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The Effect of Executions on Homicides: A New Look in an Old Light

Richard Lempert

Professor Isaac Ehrlich, in his well-known article on the death penalty, argues that previous research on the deterrent effects of capital punishment, as exemplified by the work of Thorsten Sellin, is inadequate because it focuses on the wrong issue and because it fails to control for relevant variables. Ehrlich's first point is that if one is searching for deterrence it is the law in action (i.e., the actual incidence of executions) rather than the law on the books (i.e., the presence or absence of the death penalty) which is crucial. His second point is that in order to spot deterrent effects other factors which might affect homicide rates, such as conviction rates and unemployment rates, must be held constant. Many of those who believe that Ehrlich's work is fundamentally flawed nevertheless accept these criticisms.

This article follows Sellin's approach but takes account of Ehrlich's criticisms. Instead of comparing states on the basis of whether or not they have capital punishment statutes, it compares states on the basis of the number of murderers executed. It does this by correlating differences in executions with differences in homicide rates. Focusing on differences in this way does not separate out causal factors other than executions for specific control, but it is arguably a reasonably good control for the variety of often unmeasurable factors that are historically specific to given states and likely to affect homicide rates.

The results of this analysis are consistent with the basic finding of Sellin and others who have followed his procedures. The data provide no reason to believe that executions deter homicide. At the same time nothing about the data suggests that states that do not execute murderers enjoy lower homicide rates on this account. The results of the study may be limited because only states Sellin compared are examined and a number of arbitrary decisions had to be made regarding the coding of the data and the comparisons to be made. All such decisions were made a priori on theoretical grounds and are specifically noted in the paper. However, for these reasons it might not be unfair to treat the study as a pilot for an as yet unborn larger study that would look at data from the forty-eight contiguous states.

Isaac Ehrlich's 1975 study of capital punishment[1] is best known

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1. Isaac Ehrlich, "The Deterrent Effect of Capital Punishment: A Question of Life and Death," *American Economic Review*, June 1975, pp. 397–417.

for its conclusion that executions significantly deter homicide and for its estimate that over a period of more than three decades each execution has, on the average, saved between seven and eight lives. If these findings were all there were to Ehrlich's work, his 1975 paper might well be dismissed, for his particular model and his specific results have by now been thoroughly discredited.[2] However, in criticizing earlier research, primarily that of Thorsten Sellin,[3] Ehrlich made two important methodological points that have been accepted by many of his critics and that are reflected in the techniques of econometric modeling that have come to dominate the recent empirical research into the death penalty.

THE INDEPENDENT VARIABLE

Sellin's technique was to loc•. at pairs of proximate states, one with and one without the death penalty, and to determine whether long-term differences in homicide rates might plausibly be attributed to deterrent effects associated with laws providing for capital punishment. Sellin found no evidence of deterrence using this technique. Ehrlich's first criticism is of Sellin's choice of an independent variable. Ehrlich correctly points out that many of the "capital punishment" states in Sellin's sample executed first degree murderers only infrequently or not at all over long periods of time. Ehrlich argues that the perceived risk of execution in such states is not likely to be much greater than the perceived risk of execution in states without the death penalty, so comparisons involving such states cannot be expected to yield evidence of deterrence even if executions do in fact deter.

2. William Bowers and Glenn L. Pierce, "Deterrence, Brutalization, or Nonsense: A Critique of Isaac Ehrlich's Research on Capital Punishment," unpublished manuscript printed in part as Bowers and Pierce, "The Illusion of Deterrence in Isaac Ehrlich's Research on Capital Punishment," *Yale Law Journal*, December 1975, pp. 187–208; Stephen S. Brier and Stephen E. Fienberg, "Recent Econometric Modeling of Crime and Punishment: Support for the Deterrence Hypothesis?" *Evaluation Review*, April 1980, pp. 148–91; Lawrence R. Klein, Brian Forst, and Victor Filatov, "The Deterrent Effect of Capital Punishment," in *Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates*, Alfred Blumstein, Jacqueline Cohen, and Daniel Nagin, eds. (Washington, D.C.: National Academy of Sciences, 1978), pp. 336–60; Peter Passell and John B. Taylor, "The Deterrent Effect of Capital Punishment: Another View," *American Economic Review*, June 1977, pp. 445–51; and Jon K. Peck, "The Deterrent Effect of Capital Punishment: Ehrlich and His Critics," *Yale Law Journal*, January 1976, pp. 359–67.

For Ehrlich's responses to his critics, see Isaac Ehrlich, "Deterrence, Evidence and Inference," *Yale Law Journal*, December 1975, pp. 209–27; Isaac Ehrlich, "The Deterrent Effect of Capital Punishment: A Reply," *American Economic Review*, June 1977, pp. 452–58; Isaac Ehrlich and Joel C. Gibbons, "On the Measurement of the Deterrent Effect of Capital Punishment and the Theory of Deterrence," *Journal of Legal Studies*, Janury 1977, pp. 35–50; and Isaac Ehrlich and Randall Mark, "Fear of Deterrence," *Journal of Legal Studies*, June 1977, pp. 293–316.

3. Thorsten Sellin, *The Death Penalty* (Philadelphia: American Law Institute, 1959); Thorsten Sellin, *Capital Punishment* (New York: Harper & Row, 1967).

As Baldus and Cole[4] point out, given Sellin's desire to speak to the legal debate on the death penalty, it is by no means clear that he asked the wrong question. The deterrence issue, insofar as it bears on the constitutionality or desirability of a regime of capital punishment, is whether the laws that institute the regime result in fewer homicides than would otherwise occur. If a state executes murderers so rarely or so capriciously as to not achieve a deterrent effect, the value of the death penalty in that state is not enhanced simply because some other way of selecting people for death might have substantial benefits. Indeed, the experience in such states may be a proper measure of the potential utility of the death penalty if powerful sociological and legal forces guarantee that the death penalty is never likely to be regularly or systematically applied.

Nor is the question Sellin addresses theoretically sterile. There is no logical reason why a law allowing a fearsome penalty cannot deter even if that penalty is rarely invoked. Subjective fear of sanctions may turn more on the legal options open to the state than on those actually employed. The moral-educative effects of sanctions[5] may also reflect permissible rather than enforced threats. If severe sanctions are rarely applied it could be because the threat of the sanction is sufficient to prevent almost all behavior that would make people eligible for it. Sellin's research directly addresses this possibility. One must conclude from his results that it is not the case that death penalty statutes accompanied only rarely by executions are sufficient to deter homicide. Since in a number of Sellin's death penalty states executions occurred almost every year, one also finds in his results evidence that executing several murderers a year is insufficient to bring about substantial deterrent effects.

However, the fact that Sellin asked *a* right question does not mean that Ehrlich's preferred question, whether executions deter, is not right in its own way. Given the accumulated learning and research on deterrence,[6] it is plausible to suppose that the deterrent effect of a sanction is a function not of its legal status but of the incidence with which it is imposed. If discrete executions deter,[7] historic systems of capital punishment may, to the extent

4. David C. Baldus and James W. L. Cole, "A Comparison of the Work of Thorsten Sellin and Isaac Ehrlich on the Deterrent Effect of Capital Punishment," *Yale Law Journal*, December 1975, pp. 170–86.

5. Johannes Andenaes, "The General-Preventive Effects of Punishment," *University of Pennsylvania Law Review*, May 1966, pp. 949–83; Jack P. Gibbs, "Preventive Effects of Capital Punishment Other than Deterrence," *Criminal Law Bulletin*, January-February 1978, pp. 34–50.

6. See, e.g., Franklin Zimring and Gordon Hawkins, *Deterrence* (Chicago: University of Chicago Press, 1973); Gibbs, "Preventive Effects of Capital Punishment."

7. See the puzzling results of William F. Graves, "The Deterrent Effects of Capital Punishment in California," in *The Death Penalty in America: An Anthology*, rev. ed., Hugo A. Bedau, ed. (New York: Doubleday, 1967), pp. 322–32; and David P. Phillips, "The Deterrent Effect of Capital Punishment: New Evidence on an Old Controversy," *American Journal of Sociology*, July 1980, pp. 139–48.

that people were actually executed, have had a deterrent effect. Sellin's failure to find deterrence could result from his choice of an independent variable (i.e., the legal status of capital punishment), which confounds periods when deterrent effects might be expected (i.e., years with executions) with periods when no effect is likely (i.e., years with no executions). Misspecifying the independent variable in this way would attenuate associated effects, perhaps to the point where available statistical techniques will not allow the identification of deterrent effects at the requisite level of certainty.

The effects of executions are also important in the policy debate. Although the core issue involves the constitutionality and desirability of ongoing systems of capital punishment, if executions engender substantial deterrence, states may choose to change their ongoing systems, and assessments of constitutionality may change as well.

Empirical research on the deterrent effects of capital punishment is limited by historical patterns of executions. This is obvious in studies using the legality of the capital sanction as the independent variable, but it is no less true when the focus is on executions. It may be that executions have historically not deterred homicide, but that if executions occurred more frequently each execution would have a deterrent effect. However, if actual executions have historically not deterred, there is no good reason to believe that additional executions would have had a deterrent effect associated with them. Similarly, if executions have deterred historically there is no way of proving that additional executions would have had an incremental deterrent effect, but so long as any deterrence associated with executions in jurisdictions executing more than once does not diminish substantially in a given time period with each execution after the "nth," it is likely that additional executions in that time period would have saved more lives.

ADEQUATE CONTROL

Ehrlich's second criticism of Sellin's work is that it does not adequately control for variables other than the penalty structure which might explain homicide rate differences across states. Judging by Ehrlich's own work, these factors should include the probability of arrest for homicide, the conditional probability of conviction given arrest, labor force participation, unemployment rates, government expenditures, expenditures on the police, per capita income, age structure, and racial composition. Many researchers otherwise quite critical of Ehrlich's work share his view that Sellin's statistical techniques inadequately control for factors other than the penalty structure that may affect homicide rates or the incidence of executions.

Sellin is not, however, unconcerned with controlling for relevant variables. His technique of contrasting *contiguous* death penalty and abolitionist states *over the same period of time* is an attempt to do this. It is based on the expectation that the multitude of factors that conduce to or inhibit homicide will be relatively similar in neighboring states and will change in similar ways over time.

There is, admittedly, considerable error built into this approach, but it is arguably more adequate than the techniques of multiple regression favored by Ehrlich and other econometricians.[8] Data on the kinds of variables (or on likely proxies for such variables) that one would want to incorporate into a model of deterrence are often not available over time, and where such data are available their validity is often suspect.[9] One consequence of omitting an important variable from a regression model or of measuring included variables with invalid indicators is that effects associated with variables in the model may substantially distort reality. Given these data problems, Sellin's approach may control for relevant variables more adequately—if less specifically—than does Ehrlich's approach. Indeed, Sellin's particular research, which involves cross-state comparisons for the years 1920 through 1955, could not have been done with a model that was anything like Ehrlich's. The quality and availability of relevant data diminish substantially before 1940 and most of the necessary state-related information is not available except in census years.

However, the fact that Sellin's approach is defensible does not mean that his cross-state comparisons adequately control for factors other than penalty structure which affect homicide rates. Indeed, Sellin's own data indicate that the assumption that neighboring states are initially similar on factors affecting the homicide rate is untenable as a general proposition. If neighbors were similar there would not be, absent an implausibly large brutalizing effect attributable to capital punishment, the markedly different homicide rates that characterize such neighboring states as Michigan and Ohio or Colorado and Kansas.

There is substantially more support in Sellin's data for the proposition that neighboring states, given their initial positions, are affected in much the same way by changes in those factors that affect homicide rates. Generally speaking, the homicide trends in Sellin's paired states are similar over time. Thus, whatever the initial difference in factors that predict to homicide, changes in adjacent states tend not to be dramatically different. It is with this observation that my analysis begins.

A MODIFICATION OF SELLIN

If, as Ehrlich suggests, each execution saves an average of eight lives, one would expect the magnitude of differences in annual homicide rates to re-

8. Baldus and Cole, "A Comparison of the Work."
9. Bowers and Pierce, "Deterrence, Brutalization, or Nonsense."

flect yearly differences in executions. The advantage of the more persistently executing state should increase with each additional execution. If we are willing to hypothesize an execution-homicide trade-off, we may calculate the expected effect of executions on homicide rates by multiplying the difference in the number of executions by the reciprocal of the trade-off ratio and dividing by the population in 100,000s of the executing state. Thus, if a state of four million people executes five murderers in a given year, a trade-off ratio of 1:8 leads one to expect that that state's homicide rate for the year will be one per 100,000 less than it would have been had that state executed no one. In such a year, that state's homicide rate should compare more favorably with the rate of an abolitionist neighbor than it does in years that that state executes fewer people, even though the executing state may for historical reasons have always had a higher homicide rate than its neighbor.

Focusing on the association between differences in rates of execution and rates of homicide in neighboring states has several advantages over Sellin's approach, two of which respond to Ehrlich's criticisms. First, it offers better control for a host of unmeasured relevant variables since historical factors peculiar to each state are controlled along with factors that change in neighboring states in much the same way over time. Second, it focuses on actual executions rather than on penalty structures; thus, the effort to spot deterrence is not confounded by situations where an authorized death penalty has fallen into disuse or by years where a state that is willing to execute does not. Finally, with the focus on the relationship between execution rate and homicide rate differences it is possible to compare executing states with each other as well as with abolitionist neighbors.[10] For reasons of convenience I have restricted my analysis to those states examined by Sellin, but the technique can be used with any pairs of neighboring, or within certain limits, nonneighboring states.

THE DATA

The information on homicide rates in this analysis is taken from Sellin,[11] who relies on vital statistics data for the years 1920-55. The execution data are drawn from the Teeters and Zibulka inventory of executions under state

10. In comparing two death penalty states the number of executions in one state must be adjusted if rate data are used because if each execution saves the same number of lives, homicide rates will be more substantially affected by a given number of executions in the less populous state. Thus, if a state of two million people and a state of four million each execute a person, Ehrlich's results predict that the execution should decrease the smaller state's rate by .4 per 100,000 but should decrease the larger state's rate by only .2. Where death penalty states are compared in this study, the number of executions is appropriately, although loosely, adjusted and rounded to the nearest integer. (Population data were only available for census years. The adjustments reflect an estimated average population for each decade.)

11. Thorsten Sellin, *The Death Penalty* (Philadelphia, Pa.: American Law Institute, 1959).

authority as presented in Bowers.[12] Executions in year T are defined as the total number of executions in the last four months of year T-1 plus those in the first eight months of year T. For example, executions occurring from September through December 1939 are considered 1940 executions, while those occurring in the same months of 1940 are attributed to 1941. There are two reasons for this. First, the deterrent effect of an execution, although probably greatest immediately after the execution, is likely to linger for several months. I made the arbitrary, but, it is to be hoped, plausible decision that executions as early as four months before the beginning of calendar year T might be expected to have the bulk of their deterrent effect in year T. Second, there is the possibility that defendants are more likely to receive a death sentence when homicide rates are high than when the rates are low. Thus, the research would be biased against finding a deterrent effect if some lag were not built in. I felt, based on fragmentary evidence, that in most cases there would be at least an eight-month lag between the date of sentencing, which might be expected to be affected by the crime rate, and the date of execution, which is less likely to be so affected.[13] However, if the lag built into these data does correct for a tendency to sentence more severely when homicide rates are high, statistical regression could bias the results in the direction of showing a deterrent effect. A crude check indicates that for some states these assumptions regarding sentencing are plausible, but that regression artifacts do not substantially bias these comparisons.[14] They might, however, explain small differences.

12. William J. Bowers, *Executions in America* (Lexington, Mass.: D. C. Heath, 1974).

13. If the rate of pardons or stays of executions were affected by current crime rates this assumption would be false, and executions would be more likely when crime rates and concern for crime were high. This danger does not seem great. Pardons often seem to be a function of the personal values of state governors and stays of executions, often automatic upon proper appeals, are granted by judges who may be relatively insulated from political pressures in the state. The validity of the delay assumption will vary with the time and personal characteristics of the defendant. In the early years of the time series examined and with less privileged defendants the typical delay may have been less than eight months. In later years and with more advantaged defendants the delay between sentence and execution might have been two years or more.

14. If a homicide rate reflects both consistent causes and random effects, years following years with exceptionally high homicide rates are likely to have lower rates because random effects in the extreme year are likely to have been consistently conducive to homicide (thus making it extreme) while in the subsequent year they are likely to be more balanced since they are random. If homicide sentences are more likely in years with high murder rates one would expect that subsequent years, when executions are carried out, will have lower rates because of expected statistical fluctuations even if the death penalty does not deter.

Statistical regression is clearly evident in the data we shall examine and usually dominates time trends. Crude calculation by hand (which is to say there is a possibility of slight error) reveals that for the dozen states we shall look at over the time period we shall examine there were 111 instances where a homicide rate fell or rose by .7. In 69 percent of those cases, that state's rate in the next year moved in the opposite direction (i.e., a year with a rate .7 or more higher than the preceding year was followed by a year with a lower rate with the reverse being true for

Since controlling for other factors, however crudely, is crucial to the analysis, not all periods are of equal theoretical relevance. In situations where the typical difference in the homicide rates of paired states changes abruptly and substantially from one period to another or when, at a certain point, yearly homicide rates change abruptly and substantially in one or both of the comparison states, the *ceteris paribus* assumption becomes untenable. Thus, in different states different time periods are examined. They were chosen by eye to be periods in which the fluctuations in rate differences, however broad, tended to be about the same point and periods in which the homicide rates within states were either relatively constant, fluctuating about the same point, or tending gradually in one direction.

It should be obvious that a number of arbitrary decisions have been made in the course of this research. All such decisions were made on theoretical grounds for reasons I have noted. Thus, I intentionally have not examined the data with execution dates lagged by more or less than the four-month period which was selected a priori for the reasons given. Decisions concerning the appropriate periods for analysis were made by looking at the data, but without attending to the relationship between differences in executions and differences in homicide rates. Because of suspicions of investigator bias which plague death penalty research, I present almost every result I gener-

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years in which rates fell by .7). In 25 percent of the cases trends continued and in 5 percent of the cases the subsequent rate showed no change.

Nevertheless, it appears that the data I shall report are not greatly biased by regression artifacts even though for a number of states an increase in the annual number of executions is associated with the decrease in homicide rates that one would expect from regression (if it is correct to assume that death sentences, which are carried out in the next year, are likely when murder rates are high) and/or from deterrence. This is because the rates of neighboring states tend to rise and fall with those of the executing state. For example, in ten of seventeen years when Ohio's execution rate rises its homicide rate falls, in six years the situation is reversed, and in one there is no change. However, in Michigan for those same years there are eleven instances where the preceding year's rates were higher, four where they were lower, and two where they were the same. For Indiana during Ohio's years of increasing executions (six of which saw executions increase in Indiana) homicide rates in the preceding year were higher nine times, lower six times, and the same twice. This is what would be expected if large apparently random fluctuations in homicide rates over time are in fact caused by factors such as the weather which are common to regions. It is not consistent with deterrence theory unless executions in death penalty states convince those in neighboring states that there is a chance they will be executed or that they will be punished more severely if they kill. The pattern is also consistent with the hypothesis of spillover effects from normative validation.

When a hypothesis of randomness is clearly competitive with the hypothesis of a systematic cause, the assumption that one is witnessing a random process is usually preferred unless one has good evidence to the contrary. Here the good evidence, that is, the general presence of regression artifacts in these data, suggests we are witnessing effects that are random in time but relatively widespread when they exist. However, it should be emphasized that my tests for regression are rather crude (e.g., they consider the direction but not the size of effects). The matter deserves more thorough testing.

ated.[15] It is possible that a different set of theoretically defensible but arbitrary decisions as to comparison periods or execution lags would yield different results, but unless the periods were chosen with an eye to the desired results it is most unlikely. The decision to minimize preliminary data analysis and to sacrifice possibilities of induction is in this study a core methodological principle.

RESULTS

The more stringent test of the deterrence hypothesis is the correlation analysis presented in Table 1, while the more informative involves the grouped data reported in Tables 2 through 13. The correlation analysis is the more stringent test because it assumes a linear relationship. Executions might save lives, but if six executions are likely to save no more lives than four, linear correlations will be attenuated.[16]

Table 1 presents the array of correlations that were generated. In all cases the coding was done so that deterrence theory would predict an inverse re-

15. Everything except the correlation analysis was done by hand because it appeared when I started that this would be quicker than putting the data on the computer. Although all work was double checked, there may be some "calculator error." Because the work was done by hand and because the test of deterrence theory would only be fair if comparisons were chosen beforehand on theoretical grounds, exploratory data analysis was kept to a minimum. Data are reported on all states compared. The New England group of New Hampshire, Vermont, and Maine was ignored because the two states that had the death penalty rarely executed. I did a few comparisons not presented because upon reflection they did not make as much theoretical sense as what is presented below. For example, at an early stage I compared Ohio and Michigan by decade because I sensed that time was an important dimension. As I thought about this I realized that time was an important control insofar as different periods were probably characterized by differences in the characteristics of variables affecting the homicide rates. Once I appreciated this, I looked at the homicide rates and their differences to determine appropriate comparison periods. The decade data are not presented. I also initially compared pairs of death penalty states without correcting for population differences. The results of the analyses not presented are consistent with what is reported here.

16. Ehrlich does not estimate his equations in linear form. With different values of execution and homicide rates the elasticities he found might suggest different trade offs. Ehrlich summarized his results in their linear form: *Each* execution saves eight lives. It is the well-publicized statement, often presented as a finding or prediction, that is used as a bench mark in assessing these data. A fairer test of Ehrlich would evaluate the trade off separately for each state using Ehrlich's elasticities and homicide and execution data for each state. Treating Ehrlich's conclusions as a hypothesis for purposes of policy analysis, I have chosen to specify that hypothesis in the terms in which Ehrlich's findings have entered the popular debate. As we shall see, the data generally do not support the conclusion of an inverse relation between the conditional probability of execution and the homicide rate whatever the trade off implied by Ehrlich's elasticities, even if evaluated on a state-by-state basis.

Ehrlich makes his prediction of a 1:8 trade-off ratio without the *ceteris paribus* incorporated in his model that conviction probabilities be held constant. If the data show no relationship between executions and homicide rate differences it is possible, but not plausible, that executions have the effect Ehrlich predicts but that differences in conviction (or arrest) probabilities offset lationship between differences in homicide scores and differences in execution scores. Years in parentheses were judged before the correlations were run to be not theoretically interesting because it was either doubtful whether other factors were the same throughout the period or there was no reason to believe that other factors were any different in the years in question than over some longer time frame.

There are twenty-seven correlations for eleven pairs of states that were defined on an a priori basis as theoretically interesting. In only one case, the Ohio-Michigan comparison for the years 1931-36, is there a correlation in the predicted direction that reaches conventional levels of significance. However, even this correlation may not be regarded as statistically significant since there are enough independent correlations that one correlation at the .05 level might well be a random effect. Most correlations are small. Of the twenty-seven correlations, twelve are in the predicted direction (negative), fourteen are not, and one rounds off to zero. Looking only at correlations for periods that do not overlap and choosing where we are confronted with a choice so as to favor the deterrence hypothesis, we find nine correlations in the predicted direction, nine in the opposite direction, and one that is essentially zero. Looking at all correlations suggests that we should not have expected the period 1938-55 in the Connecticut-Massachusetts comparison to be uninteresting since the pattern is very different during those years from what it was before 1937 or over the entire period. We also need not have looked separately at the 1920-37 and 1938-55 correlations for Massachusetts-Rhode Island since they both reflect the pattern of the entire period.

In addition to these correlations, the data were transformed in comparisons involving executing and abolitionist states so that for any period without executions the probability of an execution in a death penalty state during the first six nonexecuting years after an executing year would be treated as .5, .3, .1, .01, .001, and .0001, respectively. The theory behind this transformation is that the subjective probability of execution (which is what is crucial in deterrence theory) remains relatively high the first year after an execution, sinks gradually as the next nonexecution years follow, and then sinks more

these effects. If in the real world conviction (and/or arrest) probabilities have such an inverse relationship with execution probabilities and if there is no prospect of changing the system to eliminate this, the policy implications argue in favor of abolishing capital punishment since without capital punishment we would have the same degree of deterrence and be apprehending and convicting more guilty murderers. It is also true that Ehrlich's prediction is based on aggregated national data while we are looking separately at state rates. It is possible but not plausible that the states we shall examine did not contribute to the effect Ehrlich spotted. If so, in those states where executions save lives they save many more than eight, and Ehrlich's model fails to capture substantial interaction between state characteristics and penal structure which has important policy implications.

Table 1. Correlations between Differences in Execution Rates and Differences in Homicide Rates for Selected States and Selected Time Periods

Ohio—Michigan	
(1920 - 1955)	.28
1920 - 1929	.16
1931 - 1936	-.87[a]
1937 - 1949	.13
Ohio—Indiana	
(1920 - 1955)	.07
1920 - 1935	-.34
1936 - 1949	.06
1950 - 1955	.31
Iowa—Minnesota	
(1923 - 1955)	-.14
1923 - 1936	-.09
1938 - 1955	-.17
1926 - 1936	.08
1926 - 1955	-.07
Iowa—Wisconsin	
1923 - 1955	-.05
(1923 - 1938)	-.03
(1939 - 1955)	-.06
Indiana—Michigan	
(1920 - 1955)	.03
1920 - 1939	.00
1940 - 1955	.12
Missouri—Colorado	
1938 - 1955	.04
Colorado—Kansas	
1920 - 1955	.09
1926 - 1955	.30
1920 - 1938	-.30
1926 - 1938	.03
1939 - 1955	.37
Massachusetts—Rhode Island	
1920 - 1955	-.06
1920 - 1937	-.05
1938 - 1955	-.07
Connecticut—Massachusetts	
1920 - 1955	.05
1920 - 1937	.28
1938 - 1955	-.36
Connecticut—Rhode Island	
1920 - 1955	-.18
1920 - 1937	-.08
(1938 - 1955)	-.29
Missouri—Kansas	
1938 - 1955	.37

Note: Negative correlations support the deterrence hypothesis.
[a]Significant at the .05 level.

precipitously until it levels off, at .0001. The numbers used were quite arbitrarily chosen—they seemed plausible to me—and may utterly fail to capture changes in subjective probabilities of death. In any case there was not one instance where executing, measured either by actual executions and the specified transformations for zero execution years or by the natural logs of these numbers, correlated significantly with difference scores.

The overwhelming impression that one gets from this correlation analysis is that there is no linear relationship between a state's willingness to execute

Table 2. The Influence of Executions on Relative Homicide Rates for Selected Pairs of States and Selected Time Periods

Comparison (Rows on tables 3-13)	*Ohio-Indiana*			*Ohio-Michigan*			*Indiana-Michigan*		*Iowa-Minnesota*		*Iowa-Wisconsin*
	1920-35	*1936-49*	*1950-55*	*1920-29*	*1930-36*	*1937-49*	*1920-39*	*1940-55*	*1923-36*	*1938-55*	*1923-55*
A-B	8.6	1.5	X	X	10.8	X	X	X	.7	2.9	5.8
A-C				X	8.6		X		1.5	1.0	X
A-D							.2				
B-C				.6	3.4		X		2.2	X	X
B-D							4.2				
C-D							13.8				
Relationships in predicted direction	Yes	Yes	No	MN	Yes	No	Mixed	No	Yes	MY	MN

Table 2. Cont'd

	Comparison							
(Rows on tables 1-13)	*Colorado-Kansas* 1920-38	*Colorado-Kansas* 1939-55	*Missouri-Kansas* 1938-55	*Missouri-Colorado* 1938-55	*Massachusetts-Rhode Island* 1920-55	*Connecticut-Rhode Island* 1920-55	*Connecticut-Massachusetts* 1920-37	*Connecticut-Massachusetts* 1938-55
A-B	X	X	X	X	6.3	2.9	2.7	X
A-C	8.4	X	X		4.5	3.1	X	2.2
A-D	2.6	X			.6			
B-C	24.6	10.4	.2		2.7	3.4	X	8.6
B-D	6.0	X			X			
C-D	X	X			X			
Relationships in predicted direction?	MY	MN	MN	No	MY	Yes	MN	MY

Notes: An "X" indicates a relationship that is not in the predicted direction. Numbers are found where relationships are consistent with deterrence theory. They indicate the number of lives that each execution would have to save if homicide rate differences between states were entirely attributable to the deterrent effects of executions.

MN indicates the direction of the results are m ixed depending on the number of executions compared, but the majority of results are inconsistent with deterrence theory. MY indicates similarly mixed results with the majority of comparisons favoring deterrence theory.

and the number of lives that state loses to homicides. When the data are taken as a whole, there is no evidence suggesting that executions save lives.

The second test presents homicide rate differences in comparison states controlling for the number of executions. In all cases comparisons are drawn so that deterrence theory predicts that more executions by the death penalty state in death penalty-abolitionist comparisons or an execution advantage for the first named of two executing states will be associated with a lower positive or a higher negative difference score. Thus, as one reads down the rows in Tables 3-13, deterrence theory is supported only when a number in a higher row is greater than a number in a row beneath it. Whenever a difference was in the predicted direction, the magnitude of the execution–homicide trade-off was determined. This required estimates of the average population of the more persistently executing state over time. Such estimates were purposely made high so as to bias the findings in favor of deterrence theory.

It is difficult to summarize these data. Different patterns of executions in the various states meant that the data were sensibly cut at different points in different jurisdictions. Furthermore, the number of cases is often so small that observed differences are statistically unreliable. Table 2, which summarizes the data, should be read in conjunction with the paired comparison tables (3-13) to determine cell sizes and the number of executions that are compared in the rows labeled A-B, A-C, A-D, and so on in the summary table. An X in a cell of Table 2 indicates a relationship that is not in the predicted direction. Where a relationship is in the predicted direction there is a number which indicates the number of lives that each execution would have had to save if the difference were due entirely to deterrence. Predicted differences in Tables 3-13 are those differences in homicide rates that one would expect if executions deterred with Ehrlich's suggested trade-off ratio of 1:8.

Look, for example, at the figures for the Ohio-Indiana comparison in the summary table and in Table 3, which reports the Ohio-Indiana data. Table 3 indicates that during those years from 1920 to 1935 when Indiana, in proportion to its population, was within five executions of Ohio, Ohio's homicide rate was relatively greater than it was when Ohio's annual "advantage" in executions was at least six. In other words, between 1920 and 1935, Ohio, for a variety of unknown reasons, almost always had a higher annual homicide rate than Indiana, but Ohio's homicide rate did not exceed Indiana's by as much in the years when Ohio executed relatively larger numbers of people. The figure 8.6 in the top left-hand cell of Table 2 indicates that the diminution in the rate differences is almost exactly what one would predict if each execution in both Ohio and Indiana saved an average of eight lives. This coincides with the trade-off ratio that best characterized Ehrlich's (1975) results.

Table 3 also summarizes the results for the years 1936-49, and indicates that the dividing point on executions for this comparison is where Ohio annually executes at least four more people than does Indiana. The presence of a number for this comparison in Table 2 indicates that the difference is in the predicted direction, but the trade-off in this case is 1.5. Deterrence theory is supported, but Ehrlich's publicized estimation is not.

The third cell on Table 2 for the Ohio-Indiana comparison has an *X*, indicating that for the years 1950-55 the results were not in the predicted direction. A glance at Table 3 indicates that these results might well be unstable since they contrast two years when neither Indiana nor Ohio executed with four years when Ohio executed four or five more persons than did Indiana.

The 1920-29 column on Table 2 for the Ohio-Michigan comparison presents a situation where results are trichotomized. Table 4 reveals that Ohio's homicide rate compared more favorably with Michigan's when Ohio executed six or fewer people than when it executed either seven exactly or eleven or more. Thus, an *X* appears on Table 2 for the A-B and A-C comparisons. Since Ohio did better when it executed eleven or more than when it executed seven, one comparison, that for rows B-C, is in the predicted direction. However, the number, .6, indicates a relatively slight savings in moving from seven to eleven or more executions. The overall pattern for the Ohio-Michigan comparison is inconsistent with the deterrence hypothesis.

Looking at the overall summary sheet (Table 2) we see that deterrence theory finds relatively little support in these data and Ehrlich's popularized prediction even less. Thirty-one of fifty-seven relationships are in the direction predicted by deterrence theory. In only thirteen of these is the trade-off ratio as much as 1:4.[17] The deterrence hypothesis receives no more support when we focus on the total picture within paired states to avoid representing some states by three time periods and others by one. The overall picture indicates that there are about as many pairs of states in which additional executions are associated with relatively higher homicide rates in the executing states as there are pairs whose relationship is in the predicted direction. These patterns suggest that nonlinear models which accord disproportionate weight to earlier or later executions would not improve the fit of the correlation analysis to the data presented in Table 1. Where results are mixed, nothing about the data suggests that the deterrent effect of the first execution that distinguishes states is consistently different from that of the *n*th, nor is there evidence suggesting that the deterrent effect on the *n*th execution that distinguishes states is consistently different from the *m*th.

Theoretically the most important predictions are those that involve rows A and B or, in the case of the executing pairs Missouri-Kansas and Massachusetts-Connecticut, rows A and C. In comparisons of death penalty and abolitionist states row A, except for Ohio-Michigan, reports rate differences when the death penalty state does not execute, while row B reports differences where the death penalty state executes once. This may well mark the difference between years when the death penalty was seen as a real threat in

17. The magnitude of a number of differences is so slight that they might be explained as regression artifacts if regression is a problem with these comparisons.

Table 3: Relationship between Execution and Homicide Rate Differences: Ohio-Indiana

	Difference for 1920-35[a]		*Difference for 1936-49*[b]		*Difference for 1950-55*
(A) Indiana within 5 executions of Ohio ($\bar{X}$ = 3)	2.02 (9)	(A) Indiana within 3 executions of Ohio ($\bar{X}$ = −.88)	1.16 (8)	(A) Neither executes	-.05 (2)
(B) Ohio executes at least 6 more than Indiana ($\bar{X}$ = 8.57)	1.46 (7)	(B) Ohio executes at least 4 more than Indiana ($\bar{X}$ = 5.83)	1.02 (6)	(B) Ohio executes 4 or 5 more ($\bar{X}$ = 4.25)	.13 (4)

[a]Predicted difference = .68 (Ohio estimated population = 6.6 million).
[b]Predicted difference = .73 (Ohio estimated population = 7.4 million). All differences in executions are adjusted for population differences.

Predicted differences in rates in this and subsequent tables are the differences one would expect if each execution saved 8 lives. Differences are calculated only when the direction of the differences is in the predicted direction.

Table 4. Relationship between Execution and Homicide Rate Differences: Ohio-Michigan

	Difference for 1920-29[a]		*Difference for 1930-36*[b]		*Difference for 1937-49*
(A) Ohio executes 5 or 6 ($\bar{X}$ = 5.67)	.90 (3)	(A) Ohio executes 5-8 ($\bar{X}$ = 6.67)	3.07 (3)	(A) Ohio executes 2 or 3 ($\bar{X}$ = 2.67)	1.25 (6)
(B) Ohio executes 7	1.03 (3)	(B) Ohio executes 9	2.70 (2)	(B) Ohio executes 4-15 ($\bar{X}$ = 8.43)	1.29 (7)
(C) Ohio executes 11-15 ($\bar{X}$ = 12.25)	.98 (4)	(C) Ohio executes 10	2.65 (2)		

[a]Predicted difference B-C = .67 (Ohio estimated population = 6.3 million).
[b]Predicted difference A-B = .27, A-C = .39, B-C = .12 (Ohio estimated population = 6.8 million).

Table 5. Relationship between Execution and Homicide Rate Differences: Indiana-Michigan

		Difference for 1920-39[a]		Difference for 1940-55
(A)	Neither state executes	−.50		−.03
		(2)		(12)
(B)	Indiana executes 1	.05		
		(8)		
(C)	Indiana executes 2-3	.90[b]	(B)	.15
	($\bar{X}$ = 2)	(5)	($\bar{X}$ = 2.25)	(4)
(D)	Indiana executes 4 or	−.52[c]		
	more ($\bar{X}$ = 5.4)	(5)		

[a] Predicted difference A-D = 1.31, B-D = 1.07, C-D = .82 (Indiana estimated population = 3.3 million).
[b] Equals .23 if 1 year with an exceptionally large difference because of a sharp rise in the Indiana homicide rate is eliminated.
[c] Equals .50 if 1 year with an exceptionally large difference because of a sharp rise in the Michigan homicide rate is eliminated.

Table 6. Relationship between Execution and Homicide Rate Differences: Iowa-Minnesota

		Difference for 1923-55[a]	*Difference for 1926-55*[d]	*Difference for 1923-36*[b]	*Difference for 1938-55*[c]
(A)	Iowa executes	-.12	-.12	-.43	.01
	none	(19)	(19)	(7)	(11)
(B)	Iowa executes	-.30	-.11	-.46	-.10
	1	(9)	(7)	(5)	(4)
(C)	Iowa executes	-.28	-.23	-.55	-.10
	2 or more ($\bar{X}$ = 2.4)	(5)	($\bar{X}$ = 2.5) (4)	($\bar{X}$ = 2) (2)	($\bar{X}$ = 2.67) (3)

[a]Predicted difference A-B = .31, A-C = .74, B-C = .43 (Iowa estimated population = 2.6 million).

[b]Predicted difference A-B = .32, A-C = .64, B-C = .32 (Iowa estimated population = 2.5 million).

[c]Predicted difference A-B = .31 (Iowa estimated population = 2.5 million). Note that this period begins at 1938 and not 1937 because Minnesota's homicide rate shows a sharp increase in 1937 which does not occur in Iowa until 1938.

[d]The years 1920-25 include five of the eight years from 1920 to 1955 when Minnesota's homicide rate was above 3.0. The average rate during this period of 3.5 was exceeded by only one year after 1925. Thus a fairer comparison of Iowa and Minnesota may begin in 1926.

Table 7. *Relationship between Execution and Homicide Rate Differences: Iowa-Wisconsin*

		Difference for 1923-55[a]
(A)	Neither state executes	.14
		(19)
(B)	Iowa executes 1	-.08
(C)	Iowa executes 2 or more ($\bar{X}$ = 2.4)	.36
		(5)

[a]Predicted difference A-B = .36 (Iowa estimated population = 2.6 million).

Table 8. Relationship between Execution and Homicide Rate Differences: Colorado-Kansas

	Difference for 1920-55[a]		*Difference for 1926-55*[b]		*Difference for 1920-38*[c]	*Difference for 1926-38*		*Difference for 1939-55*[d]
(A) Neither executes or								
Kansas executes more	1.52		.97		2.63	1.73		.69
($\bar{X}$ = -.21)	(14)	($\bar{X}$ = -.27)	(11)	($\bar{X}$ = 0)	(6)	(3)	($\bar{X}$ = -.5)	(6)
(B) Colorado executes	2.43		1.72		3.37	2.12		1.50
1 more than Kansas	(12)		(9)		(6)	(3)		(6)
(C) Colorado executes	.90		.90		1.03	1.03		.70
2 more than Kansas	(5)		(5)		(3)	(3)		(2)
(D) Colorado executes at								
least 3 more than	1.80		1.80	($\bar{X}$ = 4)	1.65	1.65	($\bar{X}$ = 3)	2.40
Kansas ($\bar{X}$ = 3.8)	(5)		(5)		(4)	(4)		(1)

[a]Predicted difference A-C = 1.47, B-C = .67, B-D = 1.87 (Colorado estimated population = 1.2 million).
[b]Colorado's homicide rate decreased sharply between 1924 and 1925 and the difference between the two dropped sharply between 1925 and 1926. All but one rate difference greater than 3.0 was before 1926.
[c]Predicted difference A-C = 1.52, A-D = 3.05, B-C = .76, B-D = 1.52 (Colorado estimated population = 1.05 million).
[d]Predicted difference B-C = .62 (Colorado estimated population = 1.3 million).

Table 9. Relationship between Execution and Homicide Rate Differences: Missouri-Colorado

		Difference for 1938-55
(A)	Missouri executes less than Colorado ($\bar{X}$ = -4.3[a])	1.52 (10)
(B)	Neither executes or Missouri executes more ($\bar{X}$ = 1.38)	1.68 (8)

[a]Adjusted for population differences.

Table 10. Relationship between Execution and Homicide Rate Differences: Missouri-Kansas

		Difference for 1938-55
(A)	Kansas executes more than Missouri ($\bar{\chi} = -3.0^a$)	2.20 (5)
(B)	Neither executes	2.87 (3)
(A)	Missouri executes more than Kansas ($\bar{X} = 2.2^a$)	2.86 (10)

[a]Adjusted for population differences.

Table 11. Relationship between Execution and Homicide Rate Differences: Massachusetts—Rhode Island

		Difference for 1920-55[a]	*Difference for 1920-37*[b]	*Difference for 1938-55*[c]
(A)	Neither state executes	.04 (17)	.17 (6)	-.04 (11)
(B)	Massachusetts executes 1	-.10 (7)	-.22 (5)	.20 (2)
(C)	Massachusetts executes 2	-.16	-.33	.00
(D)	Massachusetts executes at least 3 ($\bar{X}$ = 3.17)	.00 (6)	($\bar{X}$ = 3.25) .13 (4)	($\bar{X}$ = 3) -.25 (2)

[a]Predicted difference A-B = .18, A-C = .36, A-D = .58, B-C = .18 (Massachusetts estimated population = 4.4 million).
[b]Predicted difference A-B = .19, A-C = .38, A-D = .62, B-C = .19 (Massachusetts estimated population = 4.2 million).
[c]Predicted difference A-D = .53, B-D = .36, C-D = .18 (Massachusetts estimated population = 4.5 million).

Table 12. Relationship between Execution and Homicide Rate Differences: Connecticut—Rhode Island

	Difference for 1920-55[a]		*Difference for 1920-37*[b,c]
(A) Neither state executes	.63		.66
	(20)		(10)
(B) Connecticut executes	.47		.52
1	(9)		(5)
(C) Connecticut executes	.21		.33
2 or more ($\bar{X}$ = 2.4)	(7)	($\bar{X}$ = 2.3)	(3)

[a]Predicted difference A-B = .44, A-C = 1.07, B-C = .62 (Connecticut estimated population = 1.8 million).
[b]Predicted difference A-B = .50, A-C = 1.17, B-C = .67 (Connecticut estimated population = 1.6 million).
[c]The figures for the period 1920-37 were so close to the figures for the entire period that a separate rate for 1938-55 was not calculated. One can see from the information given that the predictions would be slightly closer to the actual pattern in the later period than in the two periods examined above.

Table 13. Relationship between Execution and Homicide Rate Differences: Connecticut—Massachusetts

		Difference for 1920-53[a]		*Difference for 1920-37[b]*		*Difference for 1938-55[c]*
(A)	Massachusetts executes more than Connecticut ($\bar{X}$ = -2.2[d])	.52 (10)	($\bar{X}$ = −2.67)	.50 (6)	($\bar{X}$ = −1.5)	.55 (4)
(B)	Neither state executes	.40 (11)		.33 (4)		.59 (7)
(C)	Connecticut executes more than Massachusetts ($\bar{X}$ = 3.27[d])	.58 (15)	($\bar{X}$ = 3.13)	.81 (8)	($\bar{X}$ = 3.43)	.31 (7)

[a]Predicted difference A-B = .40 (Massachusetts estimated population = 4.4 million).
[b]Predicted difference A-B = .50 (Massachusetts estimated population = 4.3 million).
[c]Predicted difference A-C = .86, B-C = .60 (Massachusetts estimated population = 4.6 million).
[d]Adjusted for population differences. The adjustment made it easier to calculate the predicted difference on the basis of Massachusett's population than on the basis of Connecticut's.

the executing state and years when it was not. In the case of the named executing pairs, row A reports rate differences where the first named state executes more and row C reports differences where the second named has the advantage. Nine of these theoretically important relationships are in the direction predicted by deterrence theory and ten are not. Only four of the nineteen comparisons suggest trade-off ratios as high as 3.0.

Perhaps the most interesting findings are those involving comparisons between executing pairs. Missouri does better vis-à-vis Colorado and Kansas when the latter states are executing *more* people relative to their populations, and Colorado does worse vis-à-vis Kansas (after 1938) when Colorado executes proportionately more people. In the case of Massachusetts and Connecticut, the more persistently executing state has its predicted advantage between 1938 and 1955 but not between 1920 and 1937. For Ohio and Indiana more executions are associated with a relatively lower rate during two of the three periods examined.

CONCLUSION

While the issue of whether actual executions deter is not necessarily of greater theoretical interest or policy importance than is the question of whether laws providing for the death penalty deter, it is undeniably an interesting and important question in its own right. This study is but one of a number that might have found evidence of a deterrent effect, if there is such an effect, but did not.

Thus, it provides further evidence that within historically given parameters the death penalty in general and executions in particular do not deter homicide. Like all studies that fail to reveal deterrence, the persuasive power of this analysis depends in large measure on how well it controls for factors which can affect homicide rates so powerfully that they might mask the effects of executions. This in turn depends on how powerful deterrent effects are. We can, for example, safely reject the hypothesis that each execution saves 1,000 lives. Whatever the weakness of the methodology used here, if the deterrent effect of executions were this strong the null hypothesis of no deterrence would have been rejected. Conversely, if it took one hundred executions to save one life there is no scientific technique which could be expected to yield evidence of deterrence given historical rates of execution and the random element in murder. More realistically, if the actual trade-off ratio were as large on the average as the 1:8 ratio that Ehrlich reports, the expected effect of executions on homicide rates would be sufficiently great that uncontrolled effects would have to be both powerful and consistently favoring abolitionist or less persistently executing states to yield the results presented in Tables 1-13. If there is no plausible reason to expect that this is the case, a trade-off ratio as high as 1:8 is implausible.

In its approach to the data this study eschews the apparent precision of modern statistical modeling and harks back to a technique that precedes the computer age. Geographical proximity, temporal congruence, and the likelihood that individual states will not change dramatically from year to year on those sociocultural factors that conduce to homicide serve as proxies for the host of variables one would want to control in a predictive model. The imprecision of these proxies is obvious, but given available data the control these proxies provide is arguably more adequate than that achieved by econometricians such as Ehrlich. Longitudinal data are simply not available for many of the variables that one would want to include in a properly specified model. Furthermore, changed methods of data collection over time often render such longitudinal data as are available of questionable reliability. While econometric models of the real world are inescapably misspecified, the problem seems particularly severe in deterrence research.

Suggesting or even establishing the inadequacy of the more "sophisticated" longitudinal models of deterrence does not, of course, establish the adequacy of the approach taken here. It is obviously less than ideal. It would not, for example, reveal strong deterrent or normative validation effects associated with executions if such effects spilled over state borders without substantial dilution. It may also yield spurious findings of deterrence or nondeterrence to the extent that factors affecting homicide rates in neighboring states are neither geographically (across states) nor historically (within states) determined. If such factors exist and are systematically associated with the absence of a death penalty or willingness to execute, results like those reported here might be found despite substantial deterrence.

These and similar possibilities take us away from science and methodology toward art and theory. The issue is not one of logical or methodological possibility. It is one of empirical and theoretical plausibility. I know of no good reason to expect a substantial spillover of deterrence from an executing state to one that does not have a death penalty. A spillover of normative validation effects is theoretically more plausible. Yet while the idea of normative validation can be traced back to Durkheim, we still lack good empirical evidence that the phenomenon exists. Even if normative validation is in some cases an important function of punishment, it might be unimportant where a crime is as widely disapproved of as murder and the effect of interest exists only at the margin between life imprisonment and death. Similarly, assuming the lag built into the data analysis adequately corrects for any tendency to sentence to death in high homicide years, I can think of no variables likely to affect execution and homicide rates in neighboring states that are not in large measure regionally or historically determined. To the extent that such variables do not exist, the results reported in this paper indicate that Sellin's findings of nondeterrence were neither the spurious result of some uncontrolled suppressor variable nor an artifact of his decision to look at the legal status of the death penalty rather than its application. These results complement Sellin's. Together they provide further reason to believe that neither the presence of the death penalty nor its application deters homicide.