Does Capital Punishment Have a Deterrent Effect? New Evidence from Post-moratorium Panel Data*

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Abstract

Evidence on the deterrent effect of capital punishment is important for many states that are currently considering a change in their position on the issue. Existing studies use only Ehrlich's data—U.S. aggregate time-series for 1933-1969 and state level cross-sectional data for 1940 and 1950—or minor extensions that lack evidence after the 1972-1976 Supreme Court imposed moratorium on capital punishment. For the first time, we examine the deterrent hypothesis using county-level post-moratorium panel data. The procedure we employ overcomes the aggregation problem, eliminates the bias arising from unobserved heterogeneity, and offers an inference which is relevant for the current crime level. Our results suggest that capital punishment has a strong deterrent effect. An increase in any of the three probabilities—arrest, sentencing, or execution—tends to reduce the crime rate. In particular, each execution results, on average, in 18 fewer murders—with a margin of error of plus and minus 10. Tests show that results are not driven by "tough" sentencing laws.

I. Introduction

The acrimonious debate over capital punishment has continued for centuries (Beccaria, 1764, and Stephen, 1864). In recent decades, the debate has heated up in the U.S. as the Supreme Court has twice changed its position on capital punishment.[1] Currently, several states are considering a change in their policies regarding the status of the death penalty.[2] 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., Sellin, 1959; Eysenck, 1970; and Cameron, 1994). Economists joined the debate with the pioneering work of Ehrlich (1975, 1977). Ehrlich's regression results, using U.S. aggregate time-series for 1933-1969 and state level cross-sectional data for 1940 and 1950, suggest a significant deterrent effect.

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 vs. exogenous variables.[3] The mixed findings prompted a series of sensitivity analyses on Ehrlich's equations, reflecting a further emphasis on specification.[4]

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[1] In 1972 the Supreme Court outlawed capital punishment but in 1976 it changed its position by allowing executions under certain carefully specified circumstances.

[2] 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," *The Economist*, July 24th 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 legislature in Nebraska and Illinois has 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.

[3] See Cameron (1994) and Avio (1998) for literature summaries.

[4] 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., Leamer (1983, 1985), McManus (1985), McAleer and Veall (1989), and Ehrlich and Liu (1999).

The extensive attention that the deterrence literature has paid to regression specification has overshadowed legitimate data concerns. For example, most studies, even those conducted recently, use time-series or cross section data that lack post-moratorium evidence.[5] The studies that use national time-series are further 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. For example, an increase in nonexecuting states' murder rates aggregated with a drop in executing states' murder rate may incorrectly lead to an inference of no deterrence, as the aggregate data would show an increase in executions leading to no change in the murder rate. 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.[6] No study has yet used recently available disaggregate time-series data to examine the deterrent effect of capital punishment. This is true despite its policy implications and timeliness. Until new evidence is presented, any policy decision has to draw on evidence that is old and perhaps affected by data limitations. To fill this vacuum, we examine the deterrent effect of capital punishment using county-level panel data that cover the post-moratorium

[5] Layson (1985) and Cover and Thistle (1988), for example, extend Ehrlich's time-series data to 1977, still, however, missing the post-moratorium executions. Grogger (1990) uses daily data for California during 1960-1963. Brumm and Cloninger (1996) use cross-sectional data covering 58 cities in 1985.
[6] See, e.g., Hoenack and Weiler (1980), Cameron (1994), and Avio (1998).

period. Our analysis has allows us to overcome the aforementioned 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 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 collinearity 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 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—that are included in commonly used murder data. We ask whether such inclusion adversely affects the deterrence inference. We draw on our discussions of these issues and the specification debate in this literature to formulate our econometric model.

The paper is organized as follows: Section II outlines the theoretical foundation of our econometric model and discusses issues related to estimation of the deterrent effect. Section III describes data and measurement issues, presents the econometric specification, and highlights important statistical issues. Section IV reports the empirical results and the corresponding analysis, including an estimate of the number of murders avoided as the result of each execution. Section V concludes the paper.

II. 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, chapter 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.[7] There were no executions in the U.S. between 1968 and 1977. Executions resumed in 1977 and have increased steadily since then.

As Table 1 illustrates, from 1977 through 1999 there have been 598 executions in 31 states. Six other states have adopted death penalty laws but have not executed anyone, and twelve states do not have death penalty laws. Several of the executing states are currently

[7] Furman v. Georgia, 408 U.S. 238 and Gregg v. Georgia, No. 74-6257, 428 U.S. 153; 96 S. Ct. 2909; 1976 U.S. Lexis 82.

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."[8]

In the U.S., the deterrence issue has been a topic of acrimonious debate for decades. The initial participants in this debate were primarily psychologists and criminologists. Their research was either theoretical or based on attempts to compare crime patterns for matched regions with different rates of execution. Results generally suggested that there is no deterrent effect (see, for example, Sellin, 1959; Eysenck, 1970; and the discussion in Cameron, 1994). Ehrlich (1975, 1977) pioneered econometric work in this area. He introduced regression analysis as a tool for examining the deterrent issue. His finding of a strong deterrent effect sharply contrast with earlier findings.

A plethora of economic studies followed Ehrlich's. Some of these studies verbally criticize or commend Ehrlich's work, while others offer alternative analyses. Most analyses offer a variant of Ehrlich's econometric model and his data (1933-1969 national time-series or 1940 and 1950 state level cross section). Results range from a substantial deterrent effect—

[8] Zimring and Hawkins (1986), p. 167.

stronger than Ehrlich's—to no effect or a small adverse effect.[9] 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.[10]

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 optimizing agent who allocates time between legal and illegal activities in such a way as to maximize expected utility. Ehrlich (1975) extends the model to murders which he argues are committed either as a by-product 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/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.

[9] See Bowers and Pierce (1975), Yunker (1976), Ehrlich and Gibbons (1977), Passel and Taylor (1977), Hoenack and Weiler (1980), Layson (1985), Cover and Thistle (1988), Chressanthis (1989), Grogger (1990), and Brumm and Clonninger (1996) among others.
[10] Fowler vs. North Carolina, 428 U.S. 904 (1976).

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

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

where y is a binary variable which 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;[11] and u is a stochastic term that includes any other relevant variable unobserved by the investigator. 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 y is the murder rate for the chosen jurisdiction. The deterrent effect of capital punishment is then the partial derivative of y 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 $f(\cdot)$.

III. 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

[11] 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*.

including the nondeterrable murders in the analysis. These discussions shape the formulation of our model.

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 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.[12] Common choices are double-log, semi-log, or linear functions.

Rather than choosing arbitrarily 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—e.g., 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

[12] The only exceptions to this general observation are Hoenack and Weiler (1980), who criticize the use of a double-log formulation suggesting a semi-log 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.

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 semi-log) murder supply equation to city, state, and national level data, assuming implicitly that a double-log (or semi-log) 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 semi-log specification.

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

$$y_{j,t} = a_i + b_1 Pa_{i,t} + b_2 Pc|a_{i,t} + b_3 Pe|c_{i,t} + g_1 Z_{j,t} + g_2 TD_t + u_{j,t}\ , \qquad (1')$$

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 such as violent TV programming or movies that have similar cross-county effects, and *u*'s are stochastic error terms with a zero mean and variance s^2. Assume there are n_i individuals in county *i*—for example, *j*=1,2,… n_i— with *i*=1,2,….*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,

$$m_{i,t} = \sum_{j=1}^{n_i} \frac{y_{j,t}}{n_i} = a_i + b_1 Pa_{i,t} + b_2 Pc|a_{i,t} + b_3 Pe|c_{i,t} + g_1 Z_{i,t} + g_2 TD_t + u_{i,t} \;, \qquad (2)$$

where m_i is capital murder rate for county i (number of capital murders divided by county population). The above averaging does not change the P_i's, but it alters the qualitative elements of Z into percentages and the level elements into per capita measures.[13] 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 heteroschedastic because its variance s^2/n_i is proportional to county population. The standard correction for the resulting heteroschedasticity in the above linear regression model is to use weighted estimation where the weights are the square roots of county population, n_i. Such linear correction for heteroschedasticity is routinely used by practitioners even in double-log or semi-log equations.

Given the above discussion we use a linear model. Ehrlich (1996) and Cameron (1994) indicate that research using a linear specification is less likely to find a deterrent effect than is a logarithmic specification. This makes our results more conservative in rejecting the "no deterrence" hypothesis.[14]

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, Glasser (1977) claims that murders committed during interpersonal disputes or noncontemplated crimes of passion are not intentionally

[13] 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.
[14] Another advantage of a linear form is that we do not need to deal with taking log of zero, as some counties have zero murders in some years. Scholars who use log specification change zero to an arbitrary small number.

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 judgement 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 judgement, which would then raise concerns about the objectivity of the analysis.

We examine this seemingly problematic issue and offer an econometric response to the above criticisms. The response applies equally to the concerns about including nonnegligent manslaughter—another possible nondeterrable crime—in the murder rate.[15] 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

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

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

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

[15] 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

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

where $\alpha_i = \alpha_i + \alpha_i' + \alpha_i''$, $\gamma = \gamma_1 + \gamma'$ and $\varepsilon_{i,t} = u_{i,t} + u'_{i,t} + u''_{i,t}$ is the compound stochastic term.[16] Note that we cannot estimate γ_1, in equation (2), or γ', 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, and neither does it affect our main inference which is about the β's.[17] Therefore, any inference about the deterrent effect is unaffected by the inclusion of the nondeterrable murders in the murder rate.

Econometric Model: The murder supply equation (3) provides the basis for our inference. We specify other equations to characterize the endogenous variables in (3). 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 use the economic model of crime to identify the additional equations and then estimate them in the context of a simultaneous equation model.

The three subjective probabilities in equation (3) are endogenous and need to be estimated through separate equations. These equations should characterize the activities of the law enforcement agencies and the criminal justice system in apprehending, convicting, and

[16] Note that the equation describing $m'_{i,t}$ may also include a national trend term ($\gamma' TD_t$). The term will be absorbed into the coefficient of TD in equation (3).

[17] The added noise due to compounding of errors may reduce the precision of estimation, but it doe not affect the statistical consistency of the estimated parameters.

punishing perpetrators. Resources allocated to the respective agencies for this purpose also enter these equations. The equations are

$$Pa_{i,t} = f_{1,i} + f_2 M_{i,t} + f_3 PE_{i,t} + f_4 TD_t + V_{i,t} \ , \tag{4}$$

$$Pc|a_{i,t} = q_{1,i} + q_2 M_{i,t} + q_3 JE_{i,t} + q_4 PI_{i,t} + q_5 PA_{i,t} + q_6 TD_t + x_{i,t} \ , \tag{5}$$

$$Pe|c_{i,t} = y_{1,i} + y_2 M_{i,t} + y_3 JE_{i,t} + y_4 PI_{i,t} + y_5 TD_t + z_{i,t} \ , \tag{6}$$

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 V, x, and z are regression error terms. Partisan influence is used to capture any political pressure to get tough with criminals, a message popular with Republican candidates. 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.[18] Also, note that all three equations include county fixed effects.

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 heteroschedasticity discussed earlier. We choose two-stage over three-stage least squares because while 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

[18] This does not include returns of parole violators, escapees, failed appeals, or transfers.

risky. Moreover, the two-stage least squares estimators are shown to be more robust to various specification problems.[19] Other variables and data are discussed next.

Data and Measurement Issues: We use a panel data set that covers 3,054 counties for the 1977-1996 period.[20] 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 way we control for the unobservable heterogeneity that arises from county specific attributes such as 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.

[19] See, e.g., Kennedy (1992, ch. 10).

[20] 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.

The data set includes crime and arrest data for murder, aggravated assault, and robbery. Given that some murders are the by-products 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 shifts the supply function. We include aggravated assault and robbery to examine this substitution effect.

The other control variables that we include in Z are real per capita personal income, real per capita unemployment insurance payments, real per capita income maintenance payments, population density, six gender and race segments of the youth population ages 10-29 (male, female; black, white, other), and the state level National Rifle Association (NRA) membership rate. We include economic and demographic variables, which are all available at the county-level, following other studies based on the economic model of crime.[21] In particular, we include population density because of the concentration of drug related activities in inner cities and their contribution to the murder rate. Age, gender, and race are included because of the differential treatment of youth by the justice system, variation in the opportunity cost of time through the life cycle, gender-influenced propensity to commit crime, and racially based differences in opportunities.

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

[21] Inclusion of the unemployment rate which is available only at the state level does not affect the results appreciably.

of the execution rate. Also, all equations include a set of time dummies that capture national trends and influences affecting all counties but varying over time.

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 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 along with 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 FBI's Uniform Crime Reports. The data on age, sex, and racial distributions, percent 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.

IV. Empirical Results

Regression results: The coefficient estimates for the murder supply equation (3) obtained using the two-stage least squares method and controlling for county-level fixed effects are reported in Tables 2 and 3.[22] Various models reported in Tables 2 and 3 differ in the way the perceived probabilities of arrest, sentencing and execution are measured. We first describe Table 2.

For model 1 in Table 2 the conditional execution probability is measured by executions at t+6 divided by number of death sentences at t. For model 2 this probability is measured by number of executions at t divided by number of death sentences at t–6. 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). 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+2 divided by the number of arrests for murder at t. For model 2 this probability is measured by number of death sentences at t divided by number of arrests for murder at t–2. 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 2 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

[22] Estimates of the coefficients of the other equations in the system are not reported, because we are mainly interested in equation (3) that provides direct inference about the deterrent effect. These estimates, however, are available from authors upon request.

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. In a similar fashion, a two-year lag and a two-year window length is used to measure the conditional death sentencing probabilities. Given the absence of an arrest lag, no averaging or lag displacement is used when computing arrest probabilities.[23]

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 3 are, respectively, similar to models 1, 2 and 3 in Table 2 except for the way we treat undefined probabilities. When estimating the models reported in Table 2, 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 2 are obtained excluding these observations. Alternatively, and to avoid losing data points, for any observation (county/year) where the probabilities of arrest or execution are undefined due to 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

[23] 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.

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 was committed, and hence no arrest was made, during this period. Rather, he will form an impression of the arrest odds based on arrests in recent years. This is consistent with Sah's (1991) argument. Table 3 uses this substitution rule to compute probabilities when they are undefined.

Results in Tables 2 and 3 suggest the presence of a strong deterrent effect. The estimated coefficient of the execution probability is negative and highly significant in all six models. This suggests that an increase in perceived probability of execution given that one is sentenced to death will lead to a lower murder rate. 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 that 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 seventy percent 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, and 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 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. More 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 teen-age 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 rate might be expected to be higher in more densely populated areas. 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 the suburbs.[24]

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 they 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.

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.

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[24] 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 density variable. The dummies were then interacted with the density variable. The estimated coefficient for models 1 through 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.

To further examine the robustness of our results, we also run similar regression models using state level data. Results are quite similar, particularly for the execution probability. In five of the six models this variable has a negative and significant coefficient estimate. In the remaining case the coefficient estimate is negative but not significant at the standard 5% level.

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,[25] and estimated the correlation between this variable and the execution variable. 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 suggest that the deterrent finding is driven by executions and not by tougher sentencing laws.

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[25] 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 (i) truth-in-sentencing laws which mandate that a violent offender must serve at least 85% of maximum sentence and (ii) "strikes" laws which significantly increase the prison sentences of repeat offenders.

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 trade-off between executions and murders? In other words, how many potential victims can be saved by executing an offender?[26] 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 are 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 which 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 trade-off. 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 trade-off between executions and murder we can use estimates of the execution deterrent coefficient $\hat{b}_3$ as reported in Tables 2 and 3. We focus on Model 4 in Table 3 which offers the most conservative (smallest) estimate of this coefficient. The coefficient b_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.

[26] Ehrlic (1975) and Yunker (1976) report estimates of such trade-offs using time-series aggregate data.

We evaluate this quantity for the U.S. using b_3 estimate in Model 4 and t = 1996, the most recent period that our sample covers. The resulting estimate is 18 with a standard error of 10 and therefore a corresponding 95% confidence interval of (8 through 28).[27] This implies that each additional execution has resulted, on average, in 18 fewer murders, or in at least 8 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 trade-off 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.

V. Concluding Remarks

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 but not dealt with. In this study, we change the focus to data issues.

We use a panel data set covering 3054 counties over the period 1977 through 1992 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 post-moratorium 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 to allow them to execute criminals. In fact, the absence of recent evidence on the effectiveness

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[27] The 95% confidence interval is given by +(-)1.96[Standard Error of ($\hat{b}_3$)](Population$_t$ /100,000) (1/S_{t-6})

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, the execution of each offender seems to save, on average, the lives of 18 potential victims. (This estimate has a margin of error of plus and minus 10). 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.

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 unfairness of the justice system and discrimination need to be considered when making a social decision regarding capital punishment.

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Table 1: Executions and Executing States

Year	Number of Executions	Number of States with Death Penalty Laws
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

Notes: Of the 38 states with death penalty laws, Connecticut, Kansas, New Hampshire, New Jersey, New York, and South Dakota have yet to execute any death row inmates. Tennessee had its first execution in April of 2000.

Table 2: Two-Stage Least Squares Regression Results for Murder Rate

Regressors	Estimated Coefficients Model 1	Model 2	Model 3
Deterrent Variables:			
Probability of Arrest	-4.037	-10.096	-3.334
	(6.941)**	(17.331)**	(6.418)**
Conditional Probability of Death	-21.841	-42.411	-32.115
Sentence	(1.167)	(3.022)**	(1.974)**
Conditional Probability of	-5.170	-2.888	-7.396
Execution	(6.324)**	(6.094)**	(10.285)**
Other Crimes:			
Aggravated Assault Rate	.0040	.0059	.0049
	(18.038)**	(23.665)**	(22.571)**
Robbery Rate	.0170	.0202	.0188
	(39.099)**	(51.712)**	(49.506)**
Economic Variables:			
Real Per Capita Personal Income	.0005	.0007	.0006
	(14.686)**	(17.134)**	(16.276)**
Real Per Capita Unemployment	-.0064	-.0077	-.0033
Insurance Payments	(6.798)**	(8.513)**	(3.736)**
Real Per Capita Income	.0011	-.0020	.0024
Maintenance Payments	(1.042)	(1.689)*	(2.330)**
Demographic Variables:			
% of Population that is	.0854	-.1114	.1852
African-American	(2.996)**	(4.085)**	(6.081)**
% of Population that is a Minority	-.0382	.0255	-.0224
other than African-American	(7.356)**	(.7627)	(4.609)**
% of Population that is Male	.3929	.2971	.2934
	(7.195)**	(3.463)**	(5.328)**
% of Population that is age 10-19	-.2717	-.4849	.0259
	(4.841)**	(8.021)**	(.4451)
% of Population that is age 20-29	-.1549	-.6045	-.0489
	(3.280)**	(12.315)**	(.9958)
Population Density	-.0048	-.0066	-.0036
	(22.036)**	(24.382)**	(17.543)**
NRA Membership Rate,	.0003	.0004	-.0002
(% state pop. in NRA)	(1.052)	(1.326)	(.6955)
Intercept	6.393	23.639	-12.564
	(.4919)	(6.933)**	(.9944)
F-Statistic	217.90	496.29	276.46
Adjusted R^2	.8476	.8428	.8624

Notes: Dependent Variable is the murder rate (murders/100,000 population). In Model 1 the execution probability is (# of executions at t)/(# of death row sentences at t-6). In Model 2 the execution probability is (# of executions at t+6)/(# 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. "**" and "*" represent significance at the 5% and 10% levels, respectively. The estimated coefficients for year and county dummies are not shown.

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

Regressors	Estimated Coefficients Model 4	Model 5	Model 6
Deterrent Variables:			
Probability of Arrest	-2.264	-4.417	-2.184
	(4.482)**	(9.830)**	(4.568)**
Conditional Probability of Death	-3.597	-47.661	-10.747
Sentence	(.2475)	(4.564)**	(.8184)
Conditional Probability of	-2.715	-5.201	-4.781
Execution	(4.389)**	(19.495)**	(8.546)**
Other Crimes:			
Aggravated Assault Rate	.0053	.0086	.0064
	(29.961)**	(47.284)**	(35.403)**
Robbery Rate	.0110	.0150	.0116
	(35.048)**	(54.714)**	(41.162)**
Economic Variables:			
Real Per Capita Personal Income	.0005	.0004	.0005
	(20.220)**	(14.784)**	(19.190)**
Real Per Capita Unemployment	-.0043	-.0054	-.0038
Insurance Payments	(5.739)**	(7.317)**	(5.080)**
Real Per Capita Income	.0043	.0002	.0027
Maintenance Payments	(5.743)**	(.2798)	(3.479)**
Demographic Variables:			
% of Population that is	.1945	.0959	.1867
African-American	(9.261)**	(4.956)**	(7.840)**
% of Population that is a Minority	-.0338	-.0422	-.0237
other than African-American	(7.864)**	(9.163)**	(5.536)**
% of Population that is Male	.2652	.3808	.2199
	(6.301)**	(8.600)**	(4.976)**
% of Population that is age 10-19	-.2096	-.6516	-.1629
	(5.215)**	(15.665)**	(3.676)**
% of Population that is age 20-29	-.1315	-.5476	-.1486
	(3.741)**	(15.633)**	(3.971)**
Population Density	-.0044	-.0041	-.0046
	(30.187)**	(27.395)**	(30.587)**
NRA Membership Rate,	.0008	.0006	.0008
(% state pop. in NRA)	(3.423)**	(3.308)**	(3.379)**
Intercept	10.327	17.035	10.224
	(.8757)	(8.706)**	(1.431)
F-Statistic	280.88	561.93	323.89
Adjusted R^2	.8256	.8062	.8269

Notes: Dependent Variable is the murder rate (murders/100,000 population). In Model 4 the execution probability is (# of executions at t)/(# of death row sentences at t-6). In Model 5 the execution probability is (# of executions at t+6)/(# 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. "**" and "*" represent significance at the 5% and 10% levels, respectively. The estimated coefficients for year and county dummies are not shown.