Does Capital Punishment Have a Deterrent Effect? New Evidence from Postmoratorium Panel Data

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Evidence on the deterrent effect of capital punishment is important for many states that are currently reconsidering their position on the issue. We examine the deterrent hypothesis by using county-level, postmoratorium panel data and a system of simultaneous equations. The procedure we employ overcomes common aggregation problems, eliminates the bias arising from unobserved heterogeneity, and provides evidence relevant for current conditions. Our results suggest that capital punishment has a strong deterrent effect; each execution results, on average, in eighteen fewer murders—with a margin of error of plus or minus ten. Tests show that results are not driven by tougher sentencing laws and are robust to many alternative specifications.

1. Introduction

The acrimonious debate over capital punishment has continued for centuries (Beccaria, 1764; Stephen, 1864). In recent decades the debate has heated up in the United States following the Supreme Court–imposed

We gratefully acknowledge helpful discussions with Issac Ehrlich and comments by Badi Baltagi, Robert Chirinko, Keith Hylton, David Mustard, George Shepherd, and participants in the 1999 Law and Economics Association Meetings, 2000 American Economics Association Meetings, and workshops at Emory University, Georgia State University, Northwestern University, and Purdue University. We are also indebted to an anonymous referee for valuable suggestions. The usual disclaimer applies.

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American Law and Economics Review Vol. 5 No. 2,
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DOI: 10.1093/aler/ahg021

moratorium on capital punishment.[1] Currently, several states are considering a change in their policies regarding the status of the death penalty. Nebraska's legislature, for example, recently passed a two-year moratorium on executions, which was, however, vetoed by the state's governor. Ten other states have at least considered a moratorium last year (''Execution Reconsidered,'' 1999, p. 27). The group includes Oklahoma, whose legislature will soon consider a bill imposing a two-year moratorium on executions and establishing a task force to research the effectiveness of capital punishment. The legislatures in Nebraska and Illinois have also called for similar research. In Massachusetts, however, the House of Representatives voted down a bill supported by the governor to reinstate the death penalty.

An important issue in this debate is whether capital punishment deters murders. Psychologists and criminologists who examined the issue initially reported no deterrent effect (See, e.g., Cameron, 1994; Eysenck, 1970; Sellin, 1959). Economists joined the debate with the pioneering work of Ehrlich (1975, 1977). Ehrlich's regression results, using U.S. aggregate time-series for 1933–69 and state-level cross-sectional data for 1940 and 1950, suggest a significant deterrent effect, which sharply contrasts with earlier findings. The policy importance of the research in this area is borne out by the considerable public attention that Ehrlich's work has received. The Solicitor General of the United States, for example, introduced Ehrlich's findings to the Supreme Court in support of capital punishment (*Fowler v. North Carolina*).

Coinciding with the Supreme Court's deliberation on the issue, Ehrlich's finding inspired an interest in econometric analysis of deterrence, leading to many studies that use his data but different regression specifications—different regressors or different choice of endogenous versus exogenous variables.[2] The mixed findings prompted a series of sensitivity analyses on Ehrlich's equations, reflecting a further emphasis on specification.[3]

1. In 1972 the Supreme Court imposed a moratorium on capital punishment, but in 1976 it ruled that executions under certain carefully specified circumstances are constitutional.

2. See Cameron (1994) and Avio (1998) for literature summaries.

3. Sensitivity analysis involves dividing the variables of the model into essential and doubtful and generating many estimates for the coefficient of each essential variable. The estimates are obtained from alternative specifications, each including some combination of the doubtful variables. See, e.g., Ehrlich and Liu (1999), Leamer (1983, 1985), McAleer and Veall (1989), and McManus (1985).

Data issues, on the other hand, have received far less attention. Most of the existing studies use either time-series or cross section data. The studies that use national time-series data are affected by an aggregation problem. Any deterrence from an execution should affect the crime rate only in the executing state. Aggregation dilutes such distinct effects.[4] Cross-sectional studies are less sensitive to this problem, but their static formulation precludes any consideration of the dynamics of crime, law enforcement, and judicial processes. Moreover, cross-sectional studies are affected by unobserved heterogeneity, which cannot be controlled for in the absence of time variation. The heterogeneity is due to jurisdiction-specific characteristics that may correlate with other variables of the model, rendering estimates biased. Several authors have expressed similar data concerns or called for new research based on panel data (see, e.g., Avio, 1998; Cameron, 1994; Hoenack and Weiler, 1980). Such research will be timely and useful for policy making.

We examine the deterrent effect of capital punishment by using a system of simultaneous equations and county-level panel data that cover the post-moratorium period. This is the most disaggregate and detailed data used in this literature. Our analysis overcomes data and econometric limitations in several ways. First, the disaggregate data allow us to capture the demographic, economic, and jurisdictional differences among U.S. counties, while avoiding aggregation bias. Second, by using panel data, we can control for some unobserved heterogeneity across counties, therefore avoiding the bias that arises from the correlation between county-specific effects and judicial and law enforcement variables. Third, the large number of county-level observations extends our degrees of freedom, thus broadening the scope of our empirical investigation. The large data set also increases variability and reduces colinearity among variables. Finally, using recent data makes our inference more relevant for the current crime situation and more useful for the ongoing policy debate on capital punishment.

Moreover, we address two issues that appear to have remained in the periphery of the specification debate in this literature. The first issue relates to the functional form of the estimated equations. We bridge the gap between theoretical propositions concerning an individual's behavior and

4. For example, an increase in nonexecuting states' murder rates aggregated with a drop in executing states' murder rates may incorrectly lead to an inference of no deterrence, because the aggregate data would show an increase in executions leading to no change in the murder rate.

the empirical equation typically estimated at some level of aggregation. An equation that holds true for an individual can also be applied to a county, state, or nation only if the functional form is invariant to aggregation. This point is important when similar equations are estimated at various levels of aggregation. The second issue relates to murders that may not be deterrable—nonnegligent manslaughter and nonpremeditated crimes of passion—and that are included in commonly used murder data. We examine whether such inclusion has an adverse effect on the deterrence inference. We draw on our discussions of these issues and the specification debate in this literature to formulate our econometric model.

The article is organized as follows: Section 2 reviews the literature on the deterrent effect of capital punishment and outlines the theoretical foundation of our econometric model. Section 3 describes data and measurement issues, presents the econometric specification, and highlights important statistical issues. Section 4 reports the empirical results and the corresponding analysis, including an estimate of the number of murders avoided as the result of each execution. This section also examines the robustness of our findings. Section 5 concludes.

2. Capital Punishment and Deterrence

Historically, religious and civil authorities imposed capital punishment for many different crimes. Opposition to capital punishment intensified during the European Enlightenment as reformers such as Beccaria and Bentham called for abolition of the death penalty. Most Western industrialized nations have since abolished capital punishment (for a list see Zimring and Hawkins, 1986, chap. 1). The United States is an exception. In 1972, in *Furman v. Georgia*, the Supreme Court outlawed capital punishment, arguing that execution was cruel and unusual punishment, but in 1976, in *Gregg v. Georgia*, it changed its position by allowing executions under certain carefully specified circumstances. There were no executions in the U.S. between 1968 and 1977. Executions resumed in 1977 and have increased steadily since then, as seen in Table 1.

As Table 2 illustrates, from 1977 through 2000 there have been 683 executions in thirty-one states. Seven other states have adopted death penalty laws but have not executed anyone. Tennessee had its first execution in April 2000, and twelve states do not have death penalty laws. Several of

Table 1. Executions and Executing States

Year	No. of Executions	No. of States with Death Penalty
1977	1	31
1978	0	32
1979	2	34
1980	0	34
1981	1	34
1982	2	35
1983	5	35
1984	21	35
1985	18	35
1986	18	35
1987	25	35
1988	11	35
1989	16	35
1990	23	35
1991	14	36
1992	31	36
1993	38	36
1994	31	34
1995	56	38
1996	45	38
1997	74	38
1998	68	38
1999	98	38
2000	85	38

Source: Snell, Tracy L. 2001. *Capital Punishment 2000*. Washington, D.C.: U.S. Bureau of Justice Statistics (NCJ 190598).

the executing states are currently considering a moratorium on executions, while a few nonexecuting states are debating whether to reinstate capital punishment.

The contemporary debate over capital punishment involves a number of important arguments, drawing either on moral principles or social welfare considerations. Unlike morally based arguments, which are inherently theoretical, welfare-based arguments tend to build on empirical evidence. The critical issue with welfare implications is whether capital punishment deters capital crimes; an affirmative answer would imply that the death penalty can potentially reduce such crimes. In fact, this issue is described as “the most important single consideration for both sides in the death penalty controversy” (Zimring and Hawkins, 1986, p. 167).

As Figure 1 demonstrates, looking at the raw data does not give a clear answer to the deterrence question. Although executing states had

Table 2. Status of the Death Penalty

Jurisdictions without a Death Penalty on December 31, 2000	Jurisdictions with a Death Penalty on December 31, 2000 (No. of Executions 1977–2000)
Alaska	Texas (239)
District of Columbia	Virginia (81)
Hawaii	Florida (50)
Iowa	Missouri (46)
Maine	Oklahoma (30)
Massachusetts	Louisiana (26)
Michigan	South Carolina (25)
Minnesota	Alabama (23)
North Dakota	Arkansas (23)
Rhode Island	Georgia (23)
Vermont	Arizona (22)
West Virginia	North Carolina (16)
Wisconsin	Illinois (12)
	Delaware (11)
	California (8)
	Nevada (8)
	Indiana (7)
	Utah (6)
	Mississippi (4)
	Maryland (3)
	Nebraska (3)
	Pennsylvania (3)
	Washington (3)
	Kentucky (2)
	Montana (2)
	Oregon (2)
	Colorado (1)
	Idaho (1)
	Ohio (1)
	Tennessee (1)
	Wyoming (1)
	Connecticut (0)
	Kansas (0)
	New Hampshire (0)
	New Jersey (0)
	New Mexico (0)
	New York (0)
	South Dakota (0)

Source: Snell, Tracy L. 2001. *Capital Punishment 2000*. Washington, D.C.: U.S. Bureau of Justice Statistics (NCJ 190598).

much higher murder rates than nonexecuting states in 1977, the rates have since converged. Hence, more sophisticated empirical techniques are required to determine if there is a deterrent effect from capital punishment.

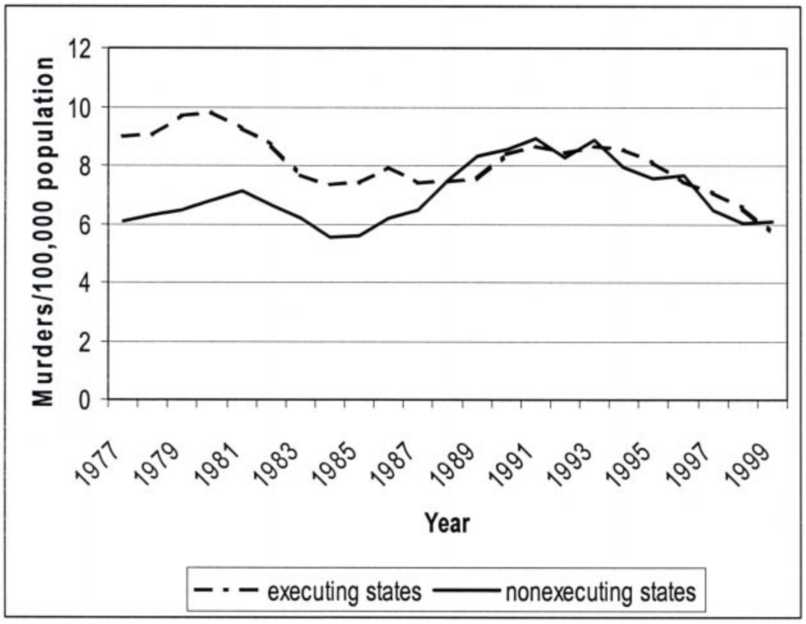

Figure 1. Murder rates in executing and nonexecuting states.

Ehrlich (1975, 1977) introduced regression analysis as a tool for examining the deterrent issue. A plethora of economic studies followed Ehrlich's. Some of these studies verbally criticize or commend Ehrlich's work, whereas others offer alternative analyses. Most analyses use a variant of Ehrlich's econometric model and his data (1933–69 national time-series or 1940 and 1950 state-level cross section). For example, Yunker (1976) finds a deterrent effect much stronger than Ehrlich's. Cloninger (1977) and Ehrlich and Gibbons (1977) lend further support to Ehrlich's finding. Bowers and Pierce (1975), Passel and Taylor (1977) and Hoenack and Weiler (1980), on the other hand, find no deterrence when they use an alternative (linear) functional form.[5] Black and Orsagh (1978) find mixed results, depending on the cross section year they use.

There are also studies that extend Ehrlich's time-series data or use more recent cross-sectional studies. Layson (1985) and Cover and Thistle (1988), for example, use an extension of Ehrlich's time-series data, covering up to 1977. Layson finds a significant deterrent effect of executions, but Cover and Thistle, who correct for data nonstationarity, find no support for the deterrent

5. Ehrlich's regression equations are in double-log form.

effect in general. Chressanthis (1989) uses time-series data covering 1966–85 and finds a deterrent effect. Grogger (1990) uses daily data for California during 1960–63 and finds no significant short-term correlation between execution and daily homicide rates.

There are also a few recent studies. Brumm and Cloninger (1996), for example, who use cross-sectional data covering fifty-eight cities in 1985 report that the perceived risk of punishment is negatively and significantly correlated with homicide commission rate. Studying the effect of concealed handgun laws on public shootings, Lott and Landes (2000) report a negative association between capital punishment and murder on a concurrent basis. Cloninger and Marchesini (2001) report that the Texas unofficial moratorium on executions during most of 1996 appears to have contributed to additional homicides. Mocan and Gittings (unpublished data) find that pardons may increase the homicide rate while executions reduce the rate. Zimmerman (2001) also reports that executions have a deterrent effect.[6] None of the existing studies, however, uses county-level postmoratorium panel data.

Becker's (1968) economic model of crime provides the theoretical foundation for much of the regression analysis in this area. The model derives the supply, or production, of offenses for an expected utility maximizing agent. Ehrlich (1975) extends the model to murders that he argues are committed either as a byproduct of other violent crimes or as a result of interpersonal conflicts involving pecuniary or nonpecuniary motives.

Ehrlich derives several theoretical propositions predicting that an increase in perceived probabilities of apprehension, conviction given apprehension, or execution given conviction will reduce an individual's incentive to commit murder. An increase in legitimate or a decrease in illegitimate earning or income opportunities will have a similar crime-reducing effect. Unfortunately, variables that can measure legitimate and illegitimate opportunities are not readily available. Ehrlich and authors who test his propositions, therefore, use several economic and demographic variables as proxies. Demographic characteristics such as population density, age, gender, and race enter the analysis because earning opportunities (legitimate or illegitimate) cannot be perfectly controlled for in an empirical investigation. Such characteristics may influence earning opportunities and can therefore serve as reasonable proxies.

6. These studies have not gone through the peer review process.

The following individual decision rule, therefore, provides the basis for empirical investigation of the deterrent effect of capital punishment:

$$\psi_t = f(Pa_t, Pc|a_t, Pe|c_t, Z_t, u_t), \tag{1}$$

where ψ is a binary variable that equals 1 if the individual commits murder during period t, and 0, otherwise; P denotes the individual's subjective probability; a, c, and e denote apprehension, conviction, and execution, respectively; Z contains individual-specific economic and demographic characteristics, as well as any other observable variable that may affect the individual's choice; and u is a stochastic term that includes any other relevant variable unobserved by the investigator.[7] Variables included in Z also capture the legitimate earning opportunities. The individual's preferences affect the function $f(\cdot)$.

Most studies of the deterrent hypothesis use either time-series or cross-sectional data to estimate the murder supply, based on equation (1). The data, however, are aggregated to state or national levels, so Ψ is the murder rate for the chosen jurisdiction. The deterrent effect of capital punishment is then the partial derivative of ψ with respect to $Pe|c$. The debate in this literature revolves around the choice of the regressors in (1), endogeneity of one or more of these regressors, and to a lesser extent the choice of $f(\cdot)$.

3. Model Specification and Data

In this section we first address two data-related specification issues that have not received due attention in the capital punishment literature. The first involves the functional form of the econometric equations, and the second concerns the allegedly adverse effect of including the nondeterrable murders in the analysis. These discussions shape the formulation of our model.

3.1. Functional Form

Most econometric models that examine the deterrent effect of capital punishment derive the murder supply from equation (1). The first step involves choosing a functional form for the equation. Ideally, the functional form of the murder supply equation should be derived from the optimizing individual's objective function. Since this ideal requirement cannot be met in

7. Note that engaging in violent activities such as robbery may lead an individual to murder. We account for this possibility in our econometric specification by including violent crime rates such as robbery in Z.

practice, convenient alternatives are used instead. Despite all the emphasis that this literature places on specification issues such as variable selection and endogeneity, studies often choose the functional form of murder supply rather haphazardly.[8] Common choices are double-log, semilog, or linear functions.

Rather than arbitrarily choosing one of these functional forms, we use the form that is consistent with aggregation rules. More specifically, note that equation (1) purports to describe the behavior of a representative individual. In practice, however, we rarely have individual-level data, and, in fact, the available data are usually substantially aggregated. Applying such data to an equation derived for a single individual implies that the equation is invariant under aggregation, and its extension to a group of individuals requires aggregation. For example, to obtain an equation describing the collective behavior of the members of a group—for instance, residents of a county, city, state, or country—one needs to add up the equations characterizing the behavior of each member. If the group has n members, then n equations, each with the same set of parameters and the same functional form but different variables, should be added up to obtain a single aggregate equation. This aggregate equation has the same functional form as the individual-level equation—it is invariant under aggregation—only in the linear case.

Because not every form has this invariance property, the choice of the functional form of the equation is important. For example, deterrence studies have applied the same double-log (or semilog) murder supply equation to city, state, and national level data, assuming implicitly that a double-log (or semilog) equation is invariant under aggregation. But this is not true, because the sum of n double-log equations would not be another double-log equation. A similar argument rules out the semilog specification.

The linear form, however, remains invariant under aggregation. Assume that the individual's murder supply equation (1) is linear in its variables,

$$\Psi_{j,t} = a_i + \beta_1 Pa_{i,t} + \beta_2 Pc|a_{i,t} + \beta_3 Pe|c_{i,t} + g_1 Z_{j,t} + \gamma_2 TD_t + u_{j,t}, \quad (1')$$

where j denotes the individual, i denotes county, a_i is the county-specific fixed effect, TD is a set of time trend dummies that captures national trends,

8. The only exceptions to this general observation are Hoenack and Weiler (1980), who criticize the use of a double-log formulation, suggesting a semilog form instead, and Layson (1985), who uses Box-Cox transformation as the basis for choosing functional form. Box-Cox transformation, however, is not appropriate for the simultaneous equations model estimated here with panel data.

such as violent TV programming or movies that have similar cross-county effects, and us are stochastic error terms with a zero mean and variance σ^2. Assume there are n_i individuals in county i—for example, $j = 1, 2, \ldots, n_i$—with $i = 1, 2, \ldots, N$, where N is the total number of counties in the U.S. Note that probabilities have an i rather than a j subscript because only individuals in the same county face the same probability of arrest, conviction, or execution.

Summing equation (1′) over all n_i individuals in county i and dividing by the number of these individuals (county population) results in an aggregate equation at the county-level for period t. For example,

$$m_{i,t} = \sum_{j=1}^{n_i} \frac{\Psi_{j,t}}{n_i} = a_i + \beta_1 Pa_{i,t} + \beta_2 Pc|a_{i,t} + \beta_3 Pe|c_{i,t} + g_1 Z_{i,t} + \gamma_2 TD_t + u_{i,t}, \tag{2}$$

where m_i is murder rate for county i (number of capital murders divided by county population). The above averaging does not change the P_i, but it alters the qualitative elements of Z into percentages and the level elements into per capita measures.[9] The subscript i obviously indicates that these values are for county i. Also, note that the new error term, $u_{i,t} = \sum_{j=1}^{n_i} u_{j,t}/n_i$, is heteroskedastic, because its variance σ^2/n_i is proportional to county population. The standard correction for the resulting heteroskedasticity is to use weighted estimation, where the weights are the square roots of county population, n_i. Such linear correction for heteroskedasticity is routinely used by practitioners even in double-log or semilog equations.

Given the above discussion, we use a linear model.[10] Ehrlich (1996) and Cameron (1994) indicate that research using a linear specification is less likely than a logarithmic specification to find a deterrent effect. This makes our results more conservative in rejecting the "no deterrence" hypothesis.

9. For example, for the gender variable, an individual value is either 1 or 0. Adding the ones and dividing by county population gives us the percentage of residents who are male. Also, for the income variable, summing across individual and dividing by county population simply yields per capita income for the county.

10. To examine the robustness of our results, we will also estimate the double-log and semilog forms of our model. These results will be discussed in section 4.

3.2. Nondeterrable Murders

Critics of the economic model of murder have argued that, because the model cannot explain the nonpremeditated murders, its application to overall murder rate is inappropriate. For example, Glaser (1977) claims that murders committed during interpersonal disputes or noncontemplated crimes of passion are not intentionally committed and are therefore nondeterrable and should be subtracted out. Because the crime data include all murders, without a detailed classification, any attempt to exclude the allegedly nondeterrable crimes requires a detailed examination of each reported murder and a judgment as to whether that murder can be labeled deterrable or nondeterrable. Such expansive data scrutiny is virtually impossible. Moreover, it would require an investigator to use subjective judgment, which would then raise concerns about the objectivity of the analysis.

We examine this seemingly problematic issue and offer an econometric response to the criticisms. The response applies equally to the concerns about including nonnegligent manslaughter—another possible nondeterrable crime—in the murder rate.[11] Assume equation (2) specifies the variables that affect the rate of the deterrable capital murders, m. Some of the nondeterrable murders would be related to economic and demographic factors or other variables in Z. For example, family disputes leading to a nonpremeditated murder may be more likely to occur at times of economic hardship. We denote the rate of such murders by m' and accordingly specify the related equation

$$m'_{i,t} = \alpha'_i + \gamma'_1 Z_{i,t} + u'_{i,t}, \tag{2$'$}$$

where u' is a stochastic term and α' and γ' are unknown parameters. Other nondeterrable murders are not related to any of the explanatory variables in equation (2). From the econometricians' viewpoint, therefore, such murders appear as merely random acts. They include accidental murders and murders committed by the mentally ill. We denote these by m'' and accordingly specify the related equation

$$m''_{i,t} = \alpha''_i + u''_{i,t}, \tag{2$''$}$$

11. Ehrlich (1975) discusses the nonnegligent manslaughter issue.

where u'' is a stochastic term and α'' is an unknown parameter. The overall murder rate is then $M = m + m' + m''$, which upon substitution for m' and m'' yields

$$M_{i,t} = \alpha_i + \beta_1 Pa_{i,t} + \beta_2 Pc|a_{i,t} + \beta_3 Pe|c + \gamma_1 Z_{i,t} + \gamma_2 TD_t + \varepsilon_{i,t}, \quad (3)$$

where $\alpha_i = a_i + \alpha'_i + \alpha''_i, \gamma_1 = g_1 + \gamma'_1$, and $\varepsilon_{i,t} = u_{i,t} + u'_{i,t} + u''_{i,t}$ is the compound stochastic term.[12] Note that we cannot estimate g_1, in equation (2), or γ'_1, in equation (2′), separately, because data on separate murder categories are not readily available. This, however, does not prevent us from estimating the combined effect γ_1, nor does it affect our main inference, which is about the βs.[13] Therefore, any inference about the deterrent effect is unaffected by the inclusion of the nondeterrable murders in the murder rate.

3.3. Econometric Model

The murder supply equation (3) provides the basis for our inference. The three subjective probabilities in this equation are endogenous and must be estimated through separate equations. Endogeneity in this literature is often dealt with through the use of an arbitrarily chosen set of instrumental variables. Hoenack and Weiler (1980) criticize earlier studies both for this practice and for not treating the estimated equations as part of a theory-based system of simultaneous equations. We draw on the economic model of crime and the existing capital punishment literature to identify a system of simultaneous equations.

We specify three equations to characterize the subjective probabilities in equation (3). These equations capture the activities of the law enforcement agencies and the criminal justice system in apprehending, convicting, and punishing perpetrators. Resources allocated to the respective agencies for this purpose affect their effectiveness and thus enters these equations:

$$Pa_{i,t} = \phi_{1,i} + \phi_2 M_{i,t} + \phi_3 PE_{i,t} + \phi_4 TD_t + \varsigma_{i,t}, \quad (4)$$

$$Pc|a_{i,t} = \theta_{1,i} + \theta_2 M_{i,t} + \theta_3 JE_{i,t} + \theta_4 PI_{i,t} + \theta_5 PA_{i,t} + \theta_6 TD_t + \xi_{i,t}, \quad (5)$$

12. Note that the equation describing $m'_{i,t}$ may also include a national trend term (γ_2 TD$_t$). The term will be absorbed into the coefficient of TD in equation (3).

13. The added noise due to compounding of errors may reduce the precision of estimation, but it does not affect the statistical consistency of the estimated parameters.

and

$$Pe|c_{i,t} = \psi_{1,i} + \psi_2 M_{i,t} + \psi_3 JE_{i,t} + \psi_4 PI_{i,t} + \psi_5 TD_t + \zeta_{i,t}, \qquad (6)$$

where PE is police payroll expenditure, JE is expenditure on judicial and legal system, PI is partisan influence as measured by the Republican presidential candidate's percentage of the statewide vote in the most recent election, PA is prison admission, TD is a set of time dummies that capture national trends in these perceived probabilities, and ς, ξ, and ζ are error terms.

If police and prosecutors attempt to minimize the social costs of crime, they must balance the marginal costs of enforcement with the marginal benefits of crime prevention. Police and judicial-legal expenditure, PE and JE, represent marginal costs of enforcement. More expenditure should increase the productivity of law enforcement or increase the probabilities of arrest, and of conviction, given arrest. Partisan influence is used to capture any political pressure to "get tough" with criminals, a message popular with Republican candidates. The influence is exerted by changing the makeup of the court system, such as the appointment of new judges or prosecutors that are "tough on crime." This affects the justice system and is, therefore, included in equations (5) and (6). Prison admission is a proxy for the existing burden on the justice system; the burden may affect judicial outcomes. This variable is defined as the number of new court commitments admitted during each year.[14] Also, note that all three equations include county fixed effects to capture the unobservable heterogeneity across counties.

We use two other crime categories besides murder in our system of equations. These are aggravated assault and robbery, which are among the control variables in *Z*. Given that some murders are the byproducts of violent activities, such as aggravated assault and robbery, we include these two crime rates in *Z* when estimating equation (3). Forst, Filatov, and Klein (1978) and McKee and Sesnowitz (1977) find that the deterrent effect vanishes when other crime rates are added to the murder supply equation. They attribute this to a shift in the propensity to commit crime, which in turn

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

shifts the supply function. We include aggravated assault and robbery to examine this substitution effect.

The other control variables that we include in Z measure economic and demographic influences. We include economic and demographic variables, which are all available at the county level, following other studies based on the economic model of crime.[15] Economic variables are used as proxy for legitimate and illegitimate earning opportunities. An increase in legitimate earning opportunities increases the opportunity cost of committing crime and should result in a decrease in the crime rate. An increase in illegitimate earning opportunities increases the expected benefits of committing crime and should result in an increase in the crime rate. Economic variables are real per capita personal income, real per capita unemployment insurance payments, and real per capita income maintenance payments. The income variable measures both the labor market prospects of potential criminals and the amount of wealth available to steal. The unemployment payments variable is a proxy for overall labor market conditions and the availability of legitimate jobs for potential criminals. The transfer payments variable represents other nonmarket income earned by poor or unemployed people. Other studies have found that crime responds to measures of both income and unemployment but that the effect of income on crime is stronger.

Demographic variables include population density and six gender and race segments of the population ages 10–29 (male or female; black, white or other). Population density is included to capture any relationship between drug activities in inner cities and murder rate. The age, gender, and race variables represent the possible differential treatment of certain segments of the population by the justice system, changes in the opportunity cost of time through the life cycle, and gender- or race-based differences in earning opportunities.

The control variables also include the state level National Rifle Association (NRA) membership rate. NRA membership is included in response to a criticism of earlier studies. Forst, Filatov, and Klein (1978) and Kleck (1979) criticize both Ehrlich and Layson for not including a gun-ownership variable. Kleck reports that including the gun variable eliminates the significance of the execution rate. Also, all equations include a set of time dummies

15. Inclusion of the unemployment rate, which is available only at the state level, does not affect the results appreciably.

that capture national trends and influences affecting all counties but varying over time.

3.4. Data and Estimation Method

We use a panel data set that covers 3,054 counties for the 1977–96 period.[16] More current data are not available on some of our variables, because of the lag in posting data on law enforcement and judicial expenditures by the Bureau of Justice Statistics. The county-level data allow us to include county-specific characteristics in our analysis and therefore reduce the aggregation problem from which much of the literature suffers. By controlling for these characteristics, we can better isolate the effect of punishment policy.

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

We have county-level data for murder arrests, which we use to estimate *Pa*. Conviction data are not available, however, because the Bureau of Justice Statistics stopped collecting them years ago. In the absence of conviction data, sentencing is a viable alternative that covers the intervening stage between arrest and execution. This variable has not been used in previous

16. We are thankful to John Lott and David Mustard for providing us with some of these data—from their 1997 study—to be used initially for a different study (Dezhbakhsh and Rubin, 1998). We also note the data on murder-related arrests for Arizona in 1980 is missing. As a result, we have to exclude from our analysis Arizona in 1980 (or 1982 and 1983 in cases where lags were involved). This will be explained further when we discuss model estimation.

studies, although authors have suggested its use in deterrence studies (see, e.g., Cameron, 1994, p. 210). We have obtained data from the Bureau of Justice Statistics on number of persons sentenced to be executed by state for each year. We use this data and arrest data to estimate $Pc|a$. We also use sentencing and execution data to estimate $Pe|c$. Execution data are at the state level because execution is a state decision. Expenditure variables in equations (4)–(6) are also at the state level.

The crime and arrest rates are from the Federal Bureau of Investigation's (FBI) Uniform Crime Reports.[17] The data on age, sex, and racial distributions, percentage of state population voting Republican in the most recent presidential election, and the area in square miles for each county are from the U.S. Bureau of the Census. Data on income, unemployment, income maintenance, and retirement payments are obtained from the Regional Economic Information System. Data on expenditure on police and judicial-legal systems, number of executions, and number of death row sentences, prison populations, and prison admissions are obtained from the U.S. Department of Justice's Bureau of Justice Statistics. NRA membership rates are obtained from the National Rifle Association.

The model we estimate consists of the simultaneous system of equations (3)–(6). We use the method of two-stage least squares, weighted to correct for the Heteroskedasticity discussed earlier. We choose two-stage over three-stage least squares because, though the latter has an efficiency advantage, it produces inconsistent estimates if an incorrect exclusionary restriction is placed on any of the system equations. Since we are mainly interested in one equation—the murder supply equation (3)—using the three-stage least squares method seems risky. Moreover, the two-stage least squares estimators are shown to be more robust to various specification

17. The FBI Uniform Crime Report Data are the best county-level crime data currently available, in spite of criticisms about potential measurement issues due to underreporting. These criticisms are generally not so strong for murder data that are central to our study. Nonetheless, there are safeguards in our econometric analysis to deal with the issue. The inclusion of county fixed effects eliminates the effects of time-invariant differences in reporting methods across counties, and estimates of trends in crime should be accurate so long as reporting methods are not correlated across counties or time. Moreover, one way to address the problem of underreporting is to use the logarithms of crime rates, which are usually proportional to true crime rates. Our general finding is robust to introduction of logs as discussed in section 4.

problems (see, e.g., Kennedy, 1992, chap. 10). Other variables and data are discussed next.

4. Empirical Results

4.1. Regression Results

The coefficient estimates for the murder supply equation (3) obtained with the two-stage least squares method and controlling for county-level fixed effects are reported in Tables 3 and 4. Various models reported in Tables 3 and 4 differ in the way the perceived probabilities of arrest, sentencing, and execution are measured. These three probabilities are endogenous to the murder supply equation (3); the tables present the coefficients on the predicted values of these probabilities. We first describe Table 3.

For Model 1 in Table 3 the conditional execution probability is measured by executions at t divided by number of death sentences at $t-6$. For Model 2 this probability is measured by number of executions at $t+6$ divided by number of death sentences at t. The two ratios reflect forward-looking and backward-looking expectations, respectively. The displacement lag of six years reflects the lengthy waiting time between sentencing and execution, which averages six years for the period we study (see Bedau, 1997, chap. 1). For probability of sentencing, given arrest, we use a two-year lag displacement, reflecting an estimated two-year lag between arrest and sentencing. Therefore, the conditional sentencing probability for Model 1 is measured by the number of death sentences at t divided by the number of arrests for murder at $t-2$. For Model 2 this probability is measured by number of death sentences at $t+2$ divided by number of arrests for murder at t. Given the absence of an arrest lag, no lag displacement is used to measure the arrest probability. It is simply the number of murder-related arrests at t divided by the number of murders at t.

For Model 3 in Table 3 we use an averaging rule. We use a six-year moving average to measure the conditional probability of execution, given a death sentence. Specifically, this probability at time t is defined as the sum of executions during ($t+2$, $t+1$, t, $t-1$, $t-2$, and $t-3$) divided by the sum of death sentences issued during ($t-4$, $t-5$, $t-6$, $t-7$, $t-8$, and $t-9$). The six-year window length and the six-year displacement lag capture the average time from sentence to execution for our sample. Similarly, a two-year lag and a two-year window length is used to measure the conditional

Table 3. Two-Stage Least Squares Regression Results for Murder Rate

	Estimated Coefficients		
Regressor	**Model 1**	**Model 2**	**Model 3**
Deterrent Variable			
Probability of arrest	− 4.037	− 10.096	− 3.334
	(6.941)**	(17.331)**	(6.418)**
Conditional probability of death sentence	− 21.841	− 42.411	− 32.115
	(1.167)	(3.022)**	(1.974)**
Conditional probability of execution	− 5.170	− 2.888	− 7.396
	(6.324)**	(6.094)**	(10.285)**
Other Crime			
Aggravated assault rate	0.0040	0.0059	0.0049
	(18.038)**	(23.665)**	(22.571)**
Robbery rate	0.0170	0.0202	0.0188
	(39.099)**	(51.712)**	(49.506)**
Economic Variable			
Real per capita personal income	0.0005	0.0007	0.0006
	(14.686)**	(17.134)**	(16.276)**
Real per capita unemployment insurance payments	− 0.0064	− 0.0077	− 0.0033
	(6.798)**	(8.513)**	(3.736)**
Real per capita income maintenance payments	0.0011	− 0.0020	0.0024
	(1.042)	(1.689)*	(2.330)**
Demographic Variable			
African American (%)	0.0854	− 0.1114	0.1852
	(2.996)**	(4.085)**	(6.081)**
Minority other than African American (%)	− 0.0382	0.0255	− 0.0224
	(7.356)**	(0.7627)	(4.609)**
Male (%)	0.3929	0.2971	0.2934
	(7.195)**	(3.463)**	(5.328)**
Age 10–19 (%)	− 0.2717	− 0.4849	0.0259
	(4.841)**	(8.021)**	(0.4451)
Age 20–29 (%)	− 0.1549	− 0.6045	− 0.0489
	(3.280)**	(12.315)**	(0.9958)
Population density	− 0.0048	− 0.0066	− 0.0036
	(22.036)**	(24.382)**	(17.543)**
NRA membership rate, (% state pop. in NRA)	0.0003	0.0004	− 0.0002
	(1.052)	(1.326)	(0.6955)
Intercept	6.393	23.639	− 12.564
	(0.4919)	(6.933)**	(0.9944)
F-statistic	217.90	496.29	276.46
Adjusted r^2	0.8476	0.8428	0.8624

Notes: Dependent variable is the murder rate (murders/100,000 population). In Model 1 the execution probability is (number of executions at t)/(number of death row sentences at $t-6$). In Model 2 the execution probability is (number of executions at $t+6$)/(number of death row sentences at t). In Model 3 the execution probability is (sum of executions at $t+2+t+1+t+t-1+t-2+t-3$)/(sum of death row sentences at $t-4+t-5+t-6+t-7+t-8+t-9$). Sentencing probabilities are computed accordingly, but with a two-year displacement lag and a two-year averaging rule. Absolute value of t-statistics are in parentheses. The estimated coefficients for year and county dummies are not shown.
*Significant at the 90% confidence level, two-tailed test.
**Significant at the 95% confidence level, two-tailed test.

Table 4. Two-Stage Least Squares Regression Results for Murder Rate

	Estimated Coefficients		
Regressor	**Model 4**	**Model 5**	**Model 6**
Deterrent Variable			
Probability of arrest	− 2.264	− 4.417	− 2.184
	(4.482)**	(9.830)**	(4.568)**
Conditional probability of death sentence	− 3.597	− 47.661	− 10.747
	(0.2475)	(4.564)**	(0.8184)
Conditional probability of execution	− 2.715	− 5.201	− 4.781
	(4.389)**	(19.495)**	(8.546)**
Other Crime			
Aggravated assault rate	0.0053	0.0086	0.0064
	(29.961)**	(47.284)**	(35.403)**
Robbery Rate	0.0110	0.0150	0.0116
	(35.048)**	(54.714)**	(41.162)**
Economic Variable			
Real per capita personal income	0.0005	0.0004	0.0005
	(20.220)**	(14.784)**	(19.190)**
Real per capita unemployment insurance payments	− 0.0043	− 0.0054	− 0.0038
	(5.739)**	(7.317)**	(5.080)**
Real per capita income maintenance payments	0.0043	0.0002	0.0027
	(5.743)**	(0.2798)	(3.479)**
Demographic Variable			
African American (%)	0.1945	0.0959	0.1867
	(9.261)**	(4.956)**	(7.840)**
Minority other than African American (%)	− 0.0338	− 0.0422	− 0.0237
	(7.864)**	(9.163)**	(5.536)**
Male (%)	0.2652	0.3808	0.2199
	(6.301)**	(8.600)**	(4.976)**
Age 10–19 (%)	− 0.2096	− 0.6516	− 0.1629
	(5.215)**	(15.665)**	(3.676)**
Age 20–29 (%)	− 0.1315	− 0.5476	− 0.1486
	(3.741)**	(15.633)**	(3.971)**
Population density	− 0.0044	− 0.0041	− 0.0046
	(30.187)**	(27.395)**	(30.587)**
NRA membership rate, (% state pop. in NRA)	0.0008	0.0006	0.0008
	(3.423)**	(3.308)**	(3.379)**
Intercept	10.327	17.035	10.224
	(0.8757)	(8.706)**	(1.431)
F-Statistic	280.88	561.93	323.89
Adjusted r^2	0.8256	0.8062	0.8269

Notes: Dependent variable is the murder rate (murders/100,000 population). In Model 4 the execution probability is (number of executions at t)/(number of death row sentences at $t-6$). In Model 5 the execution probability is (number of executions at $t+6$)/(number of death row sentences at t). In Model 6 the execution probability is (sum of executions at $t+2+t+1+t+\ t-1+t-2+t-3$)/(sum of death row sentences at $t-4+t-5+t-6+t-7+t-8+t-9$). Sentencing probabilities are computed accordingly, but with a two-year displacement lag and a two-year averaging rule. Absolute value of t-statistics are in parentheses. The estimated coefficients for year and county dummies are not shown.
*Significant at the 90% confidence level, two-tailed test.
**Significant at the 95% confidence level, two-tailed test.

death sentencing probabilities. Given the absence of an arrest lag, no averaging or lag displacement is used when arrest probabilities are computed.[18]

Strictly speaking, these measures are not the true probabilities. However, they are closer to the probabilities as viewed by potential murderers than would be the "correct" measures. Our formulation is consistent with Sah's (1991) argument that criminals form perceptions based on observations of friends and acquaintances. We draw on the capital punishment literature to parameterize these perceived probabilities.

Models 4, 5, and 6 in Table 4 are, respectively, similar to Models 1, 2, and 3 in Table 3, except for the way we treat undefined probabilities. When estimating the models reported in Table 3, we observed that in several years some counties had no murders and some states had no death sentences. This rendered some probabilities undefined because of a zero denominator. Estimates in Table 3 are obtained excluding these observations. Alternatively, and to avoid losing data points, for any observation (county/year) in which the probabilities of arrest or execution are undefined because of this problem, we substituted the relevant probability from the most recent year when the probability was not undefined. We look back up to four years, because in most cases this eradicates the problem of undefined probabilities. The assumption underlying such substitution is that criminals will use the most recent information available in forming their expectations. So a person contemplating committing a crime at time t will not assume that he will not be arrested if no crime has been committed, and hence no arrest has been made, during this period. Rather, he will form an impression of the arrest odds, an impression based on arrests in recent years. This is consistent with Sah's (1991) argument. Table 4 uses this substitution rule to compute probabilities when they are undefined.[19]

Results in Tables 3 and 4 suggest the presence of a strong deterrent effect.[20] The estimated coefficient of the execution probability is negative and highly significant in all six models. This suggests that an increase in

18. The absence of arrest data for Arizona in 1980, mentioned earlier, results in the exclusion of Arizona 1980 from estimation of all three models, Arizona 1982 from estimation of Models 2 and 3, and Arizona 1983 from estimation of Model 3.

19. For the states that have never had an execution, the conditional probability of execution takes a value of 0. For the states that have never sentenced anyone to death row, the conditional probability of a death row sentence takes a value of 0.

20. In all of our estimations we correct the residuals from the second-stage least squares to account for using predicted values rather than the actual arrest rates,

perceived probability of execution, given that one is sentenced to death, will lead to a lower murder rate.[21] The estimated coefficient of the arrest probability is also negative and highly significant in all six models. This finding is consistent with the proposition set forth by the economic models of crime, which suggests an increase in the perceived probability of apprehension leads to a lower crime rate.

For the sentencing probability the estimated coefficients are negative in all models and significant in three of the six models. It is not surprising that sentencing has a weaker deterrent effect, given that we are estimating the effect of sentencing, *holding the execution probability constant*. What we capture here is a measure of the "weakness" or "porosity" of the state's criminal justice system. The coefficient of the sentencing probability picks up not only the ordinary deterrent effect, but also the porosity signal. The latter effect may, indeed, be stronger. For example, if criminals know that the justice system issues many death sentences but the executions are not carried out, then they may not be deterred by an increase in probability of a death sentence. In fact, an unpublished study by Leibman, Fagan, and West reports 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. This reveals the indeterminacy of a death sentence and its ineffectiveness when it is not carried out. Such indeterminacy affects the deterrence of a death sentence.

The murder rate appears to increase with aggravated assault and robbery, as the estimated coefficients for these two variables are positive and highly significant in all cases. This is in part because these crimes are caused by the same factors that lead to murder, so measures of these crimes serve as additional controls. In addition, this reflects the fact that some murders are the byproduct of robbery or aggravated assault. In fact, several studies

death row sentencing rates, and execution rates in the estimation of the murder equation (Davidson and MacKinnon, 1993, chap. 7).

21. We also repeat the analysis, using as our dependent variable six other crimes: aggravated assault, robbery, rape, burglary, larceny, and auto theft. If executions were found to deter other crimes besides murder, it may be the case that some other omitted variable that is correlated with the number of executions is causing crime to drop across the board. However, we find no evidence of this. Of the thirty-six models that we estimate (six crimes and six models per crime), only six exhibit a negative correlation between crime and the number of executions. These cases are spread across crimes with no consistency as to which crime decreases with executions.

have documented that increasing proportions of homicides are the outcome of robbery (see, e.g., Zimring, 1977).

Additional demographic variables are included primarily as controls, and we have no strong theoretical predictions about their signs. Estimated coefficients for per capita income are positive and significant in all cases. This may reflect the role of illegal drugs in homicides during this time period. Drug consumption is expensive and may increase with income. Those in the drug business are disproportionately involved in homicides because the business generates large amounts of cash, which can lead to robberies, and because normal methods of dispute resolution are not available. An increase in per capita unemployment insurance payments is generally associated with a lower murder rate.

Other demographic variables are often significant. A larger number of males in a county is associated with a higher murder rate, as is generally found (e.g., Daly and Wilson, 1988). An increase in percentage of the teenage population, on the other hand, appears to lower the murder rate. The fraction of the population that is African American is generally associated with higher murder rates, and the percentage that is minority other than African American is generally associated with a lower rate.

The estimated coefficient of population density has a negative sign. One might have expected a positive coefficient for this variable; murder rates are higher in large cities. However, this may not be a consistent relationship: the murder rate can be lower in suburbs than it is in rural areas, although rural areas are less densely populated than suburbs. But the murder rate may be higher in inner cities where the density is higher than in the suburbs.[22] Glaeser and Sacerdote (1999) also report that crime rates are higher for cities with 25,000 to 99,000 persons than for cities with 100,000 to 999,999 persons and then higher for cities over one million, although not as high as for the smaller cities. (Glaeser and Sacerdote, 1999, Figure 3.) Because there are relatively few counties containing cities of over one million, our measure of

22. To examine the possibility of a piecewise relationship, we used two interactive (0 or 1) dummy variables identifying the low and the high range for the density variable. The dummies were then interacted with the density variable. The estimated coefficient for Models 1–3 were negative for the low density range and positive for the high density range, suggesting that murder rate declines with an increase in population density for counties that are not too densely populated, but increases with density for denser areas. This exercise did not alter the sign or significance of other estimated coefficients. For Models 4–6, however, the interactive dummies both have a negative sign.

density may be picking up this nonlinear relationship. They explain the generally higher crime rate in cities as a function of higher returns, lower probabilities of arrest and conviction, and the presence of more female-headed households.

Finally, the estimates of the coefficient of the NRA membership variable are positive in five of the six models and significant in half of the cases. A possible justification is that in counties with a large NRA membership guns are more accessible and can therefore serve as the weapon of choice in violent confrontations. The resulting increase in gun use, in turn, may lead to a higher murder rate.[23]

The most robust findings in these tables are as follows: The arrest, sentencing, and execution measures all have a negative effect on murder rate, suggesting a strong deterrent effect as the theory predicts. Other violent crimes tend to increase murder. The demographic variables have mixed effects; murder seems to increase with the proportion of the male population. Finally, the NRA membership variable has positive and significant estimated coefficients in all cases, suggesting a higher murder rate in counties with a strong NRA presence.

We do not report estimates of the coefficients of the other equations in the system (equations [4]–[6]), because we are mainly interested in equation (3), which allows direct inference about the deterrent effect. Nevertheless, the first-stage regressions do produce some interesting results. Expenditure on the police and judicial-legal system appears to increase the productivity of law enforcement. Police expenditure has a consistently positive effect on the probability of arrest (equation [4]); expenditure on the judicial-legal system has a positive and significant effect on the conditional probability of receiving a death penalty sentence in all six models of equation (5). The partisan-influence variable also has a consistently positive and significant impact on the probability of receiving a death sentence (equation [5]). This result indicates that the more Republican the state, the more common the death row sentences. The partisan-influence variable has a consistently positive

23. If the NRA membership variable is a good proxy for gun ownership, our results appear to contradict the finding that allowing concealed weapons deters violent crime (Lott and Mustard, 1997). However, the results may be consistent with theirs if the carrying of concealed weapons is negatively related to NRA membership. See also Dezhbakhsh and Rubin (1998), who find results much weaker than those of Lott and Mustard.

and significant impact on the conditional probability of execution in equation (6). This suggests that the more Republican the state, the more likely the executions. The expenditure on the judicial-legal system has a negative and significant effect on the conditional probability of execution in all six models (equation [6]). This result implies that more spending on appeals and public defenders results in fewer executions.

4.2. Effect of Tough Sentencing Laws

One may argue that the documented deterrent effect reflects the overall toughness of the judicial practices in the executing states. For example, these states may have tougher sentencing laws that serve as a deterrent to various crimes, including murder. To examine this argument, we constructed a new variable measuring "judicial toughness" for each state, and estimated the correlation between this variable and the execution variable.[24] The estimated correlation coefficient ranges from $-.06$ to .26 for the six measures of the conditional probability of execution that we have used in our regression analysis. The estimated correlation between the toughness variable and the binary variable that indicates whether or not a state has a capital punishment law in any given year is .28.

We also added the toughness variable to equation (3), our main regression equation, to see whether its inclusion alters our results. The inclusion of the toughness variable did not change the significance or sign of the estimated execution coefficient. Moreover, the toughness variable has an insignificant coefficient estimate in four of the six regressions. The low correlation between execution probability and the toughness variable, along with the observed robustness of our results to inclusion of the toughness variable, suggests that the deterrent finding is driven by executions and not by tougher sentencing laws.

4.3. Magnitude of the Deterrent Effect

The statistical significance of the deterrent coefficients suggests that executions reduce the murder rate. But how strong is the expected tradeoff

24. This variable takes values 0, 1, or 2, depending on whether a state has zero, one, or two tough sentencing laws at a given year. The tough sentencing laws we consider are (1) truth-in-sentencing laws, which mandate that a violent offender must serve at least 85% of the maximum sentence and (2) "strikes" laws, which significantly increase the prison sentences of repeat offenders. See also Shepherd (2002a, 2002b).

between executions and murders? In other words, how many potential victims can be saved by executing an offender?[25] Neither aggregate time-series nor cross-sectional analyses can provide a meaningful answer to this question. Aggregate time-series data, for example, cannot impose the restriction that execution laws be state specific, and any deterrent effect should be restricted to the executing state. Cross-sectional studies, on the other hand, capture the effect of capital punishment through a binary dummy variable that measures an overall effect of the capital punishment laws instead of a marginal effect.

Panel data econometrics provides the appropriate framework for a meaningful inference about the tradeoff. Here an execution in one state is modeled to affect the murders in the same state only. Moreover, the panel allows estimation of a marginal effect rather than an overall effect. To estimate the expected tradeoff between executions and murder, we can use estimates of the execution deterrent coefficient $\hat{\beta}_3$ as reported in Tables 3 and 4. We focus on Model 4 in Table 4, which offers the most conservative (smallest) estimate of this coefficient. The coefficient β_3 is the partial derivative of murder per 100,000 population with respect to the conditional probability of execution, given sentencing (e.g., the number of executions at time t divided by the number of death sentences issued at time $t - 6$). Given the measurement of these variables, the number of potential lives saved as the result of one execution can be estimated by the quantity $\beta_3(\text{POPULATION}_t/100{,}000)$ $(1/S_{t-6})$, where S is the number of individuals sentenced to death.

We evaluate this quantity for the United States, using β_3 estimate in Model 4 and $t = 1996$, the most recent period that our sample covers. The resulting estimate is 18, with a margin of error of 10 and therefore a corresponding 95% confidence interval (8–28).[26] This implies that each additional execution has resulted, on average, in eighteen fewer murders, or in at least eight fewer murders. Also, note that the presence of population in the above expression is because murder data used to estimate β_3 is on a per capita basis. In calculating the tradeoff estimate, therefore, we use the population of the states with a death penalty law, since only residents of these states can be deterred by executions.

25. Ehrlich (1975) and Yunker (1976) report estimates of such tradeoffs, using time-series aggregate data.

26. The 95% confidence interval is given by $+(-)1.96$ [SE of $(\hat{\beta}_3)$] $(\text{POPULATION}_t/100{,}000)$ $(1/S_{t-6})$.

Table 5. Estimates of the Execution Probability Coefficient under Various Specifications (Robustness Check)

Specification	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
State-level data	– 5.343	– 2.257	– 6.271	– 1.717	– 4.046	– 2.895
	(2.774)**	(2.151)**	(4.013)**	(0.945)	(6.486)**	(1.867)*
Semilog	– 0.145	– 0.191	– 0.218	– 0.142	– 0.420	– 0.419
	(1.449)	(3.329)**	(2.372)**	(0.878)	(6.518)**	(2.902)**
Double log	– 0.155	– 0.078	– 0.144	– 0.150	– 0.181	– 0.158
	(3.242)**	(2.987)**	(6.283)**	(1.871)*	(3.903)**	(3.818)**
1990s data	– 3.021	0.204	– 3.251	– 1.681	– 4.079	– 2.791
	(3.250)**	(0.301)	(3.733)**	(2.182)**	(4.200)**	(3.633)**
Execution dummy added	– 7.431	– 3.074	– 7.631	– 4.442	– 5.109	– 5.669
	(9.821)**	(6.426)**	(11.269)**	(7.143)**	(19.564)**	(9.922)**
Other crimes dropped	– 0.088	– 7.085	– 4.936	– 1.688	– 7.070	– 1.599
	(0.090)	(11.471)**	(5.686)**	(2.394)**	(22.282)**	(2.531)**
Exogenous execution	– 0.494	– 0.428	– 2.515	– 0.309	– 0.377	– 1.761
probability	(2.888)**	(3.236)**	(8.284)**	(2.464)**	(5.102)**	(7.562)**

Notes: Absolute value of *t*-statistics are in parentheses. The estimated coefficients for the other variables are available upon request.
*Significant at the 90% confidence level, two-tailed test.
**Significant at the 95% confidence level, two-tailed test.

4.4. Robustness of Results

Although we believe that our econometric model is appropriate for estimating the deterrent effect of capital punishment, the reader may want to know how robust our results are. To provide such information, we examine the sensitivity of our main finding—that capital punishment has a deterrent effect on capital crimes—to the econometric choices we have made. In particular, we evaluate the robustness of our deterrence estimates to changes in aggregation level, functional form, sampling period, modeling death penalty laws, and endogenous treatment of the execution probability.

For each specification, we estimate the same six models as described above. The results are reported in Table 5. Each row includes the estimated coefficient of the execution probability (and the corresponding *t*-statistics) for the six models.[27] Results are in general quite similar to those reported for the main specification. For example, where we use state-level data the estimated coefficient of the execution probability is negative and significant in five of the six models, suggesting a strong deterrent effect for executions. In the remaining case, Model 4, the coefficient estimate is insignificant.

27. For brevity, we do not report full results, which are available upon request.

We also estimate our econometric model in double-log and semilog forms. These, along with the linear model, are the commonly used functional forms in this literature. For the semilog form, this coefficient estimate is negative in all six models and significant in four of the models. For the double-log form the estimated coefficient of the execution probability is negative and significant in all six models. These results suggest that our deterrence finding is not sensitive to the functional form of the model.

Given that the executions have accelerated in the 1990s, we think it worthwhile to examine the deterrent effect of capital punishment, using only the 1990s data. This will also get at a possible nonlinearity in the execution parameter. We, therefore, estimate Models 1–6, using only the 1990s data. The coefficient estimate for the execution probability is negative and significant for all models but Model 2, which has a positive but insignificant coefficient.

As an additional robustness check, we added to our linear model a dummy variable that identifies the states with capital punishment. This variable takes a value of 1 if the state has a death penalty law on the books in a given year, and 0 otherwise. This variable allows us to make a distinction between having a death penalty law and using it. The addition of this variable did not change the sign or the significance of the estimated coefficient of the execution probability. The estimated coefficient remains negative and significant in all six models. The estimated coefficient of the dummy variable, on the other hand, does not show any additional deterrence. This suggests that having a death penalty law on the books does not deter criminals when the law is not applied.

In addition, we estimate the models after dropping the crime rates of aggravated assault and robbery. The coefficient for the conditional probability of execution is negative and significant in four of the models. In Model 1 the coefficient is negative and insignificant, and in Model 4 the coefficient is positive and significant.

We also estimated all six models reported in Tables 3 and 4, assuming that the execution probability is exogenous. In all six cases the estimated coefficient of this variable turned out to be negative and significant, suggesting a strong deterrent effect.

The numerator of murder rate, our dependent variable, is murder that also appears as the denominator of arrest rate, which is one of the regressors, and is perhaps proportional to other probabilities that we use as regressors. To make certain that we are not observing a spurious negative correlation between these variables, we estimate the primary system of equations (3)–(6), using variables that are in levels. We use the number of murders in year t as the dependent variable and the number of executions, the number of death row sentences, and the number of arrests in year t as the deterrent variables. The estimated coefficient on the number of executions in this specification is -16.008 with a t-statistic of 25.440 (significant at the 95% confidence level), indicating deterrence and suggesting that our results are not artifacts of variable construction.

Overall, we estimate fifty-five models. Six models are reported in Tables 3 and 4; forty-four models in Table 5. One model is discussed in the previous paragraph, and 6 models are discussed in the section examining the effect of tough sentencing laws); the estimated coefficient of the execution probability is negative and significant in forty-nine of these models and negative but insignificant in four (see note 27). The above robustness checks suggest that our main finding that executions deter murders is not sensitive to various specification choices.

5. Concluding Remarks

Does capital punishment deter capital crimes? The question remains of considerable interest. Both presidential candidates in the fall 2000 election were asked this question, and they both responded vigorously in the affirmative. In his pioneering work, Ehrlich (1975, 1977) applied a theory-based regression equation to test for the deterrent effect of capital punishment and reported a significant effect. Much of the econometric emphasis in the literature following Ehrlich's work has been the specification of the murder supply equation. Important data limitations, however, have been acknowledged.

In this study we use a panel data set covering 3,054 counties over the period 1977–96 to examine the deterrent effect of capital punishment. The relatively low level of aggregation allows us to control for county-specific effects and also avoid problems of aggregate time-series studies. Using comprehensive postmoratorium evidence, our study offers results that are relevant for analyzing current crime levels and useful for policy purposes.

Our study is timely because several states are currently considering either a moratorium on executions or new laws allowing execution of criminals. In fact, the absence of recent evidence on the effectiveness of capital punishment has prompted state legislatures in, for example, Nebraska to call for new studies on this issue.

We estimate a system of simultaneous equations in response to the criticism levied on studies that use ad hoc instrumental variables. We use an aggregation rule to choose the functional form of the equations we estimate: linear models are invariant to aggregation and are therefore the most suited for our study. We also demonstrate that the inclusion of nondeterrable murders in murder rate does not bias the deterrence inference.

Our results suggest that the legal change allowing executions beginning in 1977 has been associated with significant reductions in homicide. An increase in any of the three probabilities of arrest, sentencing, or execution tends to reduce the crime rate. Results are robust to specification of such probabilities. In particular, our most conservative estimate is that the execution of each offender seems to save, on average, the lives of eighteen potential victims. (This estimate has a margin of error of plus and minus ten). Moreover, we find robbery and aggravated assault associated with increased murder rates. A higher NRA presence, measured by NRA membership rate, seems to have a similar murder-increasing effect. Tests show that results are not driven by "tough" sentencing laws and are robust to various specification choices. Our main finding, that capital punishment has a deterrent effect, is robust to choice of functional form (double-log, semilog, or linear), state-level versus county-level analysis, sampling period, endogenous versus exogenous probabilities, and level versus ratio specification of the main variables. Overall, we estimate fifty-five models; the estimated coefficient of the execution probability is negative and significant in forty-nine of these models and negative but insignificant in four models.

Finally, a cautionary note is in order: deterrence reflects social benefits associated with the death penalty, but one should also weigh in the corresponding social costs. These include the regret associated with the irreversible decision to execute an innocent person. Moreover, issues such as the possible unfairness of the justice system and discrimination must be considered when society makes a social decision regarding capital punishment. Nonetheless, our results indicate that there are substantial costs in deciding not to use capital punishment as a deterrent.

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