The Deterrent Effect of Capital Punishment:
Evidence from a "Judicial Experiment"*

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Abstract

Does capital punishment deter capital crimes? We use panel data covering the fifty states during the period 1960-2000 period to examine the issue. Our study is novel in four ways. First, we estimate the moratorium's full effect by using both pre- and postmoratorium evidence. Second, we exploit the moratorium as a judicial experiment to measure criminals' responsiveness to the severity of punishment; we compare murder rates immediately before and after changes in states' death penalty laws. The inference draws on the variations in the timing and duration of the moratorium across states provide a cross section of murder rate changes occurring in various time periods. Third, we supplement the before-and-after comparisons with regression analysis that disentangles the impact of the moratorium itself on murder from the effect on murder of actual executions. By using two different approaches, we avoid many of the modeling criticisms of earlier studies. Fourth, in addition to estimating 84 distinct regression models—with variations in regressors, estimation method, and functional form—our robustness checks examine the moratorium's impact on crimes that are not punishable by death. Our results indicate that capital punishment has a deterrent effect, and the moratorium and executions deter murders in distinct ways. This evidence is corroborated by both the before-and-after comparisons and regression analysis. We also confirm that the moratorium and executions do not cause similar changes in non-capital crimes. The results are highly robust.

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.[1] The death penalty's deterrent effect is certainly an important consideration for several states that are now considering moratoriums on executions.[2]

Psychologists and criminologists who initially studied the death penalty reported no deterrent effect.[3] Economists joined the debate with Ehrlich's 1975 and 1977 studies.[4] He applied regression analysis to both U.S. aggregate data for 1933-1969 and state-level data for 1940 and 1950 and found a significant deterrent effect. 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 ranged from a substantial deterrent effect to no effect or a small adverse effect.[5]

This study advances the deterrence literature by exploiting an important characteristic that other studies overlooked: 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 capital punishment's deterrent effect. As we show, the data indicate that murder rates increased immediately after the

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[1] Frankin E. Zimring & Gordon Hawkins, Capital Punishment and the American Agenda 167 (1986).
[2] North Carolina, Illinois, and Maryland have suspended executions and moratorium bills have recently been introduced in Pennsylvania, Tennessee, Oklahoma, Ohio, Delaware, and Nebraska.
[3] For example, J.T. Sellin, J. T., The Death Penalty (1959); H. Eysenck, Crime and Personality (1970).
[4] Isaac Ehrlich, The Deterrent Effect of Capital Punishment: A Question of Life and Death, 65 Am. Econ. Rev., 397 (1975); Isaac Ehrlich, Capital Punishment and Deterrence: Some Further Thoughts and Additional Evidence, 85 J. Pol. Econ. 741 (1977).
[5] A brief description of the deterrence studies appears in the next section. For literature summaries see Samuel Cameron, A Review of the Econometric Evidence on the Effects of Capital Punishment, 23 Journal of Socio-Economics 197 (1994) and K.L. Avio, Measurement Errors and Capital Punishment, 20 Applied Economics 1253 (1988).

moratorium was imposed and decreased directly after the moratorium was lifted, providing support

for the deterrence hypothesis

We compare the murder rate for each state immediately before and after it suspended or

reinstated the death penalty. Such comparison provides the basis for a strong inference for several

reasons. First, many factors that affect crime—e.g., law enforcement, judicial, demographic, and

economic variables—change only slightly over a short period of time. Therefore, quick changes in a

state's murder rate following a change in its death penalty law can be attributed to the legal change.

Second, the moratorium began and ended in different years in different states. For example,

twelve states abolished their death penalty statutes before the 1972 Supreme Court decision.[6] The

death penalty statutes of Massachusetts and Rhode Island were abolished twice, once in 1972 and a

second time in 1984.[7] In addition, for the states that rewrote their death penalty statutes during

1972-1976, the moratorium officially ended with the 1976 *Gregg* decision. However, twelve other

states did not reenact their death penalty statutes until years later, and as late as 1995 (New York,

see Table 2). Considering the different start and end dates, the duration of the moratorium varied

considerably across states, ranging from four to thirty years. Observing similar changes in murder

rates immediately after the same legal change in different years and in various states provides

compelling evidence of the moratorium's effect on murder.

Third, our results gain strength from our examination of the states that suspended and

reinstated their death penalty statutes several times. For example, Rhode Island suspended the

death penalty in 1972, reinstated it in 1977, and abolished it in 1984. Although many factors that

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[6] These states include Alaska, Hawaii, Iowa, Maine, Michigan, Minnesota, New Mexico, New York, Oregon, Vermont, West Virginia, and Wisconsin (see tables 1 and 2). Six of these states abolished their statutes during our sampling period, contributing to our analysis with a suspension date other than 1972.

[7] Note that Massachusetts reinstated the death penalty in 1982 before it abolished it all together in 1984, and Rhode Island reinstated the death penalty in 1977 before it abolished it in 1984. They are both included in this count. Delaware restored its death penalty statute twice (once in 1961); so it adds a thirteenth observation to the sample of non-1976 switches.

affect Rhode Island's murder rate differ in 1972 and 1984, the murder rate increased after both suspensions: by 13 percent after 1972 and by 25 percent after 1984 in one-year comparisons. The 1984 increase even reversed a declining trend in Rhode Island's murders. For Rhode Island and other 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 analysis that, unlike many existing studies, uses both pre- and postmoratorium data. The regressions disentangle the impact of the moratorium itself on murder from the effect of actual executions on murder. Basing our inference on two different approaches allows us to de-emphasize the difficult modeling issues that have impeded earlier regression studies.

We also apply a battery of tests to examine the robustness of our findings. These include regression sensitivity checks with 77 additional regression equations that differ in regressors, functional form, data, and estimation method. In addition, we confirm that the deterrent effect of capital punishment is not driven, spuriously, by common crime patterns. 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. However, our results indicate that the moratorium and executions deter murder, but not property crime.

The paper 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 U.S. 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 the paper.

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.

A. 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 executions each year. Although the use of capital punishment declined somewhat in the 1940s

and 1950s, executions were still much more frequent than today: approximately 130 a year in the

1940s and 75 a year during the 1950s, compared to an average of 48 per year in the 1990s. Over 65

percent of the American public approved of the death penalty during these decades.[8]

In the late 1950s, however, public support for the death penalty started to decline, reaching a

low of 42 percent in 1966.[9] 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

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[8] Raymond Paternoster, Capital Punishment in America 20 (1991).
[9] Paternoster, *supra* note 8 at 19.

of death penalty sentences.[10] Reflecting the public's growing disapproval, executions declined

steadily throughout the 1960.

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 5-4 decision, the justices held that

discretionary capital statutes resulted in arbitrary sentencing, violating the 8th 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, while 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 twenty 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

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[10] Hugo Adam Bedau, The Death Penalty in America 25 (1982).

executions in 32 states. Today, the approval rating for the death penalty is over 74 percent, down

from an all-time high of 80 percent in 1994.[11]

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, six have performed no

postmoratorium 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.

Although some of the states have performed only one or two postmoratorium executions.

B. Literature on the Deterrent Effect of the Death Penalty

In the U.S., 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.[12]

The debate in the economics literature began with Ehrlich's seminal work that first used

regression analysis to study the deterrent effect of capital punishment.[13]   His finding of a strong

deterrent effect 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

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[11] Jeffrey M. Jones, Gallop Poll Analysis, Gallop News Service, May 19, 2003.

different specifications and functional forms. For example, using the same data, Yunker,

Cloninger, Ehrlich and Gibbons, Ehrlich and Liu, and Liu have found a deterrent effect of capital

punishment.[14] In contrast, Bowers and Pierce, Passel and Taylor, and Hoenack and Weiler find no

deterrence when they use the same data with alternative specifications.[15] Similarly, McAleer and

Veall, Leamer, and McManus, find no deterrent effect when different variables are included over

the same sample period.[16] Finally, Black and Orsagh find mixed results depending on the cross-

section year they use.[17]

   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 and Cover and Thistle, for example, use identical extensions of Ehrlich's

time-series data: 1933-1977.[18] 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 employs time-series data covering 1966 through 1985 and Brumm and Cloninger use

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[12] Sellin, *supra* note 3; Eysenck, *supra* note 3; and for a general discussion, see Cameron, *supra* note 5.

[13] Ehrlich (1975), *supra* note 4; Ehrlich (1977), *supra* note 4.

[14] James A. Yunker, Is the Death Penalty a Deterrent to Homicide? Some Time Series Evidence, 5 Journal of Behavioral Economics 45 (1976); Dale O. Cloninger, Deterrence and the Death Penalty: A Cross-Sectional Analysis, 6 Journal of Behavioral Economics 87 (1977); Isaac Ehrlich & Joel Gibbons, On the Measurement of the Deterrent Effect of Capital Punishment and the Theory of Deterrence, 6 Journal of Legal Studies 35 (1977); Isaac Ehrlich & Zhiqiang Liu, Sensitivity Analysis of the Deterrence Hypothesis: Lets Keep the Econ in Econometrics, 42 Journal of Law and Economics 455 (1999); Zhiqiang Liu, Capital Punishment and the Deterrence Hypothesis: Some New Insights and Empirical Evidence, Eastern Economic J. (forthcoming).

[15] W. J. Bowers & J.L. Pierce, The Illusion of Deterrence in Isaac Ehrlich's work on Capital Punishment, 85 Yale Law Journal 187 (1975); Peter Passell & John B. Taylor, The Deterrent Effect of Capital Punishment: Another View, 67 American Economic Review 445 (1977); Stephen A. Hoenack & William C. Weiler, A Structural Model of Murder Behavior and the Criminal Justice System, 70 American Economic Review 327 (1980).

[16] Michael McAleer & Michael R. Veall, How Fragile are Fragile Inferences? A Re-Evaluation of the Deterrent Effect of Capital Punishment, 71 Review of Economics and Statistics 99 (1989); Edward E. Leamer, Let's Take the Con out of Econometrics, 73 American Economic Review 31 (1983); Walter S. McManus, Estimates of the Deterrent Effect of Capital Punishment: The Importance of the Researcher's Prior Beliefs, 93 Journal of Political Economy 417 (1985).

[17] T. Black & T. Orsagh, New Evidence on the Efficacy of Sanctions as a Deterrent to Homicide, 58 Social Science Quarterly 616 (1978).

[18] Stephen A. Layson, Homicide and Deterrence: A Reexamination of the United States Time-Series Evidence, 52 Southern Economic Journal 68 (1985); James P. Cover & Paul D. Thistle, Time Series, Homicide, and the Deterrent Effect of Capital Punishment, 54 Southern Economic Journal 615 (1988).

cross-sectional data covering 58 cities in 1985; both studies find a deterrent effect.[19] In contrast, Grogger uses daily data for California during 1960-1963 and finds no significant deterrent effect.[20]

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, Cloninger and Marchesini, Mocan and Gittings, Zimmerman, Zimmerman, and Shepherd find a deterrent effect of capital punishment. However, Albert finds no deterrent effect with state data.[21] Dezhbakhsh, Rubin, and Shepherd also find a deterrent effect using county-level panel data.[22]

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 datasets. No study has used the 1970s death penalty moratorium in the context of a controlled-group experiment.

III. Analysis of National Data

We begin our empirical analysis by examining the aggregate crime and execution data for the

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[19] George A. Chressanthis, Capital Punishment and the Deterrent Effect Revisited: Recent Time-Series Econometric Evidence, 18 Journal of Behavioral Economics 81 (1989); Harold J. Brumm and Dale O. Cloninger, Perceived Risk of Punishment and the Commission of Homicides: A Covariance Structure Analysis, 31 Journal of Economic Behavior and Organization 1 (1996).

[20] Jeffrey Grogger, The Deterrent Effect of Capital Punishment: An Analysis of Daily Homicide Counts, 85 J. of the American Statistical Association 295 (1990).

[21] John R. Lott, Jr. & William M. Landes, Multiple Victim Public Shootings, Bombings, and Right-to-Carry Concealed Handgun Laws: Contrasting Private and Public Law Enforcement, (John M. Olin Law & Economics Working paper No. 73, University of Chicago Law School, 2000); Dale O. Cloninger & Roberto Marchesini, Execution and Deterrence: A Quasi-Controlled Group Experiment, 35 Applied Economics 569 (2001); H. Naci Mocan and R. Kaj Gittings, Getting Off Death Row: Commuted Sentences and the Deterrent Effect of Capital Punishment, Journal of Law and Economics (forthcoming); Paul R. Zimmerman, Estimates of the Deterrent Effect of Alternative Execution Methods in the United States: 1978-2000, Federal Communications Commission Working Paper (2003); Paul R. Zimmerman, State Executions, Deterrence, and the Incidence of Murder, Federal Communications Commission Working Paper (2003), Joanna M. Shepherd, Murders of Passion, Execution Delays, and the Deterrence of Capital Punishment, Clemson University Working Paper (2003); Craig J. Albert, Challenging Deterrence: New Insights on Capital Punishment Derived from Panel Data, 60 University of Pittsburgh Law Review 321 (1999).

[22] Hashem Dezhbakhsh, Paul Rubin, and Joanna M. Shepherd, Does Capital Punishment Have a Deterrent Effect? New Evidence from Postmoratorium Panel Data, American Law and Economics Review (forthcoming).

United States for the period 1960-2000. We first discuss the general trends in the data. Then, we present before-and-after moratorium comparisons of murder rates and regression results. The data and sources are described in the Data Appendix. The analysis of national data motivates the more involved state-level analysis that follows.

A. General Trends

Figure 1 displays the murder rate and the number of executions in the United States for the period 1960-2000.[23] 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 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 rate.   The pattern during the remaining period (1980-1989) is not as clear or persistent, although the two series still have opposite trends.

B. 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 zero 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.[24] 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 .8 or 9.3% when the moratorium

was imposed and dropped by .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 .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 .4 or 4.1% for the respective effects.

C. Regression Results

We also 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 run a regression for each deterrent variable—executions,

lagged executions, and a moratorium dummy variable—on the murder rate over our entire sample

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[23] Following the common practice, the murder rate used in our analysis is the number of murders and nonnegligent

period, 1960-2000. We also run regressions of executions and lagged executions on the murder rate

for two sub-periods 1960-1976 (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.[25] 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 vs. 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.[26] In our state-level analysis, we explore factors that might

contribute to differences in the magnitude of the relationship between murders and executions.

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 link is more likely

to run from executions to murders.

The regression of the murder rate on the moratorium binary variable reflect the fact that

murder rates were higher during the moratorium years. The moratorium binary variable, which

takes a value of one for every year during the moratorium period and zero otherwise, is intended to

extract the gross effect of the moratorium on the probability distribution of the U.S. murder rate.

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manslaughters per 100,000 population.
[24] The year of the change (1972 or 1976) is discarded since it covers two regimes.
[25] It is interesting to note that these results are robust to the choice of functional form, as neither taking the log nor differencing the variables alters the significance of the coefficient estimates. Moreover, the observed negative relationship also holds between the murder rate and higher lags of executions.
[26] We also repeated the two-equation estimation for two other sub-periods: the 1980s and 1990-2000. Unreported results confirm that the negative relationship is significant during both decades, but much stronger during the 1990s; e.g., p-values for the coefficient estimates are (.003 and .004) and (.000 and .000) for the 1980s and 1990-

The estimated coefficient is positive and significant at the five percent 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 exploratory 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.

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 also report that unemployment has a positive effect and income has an insignificant effect on murders and other crimes. [27]

2000, respectively. While the strength of the relationship seems to change over time, its direction and significance does not.

[27] John J. Donohue and Steven D. Levitt, The Impact of Legalized Abortion on Crime, 116 Quarterly Journal of Economics 379 (2001).

IV. Analysis 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.[28] 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 fifty states and the District of Columbia for the period 1960 through 2000. The data and sources are described in the Data Appendix.

A. 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.[29] 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

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[28] Executions carried out by the federal government are quite rare; only one occurred in 1963 during our sample.
[29] The year of the change is discarded since it covers two regimes.

following the suspension (or abolition) of the death penalty.[30] The three 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.[31] 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.[32] 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 percent 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

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[30] Kernel density estimates have the advantage of being smooth and also independent of the choice of origin—
unlike histograms that 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%). Bernard
Silverman, Density Estimation for Statistics and Data Analysis (1994). Also, using optimal bandwidth diminishes
the arbitrariness inherent in choosing bandwidth—which is similar to choosing the number of bins in a histogram
[31] 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 percent, 360 percent, and 458 percent (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 percent 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.
[32] Note that the analysis, obviously, does not include several non-switching 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.

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 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.[33] The number of negative observations in Table 6 indicates that, in about 70 percent 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 re-adoption 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 reinstating

their death penalty experience 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 seemingly asymmetric strength of the deterrent

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[33] 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.

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.

B. Regression Analysis and Results

In this section, we use a panel of fifty states and the District of Columbia over the 1960-2000 period. Our data include economic, demographic, crime, punishment, legal, and law enforcement variables. Panel data allow us to make a strong deterrent inference by exploiting cross-states differences, and 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.[34]

We also control for possible heteroskedasticity and nonnormality of regression errors that result from variation in the size of states. Since 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). 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.[35] We use the economic model of crime and the death penalty literature to specify these regressors.[36] 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 one for every year during a state's moratorium and zero otherwise. The economic variables include real per capita personal income and the unemployment rate. The demographic variables are the percentages of population age 15 to 19, age 20 to 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 non-punishment deterrent factor; enhanced police presence may increase detection and apprehension, deterring some criminal activities.[37] Other

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[34] Another advantage of the dataset is its resilience to common panel problems such as self-selectivity, nonresponse, attrition, or sampling design shortfalls.

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

[36] We do not include death sentences (or convictions) as a control for two reasons. First, data for this variable is 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, Leibman, Fagan and West report that nearly 70 percent of all death sentences issued between 1973 and 1995 were reversed on appeal at the state or federal level. Liebman, J.S., J. Fagan, & V. West, Capital attrition: Error rates in capital cases, 1973-1995, 78 Texas Law Review 1839 (2000). 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, Rubin, and Shepherd examine the effect of death sentences on murder and find support for this argument. Dezhbakhsh, Rubin, and Shepherd, *supra* note 22.

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

controls include state-specific unobservable characteristics that are estimated through fixed effects, and time-specific binary variables that capture long-term national trends in crime.[38]

The seven primary models reported in Table 7 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 three 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 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

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[38] Estimates of over fifty coefficients corresponding to these variables are not reported for space economy.

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.[39] In contrast, the negative relationship between murder and unemployment is not intuitive. Nevertheless, this result accords with existing empirical evidence.[40] 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.[41] Other studies have found similar relationships between these demographic variables and murder.[42]

Only police employment has no significant relationship with murder.[43] Although one might expect that more police would improve arrest rates and lower crime, existing empirical evidence does not support this theory. Instead, other studies find that police employment can be either positively or negatively related to crime rates, depending on the racial and gender makeup of the

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[39] Eric D. Gould, David B. Mustard, & Bruce A. Weinberg, Crime Rates and Local Labor Market Opportunities in the United States: 1979-1997, 84 Rev. Econ. & Stat. (forthcoming 2003). Shepherd, *supra* note 21.
[40] Mocan & Gittings, *supra* note 21; Lawrence Katz, Steven D. Levitt, & Ellen Shustorovich, Prison Conditions, Capital Punishment, and Deterrence, (manuscript, 2001); Christopher Ruhm, Are Recessions Good for Your Health?, 115 Quarterly Journal of Economics 617 (2000).
[41] Michael Tonry, Sentencing Matters 139 (1996).
[42] Ehrlich (1977), *supra* note 4; Joanna M. Shepherd, Fear of the First Strike: The Full Deterrent Effect of California's Two- and Three-Strikes Legislation, 31 J. Legal Stud. 159 (2002).
[43] Dezhbakhsh and Rubin find a negative relationship between police payroll and crimes, but such data is not available for our sampling period. Hashem Dezhbakhsh and Paul H. Rubin, Lives Saved or Lives Lost? The Effect of Concealed-Handgun Laws on Crime, 88 American Economic Review 468 (1998).

police force.[44] 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 deterrable.[45] 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 our results to further strengthen the deterrence inference. We first examine the sensitivity of our results to changes in regressors, functional form, data, and estimation method. Then, we confirm that the deterrent effect of capital punishment is not a spurious finding stemming from common crime patterns.

A. Regression Specification

To check the robustness of our regression results, we conduct a battery of tests involving 77 additional regression equations. Estimates are not reported for brevity, but are available upon request. A discussion of the findings follows.

We first change the regressors in the seven primary models reported in Table 7. Five variations are considered leading to 35 models. 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 variables.

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[44] Donohue & Levitt *supra* note 27; John R. Lott, Jr., Does a Helping Hand Put Others at Risk?: Affirmative Action, Police Departments, and Crime, 38 Economic Inquiry 239 (2000).

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 five-percent level and the rest at the one-percent level— and identical in sign to the primary models. Dropping the time trend from the set of regressors, also, does not alter our results. The deterrent variables in all seven models have the same sign as before and are significant at the one-percent 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.[46] 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 one-percent 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 variables: percentages of population age 10-14 and age 25-29.

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[45] For a study of which crimes are deterrable see Shepherd, *supra* note 21.
[46] "Get tough on crime" is a popular message with many Republican candidates.

The deterrent variables in all seven equations have the same signs and significance as those in Table 7. 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 7 using a log-linear model. All of the deterrent variables continue to have the same sign as those in Table 7 and the estimates are all significant at the one-percent 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 1960-1964 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 1960-1964 observations for the states that did not report them.[47] To examine the robustness of our findings to the inclusion of these extrapolated data, we also estimate all seven regression models without police employment and with unextrapolated police employment. In all 14 models, the deterrent and control variables have the same signs and similar statistical significance.

Furthermore, to examine the sensitivity of our results to our estimation method, we reestimate all seven models from Table 7 with different estimation choices: (i) without population weights, (ii) without controlling for the clustering effect, and (iii) without robust standard errors. Although one may expect that all three estimations will result in different standard errors and that the estimation without population weights will also produce different coefficient estimates, the results

of the 21 models are remarkably similar to those reported in Table 7. [48] Most coefficient

estimates, and especially the deterrent estimates, maintain their signs and statistical significance.

Thus, changing the estimation method does not detract from the strength of the deterrent finding.

Figure 4 presents a 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 84

regression models (7 primary models and 77 robustness checks).[49] 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, 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.

B. *Effect of the Death Penalty on Other Crimes*

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[47] 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.

[48] Controlling for the clustering effect and using robust standard errors adjusts the variance-covariance matrix of the estimated coefficients but not the estimated coefficients.

[49] These distributions do not include estimates obtained by dropping the clustering or robustness corrections, since these corrections change the standard errors but not the coefficient estimates. Their inclusion would have resulted in repetition and double counting. Biweight kernel density estimation with optimal bandwidth is used to smooth out the frequency distributions.

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.

For this robustness check, we estimate the relationship between capital punishment and property crimes. We choose property crime, rather than non-murder violent crimes, for this robustness check because violent crimes may be affected by the severity of punishment for murder as 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. [50] In contrast, the property crimes we consider—that consist of 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

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

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 percent 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 **.**304 and

.122, respectively. A similar result is found for the binary moratorium period also produced a highly

insignificant coefficient estimate with a p-value of **.**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 7 with property crime as the dependent variable. Table 8 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.[51] The minority

population also has a different relationship with property crime.[52] Consistent with other studies,

the percent of the population that belongs to a minority group is negatively related to the property

crime rate.[53] 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.

VI. Concluding Remarks

This paper provides strong evidence for the deterrent effect of capital punishment by

analyzing the moratorium on executions as a controlled "judicial experiment." Our results are

especially pertinent now because several states have imposed new moratoriums or are considering

doing so. For example, Illinois and Maryland indefinitely suspended executions in 2000 and 2002,

respectively. In 2003, North Carolina imposed a two-year moratorium while state officials conduct

a thorough examination of the state's death penalty system. 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

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[51] Gould, Mustard, & Weinberg, *supra* note 39.
[52] Other studies have also found violent and property crimes to be affected differently by various control variables; see, e.g., John Lott & David Mustard, Crime, Deterrence and Right-to-Carry Concealed Handguns, 26 Journal of Legal Studies 1 (1997) and Dennis Byrne, Hashem Dezhbakhsh, and Randall King, Unionism and Police Productivity: An Econometric Investigation, 35 Industrial Relations 566 (1996).
[53] Shepherd, *supra* note 46; Joanna M. Shepherd, Are Criminals Like Us?: Risk Attitudes, Sentencing Guidelines, and Increased Crime, Clemson University Working Paper (2003).

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, and functional form. The deterrent variables' coefficients are remarkably consistent in sign and significance across 84 different regression models. In addition, we verify that the negative relationship between the death penalty and murder is not a spurious finding. Before-and-after moratorium comparisons and regressions reveal that the death penalty does not cause a decrease in property crimes, suggesting that the deterrent effect is not reflecting general trends in crime.

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. Policymakers must weigh the benefits and costs to determine the optimal use of the death penalty.

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Table 1: Executions and Death Penalty Laws, 1960-2000
(States Currently Without the Death Penalty)

States	No. of Executions (1960–1972)	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, e.g., Bowers, 1981).

Table 2: Executions and Death Penalty Laws, 1960-2000
(States Currently With the Death Penalty)

States	Before *Furman v. Georgia* (1960-June 1972)			Since *Furman v. Georgia* (July 1972-2000)			Total
	No. of Executions	Last Execution	Death Penalty Statute Change	No. of Executions	First Execution	Death Penalty Reenacted	
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	December 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	September 1975	50
Montana	0	-----	----------------	2	1995	March 1974	2
Nebraska	0	-----	----------------	3	1994	April 1973	3
Nevada	1	1961	----------------	8	1979	July 1973	9
N. Hampshire	0	-----	----------------	0	-----	January 1991	0
New Jersey	3	1963	Abolished 1972	0	-----	August 1982	3
New Mexico	1	1960	Abolished 1969*	0	-----	July 1979	1
New York	10	1962	Abolished 1965*	0	-----	September 1995	10
N. 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	December 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	November 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 vs. Georgia*).
* The abolition of the death penalty in New Mexico and New York was restrictive as it did not cover killing an on duty police officer or a prison guard by inmates.

Table 3: Murder Rate Regression Results; U.S. National Data (1960-2000)

Regressors ↓	Entire Period (1960-2000) Coefficients Estimates			Sub-Period 1960-1976 Coefficients Estimates		Sub-Period 1977-2000 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.

Table 4: Murder Rate Regression Results; U.S. National Data (1960-2000)

Regressors ↓	Sampling Period (1960-2000)			
	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-squared	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.

Table 5- Percentage Change in Murder Rates Before and After Suspending (or Abolishing) the Death Penalty: State Panel Data

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 Standard Error	2.811	2.213	2.480
T-statistic (p-value for the T-test)	3.57*** (0.0004)	7.34*** (0.0000)	8.81*** (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%.

**Table 6- Percentage Change in Murder Rates Before and After
Reinstating the Death Penalty: State Panel Data**

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 Standard Error	3.381	2.928	2.921
T-statistic (p-value for the T-test)	−1.87** (0.034)	−2.18** (0.017)	−1.39* (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.

Table 7: Murder Rate Regression Results; State Panel Data (1960-2000)

Regressors ↓	Coefficients Estimates for Various Models						
	(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 Year 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							
Adjusted R-squared	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 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.

Table 8: Property Crime Rate Regression Results;
**State Panel Data (1960-2000)**

| Regressors ↓ | Coefficients Estimates for Various Models | | | | | | |
	(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)	.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							
Adjusted R-squared	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.

Figure 1- U.S. Murder Rate and Executions
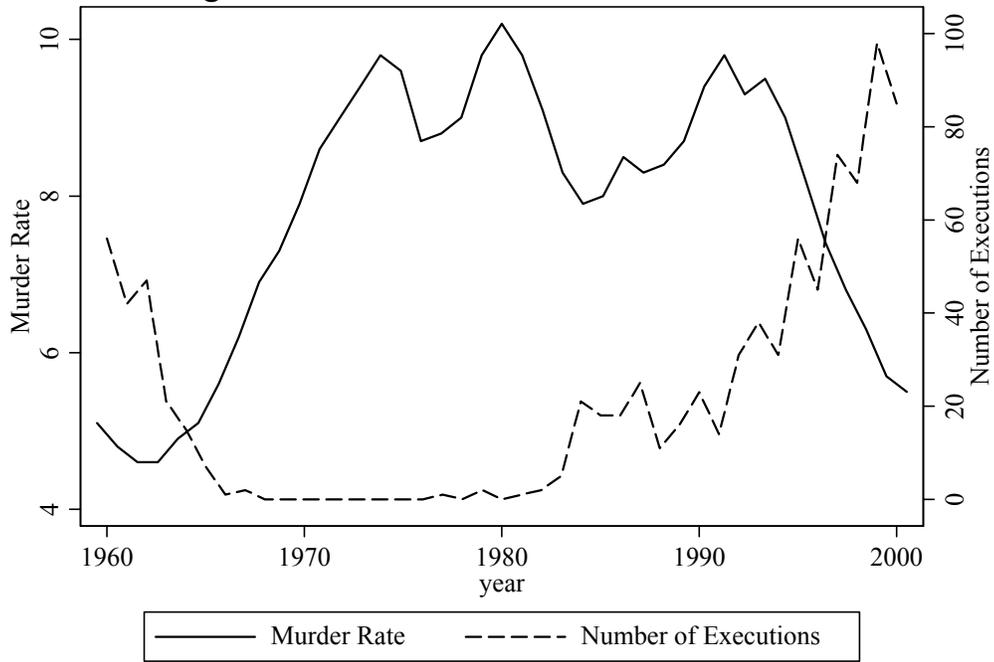
Figure 2- Distribution of the Percentage Change in Murder Rates Before and After Suspending (or Abolishing) the Death Penalty
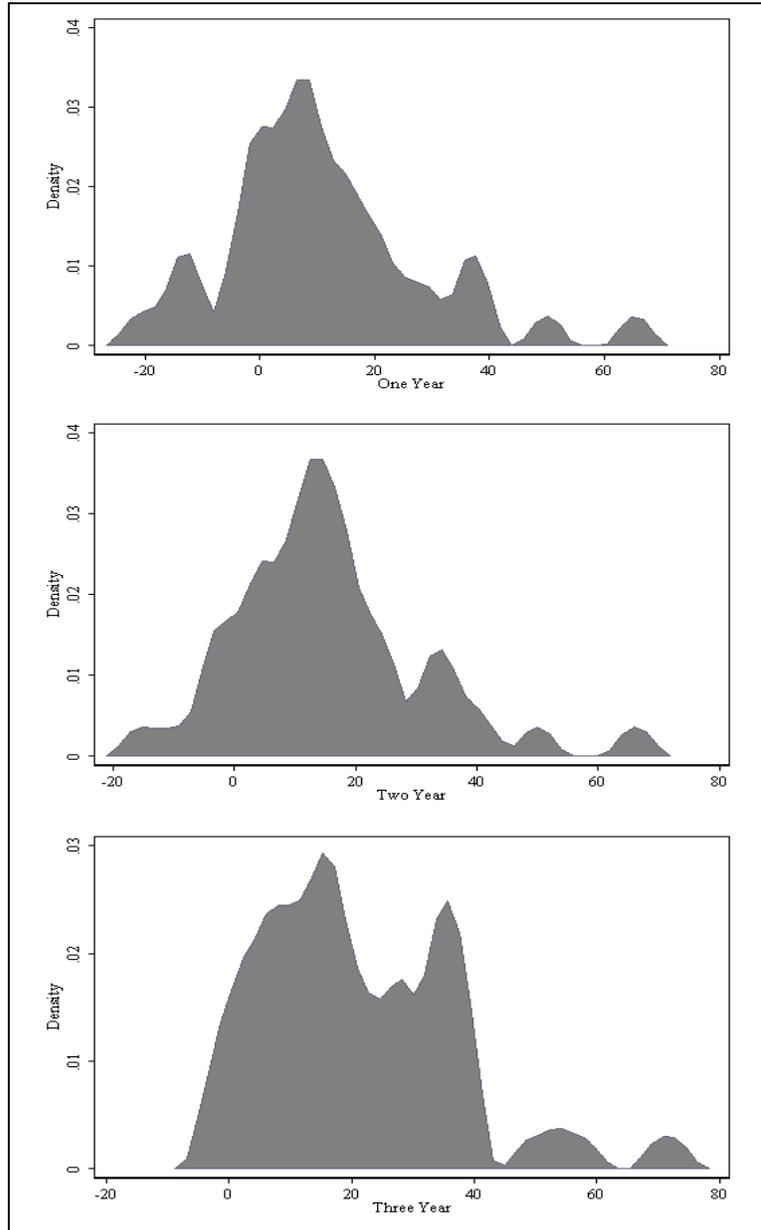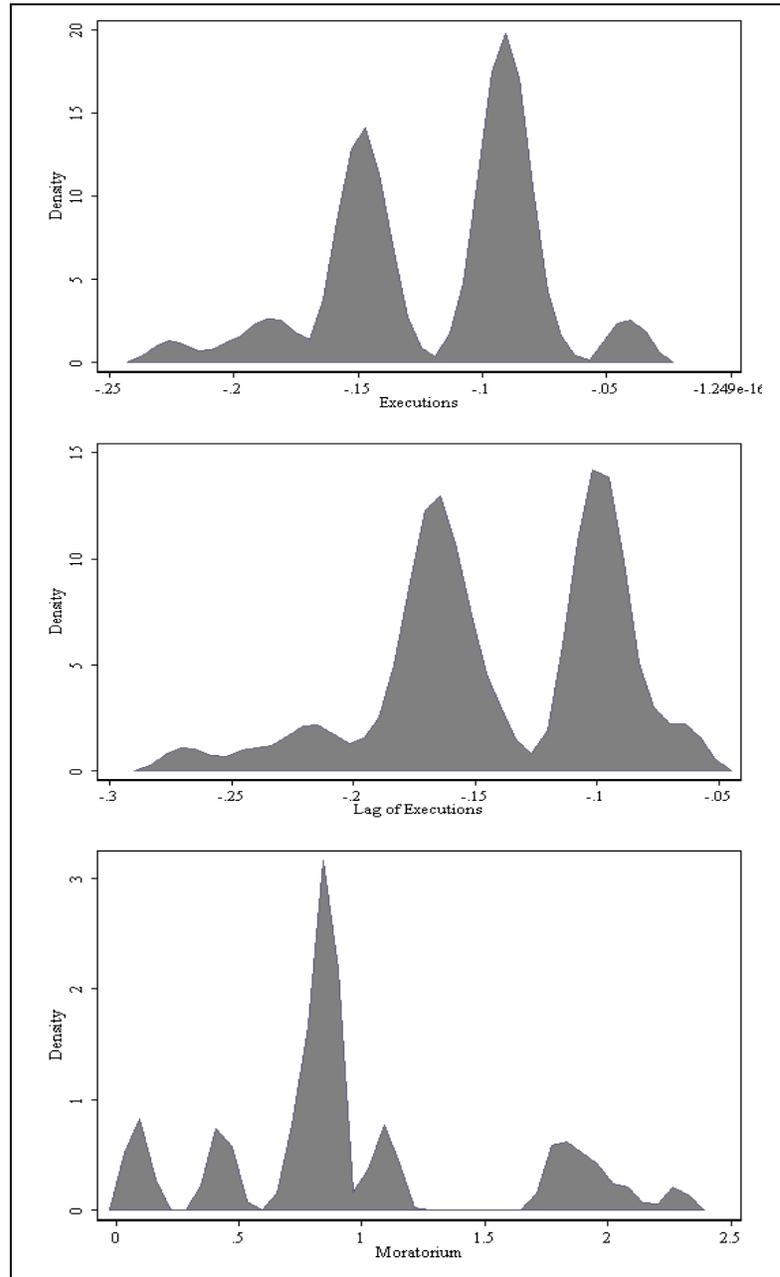

Notes: The probability densities for the murder rate changes are estimated using Biweight Kernel with optimal bandwidth. A positive value indicates an increase in the state's murder rate after the death penalty is suspended (abolished). For the one-year comparison, murder rates immediately before and 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.

**Figure 3- Distribution of the Percentage Change in Murder Rates Before
and After Reinstating the Death Penalty**
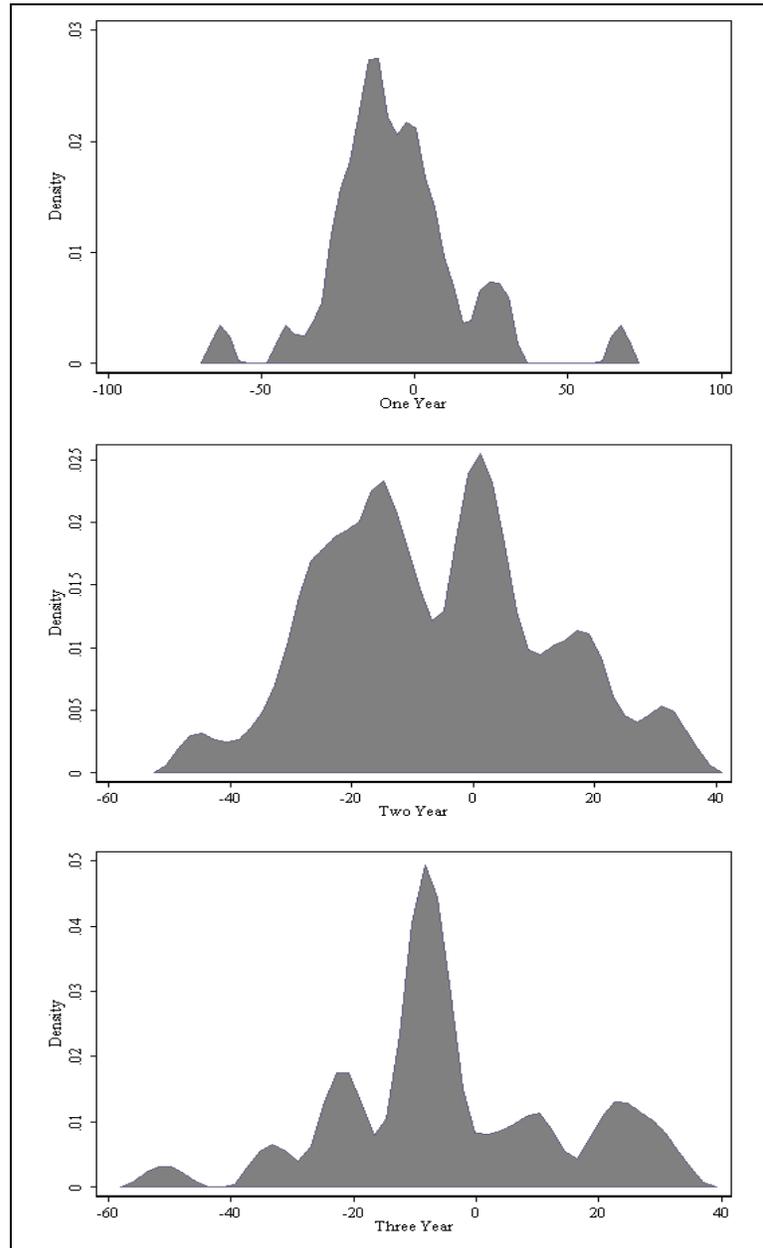
Notes: The probability densities for the murder rate changes are estimated using
Biweight Kernel with optimal bandwidth. A negative value indicates a drop
in the state's murder rate after the death penalty is reinstated. For the one-
year comparison, murder rates right before and after the change are 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.

Figure 4- Cross-Model Distributions of the Coefficient Estimates for the Deterrent Variables.

Notes: The probability densities for the deterrent coefficient estimates are estimated using Biweight Kernel with optimal bandwidth.

Data Appendix

Crime Rates

National data: Crime rates are defined as the number of crimes per 100,000 population.
Property crime rates and the murder & 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 on-line at:
http://149.101.22.40/dataonline/Search/Crime/State/StateCrime.cfm. The New York murder
rates for 1960 through 1964 are missing from this data set, so we extracted the missing data
from the Vital Statistics of the United States (1960-1964) published by the National Center
for Health Statistics. The New York property crime rates for 1960-1964 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

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 at http://www.deathpenaltyinfo.org/.

Execution Data

Data on executions by state is available from the Death Penalty Information Center:
http://www.deathpenaltyinfo.org/article.php?did=414&scid=8

Income

Per capita income data were obtained from the Bureau of Economic Analysis at
http://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.

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.* (1960-1992), 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 (1960-1992) 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 1960-1964. For these states, we obtained estimates of the missing
data using linear extrapolated 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. Washington, D.C. 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, http://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 (Complied in association with the Urban Institute). The intervening data (non-census years) are estimated using interpolation.

Unemployment Rate

Unemployment rate series were collected from the Manpower Report of the President (1960-1969) and the Bureau of Labor Statistics, http://www.bls.gov/lau/staadoc.htm, (1970-2000).

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: http://www.uselectionatlas.org/.