. Although the negative range concentration in these charts is not as strong as the positive range concentration of the charts

14. To avoid undue outlier influence on the statistics reported in Table 5 and the distribution estimates in Figure 2, we dropped Vermont's observations of murder rate increases of 200%, 360%, and 458% (for one-year, two-year, and three-year comparisons) following the ban on executions. We also dropped the one-year change for South Dakota that shows a 216% increase in murder. These four observations would have added to the strength of the deterrence evidence provided here, but we feel more comfortable excluding them given the small size of these states and their small murder rates. All other observations used in Tables 5 and 6 are not large enough to cause any concern.

15. Note that the analysis obviously does not include several nonswitching states—states that abolished the death penalty prior to 1960 (the start of our sample) and never reinstated it. These states are included, however, in our regression analysis of the next section.

FIGURE 3

Frequency Distribution of the Percentage Change in Murder Rates before and after Reinstating the Death Penalty



Notes: The frequency distributions for the murder rate changes are estimated using biweight kernel with optimal bandwidth. A negative value indicates a drop in the state's murder rate after the death penalty is reinstated. For the one-year comparison, murder rates right before and after the change are compared. For two-year (three-year) comparisons, the average crime rate two years (three years) before and the average crime rate two years (three years) after the legal change are compared. All changes are in percents.

in Figure 2, the deterrent effect of reinstating the death penalty is obvious.

Table 6 reports the descriptive statistics for these distributions; it has the same format as Table 5.[16] The number of negative observations in Table 6 indicates that in about 70% of the cases, murder drops after the state reinstates the death penalty. Moreover, both the sample median and mean are negative, suggesting an overall drop in murders following a readoption of the death penalty. The *t*-statistics are also negative and significant for all three measures.

Overall, these statistics and the distribution estimates in Figure 3 suggest that states that reinstated death penalty experienced a drop in murder rates. The drop, however, does not seem to be as widespread as the increase in the number of murders resulting from suspending the death penalty. There are several plausible explanations for the asymmetric strength of the deterrent effect. First, in 1972 the Supreme Court not only suspended executions but also commuted the sentences of many death row inmates. The combined effect was perhaps more potent than its reversal that involved only a change in the death penalty statutes. Second, the ban on executions that took place simultaneously across the country was more dramatic and caught more public attention than the switch back to the death penalty that occurred at a staggered pace over two decades. Finally, suspending the death penalty necessarily stops executions, but reinstating the death penalty does not guarantee new executions. A glance at Table 2 reveals that many states that reinstated the death penalty years ago have yet to execute a convict. The regression analysis of the next section, which uses the number of executions as well as the moratorium as explanatory variables, sheds more light on this finding.

Regression Analysis and Results

Before presenting the state-level regression analysis, we report selected summary statistics for state-level data. These statistics are

16. There are fewer observations in Table 6 than in Table 5 because some states that abolished capital punishment never reinstated it during our sampling period. Also, the number of observations in Table 6 is not the same across the three horizons: 41 observations in the one-year comparisons and 39 observations in the two- and three-year comparisons. Massachusetts's switch to the death penalty following the moratorium that lasted only one full calendar year (1983) only contributes to the one-year comparisons. Similarly, Delaware's reinstatement of the death penalty in 1961 is too close to the beginning of our sample to be measured by the two-year and three-year comparisons.

TABLE 6
Percentage Change in Murder Rates before and after Reinstating the Death Penalty; State Panel Data (1960–2000)

Statistics	One-Year Comparison	Comparison of Two-Year Averages	Comparison of Three-Year Averages
No. of observations	41	39	39
No. of negative observations	29	23	26
Median	−9.30	−6.82	−7.50
Mean	−6.33	−6.39	−4.07
Sample SE	3.381	2.928	2.921
t-statistic	−1.87**	−2.18**	−1.39*
p-value for the *t*-test	0.034	0.017	0.085

Notes: t-statistics test the hypothesis of no change against the alternative of fewer murders. ** and *** indicate significance at the 5% and 1% levels, respectively. For the one-year comparison, murder rates in the year before and after the change are compared. For two-year (three-year) comparisons, the average crime rate two years (three years) before and the average crime rate two years (three years) after the legal change are compared. All changes are in percents.

reported in Table 7 for three time periods: the pre-moratorium period from 1960–1971, the period during the moratorium from 1972–1976, and the post-moratorium period from 1977–2000. Two different standard deviations are presented for each variable corresponding to the overall sample standard deviation (in brackets) and only within a state variation over time (in parentheses). Although removing the fixed effects decreases the variability in the murder and execution variables, substantial variability remains. In contrast, the variability in the demographic variables such as the percent minority and the percent in each age group decreases substantially without fixed effects. Because state fixed effects are included in the regressions, caution should be used when interpreting the coefficient estimates on the demographic variables as identification comes from such small fluctuations.

Next, we use a panel of 50 states and the District of Columbia over the 1960–2000 period. Our data include economic, demographic, crime, punishment, legal, and law enforcement

TABLE 7
Selected Descriptive Statistics; State Panel Data (1960–2000)

	Mean [Full-Sample SD] (Within-State SD) of Variable during Each Period		
Variables	Premoratorium: 1960–71	During Moratorium: 1972–76	Postmoratorium: 1977–2000
Murder rate	5.731 [4.211] (1.484)	8.394 [5.506] (1.077)	7.567 [7.471] (1.750)
Number of executions	0.312 [1.134] (0.607)	0 [0] (0)	0.558 [2.464] (0.827)
Per capita real income	8732.938 [1861.607] (1088.086)	11284.240 [1825.755] (385.030)	13892.350 [2583.784] (1524.888)
Unemployment rate	4.662 [1.541] (1.004)	6.119 [2.160] (1.583)	6.149 [2.126] (1.690)
Police employment	617.439 [737.761] (165.589)	904.686 [999.118] (48.183)	1037.707 [1098.832] (115.571)
Percent minority	12.640 [13.944] (0.718)	14.076 [13.965] (0.477)	17.204 [14.023] (1.502)
Percent 15–19 years old	6.591 [1.916] (0.965)	6.045 [2.132] (0.171)	7.003 [1.141] (0.809)
Percent 20–24 years old	5.648 [1.798] (0.729)	5.859 [2.092] (0.311)	7.040 [1.215] (0.692)

Notes: Each cell in the table reports the mean, full-sample standard deviation in brackets, and within-state standard deviation in parentheses of annual, state-level observations for the three periods. See appendix for data details and sources.

variables. Panel data allow us to make a strong deterrent inference by exploiting cross-state differences, particularly the staggered timing of the moratorium across states. We use fixed-effects estimation to control for unobserved heterogeneity across states and avoid the bias caused by the correlation between state-specific effects and other control variables.[17]

We also control for possible heteroskedasticity and nonnormality of regression errors that result from variation in the size of states. Because the dependent variable and most control variables are in per capita rates, we use state population as the weight in our generalized least squares estimation. In addition, we use robust standard errors to correct for any residual heteroskedasticity of unknown form or nonnormal error distributions. These corrections yield consistent estimates of the variance of coefficient estimates, leading to estimation efficiency. The standard errors are further corrected for possible clustering effects—dependence within clusters (groups, which are states here). The correction leads to a more efficient estimation with little loss in accuracy if the clustering effect is absent.

Our baseline regression model consists of a single equation with the murder rate as the dependent variable and various regressors as control variables.[18] We use the economic model of crime and the death penalty literature to specify these regressors.[19] They include three deterrent variables: the number of executions in the states, the number of executions lagged by one year to allow for adjustment in criminals' behavior, and a binary variable that equals 1 for every year during a state's moratorium and 0 otherwise. The economic variables include real per capita personal income and the unemployment rate. The demographic variables are the percentages of population age 15–19, age 20–24, and belonging to a minority group. Age and race variables are included because of the differential treatment of youth by the justice system, variation in the opportunity cost of time through the life cycle, and racially based differences in opportunities. We also include full-time state police employees as a nonpunishment deterrent factor; enhanced police presence may increase detection and apprehension, deterring some criminal activities.[20] Other controls include state-specific unobservable characteristics that are estimated through fixed effects and a set of decade-specific dummy variables that capture long-term national trends in crime.[21]

The seven primary models reported in Table 8 differ in terms of their deterrent variables: executions, lagged executions, and the state moratorium binary variable. The first three models include only one deterrent variable; models 4, 5, and 6 include some combination of two deterrent variables; and model 3 includes all three deterrent variables.

In all models, the estimated coefficients of the deterrent variables are highly significant. Executions and lagged executions have negative coefficients, indicating that executions reduce murders. The state moratorium variable has a positive coefficient, suggesting that banning executions increases the murder rate, or alternatively, reinstating the death penalty reduces the murder rate. These estimates suggest that both adopting a capital statute and exercising it have strong deterrent effects.

Moreover, the significant execution and moratorium coefficients in the equations that include both variables (models 5, 6, and 7) suggest that the deterrent effect of executions is quite distinct from the deterrent effect of a death penalty statute. The frequency of executions increases the magnitude of the deterrent

17. Another advantage of the data set is its resilience to common panel problems, such as self-selectivity, nonresponse, attrition, or sampling design shortfalls.

18. In this section, we only report the main results. The robustness of the results to estimation and specification choices is examined in the next section.

19. We do not include death sentences (or convictions) as a control for two reasons. First, data for this variable are not available during one-third of our sample period; including this variable would lead to excluding many years of data. Second, this variable is not expected to have a significant effect on murders due to the "weakness" or "porosity" of the state's criminal justice system and reversibility of the death sentences. For example, if criminals know that the justice system issues many death sentences but that the executions are not carried out, then they may not be deterred by an increase in the likelihood of a death sentence. In fact, Liebman et al. (2000) report that nearly 70% of all death sentences issued between 1973 and 1995 were reversed on appeal at the state or federal level. Also, six states sentence offenders to death but have performed no executions, reflecting the indeterminacy of a death sentence and its ineffectiveness in deterring murders. Dezhbakhsh et al. (2003) examine the effect of death sentences on murder and find support for this argument.

20. Ideally, we could also use the arrest rate as a deterrent variable; unfortunately, state-level arrest data are not available for many years of our sampling period.

21. Estimates of over 50 coefficients corresponding to these variables are not reported for space economy.

TABLE 8
Murder Rate Regression Results; State Panel Data (1960–2000)

	Coefficients Estimates for Various Models						
Regressors	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Executions	−0.1452 (−10.58***)	—	—	−0.0874 (−3.19***)	−0.1446 (−10.71***)	—	−0.0877 (−3.19***)
Executions lagged	—	−0.1630 (−6.59***)	—	−0.0995 (−2.97***)	—	−0.1625 (−6.66***)	−0.0988 (−2.95***)
State moratorium	—	—	0.8720 (4.25***)		0.8619 (4.02***)	0.8486 (3.90***)	0.8510 (3.86***)
Per capita real income	−0.0005 (−5.61***)	−0.0005 (−5.71***)	−0.0006 (−6.31***)	−0.0005 (−5.58***)	−0.0005 (−5.84***)	−0.0005 (−5.92***)	−0.0005 (−5.57***)
Unemployment rate	−0.1466 (−2.44**)	−0.1530 (−2.52**)	−0.1829 (−3.21***)	−0.1440 (−2.39**)	−0.1442 (−2.62***)	−0.1508 (−2.71***)	−0.1417 (−2.57***)
Police employment	0.0001 (0.63)	0.0000 (0.16)	0.0002 (1.06)	0.0000 (0.22)	0.0000 (0.40)	−0.0000 (−0.08)	−0.0000 (−0.02)
Percent minority	0.1671 (6.04***)	0.1611 (5.75***)	0.1656 (6.69***)	0.1627 (5.81***)	0.1724 (6.81***)	0.1672 (6.49***)	0.1688 (6.57***)
Percent 15–19 years old	−1.6070 (−8.38***)	−1.6805 (−9.03***)	−1.8350 (−9.38***)	−1.6242 (−9.00***)	−1.5904 (−8.89***)	−1.6635 (−9.60***)	−1.6069 (−9.62***)
Percent 20–24 years old	2.1516 (9.51***)	2.2169 (9.97***)	2.4128 (10.73***)	2.1565 (10.05***)	2.1144 (10.12***)	2.1819 (10.67***)	2.1212 (10.84***)
Time and state-specific control variables included							
Adjusted R^2	0.817	0.817	0.807	0.820	0.822	0.822	0.825

Notes: The dependent variable in all equations is the murder rate. *t*-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Estimates for the state specific fixed effects and time trend variables are not reported.

effect in states have death penalty laws. Similarly, the deterrent effect is weaker for states that have the death penalty but do not exercise it. An implication of this finding is that reinstating the death penalty has a weaker effect on criminal behavior than suspending it, because the latter stops executions but the former does not necessarily start them. This result provides justification for the asymmetric effect of suspending versus reinstating the death penalty seen in the before-and-after comparisons of the previous section.

The effects of the other variables on murder are also consistent across models. Murder is negatively related to both per capita real income and the unemployment rate, as indicated by the significant negative coefficients for these variables. The effect of income is consistent with economic theory; as income increases, the opportunity cost of apprehension increases, and murders decrease. Several studies have found a similar relationship between income and crime (see, e.g., Gould et al. 2002; Shepherd 2004). In contrast, the negative relationship between murder and unemployment is not intuitive. Nevertheless, this result accords with existing empirical evidence (see, e.g., Mocan and Gittings 2003; Katz et al. 2003; Ruhm 2000). In general, unemployment is more likely to affect property crimes than murder.

The demographic variables all have the expected relationships with murder rates. The minority variable has significantly positive coefficients in all models, suggesting a positive relationship with murder. Many minority groups have fewer legitimate earning opportunities, and thus a lower opportunity cost of criminal activities relative to their white counterparts. The variable for the percentage of the population age 20–24 has a significantly positive coefficient. The percentage of the population that is 15–19, on the other hand, has a negative and significant relationship with murder. The contrasting signs on the age variables are consistent with existing research that finds that most violent crimes are committed by offenders in their early twenties as in Tonry (1996). Other studies have found similar

TABLE 9
Alternative Specifications: State Panel Data (1960–2000)

Deterrent Coefficients Estimated	Variations on the Main Estimated Specification			
	Including State Prison Populations	Including State-Specific Trends	Including Aggravated Assault & Robbery Rates	Negative Binomial (MLE) Regression
Executions	−0.0706 (−4.63***)	−0.1490 (−4.72***)	−0.1483 (−10.86***)	−0.0182 (−8.29***)
Lag executions	−0.0824 (−3.84***)	−0.1588 (−3.91***)	−0.1660 (−6.90***)	−0.0209 (−6.51***)
State moratorium	0.6905 (3.54***)	0.9111 (4.09***)	0.5981 (3.17***)	0.0493 (2.33**)

Notes: The dependent variable is the murder rate in all regressions except for the negative binomial regression, where it is the number of murders. The regressions reported here are alternative specifications of models 1, 2, and 3 from Table 8. All regressions include the full set of controls from the primary state-level regressions that are reported in Table 8. Estimates for the control variables are not reported. *t*-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

relationships between these demographic variables and murder.[22]

Only police employment has no significant relationship with murder.[23] Police employment can play a dual role in crime. More police may result in less crime, due to the deterrence factor, but it also may increase crime reporting. Studies searching for either of the two effects may find insignificant estimates of the net effect. For example, Cornwell and Trumbull (1994) examine the latter effect, and Zimmerman (2004) searches for the former effect, but both report insignificant effects (main equation estimates in table 3 of both studies). Other studies find that police employment can be either positively or negatively related to crime rates; see Donohue and Levitt (2001) for a discussion. Thus, it is not surprising that our measure of total police employment, aggregated across gender and race, has no consistent relationship with murder.

Our results do not necessarily imply that all murders are deterrable. For example, crimes committed by the mentally ill may not be deterrable. The inclusion of nondeterrable murders in the murder rate might dilute the measurement, reducing the significance of the estimated coefficients. The fact that our regressions still produce highly significant coefficient estimates suggests that many kinds of murders are deterrable.

V. ROBUSTNESS CHECKS

We demonstrate the robustness of the results to further strengthen the deterrence inference. We first examine the sensitivity of the results to changes in regressors, functional form, data, and estimation method. These checks are performed using state-level data, because results based on national data have already been checked for sensitivity to model specification; see section II. Moreover, we examine whether the deterrent effect of capital punishment is a spurious finding stemming from common crime patterns.

Regression Specification and Estimation Method

To check the robustness of the regression results to model specification and estimation method, we conduct a battery of tests involving 89 additional regression equations. The resulting estimates for 12 regression equations are reported in Table 9. The estimation results for the remaining 77 regression equations are not reported for brevity, but frequency distributions summarizing the estimated coefficients of the variables of interest are reported graphically.[24]

We first examine whether our deterrence finding is driven by other state-specific factors that may change over time. For example, over

22. See Ehrlich (1977), Shepherd (2002), and Glaeser and Sacerdote (1999), who report that crime rates are higher for large cities than for medium-size cities. Large cities often have a large concentration of minority population.

23. Rubin and Dezhbakhsh (2003) and Dezhbakhsh and Rubin (1998) find a negative relationship between police payroll and crimes, but such data are not available for our sampling period.

24. Detailed estimates of these 77 regression equations are available on request.

the past two decades the United States has witnessed a large increase in the number of incarcerated individuals; for example, see Levitt (1996). This can be a contributing factor to the decline in crime rates during the 1990s. To ensure that our results are not attributed to the incapacitation effect of imprisoning more offenders rather than conducting more executions, we reestimate models 1, 2, and 3 in Table 8 including state per capita prison populations. Results are reported in Table 9. All of the deterrent variables continue to have the same sign as those in Table 8, and the estimates are all significant at the one-percent level.[25] The deterrence results reported here, therefore, do not seem to be an artifact of incarceration.

The deterrence results may also reflect unobservable variation in within-state murder rates, resulting from state-specific factors that change over time. To control for these effects we reestimate models 1, 2, and 3 in Table 8 including state-specific time trends. Again, the results reported in Table 9 show that all deterrent variables maintain their sign, and all estimates are significant at the 1% level. Thus, our main findings are not driven by state-specific temporal changes.

Given that some murders are the by-products of violent activities, such as aggravated assault and robbery, some authors attribute any observed deterrent effect of capital punishment to a shift in the propensity to commit crime. The incidence of other violent crimes then exerts large effects on murders. For example, Narayan and Smyth (2004) report that the deterrent effect of capital punishment in time-series data is not robust to the inclusion of aggravated assault rates and robbery rates as explanatory variables. To ensure that changes in other violent crimes are not driving our results, we estimate models 1, 2, and 3 in Table 8 including rates of aggravated assaults and robberies as control variables. The deterrent coefficients continue to maintain their sign and significance at the 1% level as indicated by the results in Table 9. This rules out that the argued substitution effect is a cause of the deterrence finding.

Next we examine the robustness of the findings to the choice of the estimation method. Some recent studies argue that the incidence of crime should be analyzed as a count measure rather than a proportion; see, for example, Plassman and Tideman (2001) and Grogger (1990). The proper estimation methods using count data are Poisson and negative binomial regressions. The Poisson model imposes the restriction that the conditional mean and variance of the count variable are equal; see Cameron and Trivedi (1990). Negative binomial regression does not make this assumption. The estimation also restricts predicted values to be nonnegative. To examine whether our findings are robust to estimation choice, we use the maximum likelihood method to reestimate models 1, 2, and 3 in Table 8 using a negative binomial model of conditional mean. The dependent variable in this case in the incidence of murder rather than its rate. The results are reported in Table 9. All deterrent coefficients estimates maintain their sign. The coefficient estimates for executions and lagged executions are significant at the 1% level, and the coefficient estimate for the moratorium variable is significant at the 5% level.

We expand the robustness checks by considering other variations in our specification and/or estimation. These checks are perhaps less substantive that the ones already performed but still important. They involve estimating 77 regression equations. Rather than reporting the resulting coefficient estimates, we present their frequency distributions. We start by changing the regressors in the seven primary models reported in Table 8. Five variations are considered leading to 35 models (7×5). The variations include dropping all regressors that are not deterrent variables, dropping the long-term trend variables, adding a measure of states' partisan tendencies, replacing the minority variable with detailed measures of racial distribution, and adding other age-related variables.

The estimated coefficients of executions, lagged executions, and the moratorium variable in the regressions that include only the deterrent variables are all significant—one at the 5% level and the rest at the 1% level— and identical in sign to the primary models. Dropping the time trend from the set of regressors

25. The estimated coefficients (t-statistics) for the state per capita prison population variable in the three regressions is: –7.90 (6.86***) when the capital punishment variable is the number of executions, –7.90 (6.88***) when the capital punishment variable is the number of lagged executions, and –9.33 (9.63***) when the capital punishment variable is the state moratorium indicator variable.

also does not alter our results. The deterrent variables in all seven models have the same sign as before and are significant at the 1% level. The control variables maintain their significance and signs in these regressions as well.

In addition, adding a measure of the states' partisan influences does not affect our results. We define partisan influence as the percentage of the statewide vote received by the Republican presidential candidate in the most recent election. This variable measures political pressure to "get tough on crime" through the appointment of new judges and prosecutors or other changes to the makeup of the justice system.[26] The variable is insignificant in all seven models, and its inclusion does not affect the signs or significance of the deterrent, demographic, and economic variables.

We next replace the minority variable with its breakdown into percent African American; percent Asian, native Hawaiian, or Pacific Islander; percent Native American or Native Alaskan; and percent other minorities (mostly Hispanics). The deterrent variables continue to be significant at the 1% level with signs identical to the primary models. The economic and age variables are also unaffected. Among the minority measures, the African American variable and the other minorities variable are significant and positive in all equations, suggesting a higher murder rate in areas with high concentrations of African Americans and Hispanics. The pattern of coefficient estimates for the other minority groups is not as consistent, although the coefficients of the Asian American variable tend to be negative in most equations.

Finally, we add two new age-related variables: percentages of population age 10–14 and age 25–29. The deterrent variables in all seven equations have the same signs and significance as those in Table 8. Other variables in the models also show little change. The 10–14-year-old population has a negative and significant effect on murders, but the 25–29-year-old population does not seem to have a significant effect on murders.

Next, we examine the robustness of our results to the functional specification by dropping the linearity assumption. We estimate the seven models in Table 8 using a log-linear model.[27] All of the deterrent variables continue to have the same sign as those in Table 8, and the estimates are all significant at the 1% level. The demographic and economic variables also have the same signs and similar statistical significance. These striking results suggest that the deterrence finding is not sensitive to the functional form of the regression equations.

Police employment data were not reported for many states during the 1960–64 period and are unavailable for Hawaii and the District of Columbia during our entire sampling period. Because police employment changes gradually and along a trend line, we extrapolated the 1960–64 observations for the states that did not report them.[28] To examine the robustness of our findings to the inclusion of these extrapolated data, we estimate all seven regression models once without police employment and once with unextrapolated police employment. In all 14 resulting models, the deterrent and control variables have the same signs and similar statistical significance.

Furthermore, to examine the sensitivity of our results to using population as weight, we also run all seven models in Table 8 without these weights. All deterrent variables maintain their sign and statistical significance, and results are remarkably similar to those reported in Table 8.

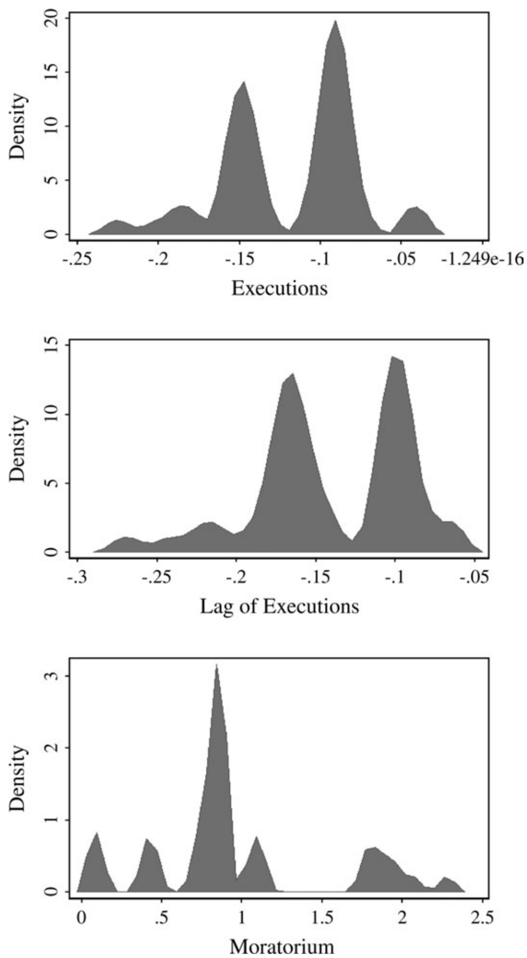
Finally, to examine the effect of a change in the way standard errors are computed on the significance of the deterrent coefficient estimates, we reestimate the seven models in Table 8 once without controlling for the clustering effect and once without robust standard errors. Note that controlling for the clustering effect and using robust standard errors adjusts the variance-covariance matrix of the estimated coefficients, but not the estimated coefficients themselves. The significance levels of the resulting coefficient estimates are remarkably similar to those reported in Table 8, so all these coefficients maintain their statistical significance even if the efficiency enhancing covariance adjustments are not made.

26. "Get tough on crime" is a popular message with many Republican candidates.

27. For observations with no executions, we changed the execution count from 0 to 0.01 to avoid undefined log values.

28. We dropped Hawaii and the District of Columbia from our regression analysis because they report no police employment data for our time period. Hawaii never had a capital statute during our sampling period, and the District of Columbia abolished the death penalty in 1972.

FIGURE 4
Cross-Model Frequency Distributions of the Coefficient Estimates for the Deterrent Variables



Notes: The frequency distributions for the deterrent coefficient estimates are estimated using biweight kernel with optimal bandwidth.

We use the regression results to construct a frequency distribution for the three main deterrent coefficient estimates, corresponding to executions, lagged executions, and moratorium. Given that dropping clustering correction or robust standard errors does not produce different coefficient estimates, only one set of these estimates are used in construction of the frequency distributions. Including both would have resulted in repetition and double counting, so the frequency distributions are based on 70 regression equations, rather than 77.

Figure 4 presents a graphical summary of these robustness checks. Each of the three charts shows the frequency distributions of the estimated coefficient of one of the deterrent variables from the 70 regression equations.[29] The distribution of the coefficient estimates for execution and lagged execution are entirely concentrated in the negative range, implying that more executions lead to fewer murders. These two distributions are bimodal, because models that include both variables yield smaller coefficient estimates than models that include only one. The distribution of the coefficient estimates for the moratorium variable is entirely concentrated in the positive range, implying that moratoriums increase murders. The few near-zero outliers arise from the scaling effect in the log-linear equations that combine regressors that are in log form with the binary moratorium variable. The concentration of distributions in all three cases suggests that the estimated models are not misspecified.

The robustness checks confirm that our results are not driven by the choices of control variables, functional form, data peculiarities, and estimation method. The consistency of the signs and significance of the deterrent variables across the 84 regression models strongly suggests that our deterrence finding is not an econometric artifact.

Effect of the Death Penalty on Other Crimes

Is the observed pattern in the murder rate driven by the death penalty statute and frequency of executions or by broader criminality and enforcement factors that affect all kinds of crimes? To answer this question, we test whether the death penalty has an effect on other crimes that are not punishable by death. Similarity of the relationship between the moratorium and these crimes with the relationship between the moratorium and murder would suggest that murder rate changes are perhaps the result of broader trends in criminal behavior affecting all crimes. An asymmetric result, however, would refute the hypothesis that the deterrence finding is spurious.

29. Biweight kernel density estimation with optimal bandwidth is used to smooth out the frequency distributions. Also, note that each deterrent variable appears in four of the seven models, so with 70 regression equations, each frequency distribution is based on 40 data points.

For this robustness check, we estimate the relationship between capital punishment and property crimes. We choose property crime, rather than nonmurder violent crimes, for this robustness check because violent crimes may be affected by the severity of punishment for murder because they often result in unintended murders. An armed robber may shoot a policeman to escape, or a rapist may kill his victim to conceal his crime.[30] In contrast, the property crimes we consider—burglary, larceny, and auto theft—do not involve contact between perpetrators and victims and should not be affected by the harshness of punishment for murder.

Because the ban on the death penalty and its subsequent reinstatement might also reflect changing judicial attitudes toward crime, we do expect some comovements between various kinds of crimes. Thus, the proper metric for this robustness check must be the extent of dissimilarity, rather than similarity, of the moratorium's effects on various crimes. We conduct before-and-after comparisons and estimate regressions using both national and state-level data for 1960–2000.

The before-and-after comparisons for property crime rates using national data reveal that the direction of the property crime changes varies over the three horizons. The two- and three-year averages show an increase in property crime after 1972 (9.9% and 20.4%) as well as after 1976 (1% and 10.2%), indicating that the postmoratorium movement in property crime is the opposite of that for murder. The one-year comparisons show no change after the introduction of the moratorium and a 4.3% drop after its lifting. In sum, the moratorium seems to have different effects on property crimes than it has on murders.

Before-and-after comparisons using state-level data yield similar results. The introduction of the moratorium in 1972 seems to coincide with an increase in property crimes that is similar to murder, especially in the two-year and three-year averages. However, contrary to murders, property crimes increase after the lifting of the moratorium. In fact, 77% of the states that reinstated the death penalty experienced an increase in their three-year average property crime rates. The opposite movements in murders and property crimes after the moratorium suggest that the deterrent finding is not the result of general trends in crime. The regression results that follow provide further support for this argument.

Using national data, the regressions of property crime both on execution and on lagged execution produce coefficient estimates that are statistically insignificant, with p-values of 0.304 and 0.122, respectively. A similar result is found for the binary moratorium period also produced a highly insignificant coefficient estimate with a p-value of 0.611. These estimates suggest that property crime during the moratorium period was not higher than its mean for the rest of the sample.

We find similar results in the state-level panel regressions. We estimate all seven primary regressions from Table 8 with property crime as the dependent variable. Table 10 reports the results of these regressions. The insignificant coefficients on execution, lagged execution, and the moratorium dummy variable in all seven regressions indicate that the death penalty has no predictable effect on property crimes. As expected, crimes that are not punishable by death are unaffected by changes in death penalty statutes or the frequency of executions.

Several of the control variables also have different relationships with property crime and murder. Although the coefficients on the income variable are mostly insignificant, the unemployment rate has a significant, positive effect on property crime. Other studies have found a similar positive relationship between unemployment and property crimes; see Gould et al. (2002). The minority population also has a different relationship with property crime.[31] Consistent with other studies, the percent of the population that belongs to a minority group is negatively related to the property crime rate. The age and police employment variables have similar effects on murders and property crime. The percentage of the population that is age 15–19 has a significant, negative effect on property crime, and the percentage of the population that is age 20–24 has a significant positive effect on property crime. The coefficients on police employment remain insignificant.

30. Another reason we examine property crimes is because many violent crimes were punishable by death in the 1960s.

31. Byrne et al. (1996) have also found violent and property crimes to be affected differently by various control variables.

TABLE 10
Property Crime Rate Regression Results; State Panel Data (1960–2000)

	Coefficients Estimates for Various Models						
Regressors	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Executions	−4.078 (−0.62)	—	—	1.254 (0.14)	−3.992 (−0.61)	—	1.203 (0.13)
Executions lagged	—	−3.907 (−0.54)	—	−4.818 (−0.48)	—	−3.824 (−0.53)	−4.698 (−0.47)
State moratorium	—	—	144.511 (1.28)	—	144.232 (1.28)	131.604 (1.17)	131.572 (1.17)
Per capita real income	0.116 (1.69*)	0.100 (1.51)	0.115 (1.71*)	0.998 (1.51)	0.117 (1.73*)	0.101 (1.56)	0.100 (1.55)
Unemployment rate	103.053 (3.18***)	98.572 (3.09***)	102.375 (3.27***)	98.441 (3.07***)	103.443 (3.27***)	98.913 (3.17***)	98.788 (3.15***)
Police employment	0.130 (1.51)	0.109 (1.14)	0.127 (1.43)	0.109 (1.14)	0.123 (1.43)	0.102 (1.06)	0.102 (1.06)
Percent minority	−81.331 (−6.84***)	−80.441 (−6.79***)	−80.634 (−6.89***)	−80.463 (−6.80***)	−80.445 (−6.89***)	−79.496 (−6.83***)	−79.518 (−6.84***)
Percent 15–19 years old	−418.944 (−3.68***)	−427.249 (−3.92***)	−422.914 (−3.75***)	−428.058 (−3.91***)	−416.165 (−3.63***)	−424.602 (−3.86***)	−425.379 (−3.86***)
Percent 20–24 year old	679.133 (4.84***)	703.396 (5.24***)	681.176 (4.91***)	704.262 (5.22***)	672.945 (4.71***)	698.010 (5.11***)	698.843 (5.09***)
Time and state-specific control variables included							
Adjusted R^2	0.835	0.832	0.836	0.832	0.836	0.833	0.833

Notes: The dependent variable in all equations is the property crime rate. t-statistics are in parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Estimates for the state specific fixed effects and time trend variables are not reported.

The asymmetry between the executions/moratorium effect on murder rates and property crime rates—one statistically significant and the other insignificant—suggests that the deterrence findings reported earlier are not spuriously caused by general crime patterns. However, one may suspect that the statistical insignificance for property crimes is driven only by one of its components (auto theft, larceny, or burglary). In that case, the other two components are affected significantly by executions and moratorium, weakening our robustness evidence.

As an additional check, we repeat the above state-level regressions for disaggregated measures of property crime: burglary, larceny, and auto theft. Table 11 reports the resulting coefficient estimates for the main variables of interest. Obviously, there is not just one component that exhibits insignificant results. Similar to what we reported for the aggregate property crimes, moratorium, executions, and lag executions have no significant effect on auto theft or larceny rates. The burglary rate shows some effect as 8 of the 12 deterrent coefficients are significant, although only 4 at the 5% level and none at the 1% level. Therefore, the asymmetry reported for the aggregate property crime seems to hold in general for the components of property crime.

VI. CONCLUDING REMARKS

This article provides evidence for the deterrent effect of capital punishment by analyzing the moratorium on executions as a controlled judicial experiment. The results are especially pertinent because several states are currently considering changing their position on capital punishment. For example, the governor of Massachusetts has recently sought to reinstate the death penalty. The governors of Illinois and Maryland suspended executions in 2000 and 2002, respectively, although Maryland resumed death penalty sentences in 2003. The North Carolina senate passed legislation imposing a two-year moratorium while state officials conduct a thorough examination of the state's death penalty system, but the

TABLE 11
Property Crime Component Regression Results, State Panel Data (1960–2000)

	Coefficients Estimates for Various Models						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable: auto theft rate							
Selected regressors							
Executions	−0.667 (−0.71)	—	—	−0.112 (−0.07)	−0.653 (−0.69)	—	−0.120 (−0.08)
Executions lagged	—	−0.492 (−0.44)	—	−0.410 (−0.23)	—	−0.480 (−0.43)	−0.392 (−0.22)
State moratorium	—	—	23.211 (1.09)	—	23.165 (1.09)	19.794 (0.94)	19.798 (0.94)
Dependent variable: larceny rate							
Selected regressors							
Executions	2.338 (0.62)	—	—	3.444 (0.62)	2.362 (0.62)	—	3.430 (0.62)
Executions lagged	—	2.762 (0.62)	—	0.260 (0.04)	—	2.784 (0.63)	0.292 (0.05)
State moratorium	—	—	39.658 (0.54)	—	39.823 (0.54)	35.436 (0.48)	35.344 (0.48)
Dependent variable: burglary rate							
Selected regressors							
Executions	−5.314 (−2.56**)	—	—	−2.513 (−0.90)	−5.274 (−2.56**)	—	−2.538 (−0.91)
Executions lagged	—	−5.613 (−2.46**)	—	−3.787 (−1.25)	—	−5.572 (−2.47**)	−3.278 (−1.24)
State moratorium	—	—	67.692 (1.81*)	—	67.324 (1.78*)	64.733 (1.71*)	64.802 (1.71*)

Notes: The dependent variables for various equations are property crime components as indicated. t-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. See Table 10 for other regressors whose coefficient estimates are not reported for space economy.

legislation failed the house in 2003. Moreover, moratorium bills have recently been introduced in Pennsylvania, Tennessee, Oklahoma, Ohio, Delaware, and Nebraska.

Most previous capital punishment studies have restricted their focus to regression specifics, such as functional form and relevant variables. Using similar and often identical data, the studies have produced contrasting results using different specifications. In contrast, we approach the deterrence question from a new angle: we exploit the judicial experiment generated by the Supreme Court's effective moratorium on the death penalty between 1972 and 1976, while also drawing on substantial pre- and postmoratorium evidence. We perform before-and-after moratorium comparisons and regressions using both national time-series data and state-level panel data for 1960–2000. The results are boldly clear: Executions deter murders, and murder rates increase substantially during moratoriums. The results are consistent across before-and-after comparisons and regressions regardless of the data's aggregation level, the time period, or the specific variable used to measure executions.

We also confirm that our results hold up to changes in our choice of regressors, estimation method, functional form, and alternative measure of the dependent variable. The deterrent variables' coefficients are remarkably consistent in sign and significance across nearly all of the 96 different regression models. In addition, we verify that the negative relationship between the death penalty and murder is not a spurious finding. Before-and-after moratorium comparisons and regression results reveal that the death penalty does not cause a decrease in property crimes, suggesting that the deterrent effect is not reflecting general trends in crime. It is also shown that the deterrence finding persists even when we control for aggravated assault and robbery that often lead to murder.

This convincing evidence for the deterrent effect does not necessarily indicate that capital punishment is sound policy. Although executions provide a large benefit to society by deterring murders, they also have costs; these include the harm from the death penalty's possibly discriminatory application and the risk of executing innocent people. Policy makers must weigh the benefits and costs to determine the optimal use of the death penalty.

APPENDIX: DATA DESCRIPTION AND SOURCES

Crime Rates

National Data. Crime rates are defined as the number of crimes per 100,000 population. Property crime rates and the murder and nonnegligent manslaughter rates (the willful killing of one human being by another) are obtained from the Bureau of Justice Statistics' Web site at http://149.101.22.40/dataonline/Search/Crime/State/statebystatelist.cfm.

State Data. State-level crime rates for 1960–2000 are available from the FBI's Uniform Crime Reports. These data can be accessed online at http://149.101.22.40/dataonline/Search/Crime/State/StateCrime.cfm. The New York murder rates for 1960–64 are missing from this data set, so we extracted the missing data from the Vital Statistics of the United States (1960–64) published by the National Center for Health Statistics. The New York property crime rates for 1960–64 are also missing from this data set and were unavailable; they were linearly extrapolated using the linear trend in the following five data points (for auto theft a two-year window was used to avoid a negative estimate).

Death Penalty Statutes and Execution Data

The data on changes in death penalty statutes of various states (reported in Tables 1 and 2) have been extracted from Bowers (1974), tables 1-1 and 2-1 and the Death Penalty Information center online at www.deathpenaltyinfo.org. Data on executions by state is available from the Death Penalty Information Center, www.deathpenaltyinfo.org/article.php?did=414&scid=8.

Income and Unemployment Data

Per capita income data were obtained from the Bureau of Economic Analysis at www.bea.gov/bea/regional/spi. The nominal data were changed into real using consumer price index series (with 1983/1984 as the base year) obtained from the Bureau of Labor Statistics, http://data.bls.gov/cgi-bin/surveymost?cu. Unemployment rate series were collected from the Manpower Report of the President (1960–69) and the Bureau of Labor Statistics, www.bls.gov/lau/staadoc.htm (1970–2000).

Police Expenditure and Employment

National Data. The data on state and local expenditures on police protection (aggregated over all states) are from the series *Uniform Crime Reports for the U.S.* (1960–92) and *Crime in the United States* (1993–2000). Few observations were missing and were linearly interpolated.

State Data. Data on full-time state police employees are from the Uniform Crime Reports for the United States (1960–92) and Crime in the United States (1993–2000), both published by the U.S. Department of Justice. Some of the states did not collect state-level police employment for the years 1960–64. For these states, we obtained estimates of the missing data using linear extrapolation of the growth trends in police employment for the following five years. A couple of states also had one or two missing years of data later in the sample period; we linearly interpolated this data as well. The District of Columbia and Hawaii did not report state-level police employment data.

Population and Other Demographic Variables

National Data. Population data (midyear population estimates of the Bureau of the Census) are obtained from the Bureau of Economic Analysis, www.bea.gov/bea/regional/spi.

State Data. These data were obtained from the Statistical Abstract of the United States, the Bureau of the Census (Historical Census Statistics), and Geolytics Collection (compiled in association with the Urban Institute); intervening data (noncensus years) are interpolated.

Voting Data

The data on voting in presidential elections is from the Atlas of U.S. Presidential Elections by Dave Leip. The data is available online at www.uselectionatlas.org/

REFERENCES

Albert, C. J. "Challenging Deterrence: New Insight on Capital Punishment Derived from Panel Data." *University of Pittsburgh Law Review*, 60, 1999, 321–71.

Avio, K. L. "Capital Punishment." in *The New Palgrave Dictionary of Economics and the Law*, edited by P. Newman. London: Macmillan Reference, 1998.

Bedau, H. A. *The Death Penalty in America.* New York: Oxford University Press, 1982.

Black, T., and T. Orsagh. "New Evidence on the Efficacy of Sanctions as a Deterrent to Homicide." *Social Science Quarterly*, 58, 1978, 616–31.

Bowers, W. J. *Executions in America.* Lexington: Lexington Books, 1974.

———. *Legal Homicide, Death as Punishment in America, 1864–1982.* Boston: Northeastern University Press, 1981.

Bowers, W. J., and J. L. Pierce. "The Illusion of Deterrence in Isaac Ehrlich's Work on Capital Punishment." *Yale Law Journal*, 85, 1975, 187–208.

Brumm, H. J., and D. O. Cloninger. "Perceived Risk of Punishment and the Commission of Homicides: A Covariance Structure Analysis." *Journal of Economic Behavior and Organization*, 31, 1996, 1–11.

Byrne, D., H. Dezhbakhsh, and R. King. "Unionism and Police Productivity: An Econometric Investigation." *Industrial Relations*, 35, 1996, 566–84.

Cameron, S. "A Review of the Econometric Evidence on the Effects of Capital Punishment." *Journal of Socio-Economics*, 23, 1994, 197–214.

Cameron, A. C., and P. K. Trivedi. "Regression-Based Tests for Overdispersion in the Poisson Model." *Journal of Econometrics*, 46, 1990, 347–64.

Chressanthis, G. A. "Capital Punishment and the Deterrent Effect Revisited: Recent Time-Series Econometric Evidence." *Journal of Behavioral Economics*, 18(2), 1989, 81–97.

Cloninger, D. O. "Deterrence and the Death Penalty: A Cross-Sectional Analysis." *Journal of Behavioral Economics*, 6, 1977, 87–107.

Cloninger, D. O., and R. Marchesini. "Execution and Deterrence: A Quasi-Controlled Group Experiment." *Applied Economics*, 35(5), 2001, 569–76.

Cornwell, C., and W. N. Trumbull. "Estimating the Economic Model of Crime with Panel Data." *Review of Economics and Statistics*, 76, 1994, 360–66.

Cover, J. P., and P. D. Thistle. "Time Series, Homicide, and the Deterrent Effect of Capital Punishment." *Southern Economic Journal*, 54, 1988, 615–22.

Dezhbakhsh, H., and P. H. Rubin. "Lives Saved or Lives Lost? The Effect of Concealed-Handgun Laws on Crime." *American Economic Review*, 88(2), 1998, 468–74.

Dezhbakhsh, H., P. H. Rubin, and J. M. Shepherd. "Does Capital Punishment Have a Deterrent Effect? New Evidence from Post Moratorium Panel Data." *American Law and Economics Review*, 5, 2003, 344–76.

Donohue, J. J., and S. D. Levitt. "The Impact of Legalized Abortion on Crime." *Quarterly Journal of Economics*, 116(2), 2001, 379–420.

Ehrlich, I. "The Deterrent Effect of Capital Punishment: A Question of Life and Death." *American Economic Review*, 65(3), 1975, 397–417.

———. "Capital Punishment and Deterrence: Some Further Thoughts and Additional Evidence." *Journal of Political Economy*, 85, 1977, 741–88.

Ehrlich, I., and J. Gibbons. "On the Measurement of the Deterrent Effect of Capital Punishment and the Theory of Deterrence." *Journal of Legal Studies*, 6(1), 1977, 35–50.

Ehrlich, I., and Z. Liu. "Sensitivity Analysis of the Deterrence Hypothesis: Lets Keep the Econ in Econometrics." *Journal of Law and Economics*, 42(1), 1999, 455–88.

Eysenck, H. *Crime and Personality.* London: Paladin, 1970.

Glaeser, E. L., and B. Sacerdote. "Why Is There More Crime in Cities?" *Journal of Political Economy*, 107(6), 1999, 225–58.

Gould, E. D., D. B. Mustard, and B. A. Weinberg. "Crime Rates and Local Labor Market Opportunities in the United States: 1979–1997." *Review of Economics and Statistics*, 84(1), 2002, 45–61.

Grogger, J. "The Deterrent Effect of Capital Punishment: An Analysis of Daily Homicide Counts." *Journal of the American Statistical Association*, 85(410), 1990, 295–303.

Hoenack, S. A., and W. C. Weiler. "A Structural Model of Murder Behavior and the Criminal Justice System." *American Economic Review*, 70, 1980, 327–41.

Jones, J. M. "Gallop Poll Analysis." *Gallop News Service,* May 19, 2003.

Katz, L., S. D. Levitt, and E. Shustorovich. "Prison Conditions, Capital Punishment, and Deterrence." *American Law and Economics Review*, 5(2), 2003, 318–43.

Layson, S. "Homicide and Deterrence: A Reexamination of the United States Time-Series Evidence." *Southern Economic Journal*, 52(1), 1985, 68–89.

Leamer, E. "Let's Take the Con out of Econometrics." *American Economic Review*, 73(1), 1983, 31–43.

Levitt, S. "The Effect of Prison Population Sizes on Crime Rates: Evidence from Prison Overcrowding Legislation." *Quarterly Journal of Economics*, 111(2), 1996, 319–52.

Liebman, J. S., J. Fagan, and V. West. "Capital attrition: Error Rates in Capital Cases, 1973–1995." *Texas Law Review*, 78, 2000, 1839–61.

Liu, Z. "Capital Punishment and the Deterrence Hypothesis: Some New Insights and Empirical Evidence." *Eastern Economic Journal*, 30(2), 2004, 237–58.

Lott, J. R. Jr., and W. M. Landes. "Multiple Victim Public Shootings." University of Chicago Law and Economics Working Paper, 2000.

Maddala, G. S., and I. M. Kim. *Unit Roots, Cointegration, and Structural Change.* Cambridge: Cambridge University Press, 1998.

McAleer, M., and M. R. Veall. "How Fragile Are Fragile Inferences? A Re-Evaluation of the Deterrent Effect of Capital Punishment." *Review of Economics and Statistics*, 71, 1989, 99–106.

McManus, W. "Estimates of the Deterrent Effect of Capital Punishment: The Importance of the Researcher's Prior Beliefs." *Journal of Political Economy*, 93, 1985, 417–25.

Mocan, H. N., and R. K. Gittings. "Pardons, Executions, and Homicides." *Journal of Law and Economics*, 46(2), 2003, 453–78.

Narayan, P. K., and R. Smyth "Dead Man Walking: An Empirical Reassessment of the Deterrent Effect of Capital Punishment Using the Bounds Testing Approach to Cointegration." Mimeo, 2004.

Passell, P., and J. B. Taylor. "The Deterrent Effect of Capital Punishment: Another View." *American Economic Review*, 67, 1977, 445–51.

Paternoster, R. *Capital Punishment in America.* New York: Lexington, 1991.

Plassman, F., and N. T. Tideman. "Does the Right to Carry Concealed Handguns Deter Countable Crimes? Only a Count Analysis Can Say." *Journal of Law and Economics,* 44, 2001, 771–98.

Rubin, P., and H. Dezhbakhsh. "The Effect of Concealed Handgun Laws on Crime: Going beyond the Dummy Variables." *International Review of Law and Economics*, 23, 2003, 199–216.

Ruhm, C. "Are Recessions Good for Your Health?" *Quarterly Journal of Economics*, 115, 2000, 617–50.

Sellin, J. T. *The Death Penalty.* Philadelphia: American Law Institute, 1959.

Shepherd, J. M. "Fear of the First Strike: The Full Deterrent Effect of California's Two- and Three-Strike Legislation." *Journal of Legal Studies*, 31(1), 2002, 159–201.

———. "Murders of Passion, Execution Delays, and the Deterrence of Capital Punishment." *Journal of Legal Studies*, 33(2), 2004, 283–321.

Silverman, B. W. *Density Estimation for Statistics and Data Analysis.* London: Chapman and Hall, 1994.

Tonry, M. *Sentencing Matters.* New York: Oxford University Press, 1996.

Yunker, J. A. "Is the Death Penalty a Deterrent to Homicide? Some Time Series Evidence." *Journal of Behavioral Economics*, 5, 1976, 45–81.

Zimmerman, P. R. "State Executions, Deterrence, and the Incidence of Murder." *Journal of Applied Economics*, 7(1), 2004, 163–93.

———. "Estimates of the Deterrent Effect of Alternative Execution Methods in the United States: 1978–2000." *American Journal of Economics and Sociology,* forthcoming.

Zimring, F. E., and G. Hawkins. *Capital Punishment and the American Agenda.* Massachusetts: Cambridge Press, 1986.