ALASKA LEGISLATURE COMMITTEE FILES

2436 HJ HB 140 (FILE 2) 2436

COMMITTEE REPORT

HOUSE

FURTHER:

Date: ______________________

Mr. Speaker:

The Committee on JUDICIARY has had ______________________

An Act authorizing capital punishment, classifying murder in the first degree as a capital felony, and establishing sentencing procedures for capital felonies.

under consideration and reports it back as follows:

[ ] do pass [ ] do not pass

[ ] do pass with attached amendments(s)

[ ] replace with CS for ______________________ [X] same title [ ] new title

and recommends ______________________

[ ] AND attaches a "Letter of Intent" [ ] New Fiscal Note [ ] Zero Fiscal Note Attached

[ ] reports it back without recommendation

[ ] referred to the ______________________ Committee

MEMBERS SIGNING
DO PASS

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MEMBERS HAVING
OTHER RECOMMENDATIONS:

____

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CHAIRMAN

H 60

The Deterrent Effect of Capital Punishment: A Cross-State Analysis of the 1960's

Brian E. Forst*

The debate over capital punishment, which for centuries has been waged over essentially nonempirical matters, has focused more recently on the extent to which executions prevent (or encourage) homicides. Interest in this aspect of capital punishment was considerably heightened when, in the amicus curiae brief submitted in *Fowler v. North Carolina*,[1] the Solicitor General of the United States cited statistical evidence[2] reported by Professor Isaac Ehrlich supporting the hypothesis that capital punishment deters murder.[3]

Before vacating and remanding *Fowler*,[4] the Court received briefs and heard oral arguments in five other death penalty cases,[5] in which the deterrence question, as before, figured prominently.[6] Soon afterward, the Court ruled that "the punishment of death does not invariably violate the Constitution,"[7] and stated that for many murderers "the death penalty undoubtedly is a significant deterrent."[8]

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* Senior Research Analyst at the Institute for Law and Social Research, Washington, D.C. The author is deeply indebted to the following persons for their helpful comments: Anthony Amsterdam, Hugo Bedau, Frank Easterbrook, Shelby Haberman, William Hamilton, Lawrence Klein, Jeffrey Roth, and Hans Zeisel. Funding for this project was provided by the Lilly Endowment. The author assumes full responsibility for any errors.

1. 428 U.S. 904 (1976).
2. Brief for the United States as Amicus Curiae at 35-38.
3. Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life or Death*, 65 Am. Econ. Rev. 397 (1975).
4. 428 U.S. 904 (1976).
5. Roberts v. Louisiana, 428 U.S. 325 (1976); Proffitt v. Florida, 428 U.S. 242 (1976); Woodson v. North Carolina, 428 U.S. 280 (1976); Jurek v. Texas, 428 U.S. 262 (1976); Gregg v. Georgia, 428 U.S. 153 (1976).
6. Brief for the United States as Amicus Curiae at 34-45, 9a-16a, Gregg v. Georgia, 428 U.S. 153 (1976).
7. Gregg v. Georgia, 428 U.S. 153, 169 (1976). *But see id.* at 231 (Marshall, J., dissenting); Commonwealth v. O'Neal, 339 N.E.2d 676, 682-85 (Mass. 1975).
8. Gregg v. Georgia, 428 U.S. 153, 185-86 (1976). Although the Court did not support its belief in the deterrent value of capital punishment with empirical evidence, this evidence was not ignored:

The Supreme Court is by no means alone in its belief that capital punishment deters crime. Eighty-four percent of the respondents to a 1977 National Observer plebiscite supported restoration of the death penalty, and belief in the deterrent effect of capital punishment was the reason most often cited.[9] Public support for the death penalty has been similarly revealed by the Gallup Poll[10] and other opinion surveys.[11]

Belief in the deterrent value of the death penalty, however, is less common within the academic community. While support for Ehrlich's research exists,[12] replications of his analysis[13] have shown that his evidence of deterrence depends upon a restrictive assumption about the mathematical relationship between homi-

"Although some of the studies suggest that the death penalty may not function as a significantly greater deterrent than lesser penalties, there is no convincing empirical evidence either supporting or refuting this view." *Id.* at 185 (footnote omitted). Professor Hans Zeisel has taken issue with this opinion, arguing that the evidence about the deterrent effect is, indeed, "quite sufficient" and that "the request for more proof is but the expression of an unwillingness to abandon an article of prejudice." Zeisel, *The Deterrent Effect of the Death Penalty: Facts v. Faiths*, 1976 SUP. CT. REV. 317, 310.

9. Egan, *Plebiscite Results: Restore the Death Penalty*, National Observer, Jan. 29, 1977, at 1, col. 3.

10. During the decade ending in April, 1976, support for the death penalty among Gallup Poll respondents rose from 42 percent to 65 percent. *Id.*

11. *See* Vidmar & Ellsworth, *Public Opinion and the Death Penalty*, 26 STAN. L. REV. 1245, 1255 (1974) ("Belief in deterrent effectiveness is probably the most frequently assessed rationale for support of capital punishment.")

12. Although Tullock characterized Ehrlich's study of capital punishment as "sophisticated," the praise was qualified: "Unfortunately, the data available for this study were not what one would hope for, and not as much reliance can be put upon his results as one normally would give to work by such a sophisticated econometrician." Tullock, *Does Punishment Deter Crime?* 36 PUB. INTEREST 103, 108 (Summer 1974). Further support has been expressed in Posner, *The Economic Approach to Law*, text of the Will E. Orgain Lecture, University of Texas Law School at 30-31 (March 1975) (on file at MINNESOTA LAW REVIEW).

13. *See* Bowers & Pierce, *The Illusion of Deterrence in Isaac Ehrlich's Research on Capital Punishment*, 85 YALE L.J. 187 (1975); Klein, Forst, & Filatov, *The Deterrent Effect of Capital Punishment: An Assessment of the Estimates* (paper commissioned by the National Academy of Sciences, September 1976 draft, to appear in DETERRENCE AND INCAPACITATION: ESTIMATING THE EFFECTS OF CRIMINAL SANCTIONS ON CRIME RATES (A. Blumstein ed. 1977) (forthcoming) [hereinafter cited as DETERRENCE AND INCAPACITATION]; Passell & Taylor, *The Deterrent Effect of Capital Punishment: Another View*, Columbia University Discussion Paper 74-7509 (March 1975), *reprinted in* Reply Brief for Petitioner, App. E., Fowler v. North Carolina, 428 U.S. 325 (1976).

cides and executions,[14] the inclusion of a particular set of observations,[15] the use of a limited set of control variables,[16] and a peculiar construction of the execution rate, the key variable.[17]

This Article first discusses briefly the strengths and weaknesses of time-series and cross-section analyses to test the hypothesis that capital punishment deters homicides. A method that avoids the more serious of these weaknesses is then described and applied to state data for 1960 and 1970 to test the above hypothesis. The results of this initial test do not support the hypothesis. To ensure that these findings reflect reality rather than simply the way in which the key variable was measured, alternative measures of the execution rate are substituted. Similar attempts are made to eliminate other possible biases that have been identified in the literature. None of these modifications of the basic model is found to alter the initial finding in any important way. It is concluded that the evidence of the 1960's supports the theory that capital punishment does not, on balance, deter homicides.

I. TIME-SERIES AND CROSS-SECTION STUDIES

Professor Ehrlich's landmark statistical test of the hypothesis that capital punishment deters homicide consisted of a regression analysis of aggregate data for the United States for the period 1933 through 1969.[18] His basic approach is commonly referred

14. Bowers & Pierce, *supra* note 13, at 199-203; Klein, Forst, & Filatov, *supra* note 13, at 31-32; Passell & Taylor, *supra* note 13, at 6-8; note 33 *infra*.

15. Ehrlich reported that his deterrence result remained when data from the 1930's were excluded, Ehrlich, supra note 3, at 410, but others found that the result disappeared when data from the latter part of the 1960's were excluded. Bowers & Pierce, *supra* note 13, at 197-204; Klein, Forst, & Filatov, *supra* note 13, at 20-28; Passell & Taylor, *supra* note 13, at 5, 21, 22.

16. Klein, Forst, & Filatov, *supra* note 13, at 14-17, 28-30. Control variables are used in the analysis of nonexperimental data to reduce the danger of erroneous inferences about relationships between variables. To the extent that the murder conviction rate influences both the execution rate and the murder rate, for example, its omission from an analysis of the deterrent effect of executions would produce the appearance of an association between executions and homicides even if, in fact, no association existed. *See* notes 21-23 *infra* and accompanying text. In the present analysis this problem is recognized and controlled for. *See* text accompanying notes 38-41 *infra*.

17. *See* Klein, Forst, & Filatov, *supra* note 13, at 17-19.

18. Ehrlich, *supra* note 3, at 406, 409. Regression analysis is a standard statistical method for determining the mathematical equation that best describes the relationship between a dependent variable (in this case, the homicide rate) and one or more predictor variables. Ehr-

to as "time-series analysis," since the units of observation con-sist of a series of time intervals—in this case, individual years.

One of the crucial unresolved issues in Ehrlich's time-series analysis centers around the sensitivity of his findings to the in-clusion of data from the 1960's. Specifically, he found no deter-rent effect when data for the period since 1964 were excluded from the analysis.[19] This conclusion is not surprising, since during the 1960's the murder rate rose precipitously, after dec-ades of slow decline, while the use of capital punishment dimin-ished until terminated in 1967;[20] but it does raise the question of the extent to which the key statistical relationship found in Ehrlich's time-series analysis reflects a true causal relationship. It is possible that the appearance of deterrence that emerges in Ehrlich's time-series study is primarily the product of variables omitted from the analysis, an omission due largely to the un-availability of data.[21] While all nonexperimental measure-ments are subject to limitations, inferences about deterrence drawn from the analysis of aggregate time-series data appear to be especially prone to error because only a limited array of fac-tors can be incorporated[22] or otherwise reflected[23] to safeguard against spurious findings.

lich's study was the first to estimate the deterrent effect of capital pun-ishment by: (1) measuring factors other than the death penalty that may have affected the homicide rate; (2) measuring the extent to which the death penalty was used when it existed; and (3) attempting to account explicitly for the reverse effect of homicides on the demand for executions.

19. Ehrlich, *Deterrence: Evidence and Inference*, 85 YALE L.J. [illegible] 217 (1975). This phenomenon was first reported by Passell & Taylor. Passell & Taylor, *supra* note 13, at 5, 21, 22.

20. *See* FEDERAL BUREAU OF PRISONS, DEPARTMENT OF JUSTICE, NA-TIONAL PRISONER STATISTICS: CAPITAL PUNISHMENT, table 2, at [illegible] (No. SD-NPS-CP-3, November 1975).

21. It is, of course, also possible that omitted variables caused his finding to understate the true effect of executions on homicides.

22. A key variable not available on an annual basis is the average term of imprisonment for persons convicted of homicide and not exe-cuted. The potential importance of this variable lies in its role as a substitute sanction for capital punishment. In an earlier study based on time-series data, Ehrlich himself found that this variable was an effective homicide deterrent. Ehrlich, *Participation in Illegitimate Activities: A Theoretical and Empirical Investigation*, 81 J. POL. ECON. 521, 551 (1973). The aggregate number of homicide convictions, a [illegible] variable in the analysis, is also not available annually. Aware of the potential importance of this variable, Ehrlich constructed rough approx-imations of its values from F.B.I. estimates of the annual number of homicides, the annual probability of arrest, and the annual probability of conviction given arrest. Ehrlich, *supra* note 3, at 407.

23. One way of reflecting regional factors is with the use of [illegible]

Professor Ehrlich's second empirical test—based on a regres-sion analysis of data for individual states, with separate results for 1940 and 1950—has been cited to further support the theory that capital punishment deters murder.[24] This "cross-section" technique has certain advantages over analysis based on aggre-gate time-series data: it allows the researcher to observe larger differences in the relevant factors,[25] to control for specific re-gional effects,[26] and to include potentially important factors about which information is not available on an annual basis.[27]

Several scholars have suggested that the existing estimates of the deterrent effect of capital punishment can be improved by analyzing data that reflect variation *both* temporally and geographically.[28] A method to accomplish this is set forth in the following section.

II. CROSS-SECTION ANALYSIS OF CHANGES

During the 1960's, after years of gradual decline, the homi-cide rate for the United States as a whole increased sharply (see Figure 1). Although the homicide rate in most states followed this general pattern, it rose much more sharply in some states

regional variables in data that manifest geographical variation. *See* notes 26 & 41 *infra*.

24. Ehrlich referred to these results in his reply to critics, Ehrlich, *supra* note 19, at 213, 217, and reported them in Punishment and Deter-rence: Some Further Thoughts and Additional Evidence, a paper delivered at the Joint Meeting of the Operations Research Society of America and the Institute of Management Science, Las Vegas (November [illegible] 1975) [hereinafter cited as Ehrlich, Punishment and Deterrence]. Since Ehrlich has described these findings as "preliminary and incom-plete," *id.* at 1, they will not be discussed in detail here.

Professor Peter Passell has also performed a cross-state analysis of the deterrent effect of capital punishment, with separate results for [illegible] and 1960. His findings differ sharply from Ehrlich's; he con-cludes that there is "no reasonable way of interpreting the cross-section data that would lend support to the deterrence hypothesis." Passell, *The Deterrent Effect of the Death Penalty: A Statistical Test*, 28 STAN. L. REV. 61, 80 (1975).

25. *See* note 69 *infra*.

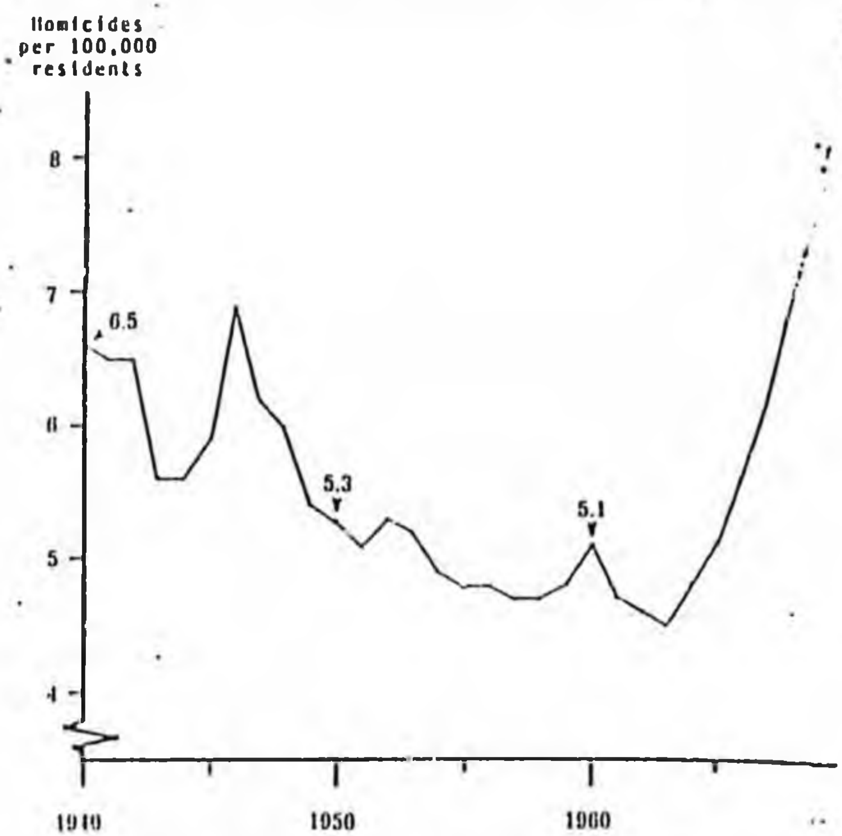
26. Aggregation errors associated with the failure to account for [illegible] effects are discussed in Baldus & Cole, *A Comparison of the Work of Thorsten Sellin and Isaac Ehrlich on the Deterrent Effect of Capital Punishment*, 85 YALE L.J. 170, 175-77 (1975).

27. *See* note 22 *supra*.

28. Peck, *The Deterrent Effect of Capital Punishment: Ehrlich and His Critics*, 85 YALE L.J. 359, [illegible] (1976); Zeisel, *supra* note 6, at 317, 330; [illegible], Book Review, 67 J. CRIM. L. & CRIMINOLOGY 359, 360 (1976) (W. [illegible], EXECUTIONS IN AMERICA).

than in others, and even declined in a few.[29] This cross-state variation, coupled with the differences from state to state in the rate at which use of the death penalty declined from 1960 to 1970,

Figure 1. The homicide rate in the United States, 1940-1970*



* Sources: UNIFORM CRIME REPORTING SECTION, FEDERAL BUREAU OF INVESTIGATION, UNITED STATES DEPARTMENT OF JUSTICE, INDEX OF CRIME, UNITED STATES, 1933-1972 (Special tabulation presented to the author in March 1975; copy on file with MINNESOTA LAW REVIEW).

BUREAU OF THE CENSUS, UNITED STATES DEPARTMENT OF COMMERCE, THE STATISTICAL ABSTRACT OF THE UNITED STATES, table 2, at 5 (197?).

29. From 1960 to 1970 the homicide rate increased in 43 states, declined in five states (Alabama, Maine, Montana, Oklahoma, and Virginia), and was unchanged in two states (Nevada and North Dakota). It increased mostly sharply in Missouri (from 4.8 homicides per 100,000 residents in 1960 to 10.7 in 1970) and New York (from 2.0 in 1960 to ? in 1970). FEDERAL BUREAU OF INVESTIGATION, DEPARTMENT OF JUSTICE, UNIFORM CRIME REPORTS, table 3, at 38-52 (1960), & table ?, at ? (1970).

provides a unique opportunity to estimate the deterrent effect of capital punishment on the commission of homicides. The changes in these and other relevant variables that occurred between 1960 and 1970 in each state for which data are available can be measured and used to estimate the average effect of reductions in the execution rate on the rate at which homicides occur in the population. To the extent that capital punishment deters homicides, the homicide rate should have increased by the largest amounts from 1960 to 1970, *ceteris paribus*, in those states with the greatest reductions in the probability that a person convicted of murder would be executed.

Examining the data in this manner should overcome the potentially serious problems associated with aggregate time-series analysis.[30] Analyzing intertemporal changes in the relevant variables across states should also improve the estimates available from conventional cross-section analysis,[31] partly by reducing biases associated with omitted variables.[32] Moreover, the results of this approach appear less sensitive to alternative assumptions about the mathematical form of the model that describes the relationships among the relevant variables than do those of either the conventional time-series or cross-sectional approaches.[33] By

30. *See* notes 10-27 *supra* and accompanying text.

31. The cross-section analyses by Ehrlich and Passell are based on the levels of variables for individual census years. *See* note 24 *supra* and accompanying text. These single-year levels are used to estimate the elasticity of the homicide rate with respect to the probability of execution, given conviction for murder. The elasticity of one variable, y, with respect to another, x, is a number indicating the percentage increase (a negative number indicates a decrease) in y that results from a one percent increase in x. Ehrlich has estimated that the elasticity of the homicide rate with respect to the probability of execution, given conviction for murder, is around −0.06. Ehrlich, *supra* note 3, at 414. Since elasticity is a measure of the effect of a *change* in one variable on another variable, estimating elasticities by analyzing actual changes in variables in a cross-section of jurisdictions has considerably more appeal than estimating them from the levels of variables for any given year.

32. According to Klein, estimates based on cross-sectional data are prone to errors of spatial heterogeneity, although in certain instances, these errors can be eliminated by "differencing" two successive cross sections. Specific biases that can be eliminated under this technique include the bias produced by the failure to measure personality effects in samples of households and that produced by failure to measure entrepreneurial effects in samples of business firms. L. KLEIN, A TEXTBOOK OF ECONOMETRICS 350, 358 (2d ed. 1974). Commenting on a draft of this article, Professor Klein suggested that interstate differences in social values may constitute a class of effects that can be accounted for by applying this method to cross-state data.

33. Whether the homicide rate is related to other factors in a linear

estimating the differential of the homicide rate rather than the parent relationship between the homicide rate and its determinants, one can be sure of describing a function that is additive in the differences of the explanatory variables.[34]

Applying this method of analysis to the 1960's is appealing for other reasons as well. More control variables are available for the most recent census years, and their measurement tends to be more accurate than it was in 1940 or 1950.[35] Moreover, there has been a great deal of controversy about the period from

or loglinear fashion is very much in controversy. Bowers & Pierce, *supra* note 13, at 190-200; Ehrlich, *supra* note 3, at 406; Ehrlich, *supra* note 19, at 217-19; Klein, Forst, & Filatov, *supra* note 13, at 3[illegible]; Passell & Taylor, *supra* note 12, at 6e-7e; Peck, *supra* note 28, at 360-61. Estimates of the deterrent effect of capital punishment have been found to be quite sensitive to whether the relationship is assumed to be linear or loglinear. *See* note 14 *supra* and accompanying text.

The assumption of loglinearity and the use of logarithms in previous studies have created additional problems. During 1968, 1969, and 1970 there were no executions. Because it is impossible to take the logarithm of zero, Ehrlich assumed that one execution took place in each of these years so that he could use the loglinear model. Ehrlich, *supra* note 3, at 409 n.6. This procedure, however, builds biases into the analysis.

While the true relationship between the homicide rate and its determinants may be nearly linear or nearly loglinear, it is likely, in fact, to be precisely neither.

34. In regressing the change in the homicide rate on the changes in the relevant independent variables, partial differential coefficients rather than slope coefficients are obtained. Letting the homicide rate, Q/N, be determined by the rate at which convicted murderers are executed, E/C, and by other factors, $X_1, X_2, \ldots$, the general relationship is written as

$$Q/N = f(E/C, X_1, X_2, \ldots).$$

The differential of the homicide rate is of the form

$$d\frac{Q}{N} = \frac{\partial \frac{Q}{N}}{\partial \frac{E}{C}}\, d\frac{E}{C} + \frac{\partial \frac{Q}{N}}{\partial X_1}\, dX_1 + \frac{\partial \frac{Q}{N}}{\partial X_2}\, dX_2 + \ldots,$$

regardless of whether the parent function is linear, loglinear, or any other continuous expression. Since the partial differential coefficients will be constants only if the parent function is linear, the differential of the homicide rate will not generally be a linear function. It will, however, be additive in the differences of the explanatory variables. Hence, the regression coefficients produced in the estimate of the differential equation may be viewed as approximations of the averages of the respective partial differential coefficients over the range of observed values.

35. Ehrlich has also suggested that more recent data are likely to be better. Ehrlich, *The Deterrent Effect of Criminal Law Enforcement*, 1 J. LEGAL STUDIES 259, 272 (1972).

1960 to 1970 in the reviews of the available time-series evidence.[36] In short, analyzing changes during this decade cross-sectionally would appear to permit one to discover more directly whether the association between the cessation of capital punishment and the upsurge in the homicide rate during the 1960's was primarily causal or coincidental.

A. THE MODEL

The model that provides the initial structure for this analysis is

(1) Q/N = f(E/C, C/Q, T, Cr, Age, NW, Male, Urb, Enr, Pop, Div, Y, Pov, Emp, S).

This equation represents the notion that the homicide rate (Q/N) is potentially influenced by the rate at which persons convicted of murder are executed (E/C), the rate at which murders result in conviction (C/Q), the average prison term served by convicted murderers (T), the factors that determine the rate at which crimes other than homicide are committed (Cr), social and demographic characteristics [age (Age), race (NW), sex (Male), urbanization (Urb), school enrollment rate (Enr), resident population (Pop), divorce rate (Div)], economic variables [median family income (Y), proportion of families in poverty (Pov), employment (Emp)], and a binary variable indicating whether the state is southern (S). The sources of data for these variables are given in the Appendix.[37]

Professor Ehrlich has provided theoretical justification for the inclusion of the criminal justice sanction variables and the economic variables.[38] The social and demographic variables have been added to minimize the degree of spuriousness in the estimates of central concern here, those reflecting the effects of the sanction variables on homicides.[39] The rate at which

36. *See* notes 15 & 10 *supra* and accompanying text.

37. *See* pp. 765-67 *infra*.

38. Ehrlich, *Participation in Illegitimate Activities: An Economic Analysis*, in ESSAYS IN THE ECONOMICS OF CRIME AND PUNISHMENT 68, 70-82 (G. Becker & W. Landes eds. 1974); Ehrlich, *supra* note 3, at 398-400.

39. Any factor that influences both the execution rate and the homicide rate, if omitted from the analysis, will tend to distort the estimated effect of executions on homicides. Age, race, sex, schooling, population density and size, and family stability are all basic characteristics that would appear to be capable of producing such distortion. Ehrlich has incorporated the first five of these factors in his study of deterrence. Ehrlich, *supra* note 38, at 93.

crimes other than homicide are committed[40] and a binary Southern variable[41] are incorporated to capture the effects of additional exogenous factors that the other control variables do not specifically measure. Values of each of these variables for 1960 and 1970 are shown in Table 1.

Table 1. Aggregate United States values, mean state values and standard deviations for the variables used in the analysis, 1960 and 1970

	U.S. total*		State means[(a)]		Standard deviations[(a)]	
	1960	1970	1960	1970	1960	1970
Variable:						
Q/N[(b)]	5.1	7.8	4.7	8.6	3.2	3.7
E/C	.0157[(c)]	0	.0192	0	.0290	0
C/Q	.4132[(d)]	.3459[(d)]	.4304	.3729	.1215	.1220
T	82.23[(e)]	67.54[(e)]	101.64	70.20	55.60	30.74
Cr[(b)]	1032.9	2738.5	942.7	2304.1	404.3	954.7
Age	.0482	.0634	.0493	.0636	.0043	.0060
NW	.1143	.1253	.1131	.1164	.1424	.1244
Male	.4926	.4887	.4957	.4893	.0094	.0082
Urb	.6986	.7348	.6189	.6610	.1494	.1444
Enr	.822	.860	.8453	.8873	.0641	.0456
Pop[(f)]	180.0	203.8	3.324	3.766	4.142	4.791
Div	.0022	.0035	.0031	.0042	.0049	.0029
Y[(g)]	5660	9586	5418	9172	1002	1467
Pov[(g)]	.184	.107	.1970	.1154	.0902	.0524
Emp	.4466	.4991	.4250	.4815	.0657	.0588
S	.3333	.3333	.2813	.2813	.4568	.4563

(a) Means and standard deviations are unweighted statistics for the 32 states for which no data were missing or undefined; (b) per 100,000 residents; (c) based on 48 states (Alaska and New Jersey did not report these statistics in 1960); (d) based on the 33 states that reported in both 1960 and 1970; (e) based on data from 34 states; (f) in millions; (g) based on income earned in the previous year.

* Aggregate U.S. values are used only in these two columns, in Figure 1, and in the concluding section of this paper.

Following the rationale described in the preceding section, the effects of interest are estimated by forming the equation of first differences:

40. A rationale for the inclusion of the nonhomicide crime rate is given in Klein, Forst, & Filatov, *supra* note 13, at 17-19, and in text accompanying notes 64-65 *infra*.

41. It has become a standard practice in cross-state econometric analysis to incorporate a binary Southern variable to reflect other social and demographic characteristics. The use of such a variable in an analysis of homicides is further warranted by the fact that the homicide rates in the South are about twice that of the rest of the nation

$$(2)\quad \Delta(Q/N) = a + b_1\Delta(E/C) + b_2\Delta(C/Q) + b_3\Delta T + c_1\Delta Cr + c_2\Delta Age + c_3\Delta NW + c_4\Delta Male + c_5\Delta Urb + c_6\Delta Enr + c_7\Delta Pop + c_8\Delta Div + c_9\Delta Y + c_{10}\Delta Pov + c_{11}\Delta Emp + c_{12}S,$$

where Δ denotes the change in a variable calculated by subtracting the 1960 level from the 1970 level, "a" denotes a constant term, b_i denotes a partial differential coefficient for a sanction variable, and c_j denotes a partial differential coefficient for a control variable.

B. Parameter Estimates

These coefficients can be estimated using ordinary least-squares regression analysis, with the full set of independent variables incorporated as regressors. These estimates are based on data from the 32 states[42] for which values of all the variables shown were reported both for 1960 and 1970:[43]

$$(3)\quad \underset{(R^2=.692)}{\Delta(Q/N)} = -\underset{(2.70)}{5.011} + \underset{(12.7)}{11.62\Delta(E/C)} \cdot \underset{(1.92)}{5.714\Delta(C/Q)}$$
$$+ \underset{(.00773)}{.001378\Delta T} - \underset{(15.8)}{38.69\Delta Pov} + \underset{(.000966)}{.001706\Delta Y}$$
$$+ \underset{(.000892)}{.001430\Delta Cr} + \underset{(17.8)}{38.97\Delta NW} - \underset{(130)}{190.2\Delta Age}$$
$$- \underset{(.877)}{.9505S} - \underset{(34.2)}{29.65\Delta Emp} - \underset{(8.53)}{9.021\Delta Enr}$$
$$+ \underset{(15.5)}{11.24\Delta Urb} + \underset{(150)}{92.58\Delta Div} + \underset{(.000000411)}{.0000002382\Delta Pop}$$
$$+ 0\Delta Male\ .$$

The numbers in parentheses are standard errors, and R^2 is the coefficient of determination, a measure of the proportion of the variance in the dependent variable that is explained by the independent variables used. Thus, 69 percent of the cross-state variance in the change in the homicide rate from 1960 to 1970 can be attributed to the set of variables in the right-hand side of equation (3).

42. The 32 states on which these estimates are based are Arizona, California, Colorado, Connecticut, Delaware, Georgia, Hawaii, Idaho, Illinois, Kansas, Kentucky, Maine, Maryland, Massachusetts, Minnesota, Mississippi, Missouri, Montana, Nevada, New Hampshire, New Mexico, New York, North Dakota, Ohio, Oklahoma, South Carolina, South Dakota, Tennessee, Utah, Washington, West Virginia, and Wyoming. Conviction data were missing for 17 states in 1970, and the average term of incarceration was not available for an additional state (Vermont) in that year.

43. Similar results are obtained when the 1960 level of the homicide rate is included as a regressor to account for nonlinearity in Q/N.

The first result provides no support for the hypothesis that capital punishment deters homicide. The positive regression coefficient for the execution rate variable is, in fact, consistent with a counterdeterrent effect,[44] but the standard error of this estimate is too large for this finding to be taken seriously.[45] Equation (3) does provide evidence, on the other hand, of a deterrent effect of convictions on homicides. Those states with the largest reductions in the ratio of homicide convictions to homicide offenses tended to have the largest increases in the homicide rate, other factors held constant.

This regression equation, however, has too many shortcomings to allow it to stand alone as an adequate test of the deterrence hypothesis. Foremost among these is the imprecision in parameter estimation caused by the inclusion of 15 independent variables—ten of which are not significant (at the .10 level)—in an equation constructed from only 32 observations.[46] Eliminating these ten variables, except for the variable of primary interest, Δ (E/C), produces a result that fits the data better:

$$
\begin{array}{llllllll}
(4)\ \Delta(Q/N) = & - & 4.222 & + & 17.64\Delta(E/C) & - & 5.970\Delta(C/Q) \\
(R^2 = .577) & & (2.10) & & (8.55) & & (1.68) \\
 & - & 24.91\Delta Pov & + & .001515\Delta Cr & + & 30.60\Delta NW \\
 & & (7.52) & & (.000507) & & (13.3) \\
 & + & .0004679\Delta Y. & & & & \\
 & & (.000526) & & & &
\end{array}
$$

44. Ehrlich explained the potential for a counterdeterrent effect as follows: "[O]ne may argue that the differential deterrent effect of capital punishment on the incentive to commit murder may be offset by the added incentive it may create for those who actually commit this crime to eliminate policemen and witnesses who can bring about their apprehension and subsequent conviction and execution." Ehrlich, *supra* note 3, at 398. Courts or juries may also be more reluctant "to convict defendants charged with murder when the risk of their subsequent execution is perceived to be undesirably high." *Id.* at 405. This latter possibility is discussed in text accompanying notes 57-59 *infra*. Von Weber has suggested as an alternative explanation that capital punishment may induce suicidally-inclined persons to commit murder. H. von Weber, *Selbstmord als Mordmotiv*, MONATSSCHRIFT FÜR KRIMINALBIOLOGIE UND STRAFRECHTSREFORM 161 (1937).

45. If executions had no effect on homicides, the probability is .37 that random factors alone would have caused the ratio of the regression coefficient for (E/C) to its standard error to be at least 0.91, the result in equation (3).

46. Estimates become increasingly precise (that is, subject to less random error) either as the number of observations increases or as the number of insignificant variables in the regression equation diminishes. The importance of precision in the estimation of the deterrent effect of capital punishment has been discussed by Ehrlich, *supra* note 10, at 2[illegible].

This result is basically similar to (3) for the variables of principal focus, except that elimination of nine weak independent variables increases the adjusted coefficient of determination, a standard measure of goodness-of-fit,[47] from .44 to .48, and increases the statistical significance of five of the six remaining variables.

Equation (4) provides evidence that the sharp increase in the homicide rate during the 1960's was the product of factors other than the abolition of the death penalty. Accounting for what appear to be the most important of these other factors—the murder conviction rate, economic variables, race, and the factors that caused non-capital offenses to escalate during the 1960's—it is apparent that these states in which the actual use of capital punishment ceased during the 1960's experienced no greater increase in the murder rate than did the states that did not use capital punishment in the first place. Under the theory that capital punishment deters murder, one would have predicted the opposite.

C. ROBUSTNESS TESTS

Before drawing inferences from data that are not produced by controlled experimentation, it is appropriate to test whether the estimates are "robust" to (that is, hold up under) departures from the assumptions on which the estimates are grounded.[48] Equation (4) is based on several assumptions: (1) the murder rate in any given year is influenced by the number of executions in that year; (2) none of the sanction variables is influenced by any of the other variables used in the regression analysis; (3) the variance in the homicide rate is no larger for highly populated states than for the less populated states; and (4) the rate at which non-capital crimes are committed is not affected by, nor does it affect, the other variables in the analysis. Each of these assumptions can be altered to test for robustness, which

47. The formula for the adjusted coefficient of determination, $\overline{R^2}$, is

$$\overline{R^2} = R^2 - (1 - R^2)\left[\frac{K}{(N - K - 1)}\right]$$

where R^2 is the coefficient of determination, K is the number of independent variables, and N is the number of observations. A. GOLDBERGER, ECONOMETRIC THEORY 217 (1964).

48. The importance of robustness tests is well established in econometric analysis. H. THEIL, PRINCIPLES OF ECONOMETRICS 615-16 (1971).

will indicate the reliability of the estimates obtained in equation (4).

1. *Alternative Constructions of the Execution Rate*

Since the execution rate is the independent variable of principal focus in this analysis, it is surely appropriate to vary the methods of measuring it.[49] The construction used in equations (3) and (4) is based on the number of executions and convictions in 1960 and 1970. One alternative is to use executions in 1961 and 1971 instead of executions in 1960 and 1970, respectively, as objective forecasts of the probability that a murder conviction will lead to execution, since executions have been reported to lag behind convictions by about a year.[50] The result corresponding to equation (4) using this alternative measure, which is denoted (E_{+1}/C), is

$$
\begin{array}{llllll}
(5)\ \Delta(Q/N) = & -\ \underset{(2.21)}{6.391} & +\ \underset{(11.5)}{2.977\Delta(E_{+1}/C)} & -\ \underset{(1.83)}{6.631\Delta(C/Q)} \\
(R^2=.508) & -\ \underset{(8.03)}{27.32\Delta Pov} & +\ \underset{(.000571)}{.001248\Delta Cr} & +\ \underset{(15.3)}{42.01\Delta NW} \\
 & +\ \underset{(.000565)}{.0007516\Delta Y}\ .
\end{array}
$$

This result is fundamentally no different from equation (4), suggesting that lagging executions does not alter the observed effect of executions on homicides.[51]

To reduce the sampling error associated with the small number of executions that occurred around 1960 and test another lag structure, one can make the numerator of the execution rate the average number of executions over the three-consecutive-year period centered about the year of the convictions in the denominator.[52] This execution rate variable is denoted (E_m/C), and is used in place of (E/C) in equation (4), giving

$$
\begin{array}{llllll}
(6)\ \Delta(Q/N) = & -\ \underset{(2.15)}{5.002} & +\ \underset{(15.4)}{15.89\Delta(E_m/C)} & -\ \underset{(1.78)}{5.823\Delta(C/Q)} \\
(R^2=.525) & -\ \underset{(7.50)}{20.03\Delta Pov} & +\ \underset{(.000551)}{.001403\Delta Cr} & +\ \underset{(14.2)}{40.57\Delta NW} \\
 & +\ \underset{(.000545)}{.0000700\Delta Y}\ .
\end{array}
$$

Again, this alternative does not produce a result that differs in any important respects from equation (4).

Another execution rate variable can be formed by combining the independent variables (E/C) and (C/Q) into the single variable (E/Q). Although this combination causes an important control variable, the murder conviction rate, to be lost, it allows all 50 states to be included in the analysis.[53] The result is

$$
\begin{array}{llllll}
(7)\ \Delta(Q/N) = & -\ \underset{(1.80)}{2.181} & +\ \underset{(30.1)}{13.17\Delta(E/Q)} & -\ \underset{(6.02)}{13.38\Delta Pov} \\
(R^2=.321) & +\ \underset{(.000400)}{.001441\Delta Cr} & +\ \underset{(13.4)}{35.00\Delta NW} & +\ \underset{(.000145)}{.0001917\Delta Y}\ .
\end{array}
$$

This result is remarkably similar to equation (4) except for the substantial reduction in the proportion of variance in the homicide rate explained by the independent variables, which is produced by the exclusion of the conviction variable and the use of a larger number of observations. This reduction provides further support for the hypothesis that convictions deter homicides, consistent with findings by Ehrlich[54] and Passell[55] and with the results of equations (3) through (6).

A final construction of the execution rate is designed to eliminate whatever bias results from the reverse effect that changes in the homicide rate may have on the execution rate. All of the above regression equations assume that the causality runs strictly from executions to homicides. These results will be biased to the extent that the execution rate is a function of the

49. The potential importance of alternative constructions has been stressed in previous analyses of the deterrent effect of capital punishment. Ehrlich, supra note 3, at 407-08; Passell, supra note 24, at [illegible] 77.

50. Ehrlich, supra note 3, at 407. Using data from the Federal Bureau of Prisons, I calculated that the median delay between sentencing and execution for persons executed during the period 1950 through [illegible] was 14 months. The distribution is skewed in the positive direction, indicating a mean delay of somewhat more than 14 months. Federal Bureau of Prisons, Department of Justice, National Prisoner Statistics: Executions 1 (No. 23, February 1960).

51. The decline in R^2 from equation (4) to (5) might be regarded as evidence that the homicide rate is less sensitive to variation in lagged executions than to variation in current executions. More fundamentally, however, it appears systematically related to neither.

52. This idea comes from Passell, supra note 24, at 66, who used instead a four-year average of executions.

53. See note 42 supra and accompanying text.

54. Ehrlich, supra note 3, at 410-11. Ehrlich's findings suggest that arrests, convictions, and executions each independently deter the commission of homicides, with arrests appearing to have the strongest effect and executions the weakest.

55. Passell, supra note 24, at 69-71.

homicide rate, which would occur, for example, if the demand for capital punishment was stimulated by an increase in the homicide rate. This bias can be reduced by replacing the variable $\Delta(E/C)$ with the estimator $\Delta^{\circ}(E/C)$, formed separately by regressing $\Delta(E/C)$ on all the predetermined variables in Table 1.[56] This alternative produces the result

$$\Delta(Q/N) = -\underset{(2.24)}{3.841} + \underset{(12.8)}{23.05\Delta^{\circ}(E/C)} - \underset{(1.71)}{6.003\Delta(C/Q)} - \underset{(7.82)}{23.60\Delta Pov} + \underset{(.000525)}{.001527\Delta Cr} + \underset{(13.7)}{38.30\Delta NW} + \underset{(.000551)}{.0004168\Delta Y} \tag{8}$$

which, again, is basically the same as the other equations. Thus, the major finding—that decreases in the execution rate are not associated with increases in the homicide rate—is robust with respect to alternative methods of constructing the execution rate variable.

2. *Alternative Structures of Simultaneity*

Although equation (1) assumes that the causation is unidirectional, some variables in the equation may be both determinants of murder and products of either the homicide rate itself or factors that influence the homicide rate. This phenomenon, known generally as "simultaneity," was assumed in equation (8). One variable other than the execution rate that may be determined simultaneously with the homicide rate is the rate at which homicide offenders are convicted; it may both affect the homicide rate, as is hypothesized in equation (1), and be produced by changes in the homicide rate. The latter would occur if, for example, the ability to convict homicide offenders was

56. The predetermined variables are ΔAge, ΔNW, ΔMale, ΔUrb, ΔEnr, ΔPop, ΔDiv, ΔY, ΔPov, ΔEmp, and S. An alternate estimator, constructed from these variables together with ΔCr, produced a similar result. This general method, called the "two-stage, least-squares regression technique," is described in most standard econometric textbooks. Because the coefficient of determination, described at equation (3), is difficult to interpret under the application of this technique, it is not reported for equations (8), (9), and (10). *See* P. DHRYMES, ECONOMETRICS: STATISTICAL FOUNDATIONS AND APPLICATIONS 240-63 (1970). Application of this technique to the analysis of crime deterrence, however, may create problems, since one cannot be confident that the control variables included in the equation of primary interest actually permit accurate identification of the crime function. Fisher & Nagin, *On the Feasibility of Identifying the Crime Function in a Simultaneous Model of Crime Rates and Sanction Levels*, in DETERRENCE AND INCAPACITATION, *supra* note 13.

hampered by an increase in the load of homicide cases. Failure to account for this reverse effect, or for the effect of changes in the execution rate on the conviction rate,[57] might bias all the regression coefficients estimated. To deal with this problem, the estimator $\Delta^{\circ}(C/Q)$ is constructed by regressing $\Delta(C/Q)$ on the predetermined variables.[58] This alternative measure of the conviction rate produces the equation

$$\Delta(Q/N) = -\underset{(3.62)}{4.007} + \underset{(10.4)}{17.68\Delta(E/C)} - \underset{(0.63)}{7.634\Delta^{\circ}(C/Q)} - \underset{(13.3)}{27.13\Delta Pov} + \underset{(.000820)}{.001300\Delta Cr} + \underset{(18.9)}{42.38\Delta NW} + \underset{(.000707)}{.0000370\Delta Y} \tag{9}$$

Once again, the homicide rate appears unaffected by changes in the execution rate.[59]

Another type of simultaneity may exist with regard to the average term of incarceration served by persons convicted of homicide, T. This would result if, for example, sentences were lengthened in response to an increase in the homicide rate, in an attempt to discourage further homicides. The potential bias produced by this simultaneity can be reduced by forming the variable $\Delta^{\circ}T$, constructed by regressing ΔT on the predetermined variables.[60] The result produced under this construction is

$$\Delta(Q/N) = -\underset{(2.12)}{4.566} + \underset{(8.55)}{17.08\Delta(E/C)} - \underset{(1.66)}{8.019\Delta(C/Q)} + \underset{(.00788)}{.000252\Delta^{\circ}T} - \underset{(7.60)}{26.00\Delta Pov} + \underset{(.000521)}{.001383\Delta Cr} + \underset{(14.1)}{44.72\Delta NW} + \underset{(.000540)}{.0000326\Delta Y} \tag{10}$$

This result is basically similar to the others reported above.

The true system of simultaneity among variables is likely to be considerably more complicated than has been hypothesized. The results obtained by treating the execution rate, the conviction rate, and the average term of incarceration as endogenous variables, however, as was done in equations (8), (9), and (10), respectively, indicate that the biases due to failure to capture

57. *See* note 44 *supra*.

58. The predetermined variables under this formulation include those already cited, *supra* note 56, and $\Delta(E/C)$.

59. Alternative estimators of $\Delta(C/Q)$, one formed without $\Delta(E/C)$ and another formed with ΔCr, produce similar results.

60. *See* note 58 *supra*.

these simultaneous effects in equations (3) and (4) are not large.[61]

3. *Use of Weighted Regressions*

In cross-section analysis the variance of the dependent variable is often larger for more heavily populated places. This condition, known in a more general form as "heteroscedasticity," produces biased estimates of standard errors of the regression coefficients and biased tests of statistical significance. The presence of heteroscedasticity is commonly identified by visual inspection of a plot of the data, although more rigorous methods are available.[62] To eliminate this bias each observation is generally adjusted by weighting it by the square root of the population. Applying this weighting technique to the observations, under equation (4), the result is

$$
\begin{array}{llllll}
(11)\ \Delta(Q/N) = & -\ \underset{(2.54)}{4.827} & +\ \underset{(10.6)}{10.59\Delta(E/C)} & -\ \underset{(1.80)}{6.340\Delta(C/Q)} \\
(R^2 = .595) & -\ \underset{(8.91)}{26.49\Delta Pov} & +\ \underset{(.000478)}{.001243\Delta Cr} & +\ \underset{(15.8)}{50.32\Delta NW} \\
 & +\ \underset{(.000564)}{.0007042\Delta Y}\ . & &
\end{array}
$$

The similarity of this equation to equation (4) suggests that the general findings are robust with respect to conventional weighting.[63]

4. *Exclusion of the Other-Crimes Variable*

One of the control variables used in equations (1) through (11) is the rate at which crimes other than homicide are committed. It was included in an attempt to account for the factors that caused crime to increase generally during the 1960's, since the failure of previous analyses to capture these effects may have interfered substantially with their ability to isolate a pure deterrent effect of capital punishment.[64] Certain offenses incorporated in this control variable, however, are likely to differ from homicide only in that the victims did not die. Since it is possible

61. Similar results are obtained by endogenizing the rate at which crimes other than homicide are committed.

62. *See* Goldfeld & Quandt, *Some Tests for Homoscedasticity*, 60 J. Am. Stat. Ass'n 539 (1965).

63. All the unweighted regression results reported in this paper have also been obtained under the weighted technique, with similar results in each instance.

64. *See* note 40 *supra*.

that some nonhomicide offenses may themselves be deterred by capital punishment, having them in the right-hand side of the regression equation may have affected the estimates of the deterrent effect that were reported above.

It is possible to test the effect of this potential bias, whose direction is not obvious, a priori, by estimating a counterpart to equation (4) without other offenses as a control variable.[65] The result is

$$
\begin{array}{llll}
(12)\ \Delta(Q/N) = & -\ \underset{(2.30)}{5.371} & +\ \underset{(9.27)}{9.564\Delta(E/C)} & -\ \underset{(1.02)}{6.040\Delta(C/Q)} \\
(R^2 = .420) & -\ \underset{(8.37)}{10.00\Delta Pov} & +\ \underset{(15.5)}{42.72\Delta NW} & +\ \underset{(.000480)}{.001412\Delta Y}\ .
\end{array}
$$

As before, the deterrent effect of capital punishment is not apparent. While the omission of factors that caused crimes other than homicide to increase during the 1960's produces a result that differs somewhat from equation (4),[66] it does not, in this analysis, materially alter the finding.

III. CONCLUSION

The aim of this Article was to investigate empirically the deterrent effect of capital punishment. Building on studies by Ehrlich[67] and Passell,[68] the influence of the execution rate on the homicide rate was estimated by controlling for the effects of other variables and for the reverse effects of the homicide rate on the sanction variables. This analysis differs from previous ones, however, both because it focuses on a unique decade during which the homicide rate increased by over 50 percent and the use of capital punishment ceased and because it examines changes in homicides and executions over time and across states.[69]

65. When ΔCr is removed from equation (3) the only independent variables that are statistically significant (at .10) are Δ(C/Q), ΔPov, ΔNW, and ΔY. Hence, our selection of an efficient subset of independent variables, used in equations (4) through (11), is unaffected by the exclusion of ΔCr.

66. The inclusion of ΔCr in equation (4) reduces substantially the appearance of a strong effect of median family income on the homicide rate obtained in (12). Behind this reduction is a large correlation coefficient (.65) for the pair ΔCr and ΔY. The actual relationships between (Q/N), Cr, Y, and other factors are likely to be extraordinarily complex, and, although the topic is important, exploration of these relationships is beyond the scope of this discussion.

67. *See* Ehrlich, *supra* note 3; Ehrlich, Punishment and Deterrence, *supra* note 24.

68. *See* Passell, *supra* note 24.

69. This approach appears also to yield more efficient estimates of

The findings do not support the hypothesis that capital punishment deters homicides. The 53 percent increase in the homicide rate in the United States from 1960 to 1970 appears to be the product of factors other than the elimination of capital punishment. Foremost among these are a decline in the rate at which homicide offenses resulted in imprisonment (from 41.3 percent in 1960 to 34.6 percent in 1970 for the states that reported in both years) and increasing affluence during the 1960's.[70]

To obtain a sense of how well the estimates, based as they are on individual observations of 32 states, generalize to the United States as a whole, the coefficients of the basic equation, (4), can be combined with changes in the respective independent variables given in the first two columns of Table 1. This produces a predicted increase of 2.68 homicides per 100,000 residents. That the actual increase was 2.7, as shown in Table 1, provides some assurance that the estimates generalize to the aggregate of 18 states not analyzed in equation (4).

The apparent strength of the incarceration rate variable and the apparent weakness of the execution rate and term of imprisonment variables as deterrents to homicide lend some support to Cesare Beccaria's two-hundred-year-old suggestion that certainty of punishment deters more effectively than its severity.[71] There are, however, other explanations for these findings. The appearance of a strong deterrent effect of imprisonments on homicides may be the result of changes in factors omitted from this analysis.[72] And the apparent weakness of the deterrent effect of long imprisonments may be the product of the inaccuracy of our term-of-imprisonment variable,[73] since random errors in the measurement of this variable will bias downward estimates of the deterrent effect of the length of imprisonment.

It seems likely, nonetheless, that this finding of a deterrent effect of imprisonments of persons convicted of murder is more real than spurious. Errors in the measure of murder imprisonments are sure to exist, and these are likely to cause estimates of the deterrent effect of incarceration to understate the true effect.[74] Moreover, this particular finding is consistent with empirical results presented by Ehrlich and Passell.[75] And it supports von Hirsch's suggestion that if penalties for homicide were eliminated entirely it is difficult to imagine that the homicide rate would not increase.[76]

The finding that capital punishment, on the other hand, does not deter homicide is remarkably robust with respect to a wide range of alternative constructions of the execution rate, alternative assumptions about simultaneity among the crime and

the relationship of primary interest, based on the coefficients of variation of the relevant variables. A variable's coefficient of variation is the ratio of its standard deviation to its mean value. In the extreme case in which a factor does not vary, it can have no relationship at all with another factor.

In fact, the coefficients of variation of both the homicide rate and the execution rate are substantially larger (0.023 and 1.51, respectively) in this study than the coefficients of variation for the homicide rate (0.157) and execution rate (0.940) based on annual aggregate United States data for the period 1933 to 1969. The coefficients of variation for the aggregate time-series data are calculated from the independent constructions of Q/N and PXQ_1, based on Ehrlich, The Deterrent Effect of Capital Punishment: A Question of Life and Death, Sources of Data at 2, 6 (May 1975) (unpublished paper on file at MINNESOTA LAW REVIEW).

70. One can only speculate as to why the homicide rate rose the most in those states with the greatest increases in wealth. Increased wealth may have provided more attractive targets to potential offenders and produced heightened expectations and frustration. Since the nation-wide increases in family income reported here are attributable to real growth and to inflation in roughly equal shares, see U.S. BUREAU OF CENSUS, STATISTICAL ABSTRACT OF THE UNITED STATES, table 1323, at 811 (1972) & Table 1, at p. 752 *supra*, the inflation component may have produced further frustration, thereby exerting additional upward pressure on the homicide rate. According to table 1323, the Consumer Price Index went from 88.7 to 116.3 between 1960 and 1970, an increase of 31.1 percent. During the same period the median family income rose from $5660 to $9500, see Table 1, at p. 752 *supra* (values for variable Y), an increase of 67.6 percent. Thus, the percentage increase in the median family income due to inflation was 46.0.

71. C. BECCARIA, ON CRIMES AND PUNISHMENTS 58 (H. Paolucci trans. 1963).

72. Suppose, for example, that exogenous changes in omitted social factors in the 1960's produced a disproportionate increase in stranger-to-stranger homicides. This would cause an increase in the homicide rate to coincide with a decrease in the imprisonment rate, since it is harder to apprehend those who commit stranger-to-stranger homicides. The appearance of a deterrent effect would then be false.

73. Passell, *supra* note 24, at 67, has discussed potential sources of error in the measurement of this variable. A particularly important potential source of error comes from the fact that the measure of the average term of imprisonment is based on released homicide offenders, exclusive of homicide offenders who die in prison, some of whom had surely already served lengthy terms of incarceration.

74. On the other hand, errors in the measurement of the number of homicides, which appears both as the numerator of the homicide rate, (Q/N), and the denominator of the conviction rate, (C/Q), are likely to bias the estimates toward the appearance of a stronger deterrent effect of incarceration than may really exist. Klein, Forst, & Filatov, *supra* note 13, at 17-19.

75. *See* notes 54-55 *supra* and accompanying text.

76. A. VON HIRSCH, DOING JUSTICE 30 (1976).

sanction variables, whether or not the observations are weighted, and the inclusion of different subsets of available control variables.

Capital punishment may be a justly deserved and appropriate sanction in some instances. It is certainly an effective way to ensure that a person convicted of murder will not commit further crimes. The results of this analysis suggest, however, that it is erroneous to view capital punishment as a means of reducing the homicide rate.

APPENDIX

DATA SOURCES FOR THE VARIABLES USED IN THIS ANALYSIS

Q/N *Criminal Homicide Rate* = Number of murders and nonnegligent manslaughters per 100,000 residents. FEDERAL BUREAU OF INVESTIGATION, DEPARTMENT OF JUSTICE, UNIFORM CRIME REPORTS, table 3, at 30-52 (1960) & table 4, at 72-81 (1970).

E/C *Execution Rate* = Ratio of the number of executions to the number of homicide prisoners received from court. E/C denotes the ratio of executions to murder convictions that occur in the same year. E_{+1}/C denotes the ratio of executions to convictions, with the executions occurring the year after convictions. E_m/C denotes the ratio of executions to convictions, where E_m is the mean annual number of executions over the three-year period centered about the year of the convictions. The source of the number of executions is FEDERAL BUREAU OF PRISONS, DEPARTMENT OF JUSTICE, NATIONAL PRISONER STATISTICS: CAPITAL PUNISHMENT, table 2, at 8-9 (No. 45, August 1969) for 1960 and 1961 data, and table 2, at 18-19 (No. SD-NPS-CP-3, November 1975) and table 3 (No. 20, February 1959) for the construction of 1959 data. The source of the number of homicide prisoners received from the court in 1960 is FEDERAL BUREAU OF PRISONS, DEPARTMENT OF JUSTICE, NATIONAL PRISONER STATISTICS: CHARACTERISTICS OF STATE PRISONERS, table A5, at 50-51 (1960); the source of the 1970 data is FEDERAL BUREAU OF PRISONS, DEPARTMENT OF JUSTICE, NATIONAL PRISONER STATISTICS, STATE PRISONERS: ADMISSIONS AND RELEASES, table A2, at 6 (1970).

C/Q *Incarceration Rate* = Ratio of the number of homicide prisoners received from court to the number of murders and nonnegligent manslaughters. The source of the number of homicide prisoners received from the court is given under E/C. The number of murders and nonnegligent manslaughters is calculated as the criminal homicide rate, described under Q/N, multiplied by the resident population. The source of the resident population is U.S. BUREAU OF THE CENSUS, DEPARTMENT OF COMMERCE, STATISTICAL ABSTRACT OF THE UNITED STATES, table 11, at 12 (1974) [hereinafter cited as CENSUS ABSTRACT].

T *Term of Incarceration* = Mean time, in months, served by homicide prisoners released from prison (including paroles). Calculated from data given in FEDERAL BUREAU OF PRISONS, DEPARTMENT OF JUSTICE, NATIONAL PRISONER STATISTICS: PRISONERS RELEASED FROM STATE AND FEDERAL INSTITUTIONS, tables 6-54, at 22-70 (1960) and FEDERAL BUREAU OF PRISONS, DEPARTMENT OF JUSTICE, NATIONAL PRISONER STATISTICS, STATE PRISONERS: ADMISSIONS AND RELEASES, table 10, at 47-81 (1970).

Cr *Nonhomicide Crime Rate* = Number of offenses other than murder and nonnegligent manslaughter reported to police per 100,000 residents, calculated as the total crime index rate minus the criminal homicide rate described under Q/N. The sources of the total crime index rate data are the same tables that were cited under the description of Q/N.

Age *Proportion of Residents of the Ages 21-24* = Ratio of the number of residents of the ages 21 through 24 to the total resident population. The source of the number of persons of the ages 21-24 for 1960 is CENSUS ABSTRACT, *supra*, table 19, at 27 (1962); the source of the 1970 data is CENSUS ABSTRACT, *supra*, table 36, at 31 (1972). The Census Bureau gives the 1960 data for persons between the ages 20-24, which we multiply by 0.8. The source of the total resident population is given under C/Q.

NW *Proportion of Nonwhites* = Ratio of the number of nonwhite residents to the total resident population. The source of the number of nonwhite residents is CENSUS ABSTRACT, *supra*, table 31, at 29 (1974). The source of the total resident population is given under C/Q.

Male *Proportion of Males* = Ratio of the number of male residents to the total resident population. The source of the number of male residents is CENSUS ABSTRACT, *supra*, table 17, at 25 (1962) & table 25, at 25 (1972). The source of the total resident population is given under C/Q.

Urb *Proportion of Urban Residents* = Ratio of urban population to the total resident population. The source of the urban population is CENSUS ABSTRACT, *supra*, table 18, at 17 (1974). The source of the total resident population is given under C/Q.

Enr *Enrollment Rate* = Ratio of the number of persons enrolled in public elementary and secondary schools to the number of residents of the ages 5-17. CENSUS ABSTRACT, *supra*, table 196, at 122 (1974).

Pop *Resident Population* = Number of residents, in millions, as of July 1. CENSUS ABSTRACT, *supra*, table 11, at 12 (1974).

Div *Divorce Rate* = Ratio of the number of divorces to the number of residents. The source of the number of divorces is CENSUS ABSTRACT, *supra*, table 95, at 67 (1974). The source of the number of residents is given under C/Q.

Y *Median Family Income* = Amount of income, in dollars, such that exactly half the resident families earn at least that much. CENSUS ABSTRACT, *supra*, table 627, at 3?? (1974).

Pov *Proportion of Families in Poverty* = Ratio of the number of families below the low income level to the total number of resident families. CENSUS ABSTRACT, *supra*, table 634, at 391 (1974).

Emp *Proportion of Adults Employed* = Ratio of the number of residents employed in nonagricultural establishments to the number of residents at least 16 years of age. The source of the number of employed residents is CENSUS ABSTRACT, *supra*, table 363, at 226 (1972). The number of residents at least 16 years of age is calculated from the CENSUS ABSTRACT, *supra*, table 19, at 27 (1962) & table 36, at 31 (1972), as follows: For 1960 we use the resident population at least twenty years of age plus 0.8 times the number of residents between 15 and 19 years of age. For 1970 we use the resident population at least 18 years of age plus one-half the number of residents between 14 and 17 years of age.

S *Binary Southern Variable* =1 if the state is Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, or West Virginia; otherwise = 0.

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Answer Critic From Utah

JUNEAU (AP) — Democratic lead[illegible] of the Alaska Legislature point[illegible] to the record today in defense of legislative activities which drew the fire of a Republican congressman.

Rep. Dawson (R-Utah), in a House speech yesterday, accused the Democratic controlled territorial Legislature of actions which might endanger Alaska statehood.

Dawson accused Democrats of "playing politics" in the rejection of [illegible] of [illegible] nominations to territorial boards and commissions by acting Gov. Waino Hendrickson.

The Utah Republican, a strong statehood supporter, also criticized a pre-legislative proposal by former Rep. Wendell Kay of Anchorage, speaker of the House in 1955, that the governor be stripped of patronage powers.

House Speaker Richard Greuel (D-Fairbanks) commented:

"Apparently Rep. Dawson is unaware of the old political custom in Alaska: the elected Legislature usually turns down a large percentage of the appointed governor's board appointments. Actually, the 1957 Legislature departed from tradition in confirming the vast majority of the governor's appointments."

"In confirming two-thirds of the Republican governor's appointees," said Senate President Victor Rivers (D-Anchorage), "the Alaska Legislature demonstrated its willingness to cooperate."

Rivers, a brother of Alaska's provisional "representative in Washington, Ralph Rivers, added:

"In the interest of harmony and good government, the democratic majority went two-thirds of the way. This is in contrast to the Republican controlled Legislature of 1953 which rejected all appointments of a Democratic governor."

Kay, a registered lobbyist at the 1957 Legislature, commented:

"Unfortunately, Rep. Dawson is [illegible] miles away from the Alaska situation. Perhaps this demonstrates how much we need statehood. I still feel it is obvious that an elected Alaskan can better manage Alaska than an appointed federal official."

There has been introduced in the territorial Legislature a much-criticized Democratic-sponsored bill calling for an elected administrative governor, to serve along side the governor appointed in Washington. The latest version of the proposal would make the appointed governor the first administrative governor, serving until the 1958 general election.

A leading Senate Republican, Sen. Mike Stepovich of Fairbanks, supported Dawson's comments. Stepovich said:

"I think he is correct. We'd bet-

ALASKA DEATH PENALTY

JUNEAU (AP) — Death by hanging, [illegible] Alaska's most severe penalty for the commission of a crime, would be abolished under terms of a bill passed by the territorial House today.

By a 14-9 vote, with one member absent, the House passed and sent to the Senate a bill that would strike all forms of capital punishment from the territorial law.

The vote followed a day and a half of debate in which the co-author of the bill, a lawyer, said "the hideous mistake of executing an innocent man" makes it [illegible] [illegible] to bring him out of the grave into God's sunshine."

"I have been unfortunate enough to witness executions," said Rep. Warren Taylor (D-Fairbanks), "and the cold-blooded, methodical way they are carried out makes you wonder if it isn't legal murder."

Taylor, who introduced the bill with Rep. Victor Fischer (D-Anchorage), told the House and a packed gallery yesterday that he had been a lawyer in Alaska 30 years next month.

"I've taken part in 31 homicide trials, quite a few in the first degree," he said, "both as a prosecutor and a defense attorney." he added:

"I recall a case in Valdez when I was prosecuting a very good friend of mine. A combination of cabin fever and raw alcohol led him to trouble, and fortunately the jury brought in a verdict in the second degree. Had it been different, I would have carried to the grave that scar on my soul."

One of the strong opponents to the bill, Rep. Dora Sweeney (D-Juneau) recalled two famous cases in support of her argument that the death penalty should be continued.

"Remember Winnie Ruth Judd," Rep. Sweeney said. "A Juneau girl who horribly hacked up two persons and tried to ship their bodies all over everywhere in a trunk, she lives today in an institution, from which she has escaped several times."

Then she pointed to the recent case of John Gilbert Graham, the Colorado youth executed for planting a bomb aboard a commercial airliner and killing more than 40 persons.

"If that happened in Alaska," Mrs. Sweeney said," and this bill passes, we could not penalize him with any more than life imprisonment—from which he eventually could be freed."

Rep. Seaborn J. Buckalew Jr. (D-Anchorage) protested that the bill "protects a class of people who don't deserve the consideration we are giving them."

Taylor said Rep. Sweeney believed in the commandment "of thou shall not kill—but," Taylor added: "There is a period after kill."

"And [illegible] Buckalew," Taylor said, "has the district attorney complex—show them no mercy."

The bill, as passed after amendments, provides for mandatory life imprisonment for persons convicted of first degree murder.

The old statute also was revised in a section which provides for first degree convictions for persons accused of wrecking a railroad train.

The amendment to that would include mandatory life for a person who "places obstruction o[illegible] [illegible]

Name Rondy Queen Tonight

(Continued from Page 1)

neau, Clark Monk and Glen Phillips of Sun Valley, Idaho; Leon Johannsen, brother of the famous Sven Johannsen, racing under the colors of the Girdwood Ski Club; Charles Burnham of Eielson Air Force Base; Erik Jorgensen, Danny McDonald and Art Stubbings of Fairbanks and seven University of Alaska students—Bill King, Norm Saunders, Hazel Owens, Sam Price, Dave Teague, Harvey Turner and Donald Boyce.

Luke Zell, president of the Anchorage Ski Club, announced the following lineups, according to classes, for the slalom and downhill racing at Arctic Valley.

Class A — Joe Young, Gary King, Clark Monk, Ronnie Seater, Glen Phillips, Bill Northway, Jerry Edwards, Johannes Berge, Harvey Turner, Ray Morris and Gene Cox.

Class B — Earle Walker, Mike Beavers, Frans Bjorklund, Merle Akers, Dick Brown, Roger Ridgeway, Erik Jorgensen, Joe Jurkiewicz, Ray Brahaney, Dan McDonald, Art Stubbings, Ron Ferch, Butch Schrweide and Pat McElroy.

Class C — Charles Burnham, Norm Saunders, Bill King, Alanson Bartholomew, Warren Henry, Edwin Harper, Roger Shaffer, Jerry Palmer, Robert Boehm, Jerry McMillan, Dave Teague, Sperry Zervas, Ron Davis, Ole Aanjensen, Don Boyce, Arne Ewald, E. B. Mackenzie, Dick Quinn, Bill Earl, Luke Zell, Sam Price, Ray Zernis, Larry Connor and DeMar Long.

Women's division — Hazel Owens, Linda Crocker, Bonnie Weimer, Marie Simon, Laurita Hefner, Marion Brown, Pat Graham and Valerie Brere.

Fur Rendezvous ski trophies will be presented at a banquet Sunday night, 8 o'clock at Forest Park Country Club.

The Alaska Sled Dog Championship races continue tomorrow and finish Sunday. A 25-mile leg is run each day [illegible] than 20 of the territory's [illegible]

DEW Line Bids Opened

SEATTLE (AP) — A Seattle firm's bids for building two DEW line stations in southwest Alaska are $1,461,[illegible] under the total of the government estimates and $[illegible] [illegible]374 under the highest pair of bids.

The bids were opened by the Alaska District Office of the Corps of Engineers here yesterday.

The low bidder was Hansen-Onberg Co. Its bid for the installation at Cold Bay, near the tip of the Alaska Peninsula, was $3,628,[illegible]. Its bid for a similar job at Cape Sarichef, about 100 miles to the west on Unimak Island, was $3,717,[illegible].

The firm also said it would knock off $100,000 if it received both jobs.

Nine bids were submitted. The government estimate for Cold Bay was $4,0[illegible],200; for Cape Sarichef $4,719,[illegible].

The other bids, with Cold Bay listed first, were:

Chris Berg, Inc., Seattle, $[illegible],077 and $4,681,280.

Baker & Ford, Bellingham, $[illegible],996,446 and $4,309,506.

Peter Kiewit Sons, $4,122,5[illegible] and $5,153,224.

Birch-Green Co., $4,[illegible],[illegible] and $5,216,742.

Morrison-Knudsen Co., $5,042,[illegible] for Sarichef; no bid on Cold Bay.

Bock Constructors and Raber & Kief, Seattle, $5,191,270 on Sarichef only.

S.S. Mullen Co., Seattle, $4,[illegible],259 on Cold Bay only.

Grove, Shepherd, Wilson & Kruge of New York and Seattle and J. A. Jones Co., of Charlottesville, N.C., and Seattle, $5,0[illegible],157 and $5,[illegible],642.

The projects are the first two of six planned in an extension of the defense warning system to the Aleutians.

Baby Show Is Tomorrow

Parents entering their children in the Fur Rendezvous Baby Contest are to bring them to the high school cafeteria tomorrow morning.

Children aged nine months to two years will be judged from 9 to 10; two to [illegible] years, 10 to 11; and 3 to 4, 11 [illegible] 11:45. Winners in each age group will compete at 2 p.m. tomorrow for the crown of baby king or queen.

Entry blanks are still available at the Spenard Hobby Shop, Northern Commercial Baby Department, Stork Shop and Jack and Jill Shop. To save time, parents are asked to have the entry forms filled in before bringing the children to the show.

The Elauret Homemakers are [illegible]

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STRONG AND WEAK RESEARCH DESIGNS FOR DETECTING THE IMPACT OF CAPITAL PUNISHMENT ON HOMICIDE

David P. Phillips*

I. Introduction

This article does not attempt to provide a systematic review of studies on the deterrent effect of capital punishment.[1] Instead, the article encompasses two related topics: (1) a discussion of some common methodological shortcomings of many studies on the deterrent effect of capital punishment, and (2) a suggestion for a type of study design that appears to avoid many of these shortcomings.

To determine whether capital punishment influences the homicide rate, one needs to disentangle the effect of capital punishment from the effect of other variables on homicide. This "disentangling" can be accomplished in two major ways. First, one can use a controlled experiment, or an approximation to one. This approach is favored in the natural sciences and has been the method of choice for most research on the effects of capital punishment. Second, one can use statistical techniques, including regression analysis, correlation, analysis of variance, path analysis, etc.[2]

This paper concentrates on studies that use the controlled experiment as a model study design. Most of these investigations have implicitly followed the study design represented in Table 1. A careful examination of this table will help to reveal systematically some of the common shortcomings of research on capital punishment.

* Associate Professor of Sociology, University of California at San Diego. B.A., Harvard University, 1964; Ph.D., Princeton University, 1970.

1. Several studies on the deterrent effect of capital punishment can be found in H. Bedau, The Death Penalty in America (1964); W. Bowers, Executions in America (1974); Klein, Forst & Filatov, *The Deterrent Effect of Capital Punishment: An Assessment of the Estimates*, in Deterrence and Incapacitation [illegible] (1978); Zeisel, *The Deterrent Effect of the Death Penalty: Facts v. Faiths*, 1976 Supreme Court Review [illegible].

2. For examples and criticisms of this type of study, see Bowers & Pierce, *The Illusion of Deterrence in Isaac Ehrlich's Research on Capital Punishment*, 85 Yale L.J. 187 (1975); Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life and Death*, 65 Am. [illegible] (1975); Ehrlich, *Deterrence: Evidence and Inference*, 85 Yale L.J. 209 (1975); Klein, Forst & Filatov, supra note 1; Zeisel, supra note 1; P. Passell & J. Taylor, The Deterrent Effect of Capital Punishment: Another View (Discussion Paper [illegible], Columbia University Department of Economics 1975).

TABLE 1

A Common Paradigm for Many Studies of the Deterrent Effects of Capital Punishment

	Time 1	Time 2	
Experimental Group	h(1)	h(2)	d = h(2) − h(1)
Control Group	h'(1)	h'(2)	d' = h'(2) − h'(1)

II. A Paradigm for Evaluating the Soundness of Some Research on the Deterrent Effects of Capital Punishment

The experimental group in Table 1 consists of a population subject to capital punishment at time 1 and not subject to capital punishment at time 2. Ideally, the control group is identical to the experimental group in every respect except one: unlike the experimental group, the control group is never subject to capital punishment. h(1) denotes the homicide rate of the experimental group at time 1, when it is still subject to capital punishment. h(2) denotes the homicide rate of the same group at time 2, after capital punishment has been abolished. d is the difference between the experimental group's homicide rate before and after capital punishment was abolished. In other words, d denotes the amount by which the experimental group's homicide rate has changed during the study period. h'(1) is the homicide rate of the control group at time 1, while h'(2) is the homicide rate at time 2. d' denotes the amount by which the control group's homicide rate has changed during the study period.

Suppose that the experimental group's homicide rate changes over time in one way, and the control group's homicide rate changes in another way. In other words, suppose that d is different from d'. Because the experimental and control groups are similar in every way except for their experience of capital punishment, a large difference between d and d' may then be plausibly attributed to the different exposure to capital punishment in the two groups.

As indicated earlier, this seemingly simple study design has frequently been the model for research on capital punishment. Therefore, it is surprising and regrettable that no investigation in the literature meets all the requirements of this study design. No study provides all four pieces of information, h(1), h(2), h'(1), and h'(2), required in Table 1. Instead, a variety of incomplete study designs prevail. In the following section, these incomplete study designs are

III. FOUR TYPES OF INCORRECT STUDY DESIGN

A. *Only Information on* $\begin{array}{|c|c|} h(1) & h(2) \end{array}$ *Is Provided*

In this type of study, there is no control group. The author merely measures the experimental group homicide rate before and after the abolition of capital punishment. If the homicide rate increases after the abolition of capital punishment, the author may be tempted to attribute this increase to the disappearance of capital punishment. In fact, however, the abolition of capital punishment may be only one of many changes experienced by the experimental group between time 1 and time 2. Any of these other changes may be the real explanation for the increase in the homicide rate.

B. *Only Information on* $\begin{array}{|c|} h(1) \\ \hline h'(1) \end{array}$ *Is Provided*

In this type of study, the homicide rate in an experimental group, $h(1)$, is compared with the homicide rate of a control group, $h'(1)$, at a time when the first group is subject to capital punishment and the second group is not. A typical example of this type of study occurs when one compares the homicide rate of a state *with* capital punishment to the homicide rates of adjacent states *without* capital punishment. If the homicide rate of the "capital punishment" state is lower than the homicide rate of the "non-capital punishment" states, the author may wish to attribute the lower rate to the presence of capital punishment. In fact, however, adjacent states differ in a great many ways that have nothing to do with capital punishment. Out of the multitude of differences between adjacent states, it seems unreasonable, without additional evidence, to select the presence of capital punishment as the definitive explanation for a low homicide rate.

C. *Only Information on* $\begin{array}{|c|c|} h(1) & \\ & h'(2) \end{array}$ *Is Provided*

At first sight, it is difficult to imagine how a careful researcher could produce a study of this sort, in which a group at one time is compared with a different group at another time. In fact, however, it is surprisingly easy to conduct a flawed investigation of this sort. Consider, for example, a comparative study of the United States homicide rates in, say, 1950 and 1960. This appears to be a study of type

$$\begin{array}{|c|c|} h(1) & h(2) \end{array}$$

already discussed, but is actually a study of type

$$\begin{array}{|c|c|} h(1) & \\ & h'(2) \end{array}$$

of forty-eight states, whereas in 1960 it consisted of fifty states. Thus the group being studied in 1950 is not the same as the group examined in 1960, even though the name of the two groups is identical.

In this example, the effect of the error is not very great, and the flaw in the study design, though embarrassing, may not be serious. In some circumstances, however, this type of flaw may be both embarrassing and serious. It is serious, for example, whenever comparisons are made using United States homicide statistics for the period 1900-1932. During this period, vital statistics rates were not known for the entire United States, only for those states eligible for inclusion in the Death Registration Area. The geographic makeup of this area changed continually from 1900 to 1932. (From 1933 on, all forty-eight states were included.) Thus, an examination of "United States" homicide rates in the early part of the century generally involves a constantly changing entity. The changes in this entity are masked, however, because the name of the entity remains unchanged.

D. *Only Information on* $\begin{array}{|c|c|} & h(2) \end{array}$ *Is Provided*

This type of study, fortunately, is more common in the popular than in the professional literature. The author of this type of study notices, for example, that California had a high homicide rate in 1970. Out of the myriad features that characterized California in 1970, the author selects one, the absence of capital punishment, and asserts that this is the reason for the high homicide rate. The author does not attempt to determine whether the California homicide rate was equally high *before* the abolition of capital punishment, nor does he attempt to compare changes in California homicide rates with changes in the homicide rates of a matched control group. It is evident that a study of this type cannot provide compelling evidence on the deterrent effect of capital punishment.

Thus, the paradigm for evaluating some studies of capital punishment

$$\begin{array}{|c|c|} h(1) & h(2) \\ \hline h'(1) & h'(2) \end{array}$$

highlights the shortcomings of four common study designs:

$$\underset{A}{\begin{array}{|c|c|} h(1) & h(2) \\ & \end{array}} \qquad \underset{B}{\begin{array}{|c|c|} h(1) & \\ h'(1) & \end{array}} \qquad \underset{C}{\begin{array}{|c|c|} h(1) & \\ & h'(2) \end{array}} \qquad \underset{D}{\begin{array}{|c|c|} & h(2) \\ & \end{array}}$$

IV. Other Shortcomings of Research on Capital Punishment

So far, only those shortcomings that are revealed by a close consideration of the paradigm illustrated by Table 1 have been discussed. Two additional shortcomings should also be mentioned.

First, almost all studies in the literature seek to determine whether laws, or the absence of laws, about capital punishment affect the homicide rate. Typically, the researcher seeks to determine whether the abolition or reinstatement of the death penalty changes the homicide rate. This type of research often has two obvious weaknesses, in addition to the shortcomings already discussed above: (1) the death penalty is abolished or reinstated very rarely in any given area, and consequently the researcher has very few cases to study; (2) the fact that a state has a law permitting capital punishment does not necessarily mean that the state actually practices capital punishment. If capital punishment is "on the books" but not practiced, it may not have the same effect as a law that is enforced. For these two reasons, studies of the deterrent effect of capital punishment *laws* may not reveal a deterrent effect, even if such an effect actually exists.

Second, almost all studies on capital punishment use yearly rather than weekly or daily statistics.[5] Research using yearly data has typically failed to find a deterrent effect of capital punishment. Such failure may have occurred because (1) there is no deterrent effect, or (2) there is a deterrent effect, but it cannot be detected with crude yearly data. Short-term deterrent effects are obviously more likely to be discovered with weekly than with yearly statistics.

These methodological shortcomings are common in the literature on capital punishment. The next section of this paper describes a type of research that appears to avoid many of them.[6]

V. The Effect of Publicized Executions on the Level of Homicides

As noted earlier, it is desirable to examine daily or weekly homicide statistics in order to detect short-term effects of capital punishment. A search of the vital statistics collections of the Library of Congress, the National Library of Medicine, and the British Museum revealed only one jurisdiction, England, that both practiced capital punishment and published weekly homicide statistics over a given period of time. The weekly homicide statistics for London from 1858 to 1921 were available for study.

A list of highly publicized English executions for the same time period was generated from a standard encyclopedia of notorious murderers. Table 2 lists these executions, ranked by the amount of publicity each received, together with the number of homicides before, during, and after the week of each execution. The week in which the execution was publicized in the newspaper will henceforth be termed "the execution week" or "the experimental period." The week just before the experimental period and the week just after will be termed "the control period." It is evident that the London in each control period is nearly identical to the London in the corresponding experimental period, except for the presence of an execution in the experimental period. Thus, this procedure for choosing controls avoids the problem of dissimilar experimental and control periods encountered in other study designs.

If executions deter homicides, then the number of homicides in the experimental period should generally be smaller than the number of homicides in the control period. Table 2, column 6, shows the average weekly number of homicides in each control period; column 7 indicates whether the number of homicides in the experimental period was less than the weekly average in the control period (indicated by a " ") or more than the weekly average (indicated by a " "). In three cases the number in the experimental period equaled the weekly average in the control period. In the remaining nineteen cases in Table 2, there are fifteen " " and four " " signs. In short, there is a general tendency for homicides to decline in the week of a publicized execution.

These data are consistent with the hypothesis that publicized executions produce a brief decline in homicides. This hypothesis is further supported by an additional piece of evidence: the more publicity given to the execution story, the more homicides decline. This is evident from an informal inspection of Table 2 and also from a more refined statistical analysis reported elsewhere.

It is difficult to think of a plausible alternative explanation for the findings. The findings are sufficiently strong so that they cannot easily be attributed to chance fluctuations in the data. In technical terms, the results are statistically significant. The findings cannot result

[5] Current research shows only two studies besides that of the author that use weekly or daily homicide statistics to examine the deterrent effect of executions. See R. Dann, The Deterrent Effect of Capital Punishment, Friends' Social Service Series Bull. No. [illegible]; Graves, *A Doctor Looks at Capital Punishment*, [illegible]; Phillips, *The Deterrent Effect of Capital Punishment: New Evidence on an Old Controversy*, [illegible]. Dann's study found no evidence of a deterrent effect, perhaps because the author examined only five executions. Graves' study also found no deterrent effect, but only a small fraction of the executions studied were well publicized.

[6] Additionally, one Pennsylvania study examined the deterrent effect of death penalty sentences rather than executions. Only four sentences were examined, and no deterrent effect was found. See Savitz, *A Study in Capital Punishment*, [illegible].

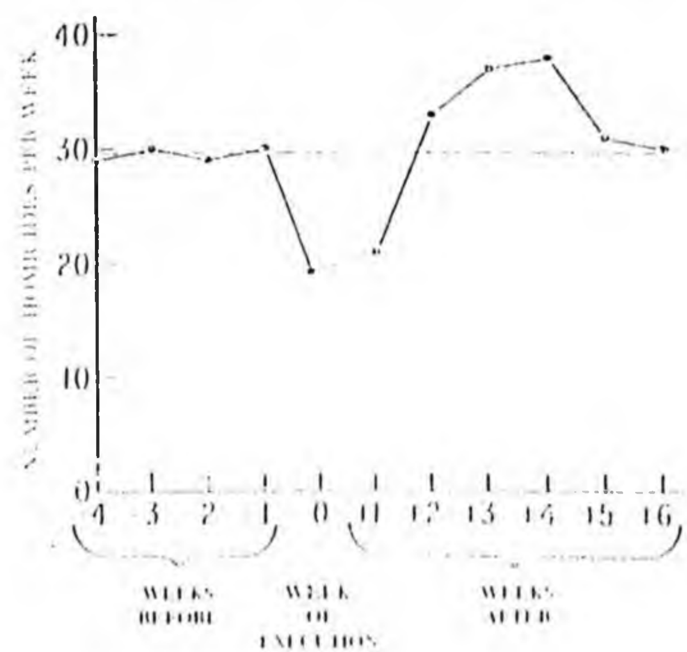
TABLE 2

WEEKLY HOMICIDES BEFORE, DURING, AND AFTER PUBLICIZED EXECUTIONS, LONDON, 1858-1921

Name of person executed and date of execution story in The Times	No. of col. inches devoted to the story in The Times	No. of homicides on week of execution story	Control Period: No. of homicides on week before experimental period	Control Period: No. of homicides on week after experimental period	Average no. of weekly homicides in control period	Difference between observed no. of deaths in execution week (col. 3) and no. expected (col. 6)
Wainwright 12/22/1875	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Crippen 11/23/1910	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Muller 11/15/1864	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Webster 2/10/1879	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Mapleton 11/29/1881	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Lamson 4/29/1882	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Seddon 4/19/1912	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Cream 11/16/1892	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Field & Gray 2/5/1921	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Chapman 4/8/1903	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Peace 2/26/1879	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Read 12/5/1894	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Milsom & Fowler 6/9/1896	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible] 8/9/1870	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible] 8/16/1905	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible] 1/10/1911	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible] 4/21/1870	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Dougal 7/15/1903	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible] 8/11/1910	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible] 1/1/1881	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible] 8/10/1910	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Stratton Brothers 5/24/1905	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

from a trend in homicides over time because the selection and treatment of controls corrects for the effects of trend. The findings also cannot be attributed to seasonal fluctuations because the control and experimental periods occur during the same seasons of the year. Similarly, the findings cannot be attributed to day-of-the-week fluctuations in homicides because the experimental and control periods each cover the same days of the week. At present, the best available explanation of the findings is that publicized executions in London from 1858 to 1921 caused a brief decline in homicides during the execution week.

So far, we have examined homicides for a three-week period centered on the execution week. It is instructive to graph the fluctuation of homicides for a longer period of time. When such a graph is made, a curious and unexpected phenomenon is discovered: the dip in homicides just after the execution is canceled by an equally large rise in homicides soon afterwards. Figure 1 shows this pattern quite



clearly. The most plausible explanation of this graph is that homicides are *temporarily* deterred for a two-week period; then these temporarily deterred homicides reappear after the publicized execution has faded from memory. Apparently, the "lesson of the scaffold" is real but only temporary, at least in London from 1858 to 1921.

Without further research, it is inappropriate to generalize these findings to contemporary America. The chief value of the study described herein lies not in its findings, but in its design. The study examines the effect of executions, rather than death penalty legislation; it employs weekly rather than yearly homicide statistics; and finally, it uses a control group that is nearly identical to the experimental group. Because of these features, the study design avoids many of the shortcomings of traditional investigations. Future research should attempt to apply this study design to contemporary American data.

A Comparison of the Work of Thorsten Sellin and Isaac Ehrlich on the Deterrent Effect of Capital Punishment*

David C. Baldus† and James W. L. Cole††

During the last 20 years, a substantial number of empirical studies—most prominent among them the work of criminologist Thorsten Sellin—have concluded that the death penalty has no measurable deterrent effect beyond that of life imprisonment.[1] A recent study by Isaac Ehrlich, an economist, challenges this traditional view.[2] Ehrlich criticizes Sellin's statistical methods and, on the basis of a more complex statistical procedure, estimates that "an additional execution per year over the period in question [1933-1969] may have resulted, on average, in 7 or 8 fewer murders."[3] In *Fowler v. North Carolina*,[4] the constitutional challenge to the death penalty now pending in the Supreme Court, the Solicitor General presented Ehrlich's findings to the Court and in his amicus brief cited them as "important empirical support for the a priori logical belief that use of the death penalty decreases the number of murders."[5] The Solicitor General asserted that earlier studies, and specifically those of Sellin, suffered from "investigatory flaws" and that only Ehrlich's work provided a reliable basis for judging whether the death penalty has a deterrent effect.[6] Now that *Fowler* has been set for reargument,[7] an assessment of the Solicitor General's claims has particular importance.

A statistical study cannot prove that executions deter murders, nor

* We are grateful for the assistance of David Engels in the preparation of this article.

† Professor of Law, University of Iowa.

†† Formerly Assistant Professor of Statistics, University of Iowa.

1. T. Sellin, The Death Penalty (ALI 1959) [hereinafter cited as The Death Penalty]; Sellin, *Capital Punishment*, 25 Fed. Probation 3 (1961); Sellin, *Homicides in Retentionist and Abolitionist States*, in Capital Punishment 135 (T. Sellin ed. 1967) [hereinafter cited as *Homicides*]. For other studies confirming these findings, see W. Bowers, Executions in America 19-20 (1974). *See generally* Allen, *Capital Punishment*, 2 Int'l Encyc. Soc. Sci. 290 (1968); F. Zimring & G. Hawkins, Deterrence: The Legal Threat in Crime Control (1973).

2. I. Ehrlich, The Deterrent Effect of Capital Punishment: A Question of Life or Death, 1973 (Working Paper No. 18, Center for Economic Analysis of Human Behavior and Social Institutions) [hereinafter cited as Ehrlich 1973]. A condensed version of the paper was recently published as Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life or Death*, 65 Am. Econ. Rev. 397 (1975) [hereinafter cited as Ehrlich 1975].

3. Ehrlich 1975, *supra* note 2, at 414.

4. State v. Fowler, 285 N.C. 90, 203 S.E.2d 803, *cert. granted sub nom.* Fowler v. North Carolina, 419 U.S. 963 (1974), *argued*, 43 U.S.L.W. 3562 (U.S. Apr. 21, 1975), *restored for reargument*, 422 U.S. 1039 (1975).

5. Brief for the United States as Amicus Curiae at 36 [hereinafter cited as Amicus Brief]. The Solicitor General submitted the Ehrlich Working Paper, *supra* note 2, to the Court on March 7, 1975.

6. *Id.* at 36-38.

can it prove that they do not. Given a hypothesis about a causal relationship, however, a statistical analysis can determine whether that hypothesis is consistent with past experience. Both Sellin and Ehrlich tested the hypothesis that capital punishment deters murders. Both used a variable to represent the threat of capital punishment, and both compared that variable with the behavior of homicide rates in the United States.[8] However, they used different statistical methods to make their comparisons and arrived at different conclusions.

Sellin used a "matching" technique.[9] He selected clusters of neighboring states "closely similar" in "social organization, composition of population, [and] economic and social conditions"; in each grouping at least one state had abolished the death penalty and at least one retained it.[10] He then compared the homicide rates for the years 1920-1955 and 1920-1962 in abolitionist and retentionist states within each group, and found that the rates in abolitionist states were not significantly or systematically different than the rates in retentionist states.[11]

8. Since the public records kept by the FBI do not include statistics on the number of murders committed each year, the researchers in this area must use the number of "murders and nonnegligent manslaughters" as a surrogate measure of the number of murders. This statistic is generally considered adequate on the assumption that the proportion of murders to nonnegligent manslaughters remains constant from one year to the next. Sellin, *Capital Punishment*, *supra* note 1, at 5-6; *see* Ehrlich 1975, *supra* note 2, at 406-07 (accounting for possible increasing trend in fraction of capital murders among all murders by including chronological time as a factor influencing the homicide rate). Throughout this paper we refer to the "homicide rate" in discussing the data analyzed by Sellin and Ehrlich, with the understanding that the underlying theoretical relationship is between executions and the *murder* rate.

9. Sellin also performed longitudinal studies of crime rates before and after a change in punishment policy in a given jurisdiction. *See* note 16 *infra*.

10. The Death Penalty, *supra* note 1, at 23.

11. Tables I and II illustrate the results reported in The Death Penalty, *supra* note 1, at 21-31, and *Homicides*, *supra* note 1, at 136-37.

TABLE I

Comparative Crude Homicide Death Rates in States with and States without the Death Penalty—Average Annual Rate 1920-1955 (Death Penalty States Are Marked D)

Midwest

Matched Group 1			*Matched Group 2*			*Matched Group 3*		
	D	D			D		D	D
Michigan	*Indiana*	*Ohio*	*Minnesota*	*Wisconsin*	*Iowa**	*North Dakota*†	*South Dakota*†	*Nebraska*
4.8	4.8	6.1	2.2	1.8	1.7	1.4	1.6	2.7

New England

Matched Group 1			*Matched Group 2*		
	D	D		D	D
Maine	*New Hampshire*	*Vermont*	*Rhode Island*	*Massachusetts*	*Connecticut*
1.6	1.3	1.2	1.7	1.7	2.5

* 1921-1955 † 1921-1955 ‡ 1930-1955

Adapted from F. Zimring & G. Hawkins, *supra* note 1, at [illegible] data from [illegible] the conclusions of the study as "completely unfounded."[18] If Ehrlich's study is reliable, it may be difficult to claim that the death penalty is "excessive and unnecessary" and therefore violates the Eighth Amendment. Because the technical merits of the Ehrlich study have thus become relevant to an important constitutional adjudication, the *Journal* is publishing what is essentially a statistical debate between Ehrlich and his critics.

In the first article, Messrs. Baldus and Cole defend Sellin and contend that his technique is better than Ehrlich's regression method for testing the deterrence hypothesis; in the second article, Messrs. Bowers and Pierce argue that Ehrlich's data are fundamentally inadequate for the method he uses and that no evidence of a deterrent effect is found when his method is correctly applied. Professor Ehrlich then responds with methodological and statistical arguments in support of his initial study and further elaborates on the basic issues underlying his research. In the next issue of the *Journal*, Professor Jon K. Peck[19] will comment on the debate between Ehrlich and his critics.

The difficulties a court faces in attempting to arbitrate an "abstruse statistical dispute" between parties to a litigation were forcefully noted by Judge J. Skelly Wright in *Hobson v. Hansen*,[20] a suit challenging different per-pupil expenditures among elementary schools in the District of Columbia. Judge Wright commented on "the added difficulties which beset the truth finding process when it is necessary to rely upon easily manipulated statistical analyses," and deplored the "overgrown garden of numbers and charts and jargon" which he suggested had obscured the basic issues in the suit.[21] He added:

> The reports by the experts—one noted economist plus assistants for each side—are less helpful than they might have been for the simple reason that they do not begin from a common data base, disagree over crucial statistical assumptions, and reach different conclusions. Having hired their respective experts, the lawyers in this case had a basic responsibility, which they have not completely met, to put the hard core statistical demonstrations into language which serious and concerned laymen could, with effort, understand.[22]

The articles which follow attempt to meet this "basic responsibility" in the context of the statistical debate over the deterrent effect of capital punishment; to the extent it is not met, the Court must rely, as did Judge Wright, "upon burden of proof, and upon straightforward moral and constitutional arithmetic."[23]

> penalty] concluded that, over the past several decades, each execution actually carried out deterred a significant number of murders. Other studies of the death penalty are infected by serious analytical flaws, and so do not provide support for a contrary conclusion.

Id. at 9.

18. Reply Brief for Petitioner, App. C at 1; See Brief, *supra* at 19 n.31. Petitioner submitted two statistical critiques of the Ehrlich study, *id.* Apps. C & E.

19. Assistant Professor of Economics, Yale University.

20. 327 F. Supp. 844, 851 (D.D.C. 1971).

21. *Id.* at 859.

22. *Id.* at 859.

These articles also illustrate the inherent vulnerability of complex statistical techniques to the adversary process. Any statistical analysis depends on a variety of explicit and implicit assumptions which can be challenged by opposing parties and on which experts may reasonably differ.[24] Since courts generally have no expertise to resolve statistical disputes, they will tend to ignore the evidence altogether once such a dispute arises.[25] The probative value of any study can be destroyed by raising a large number of technically complex objections, which, if not sufficient to disprove the results of the study, will at least undermine them to the point where the decisionmaker refuses to rely on them. This process tends toward Chief Justice Burger's characterization of the evidence on deterrence in *Furman*—an "empirical stalemate."[26] The usefulness of statistical analyses to the courts may depend on the development of procedures to resolve the technical debates which seem inevitably to arise when such studies are put before them.[27]

24. *See, e.g.*, J. Johnston, Econometric Methods 121-23 (2d ed. 1972) (mathematical statement of assumptions needed for regression analysis).

25. Finkelstein has noted this tendency in administrative proceedings in which regression studies have been introduced. *Regression Models*, *supra* note 15, at 1111. The lack of success in the use of sophisticated statistical evidence in administrative proceedings necessarily raises doubts about the chances for its success in litigation before the courts, because agencies are likely to be both more able and more willing than courts to use this evidence. An agency has a staff which can develop the expertise necessary to construct an econometric model of a regulated industry. *See, e.g.*, Khazzoom, *The FPC Staff's Econometric Model of Natural Gas Supply in the United States*, 2 Bell J. Econ. & M. Sci. 51 (1971). In addition, statistical evidence often bears directly on the predictions and policy decisions that the agency must make. For example, in a ratemaking proceeding, the agency must forecast the effect of the proposed change in rates on demand and supply in the regulated industry—a question which lends itself to econometric analysis. The relevance of the statistical evidence to regulatory decisionmaking may therefore lead agencies to encourage its use. *See, e.g.*, Southern La. Area Rate Proceeding, 40 F.P.C. 530, 620 (1968), *modified*, 41 F.P.C. 301 (1969), *aff'd*, 428 F.2d 407 (5th Cir.), *cert. denied*, 400 U.S. 950 (1970), *quoted in Regression Models*, *supra* at 1150; Madison Gas & Elec. Co., 5 P.U.R.4th 28, 49 (1974).

26. 408 U.S. 238, 395 (1972) (Burger, C.J., dissenting).

27. Finkelstein, in *Regression Models*, *supra* note 15, at 1155-61, proposes that an administrative agency decide in advance on the data to be used in regression studies put before it in a given proceeding and that the agency require a party objecting to the statistical analysis presented to demonstrate the numerical significance of its objections or even to present a superior alternative analysis of the designated data. While possibly quite useful, these procedures are not likely to be sufficient to yield what their proponent describes as

> a progression towards greater refinement and correctness in statistical methodology which will not only be apparent to the decisionmaker, but which may also achieve results meriting at least tacit agreement among experts.

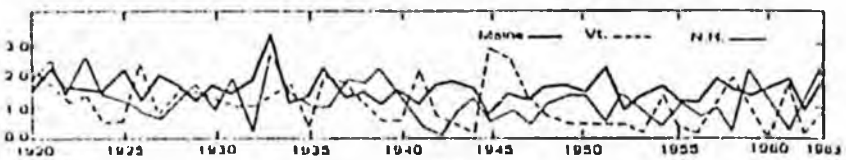
Id. at 1160. Instead, each side, using high-speed computers, may be able to "mine" a limited sample of data for results which support its hypothesis. *See id.* at 1119 n.27; Hobson v. Hansen, 327 F. Supp. 844, 859 (D.D.C. 1971) ("the studies by both experts are tainted by a vice well known in the statistical trade: data shopping and scanning to reach a preconceived result"). Although its implications have not been fully discussed, this problem of "data mining" has been recognized in economic journals. *See, e.g.*, Jorgenson, Hunter & Nadiri, *The Predictive Performance of Econometric Models of Quarterly Investment Behavior*, 38 Econometrica 213-15 (1970).

From this evidence he drew the "inevitable conclusion . . . that executions have no discernible effect on homicide death rates . . . ."[12]

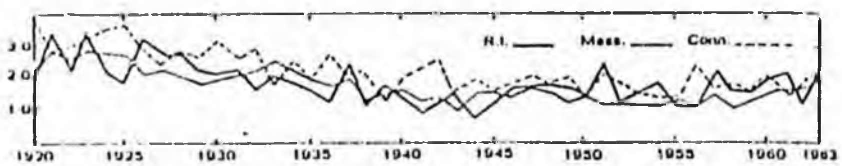
Ehrlich focused instead on the relationship in the nation as a whole between the homicide rate and "execution risk"—the fraction of persons convicted of murder who were subsequently executed. He compared the differences in homicide rate and execution risk for the years 1933-1969, and found a positive simple correlation between changes in the homicide rate and changes in execution risk—increases in execution risk were associated with increases in the homicide rate.[13] However, when he controlled for the influence of other variables on the homicide rate by using a multiple regression analysis, the relationship became negative. More precisely, he estimated that the elas-

TABLE II

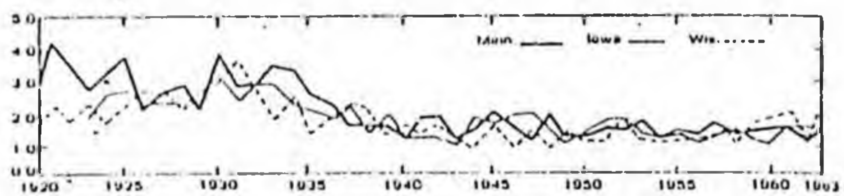
Homicide Death Rates (per 100,000 Population) in Contiguous Abolitionist and Retentionist States, 1920-1963.



Maine is an abolitionist state.



Rhode Island is an abolitionist state.



Minnesota and Wisconsin are abolitionist states.

12. THE DEATH PENALTY, *supra* note 1, at 34; *see Homicides*, *supra* note 1, at 138 ("The conclusion is inevitable that the presence of the death penalty—in law or practice—does not influence homicide death rates.")

13. Ehrlich 1975, *supra* note 1, at 409. Simple correlation analysis does not take into account the influence of other factors on the homicide rate, and hence is statistically less sophisticated than the multiple regression analysis on which Ehrlich relies for his ...

ticity of the homicide rate with respect to the execution rate was approximately −.06[14]—that is, a .06 percent decrease in the homicide rate was associated with a one percent increase in execution risk. This finding was the basis for his estimate that "on the average the tradeoff between the execution of an offender and the lives of potential victims it might have saved was of the order of magnitude of 1 for 8 for the period 1933-67 in the United States," and for his "tentative and rough calculation [that] the decline in [execution risk] alone might have accounted for about 25 percent of the increase in the murder rate between 1960 and 1967."[15]

In this paper, we compare the work of Sellin and Ehrlich and attempt to assess the reliability of their statistical evidence as a basis for making inferences about the deterrent effect of capital punishment. We consider three aspects of their research design: (1) the choice of a measure to represent capital punishment; (2) the choice of the nation or state as the unit of observation; and (3) the ability to control for factors other than the death penalty which may affect the homicide rate. We then discuss the replication or corroboration of their results by studies using the same or similar methods. While we do not argue that Sellin and others who have followed his approach have proven conclusively that the death penalty has no greater deterrent effect than life imprisonment, we believe that Sellin's work, despite its methodological shortcomings, offers a more reliable basis than Ehrlich's recent work for inferring whether the threat of capital punishment deters murders. Future studies by Ehrlich or others may weaken the credibility of work that went before him, but on the record to date Sellin makes the stronger case.

It is quite true that Ehrlich's approach is statistically more sophisticated than Sellin's. But statistical sophistication is no cure for flaws in model construction and research design. There are many questions which, because of inadequacies of data or theory, are best studied by simpler methods. The deterrent effect of capital punishment is at this point just such a question.

I. Measuring the Threat of Capital Punishment: Actual Use or Legal Status

The Solicitor General criticized Sellin's work on the ground that it "relied not upon the actual *use* of the death penalty, but upon its statutory authorization, as the independent variable against which the

14. *Id.* at 410 (Tables 3 & 4).
15. *Id.* at 398, 414 n.45.

murder rate was compared."[16] This criticism echoed Ehrlich's argument that "the actual enforcement of the death penalty [as measured by execution risk] may be a far more important factor affecting offenders' behavior than the legal status of the penalty."[17]

There is, of course, a necessary link between the legal status and actual use of capital punishment; the penalty cannot be used if it is not authorized. Moreover, the factor which is directly controlled by courts and legislatures is the legal status of the penalty. The precise question now facing the Supreme Court is whether capital punishment must be abolished, not whether its use should be increased or decreased assuming it is retained. For some purposes, it may be of interest to investigate the effects of increasing the number of executions in retentionist jurisdictions. But in the debate over abolition, the essential question is the effect of changing from a retentionist to an abolitionist jurisdiction.[18] Sellin's approach is directly addressed to this policy choice, and Ehrlich's approach is not.

Sellin compared the homicide rates within six clusters of abolitionist and retentionist states.[19] The execution levels in the retentionist states ranged from New Hampshire (one execution in the years 1920-1955) to Ohio (an average of seven executions a year).[20] Assuming that the penalty, if retained, would be applied as infrequently as in the past 15 years,[21] Sellin's comparisons of abolitionist states with retentionist states which rarely executed people become highly relevant. His other comparisons bear directly on the choice between abolition and retention at the higher execution levels of the earlier years.[22]

Ehrlich's comparison of the homicide rates with the ratio of executions to convictions—execution risk—is less relevant to the question of abolition. His analysis focuses on the marginal effects of small changes in execution risk—the number of murders deterred by one more execution—rather than on the difference between jurisdictions which do and do not use capital punishment. In Ehrlich's regression equation, the estimated homicide rate increases proportionally as execution risk declines. To predict the effect of abolition on the homicide rate, execution risk is simply set equal to zero in the equation. Representing abolition by zero execution risk fails to distinguish de facto abolition—where the death penalty is authorized but not currently applied—from de jure abolition. Yet even the legal possibility, however remote, of execution might have some deterrent effect. Because of its reliance on execution risk rather than the statutory authorization of the penalty, Ehrlich's study could not detect such an effect.

Moreover, for the particular mathematical form in which Ehrlich constructs his equation, zero execution risk implies either an infinitely large or a zero homicide rate (depending on whether the elasticity for execution risk is negative or positive).[23] These absurd implications show the equation was not designed to predict the effect of abolition. Even if one used a mathematical form which could generate a meaningful prediction of the homicide rate after abolition, such a prediction would be unreliable if based on the data used to construct Ehrlich's equation, because the possibility of error increases as the number of executions on which the prediction is based departs from the average (75) over the period which Ehrlich studied.[24]

II. Choice of Unit of Observation: Nation or State

Presumably because adequate data on arrests, convictions and executions—from which the risk of execution after conviction is computed—are not readily available on a state-by-state basis, Ehrlich compared executions and homicides for the nation as a whole. This aggregate approach cannot measure the extent to which changes in the execution

16. Amicus Brief, *supra* note 5, at 36 (emphasis in original).

17. Ehrlich 1975, *supra* note 2, at 415.

18. For a discussion of the relative reliability of various research designs as a basis for making causal inference about the impact of a particular law, see D. CAMPBELL & J. STANLEY, EXPERIMENTAL AND QUASI-EXPERIMENTAL DESIGN FOR RESEARCH (1966); SOCIAL EXPERIMENTATION: A METHOD FOR PLANNING AND EVALUATING SOCIAL INTERVENTION 97-116 (H. Riecken & R. Boruch eds. 1974); F. ZIMRING & G. HAWKINS, *supra* note 1, at 249-320; Campbell, *Legal Reforms as Experiments*, 23 J. LEGAL EDUC. 219-30 (1970).

19. Sellin did not, however, rely simply on the legal status of the penalty. He included in the category of abolitionist states those which had abolished the penalty except for treason and certain types of murders and had never applied the penalty after it was abolished for ordinary murders. THE DEATH PENALTY, *supra* note 1, at 1-2.

20. *Id.* at 25, 28, 32.

21. There was an average of 36 executions in the years 1961-1964, seven in 1965, one in 1966, two in 1967, and none since 1967. W. BOWERS, *supra* note 1, at 29.

22. [illegible] of executions for the five year periods between 1930 and [illegible]

23. Ehrlich posits a "murder supply function" in which the homicide rate is equal to the product of execution risk and six other variables which in theory influence the homicide rate. Ehrlich 1975, *supra* note 2, at 406 (equation (8)); pp. 179-80 *infra*. Each of these seven explanatory variables is raised to an exponent—the elasticity of the homicide rate with respect to that variable. If the elasticity for execution risk is negative, then as execution risk approaches zero, the product of the variables approaches infinity (because multiplication by a quantity raised to a negative exponent is equivalent to division by the same quantity raised to a positive exponent—in this case, by a quantity approaching zero). If the elasticity for execution risk is positive, then as execution risk approaches zero, the product of the variables approaches zero. To perform the regression analysis for the years 1968-1970, in which there were no executions, Ehrlich had to assume that one execution in fact occurred each year. Ehrlich 1975, *supra* note 2, at 409 n.6.

24. *See* R. WONNACOTT & T. WONNACOTT, ECONOMETRICS 27-31 (1970). The prediction is based on execution risk, not number of executions. But as the latter declines so will [illegible] as long as convictions do not decline in a greater proportion than executions.

rate are associated with changes in the murder rate for *individual* jurisdictions.

To illustrate this problem, consider the simplified example of a nation composed of three states, two retentionist (*R1* and *R2*) and one abolitionist (*A*). Assume that execution risk decreases in *R1* and remains constant in the other states, and that the murder rate increases in one state, not necessarily *R1*, and remains constant in the other two. No matter which of the three states experiences the increase in murders, the nation as a whole would show an aggregate increase in murder rate and decrease in execution risk; analyzing these aggregate figures would suggest a deterrent effect. This inference would be justified only if the increase in the murder rate occurred in *R1*, where execution risk has decreased. If instead the murder rate increased in state *A* or *R2*, the aggregate correlation would be misleading, because the increase in the murder rate in one jurisdiction could not be attributed to lower execution risk in another. The actual behavior of the murder rate and execution risk in different jurisdictions is, of course, far more complicated than in this example. But the point remains that Ehrlich's use of national data obscures the relationships between murder and execution rates and may yield results which seem consistent with a deterrent effect where no such effect actually exists. Sellin's comparison of murder rates in abolitionist and retentionist states, on the other hand, shows us whether or not homicide rates differ substantially in similar jurisdictions which do and do not use capital punishment. Because it examines differences in homicide rates among retentionist and abolitionist jurisdictions, Sellin's work does not contain the aggregation errors which may vitiate Ehrlich's results.

The aggregation approach used by Ehrlich has the further drawback of concealing regional differences. It is well-known that homicide rates are higher in the South than elsewhere in the United States,[25] and it is entirely possible that the deterrent effects, if any, of capital punishment would vary from one part of the country to another. These differences may be of considerable relevance to a decisionmaker considering the abolition of capital punishment in the United States. Sellin's method, which compares data for groups of contiguous states, would reveal regional differences if they existed; Ehrlich's approach, which aggregates the data for the entire United States, cannot. The fact that Sellin observed no deterrent effect in any region does not

25. [illegible] Sutherland & D. Cressey,

minimize the importance of taking a regional approach.[26] It merely adds plausibility to his conclusion that the threat of capital punishment has no measurable deterrent effect.

III. Holding Other Factors Constant

The Solicitor General asserted that "perhaps most importantly, [Sellin] failed to hold constant factors other than the death penalty that might influence the rate of murders." The Solicitor General also stated that Ehrlich's study alone was immune from this flaw.[27] We disagree. Sellin's matching method is simply a different technique for taking account of the influence of other variables than the multiple regression analysis used by Ehrlich. With either method there is the risk that variables not taken into account, or imperfectly taken into account, may influence the observed results. The issue is whether Sellin's or Ehrlich's method is more successful in reducing that risk.

Sellin was acutely aware of the problem of controlling for the influence of other variables on the murder rate. He recognized that the problem had been neglected by earlier work on the deterrent effect of capital punishment,[28] and used a matching method as "a deliberate attempt to eliminate differences other than those in punishment policy that might influence the crime rate."[29] A matching method controls for the effect of other variables by comparing areas which are as similar as possible with respect to those variables, but are different with respect to the variable whose effect is being isolated. Sellin assumed that the important factors influencing the murder rate were roughly similar in neighboring states. As Table III shows, this assumption is supported by a state-by-state comparison for a small sample of the law enforcement and socioeconomic factors which Ehrlich has hypothesized as determinants of the murder rate.[30]

26. The matching technique could not be applied in the Deep South, where there are no abolitionist states. However, Sellin did compare homicide rates in Missouri, which has authorized and applied the penalty, and Kansas, which did not enact it until 1935. THE DEATH PENALTY, *supra* note 1, at 32-33.

27. Amicus Brief, *supra* note 5, at 37-38.

28. *See* Sellin, *Capital Punishment*, *supra* note 1, at 6.

29. F. ZIMRING & G. HAWKINS, *supra* note 1, at 264. For this reason, Zimring and Hawkins conclude that Sellin's work is "more reliable than unmatched interstate comparisons." *Id.*

30. Table III presents comparative socioeconomic data for 1960 and law enforcement data for 1960 for the five groups of states matched by Sellin. (The law enforcement data reflect only a small, nonrandom sample of all the jurisdictions in each state, and are therefore unreliable.) The Table shows that the differences in these factors among the states in each group are generally small, and more importantly, that they do not explain the differences in the observed homicide rates. Consider, for example, the match between [illegible] homicide rate of 1.0, and retentionist states

TABLE III

Comparative Law Enforcement and Socioeconomic Data for Five Groups of States Matched by Sellin (1960 Data Except as Indicated)

State	1 Status of Death Penalty*	2 Homicide Rate	3 Probability of Apprehension Pct	4 Probability of Conviction Pct†	5 Labor Force Participation (%)	6 Unemployment Rate (%)	7 Population Aged 15-24 (%)	8 Real Per Capita Income ($)	9 Nonwhite Population (%)	10 Civilian Population (000's)	11 Per Capita Gov't Expenditures ($)‡	12 Per Capita Police Expenditures ($)
Michigan		4.5	.75	.25	54.9	6.9	12.?	1292	10.4	7811	363	11.5
Indiana	D	4.3	.83	.55	55.5	4.2	13.4	1176	6.2	4653	289	7.6
Ohio	D	3.2	.85	.33	54.9	5.5	12.9	1278	8.9	9690	338	9.0
Minnesota		1.2	.88	.44	55.5	5.0	13.1	1101	1.3	3409	383	7.5
Wisconsin		1.3	.95	.55	55.8	3.9	12.8	1173	2.4	3947	320	10.6
Iowa	D	.6	.40	.50	54.3	3.2	13.0	1078	1.1	2755	332	6.7
North Dakota		.5	1.00	1.00	53.7	5.6	14.2	899	2.1	628	394	5.5
South Dakota	D	2.1	...	1.00	54.2	4.1	13.4	877	4.2	675	329	5.1
Nebraska	D	2.3	1.00	.40	55.2	3.1	13.0	1080	2.7	1398	372	6.3
Maine		1.7	1.00	.67	53.0	6.5	13.8	999	.6	952	323	6.6
New Hampshire	D	1.3	1.00	...	57.4	4.3	13.0	1094	.5	600	334	6.9
Vermont	D	.5	1.00	0	54.4	4.5	13.9	966	.2	389	374	5.6
Rhode Island		1.0	0	.50	55.6	5.3	13.7	1194	2.3	853	296	10.8
Massachusetts	D	1.4	.80	.29	56.5	4.2	12.7	1309	2.5	5108	355	12.9
Connecticut	D	1.6	.84	.54	58.3	4.6	12.1	1542	4.6	2523	347	11.6

* D = Retentionist State
† 1966 Data (unreliable because based on very small, nonrandom sample)
‡ State and Local Governments

There are, of course, difficulties in attempting to control for other factors through a matching technique. In selecting areas for matching, it always is possible that an important difference among states will be overlooked. But such a difference would not vitiate the results of the matching study unless the difference were consistently and systematically related to the choice of a punishment policy.[31] The Solicitor General suggested that a difference in arrest and conviction rates might be systematically related to a difference in punishment policy, because jurisdictions which abolish capital punishment could use an increased likelihood of arrest and conviction as an alternative deterrent.[32] A finding that the homicide rate in a retentionist state was similar to the rate in an abolitionist state with higher arrest and conviction rates would then be consistent with the hypothesis of a deterrent effect for capital punishment. However, neither the Solicitor General nor Ehrlich offered evidence to support this relationship.[33]

Ehrlich's multiple regression analysis controls for the influence of variables other than execution risk by incorporating a set of additional explanatory variables into a mathematical formula for the homicide rate. Ehrlich posits a "murder function" in which the national homicide rate is determined by execution risk and six other factors. Of the six, two are law enforcement variables: the arrest rate (the fraction of all murders which are cleared by the arrest of a suspect) and the conviction rate (the fraction of suspects arrested for murder who are subsequently convicted of murder). The other four are socioeconomic variables: the labor force participation rate, the unemployment rate,

Massachusetts and Connecticut with homicide rates of 1.4 and 1.6 respectively. All but two of the variables—civilian population and percentage of nonwhites—suggest that Rhode Island's homicide rate should be *higher* than the neighboring states, while in fact it is lower than the rates in both Massachusetts and Connecticut.

The sources of data in Table III are as follows: *Column 1*—legal status of death penalty, F. Zimring & G. Hawkins, *supra* note 1, at 265; *Column 2*—homicide rate 1960, FBI, Uniform Crime Reports for the United States 31-37 (1961) (Table 2); *Columns 3 & 4*—FBI (unpublished data on file with *Yale Law Journal*); *Columns 5 & 6*—calculated from Bureau of the Census, U.S. Dep't of Commerce, General Social and Economic Characteristics: 1960 Table 5-3 (1961); *Column 7*—calculated from *id.* Table 69; *Column 8*—calculated from formula in M. Friedman, A Theory of the Consumption Function 1 (1957), using data from F. Karni, The Value of Time and the Demand for Money, 1971, Table 33 (unpublished doctoral dissertation, Univ. of Chicago), U.S. Public Health Serv., Dep't of Health, Education, and Welfare, Vital Statistics Rates in the United States 1940-1960 Table 71 (R. Grove & A. Hetzel eds. 1968), U.S. Dep't of Commerce, Survey of Current Business 10 (Aug. 1966) (Table 1), and U.S. Dep't of Commerce, Survey of Current Business 3 (Aug. 1971) (Table 2); *Columns 9 & 10*—calculated from General Social and Economic Characteristics: 1960, *supra*, Table 5-3; *Column 11*—Bureau of the Census, U.S. Dep't of Commerce, Data by States, 1960, at 27 (1961) (Table 15); *Column 12*—*id.* at 29-30 (Table 16).

31. F. Zimring & G. Hawkins, *supra* note 1, at 266.
32. Amicus Brief, *supra* note 5, at 37-38.
33. *Id. But see* Ehrlich 1975, *supra* note 1, at 411.

the fraction of the population in the age group 14 to 24, and estimated per capita income.[34] The regression analysis applied to these variables yields an algebraic equation which gives a numerical estimate of the effect of each explanatory variable on the homicide rate. In this sense, Ehrlich's study accounts more precisely for the influence of this set of factors than does Sellin's study.

This precision, however, is misleading. In order for the statistical results to be reliable, the equation must include all variables which significantly affect the homicide rate. The omission of any significant variable not only renders the model incapable of fully explaining the behavior of the homicide rate, but distorts the effects of those variables which have been included.[35] The regression method is therefore best suited to testing a hypothesis based on a well-developed theory which isolates a few determinants of the variable under study.

Hypotheses about the causes of murder cannot rely on such a theory. Ehrlich's analysis relies on an economic postulate—"that the propensity to perpetrate such crimes [as murder] is influenced by the prospective gains and losses associated with their commission."[36] There is no reason to think that economics or any other discipline has yet identified the determinants of the murder rate with enough confidence to rely on results obtained from regression analysis. Indeed, there are strong a priori reasons for thinking that the murder rate would be influenced by a number of variables not considered by Ehrlich, such as rates of migration from rural to urban areas, per capita ownership of weapons, and the level of violent crimes against property. As Zimring and Hawkins point out in their discussion of deterrence:

> Very few, if any, studies done on the impact of criminal law variations on crime give us reason to believe that most of the many factors which should be included in such a statistical analysis are present and accounted for.
>
> . . . .
>
> . . . Only when the statistical complexity of such methods lulls the researcher into a false sense that all relevant variables have been accounted for, or that natural variations are in fact present, does multiple-correlation analysis become more dangerous than helpful.[37]

Our a priori skepticism about the adequacy of the variables included in Ehrlich's model is supported by the results of reanalyses of his data, which we discuss below.[38] These reanalyses found that the sign of the elasticity for execution risk changes from positive to negative when the recent years are dropped from Ehrlich's data series. The apparent change in the effect of execution risk on the homicide rate indicates that the variables included by Ehrlich do not explain the behavior of the homicide rate in a consistent manner over subperiods in his sample and suggests that variables not included by Ehrlich may be necessary for a better explanation.

Regression analysis requires the assumption not only that the homicide rate is a function of a fixed set of variables, but also that the function has a particular mathematical form. Ehrlich postulated that the homicide rate was equal to the product of seven explanatory variables and a random error term. The equation which Ehrlich actually estimates is a particular characterization of this relationship in which the logarithm of the homicide rate is set equal to the weighted sum of the logarithms of the explanatory variables. The weights provide estimates of the effect of each explanatory variable on the homicide rate. Since other investigators who have performed Ehrlich's regression analysis using natural numbers rather than logarithms have found no evidence of a deterrent effect, Ehrlich's results seem to depend on his assumptions about the mathematical form of the relationship.[39]

Even assuming that Ehrlich's regression equation successfully isolated the true determinants of the murder function and correctly specified its mathematical form, a serious problem remains with Ehrlich's use of the equation to estimate the tradeoff between executions and murders. Ehrlich measures this tradeoff by the partial elasticity of the homicide rate with respect to the execution rate—the percent decrease in homicide rate produced by a one percent increase in execution risk, assuming that the other variables affecting the murder rate are held constant as execution risk varies. The estimated elasticity of −.065 im-

34. Ehrlich 1975, *supra* note 2, at 398-402 (theoretical discussion of factors influencing murder), 406-09 (empirical discussion of data chosen to measure those factors). In substituting the measured for the true homicide rate, Ehrlich inserts a time trend as an explanatory variable. *Id.* at 406-07.

35. The extent to which the regression analysis explains the behavior of the homicide rate is measured by the coefficient of determination, which is not less than zero and not greater than one. A regression which omits a relevant variable will have a lower coefficient of determination than a regression which correctly specifies the relationship. The omission of important variables from the regression model will also bias the results obtained for the variables which are included. *See* J. JOHNSTON, ECONOMETRIC METHODS 169 (2d ed. 1972) ("*exclusion* of relevant variables from the regression may be a very serious error . . . .") (emphasis in original), 211 (omitted variables may cause the statistical problem of serial correlation in the disturbance term).

36. Ehrlich 1975, *supra* note 2, at 398-99. His other "basic proposition" is that those [illegible] involving pecuniary

37. F. ZIMRING & G. HAWKINS, *supra* note 1, at 267-68. Multiple correlation analysis is closely related to, but less sophisticated than, the multiple regression technique used by Ehrlich. *See* R. WONNACOTT & T. WONNACOTT, *supra* note 21, at 103-30; note 14 *supra*.

38. Pp. 181-85 *infra*.

39. *See id.*

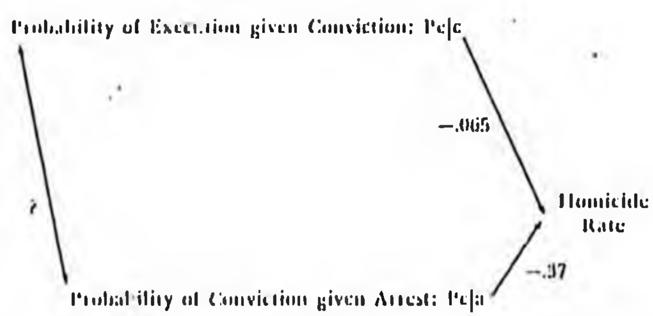
plies that at the average levels of executions and murders during the period studied, an additional execution would result in seven or eight fewer murders.[40] Yet, as Ehrlich recognizes, the conviction rate is likely to decline as execution risk increases, because juries seem less inclined to convict a defendant charged with murder the greater the chance the defendant will subsequently be executed.[41] Since in Ehrlich's model the conviction rate also has a deterrent effect, the effect of the decline in the conviction rate will tend to offset the effect of the increase in execution risk. Indeed, if a one percent increase in execution risk produces more than a .175 percent decrease in the conviction rate, then the effect on the homicide rate of the decrease in the conviction rate will outweigh the effect of the increase in execution risk. As a result, the increase in execution risk will cause a net increase, rather than a decrease, in the homicide rate.[42] In re-

40. Ehrlich 1975, *supra* note 2, at 414 & n.15.

41. *Id.* at 405. In estimating his equations, Ehrlich used a two-stage procedure designed to take account of the interdependence of the law enforcement variables. Ehrlich 1973, *supra* note 2, at 50-60 (statistical appendix). We note that Ehrlich's second stage regression does not hold fixed the four socioeconomic variables which he uses in the first stage but which he does not explicitly include in his murder supply function. Ehrlich 1975, *supra* note 2, at 409 (variables denoted X_4 in Table 2—fraction of nonwhites in residential population, civilian population, per capita real expenditures of all governments, per capita real expenditures on police). Our fundamental criticism, however, is that in using those estimates to calculate the tradeoff between murders and executions, he neglected the interdependence.

42. We owe this point to P. Passell & J. Taylor, The Deterrent Effect of Capital Punishment: Another View, Feb. 1975, at 9-11 (Discussion Paper 74-7509, Columbia Univ. Dep't of Economics) (on file with *Yale Law Journal*). Figure 1 depicts the net effect of the increase in execution risk Pe|c.

Figure 1



The arrows represent causal influences, and the numbers accompanying the arrows represent the elasticities reported by Ehrlich in Equation 3 of his Table 4. Ehrlich 1975, *supra* note 2, at 410. The question mark represents the elasticity of the probability of conviction given arrest, Pc|a, with respect to the probability of execution given conviction, Pe|c. Given the reluctance of juries to convict where execution may follow, this elasticity is expected to be negative, *i.e.*, as Pe|c increases, Pc|a decreases. According to these elasticities, a decline of one percent in the probability of conviction would be associated with a .065 percent increase in the homicide rate. But that same one percent

porting his estimated tradeoffs between executions and murders, Ehrlich conceded that "[t]he actual tradeoffs . . . depend partly upon the ability of law enforcement agencies to control simultaneously all [law enforcement variables]"—in other words, to hold arrest and conviction rates constant as execution risk increases.[43] Moreover, in performing another regression which eliminated this problem of interaction between execution risk and the conviction rate, he found smaller, although in most cases still significant, deterrent effects.[44]

Given the difficulty of isolating the determinants of the homicide rate, specifying their mathematical form, and adjusting for the interaction between execution risk and the other law enforcement variables, it is not at all clear that Ehrlich's analysis is more reliable than Sellin's. It is certainly not true that Ehrlich's study of the deterrent effect of the death penalty is uniquely successful in holding other factors constant. The problems are not that simple, nor Ehrlich's approach that satisfactory.

IV. Replication and Corroboration by Other Studies

As we have shown, both matching and regression techniques are necessarily imperfect methods for testing the deterrent effect of capital punishment. Given these inherent imperfections in research technique, the credibility attaching to each study depends on the extent to which consistent results are obtained when a similar approach is applied to different data. A crucial test of the reliability of the results is whether they can be reproduced or corroborated by other studies.

Sellin consistently found no discernible effect of capital punishment on homicide rates in matching a large number of clusters of states. At least four other investigators have used the matching method on different data and reached similar conclusions.[45] Moreover, Sellin's re-

decline in the probability of execution given conviction would lead to some unknown (X percent) increase in the probability of conviction given arrest. And this increase would lead in turn to a decrease of .37 × X percent in the homicide rate, compensating more or less for the increase in the homicide rate resulting directly from the decline in the probability of execution given conviction. In particular, if the elasticity of Pc|a with respect to Pe|c is .175 percent, the result from this causal chain will be a .065 percent (.175 × .37) decrease in the homicide rate. This decrease will exactly offset the .065 percent increase in the homicide rate resulting from the decline in the probability of execution given conviction.

43. Ehrlich 1975, *supra* note 2, at 415.

44. *Id.* at 415 n.16. *But see* P. Passell & J. Taylor, *supra* note 12, at 10-11 (arguing these results are unreliable).

45. W. Bowers, *supra* note 1, at 137-47 (homicide rates in abolitionist states and contiguous retentionist states for four years prior and subsequent to the 1967 judicial moratorium on capital punishment); Bailey, *Murder and the Death Penalty*, 65 J. Crim.

sults are independently corroborated by his comparisons of crime rates in particular jurisdictions before and after capital punishment was either adopted or repealed.[46] These longitudinal studies have imperfections of their own, but as Zimring and Hawkins point out, "the combination of two or more imperfect research approaches may reveal a relatively clear picture about the relation of the variables being studied to rates of crime."[47]

Ehrlich also recognized the need for the broadest possible empirical base for his conclusions. He analyzed his data with six different measures for the key explanatory variable—execution risk—and obtained similar results.[48] He indicated concern that his analysis not be unduly sensitive to changes in the time period to which it was applied, to minor modifications in the selection or computation of variables included in the analysis, or to the use of natural values rather than logarithms as a functional form.[49] Reapplying the statistical analysis to data spanning shorter time periods, he reported results generally consistent, if not in perfect agreement, with his basic conclusions.[50]

However, the efforts of Passell and Taylor to reproduce Ehrlich's results using identical estimation procedures have yielded significant discrepancies, apparently due to minor differences in the data on which the replication was based.[51] These discrepancies necessarily call into

1. & Crim'y 116, 121 (1971) (eight groups of states); Schuessler, *The Deterrent Influence of the Death Penalty*, 284 Annals 54, 58 (1952) (five groups); Sutherland, *Murder and the Death Penalty*, 15 J. Crim. L. & Crim'y 522, 526 (1925) (one group of states and one group of cities).

46. The Death Penalty, *supra* note 1, at 34-38 (American states), 38-50 (foreign countries). From the behavior of homicide rates before and after the change in punishment policy, Sellin concluded that the death penalty "exercises no influence on the extent or fluctuating rates of capital crimes." *Id.* at 63. Other longitudinal studies confirm Sellin's results. W. Bowers, *supra* note 1, at 147-57 (comparing homicide rates in states changing from mandatory to discretionary capital punishment); Fattah, *The Canadian Experiment with Abolition of the Death Penalty*, in W. Bowers, *supra* at 121; Samuelson, *Why was Capital Punishment Restored in Delaware?*, 60 J. Crim. L.C. & P.S. 148, 149 (1969); Schuessler, *supra* note 45, at 58-59.

47. F. Zimring & G. Hawkins, *supra* note 1, at 270.

48. Ehrlich 1975, *supra* note 2, at 407-08 (definition of the six measures of execution risk), 410-11 (regression results for each measure).

49. *Id.* at 412-13.

50. *Id.* at 410 (Table 1), 413 ("[T]he qualitative results . . . are for the most part insensitive to changes in the specific interval of time . . . . However, the absolute magnitudes of some of the estimated elasticities . . . do change when estimated from different subperiods.")

51. P. Passell & J. Taylor, *supra* note 12, at 2-4. Ehrlich has provided complete documentation of the data sources for his study in a memorandum released in August, 1975, and dated May, 1975. The Deterrent Effect of Capital Punishment: A Question of Life and Death, American Economic Review (June, 1975): Sources of Data (on file with *Yale Law Journal*). This documentation was not available at the time of the replications by Passell and Taylor. Consequently these authors were forced to reconstruct parts of the data base using whatever procedures seemed most appropriate.

question the reliability of Ehrlich's conclusions. More importantly, further analysis of Ehrlich's data by Passell and Taylor, and later by Bowers and Pierce, indicates that the evidence of a deterrent effect reported by Ehrlich disappears when the model is estimated with natural numbers rather than logarithms or when it is estimated for shorter time periods which exclude the recent years from 1963 to 1969.[52] Furthermore, Ehrlich's model does not explain the homicide rate as well over the long run (1935-1969) as it does in shorter periods, for which it reveals no significant relationship between execution risk and the homicide rate. If a model correctly explains a set of relationships, it will not decrease in explanatory power as more data (years, in the present context) are brought into the analysis. Ehrlich's model has just the opposite property—showing less predictive power over the long run than over the short run. A final piece of evidence on the issue of corroboration is a recent regression study, based on Ehrlich's theoretical model but using 1950 and 1960 data for more than 40 states, which found no deterrent effects associated with execution risk.[53]

Conclusion

It is quite possible that because of the complexity of the social phenomenon involved, we will never know with certainty whether capital punishment does or does not deter murder. Statistical analyses can only test with the available data the hypothesis that a significant deterrent effect exists. On the basis of the work of Sellin and others who have taken his approach, we are inclined to attach more credibility to their view that capital punishment does not have a significant deterrent effect. The credibility we assign to this hypothesis is based upon our confidence in Sellin's choice of a variable to measure the threat of capital punishment, in his use of the state rather than the nation as his unit of observation, his technique for controlling for the influence of other variables which affect the homicide rate, and in the consistent results which he and others have produced applying these methods to different time periods and different jurisdictions.

estimated permanent income and labor force participation as well as particular values from the police expenditure and conviction rate series which Ehrlich had estimated by an unspecified process of interpolation. P. Passell & J. Taylor, *supra* at 3.

52. P. Passell & J. Taylor, *supra* note 12, at 4-8; Bowers & Pierce, *The Illusion of Deterrence in Isaac Ehrlich's Research on Capital Punishment*, 85 Yale L.J. 187 (1975). Ehrlich reported that his "regression results [were] found to be robust with respect to the functional form of the regression equation," but offered no results in support of this statement. Ehrlich 1975, *supra* note 2, at 412.

53. Passell, *The Deterrent Effect of the Death Penalty: A Statistical Test*, 28 Stan.

Given this substantial body of competent research, we are unwilling to abandon the view that it supports on the basis of Ehrlich's single study. Ehrlich's study relies on a measure of the death penalty threat which does not reflect the relationship between executions and murders in specific jurisdictions and which does not focus on the relevant policy question of the effect of abolition. Moreover, Ehrlich's estimated tradeoff rests on the highly doubtful assumptions (1) that the probability of conviction could be kept constant while the probability of execution varies, and (2) that the equation used to control for the effects of variables other than execution risk combines all the significant determinants of the homicide rate in the proper mathematical form.

The use of the Sellin and Ehrlich studies in the context of a constitutional challenge to capital punishment illustrates the need for judicial procedures to evaluate statistical analysis presented by litigants in support of their positions. There is a certain danger in relying on academic work, designed to promote inquiry and further research, as a basis for deciding disputes in a court of law—especially where the stakes involved are high and the implications for society are great. The courts presently do not provide systematic factfinding procedures to resolve issues of "legislative" fact[51] that are critical to the policy judgments courts must make. Until the courts develop procedures to bring complex statistical studies under the scrutiny of the adversary process, it will be necessary to carry on the technical debate over such legislative facts largely in the law reviews.

51. K. Davis, Administrative Law Text § 7.03, at 161 (3d ed. 1972).

The Illusion of Deterrence in Isaac Ehrlich's Research on Capital Punishment*

William J. Bowers† and Glenn L. Pierce††

In this critique of Professor Ehrlich's recent research on capital punishment,[1] we conclude that he has failed to provide any reliable evidence that the death penalty deters murder. His data are inadequate for the purposes of his analysis and he misapplies the highly sophisticated statistical techniques he employs. We begin with an evaluation of the data he uses to measure the critical variables in his theoretical formulation and then consider flaws in his analysis which would invalidate his conclusions even if his data were adequate. We conclude by explaining how Ehrlich's analysis produces results which seem consistent with the deterrence hypothesis when in fact they are not.[2]

I. Inadequacies in Ehrlich's Data

The credibility of Ehrlich's conclusions depends on the quality of the data he has used. For measures of the variables at the core of his theoretical analysis, he relies on the Uniform Crime Reporting System (UCRS) of the FBI.[3] The behavior he seeks to explain (the dependent

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† Director, Center for Applied Social Research, Northeastern University.
†† Research Assistant, Center for Applied Social Research, Northeastern University.

1. Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life and Death*, 65 Am. Econ. Rev. 397 (1975) [hereinafter cited as Ehrlich 1975]; I. Ehrlich, The Deterrent Effect of Capital Punishment: A Question of Life and Death, 1973 (Working Paper No. 18, Center for Economic Analysis of Human Behavior and Social Institutions) [hereinafter cited as Ehrlich 1973].

2. The findings presented here are drawn from a more extensive and detailed critique of Ehrlich's work by the present authors. Deterrence, Brutalization, or Nonsense, 1975 (unpublished manuscript, Center for Applied Social Research, Northeastern Univ.). This manuscript includes as appendixes the full regression results for equations used to estimate coefficients in the present paper [illegible] *Yale Law Journal*) and a complete listing of the data values [illegible] analysis [illegible]

variable) is the annual criminal homicide rate for the United States as reported by the UCRS, and his deterrence variables are the rates of arrest, conviction, and execution for homicide, which also come entirely or in part from the UCRS. Only if these data are sound throughout the full time period covered by Ehrlich's analysis do his findings deserve serious consideration.

A. *The Dependent Variable*

The FBI's national homicide statistics collected in the early years of the UCRS are unreliable. A staff report of the National Commission on the Causes and Prevention of Violence emphasizes this problem:

> [M]any reporting agencies, especially in the nonurban areas, were slow in joining the *UCR* network; there were only 400 agencies reporting to the *UCR* in the 1930's, while today there are about 8,500. Thus, trends of both violent and nonviolent crimes during the early years of the *UCR* are highly questionable as representative of national figures.[4]

Furthermore, the President's Commission on Law Enforcement and the Administration of Justice warns that "figures prior to 1958, and particularly those prior to 1940, must be viewed as neither fully comparable with nor nearly so reliable as later figures."[5]

Ehrlich indicates that he used "readjusted" estimates of the homicide rate supplied by the FBI.[6] The FBI has periodically adjusted their estimates of offenses for earlier years on the basis of *recent* data on offenses from jurisdictions that entered the reporting program after 1958. Yet to our knowledge there are no published indications of how the readjustments are performed. In any case, the adjustment of figures for as long ago as 40 years on the basis of the current homicide levels of agencies recently added to the sample is of dubious value.

Less problematic are the willful homicide figures compiled by the Bureau of the Census. Unlike the voluntary reporting system of the FBI, Census reports of willful homicide are mandated by law in each state. The annual collection of mortality statistics including willful homicide began in 1900, 30 years before the beginning of the FBI reporting system. By 1933, all states had met the 90 percent coverage

4. [illegible] Staff of the Nat'l Comm'n on the Causes and Prevention of Violence, Crimes of [illegible]

[illegible] Law Enforcement and Administration of Justice,

requirement for admission to the national Vital Statistics program.[7] Thus, the Census homicide statistics for the nation have been relatively complete since the early 1930's.[8] Furthermore, the classification of "willful homicide" has remained essentially constant over time.[9] For these reasons, perhaps, the Census homicide figures have gained a reputation for reliability, and have been used more widely than the FBI figures in previous studies of the deterrent effects of capital punishment.[10]

If both FBI and Census data provided accurate estimates of the homicide rate, the statistics would, of course, agree. Table I[11] shows that the figures drawn from the two agencies are reasonably well correlated except during the 1930's, when the FBI's reporting system was in its inception. Notably, the FBI homicide estimates are 15 percent below the Census figures for the 1930's, whereas the difference is only about three percent lower for the period after 1940. By all indications, these discrepancies are the result of inadequate sampling, reporting, and estimating in the early years of the UCRS.

7. 1 U.S. Public Health Serv., Dep't of Health, Education, and Welfare, Vital Statistics of the United States 1950, at 29 (1954).

8. Tests of the completeness of birth registrations made in 1940 and 1950 indicated that these statistics were, respectively, 92.5 and 97.9 percent complete. Although precise studies of the completeness of death registrations are not available for this period, the compilers of the Vital Statistics believe that they are even more complete than birth registrations. *Id.*

9. The Department of Health, Education, and Welfare reports:

> Since 1900, the causes of death have been classified according to seven different revisions of the International Classification of Diseases. Each revision has produced some breaks in the comparability of cause-of-death statistics. However, homicide is among the causes for which the classifications are essentially comparable for all revisions.

National Center for Health Statistics, U.S. Dep't of Health, Education, and Welfare, Homicide in the United States 1950-1964, at 9 (1967).

10. *E.g.*, T. Sellin, The Death Penalty (ALI 1959); Schuessler, *The Deterrent Influence of the Death Penalty*, 284 Annals 54 (1952).

11. TABLE I

Correlations Between Homicide Rates Based on FBI and Census Data by Decade

Effective Period	*Annual Homicide Rates*	*Year to Year Changes in Homicide Rates*
1933-1939	.24	−.69
1940-1949	.81	.96
1950-1959	.95	.76
1960-1969	.98	.79

For data sources, see Appendix, nos. 1, 13.

A correlation of .24 between FBI and Census annual homicide rates for the 1930's means that there is little consistency (only six percent common variance) between the two data sets in this decade. A correlation of −.69 between year to year changes in FBI and Census homicide rates means that an increase between adjacent years in one set [illegible] years

B. *The Deterrence Variables*

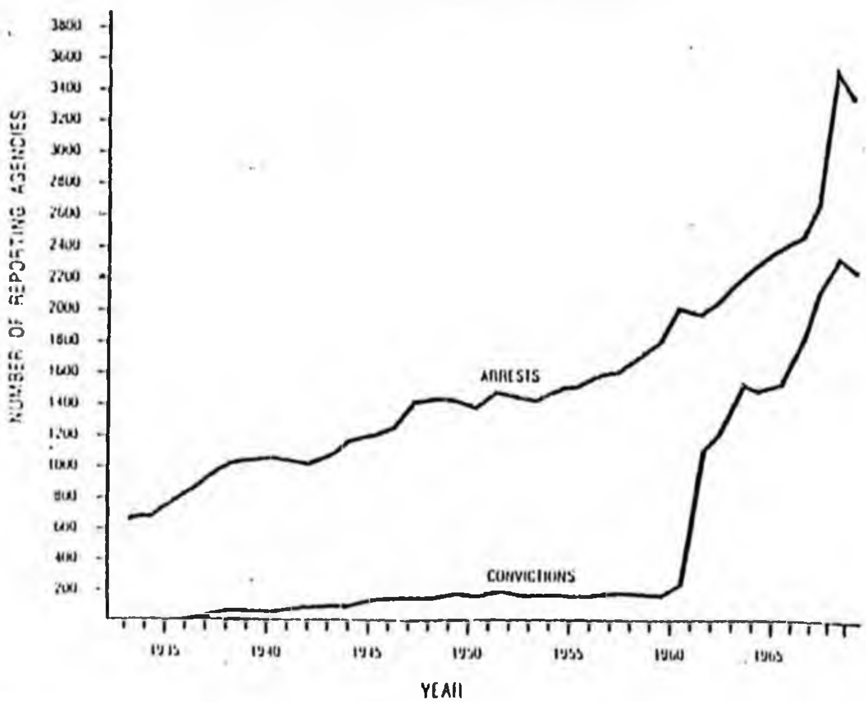
The FBI data on arrest and conviction rates are even less reliable. The agencies reporting arrest and conviction statistics have remained a relatively small, self-selected subsample throughout most of the period during which these statistics have been compiled. Indeed, the arrest and conviction figures are based on such small and unrepresentative samples of law enforcement agencies that the FBI has made no effort to readjust earlier arrest and conviction figures on the basis of more recent returns.

As shown in Figure 1,[12] the number of agencies reporting arrest data did not reach 2,000, or about one-quarter of the total number of agencies, until the 1960's, and the number of agencies reporting convictions did not exceed 300 until the 1960's. The abrupt increase between 1960 and 1961 in agencies reporting convictions represents a major change in reporting practices for conviction statistics by the UCRS. Notably, in 1936, the first year in which conviction rates were

12.

Figure 1

NUMBER OF AGENCIES REPORTING ARREST AND CONVICTION STATISTICS BY YEAR



For data sources, see Appendix, nos. 2, 3

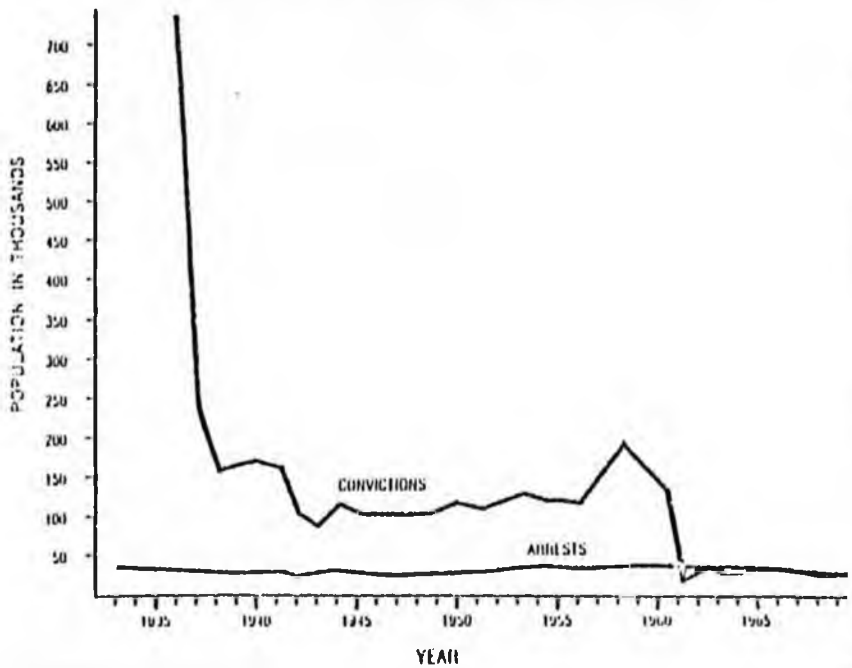
reported, the figure Ehrlich used as a national estimate was based on only 13 jurisdictions.

Figure 2[13] shows that the average size of jurisdictions reporting conviction data declined substantially in the late 1930's and the early 1960's. According to recent evidence, conviction rates are relatively low in the nation's largest jurisdictions.[14] Hence the conviction data drawn from disproportionately large jurisdictions in 1936 and 1937 are apt to underestimate the national conviction levels for these years. In fact, the reported conviction levels for these two years are far below those for other years—respectively −5.91 and −4.51 standard deviations below the mean conviction level for the period 1938-1969. Because the 1936 and 1937 conviction levels figure prominently in Ehrlich's estimation of missing conviction values for the years 1933-1935, the conviction rates for the entire period 1933-1937 are apt to be grossly biased in his analysis.[15]

13.

Figure 2

POPULATION COVERAGE PER AGENCY REPORTING ARREST AND CONVICTION STATISTICS BY YEAR



For data sources, see Appendix, nos. 2, 3

14. In 1974, only 31.9 percent of those charged with homicide in jurisdictions with populations of 250,000 or more were convicted, as compared to approximately 50 percent in smaller jurisdictions. Unpublished FBI data (on file with *Yale Law Journal*).

15. [illegible] May 1975 memorandum prepared by Ehrlich (The Deterrent Effect of Capital

Finally, the measurement of execution risk—the key explanatory (or independent) variable in Ehrlich's work—is confounded by the inadequacies in the homicide, arrest, and conviction data, because execution risk, as defined by Ehrlich, incorporates all three of these variables.[16] Thus, like his dependent variable, all three of his deterrence variables are subject to potentially serious measurement error. While we do not contend that all of Ehrlich's data are inaccurate, we have identified substantial problems with his core variables which cast doubt on his ability to perform a meaningful regression analysis.

II. Errors in Ehrlich's Regression Analysis

We have independently applied Ehrlich's regression technique to comparable data. On the basis of this replication, we find that his evidence of deterrence emerges only under restrictive assumptions about

Sources of Data (on file with *Yale Law Journal*)) supplies the auxiliary equations he used to estimate the missing conviction values for 1933 through 1935. To show the importance of the effect of the values for 1936 and 1937 on the conviction estimates Ehrlich obtains for the missing years, we have estimated conviction rates for the years 1933-1937 on the basis of the data for the 1938-1969 period with the auxiliary equation used by Ehrlich to estimate the years 1933-1935 for the data from the 1936-1969 period. It is instructive to compare our estimated conviction rates with the corresponding values used by Ehrlich:

	Ehrlich's Conviction Rates		*Alternative Estimates*	
	Annual Conviction Estimates	*Standard Deviations from the 1938-1969 Mean*	*Annual Conviction Estimates*	*Standard Deviations from the 1938-1969 Mean*
1937	30.3	−4.54	43.7	−.40
1936	25.8	−5.91	44.1	−.29
1935	28.3	−5.18	44.1	−.26
1934	26.9	−5.59	42.8	−.69
1933	30.2	−4.58	43.3	−.53

We have already noted that the reported conviction rates for 1936 and 1937 are far below the mean for the 1938-1969 period. The above comparison also makes clear that these reported rates are far below the values that would be estimated for these two years by applying Ehrlich's auxiliary equation to the data for the 1938-1969 period. Furthermore, Ehrlich's estimated conviction rates for 1933-1935 are also four to five standard deviations below the mean for 1938-1969; as the alternative estimates show, they would be far higher if they were estimated on the basis of the 1938-1969 time period rather than the 1936-1969 time period.

16. Ehrlich 1975, *supra* note 1, at 401. All of his measures of execution risk are in various ways and degrees biased negatively with respect to the criminal homicide rate. Thus, random error in the number of homicides reported will tend to produce a negative correlation between the homicide rate and execution risk as measured by PXQ_1 and PXQ_2, because the number of reported homicides in a given year, Q_t, is both the numerator of the homicide rate and a component in the denominator of these two execution measures. *See* Appendix, nos. 1, 4. The remaining four measures of execution risk will tend to be negatively correlated with the homicide rate as a result of autocorrelated error in the reported number of homicides, because these measures all incorporate prior (lagged) homicide levels, in one way or another.

the form of relationships among the variables and only under a narrow selection of the time period for analysis. The limitations required to obtain these results are not justified, since Ehrlich's regression model fits the data better without them. Thus, even if Ehrlich's data were free of errors, the analysis, when prope[illegible]onducted, would not show that the death penalty has a deterrent [illegible]fect.

A. *Replication of Data and Regression Results*

To ensure comparability in the replication of Ehrlich's regression analysis, we attempted to use exactly the same data as Ehrlich. Table II[17] describes Ehrlich's and our variables and gives the means and

17.

Table II

Variables Used in the Regression Analysis: Annual Observations 1933-1969
(Means and Standard Deviations in Natural Logarithms)

	Variable		*Our Mean*	*Ehrlich's Mean*	*Our Standard Deviation*	*Ehrlich's Standard Deviation*
Y_1	$\left(\frac{Q}{N}\right)^*$	= Crime rate: offenses known per 1,000 civilian population.	−2.853	−2.857	.150	.156
X_1	P^*a	= Probability of arrest: clearance rates.	4.497	4.997	.038	.038
	$P^*c\|a$	= Conditional probability of conviction: fraction of those charged who were convicted of murder.	3.712	3.711	.172	.175
	$P^*e\|c$	= Conditional probability of execution. PXQ_1 = the number of executions for murder in year t + 1 as a percent of the total number of convictions in year t.	.172	.176	1.718	1.719
X_2	L	= Labor force participation rate of the civilian population.	−.511	−.516	.029	.030
	U	= Unemployment rate of the civilian labor force.	1.743	1.713	.728	.728
	A	= Fraction of residential population in the age group 14-24.	−1.759	−1.710	.122	.118
	Y	= Friedman's estimate of permanent income per capita.	6.889	6.868	.337	.338
	T	= Chronological time (years).	2.685	2.685	.867	.867
X_3	NW	= Percent of nonwhite residential population.	−2.216	−2.212	.061	.063
	N	= Civilian population in 1,000's.	11.911	11.911	.161	.161
	XGOV	= Per capita (real) expenditures on all governments in million dollars.	−7.753	−7.661	.256	.501
	$XPOL_{-1}$	= Per capita (real) expenditures on police in dollars lagged one year.	2.200	2.111	.116	.306

For data sources, see Appendix and Ehrlich 1975, *supra* note 1, at 409. In his memorandum, *supra* note 15, Ehrlich indicated that the mean of P^*a is incorrectly stated in his table, and standard deviations of their logarithms. In most cases, our means and standard deviations correspond quite closely to Ehrlich's. For 11 of the 13 variables, they differ by no more than three percent, and generally by less than one percent. Differences of two or three percent may indicate that where alternates were available we chose a different data source than Ehrlich did. The two variables that show discrepancies of greater than three percent between the two data sets—the indexes of total per capita expenditures of governments ("XGOV") and of per capita expenditures on police ("$XPOL_{-1}$")—differ primarily in standard deviations.[18] In all cases where discrepancies exist, however,

18. Ehrlich's memorandum, *supra* note 15, describes the procedures that he used in obtaining his variables. For XGOV and $XPOL_{-1}$, the two variables with significant differences between the two data sets, we believe that our measures more faithfully reflect Ehrlich's definitions of the variables. Although Ehrlich described XGOV as per capita expenditures of local, state, and federal governments, his memorandum indicates that he actually used government purchases of goods and services and a price deflator for government purchases, instead of government expenditures and an appropriate price deflator. His memorandum also indicates that he failed to exclude defense purchases or expenditures. This is a serious oversight since defense expenditures and purchases represent resources not available for law enforcement activity. Our measure is based on government expenditures and excludes defense expenditures and purchases.

It was virtually impossible to replicate $XPOL_{-1}$ exactly because Ehrlich used an unspecified auxiliary regression equation to estimate unavailable police expenditure data for odd years prior to 1952. Furthermore, his memorandum indicates that he used a price deflator for government purchases rather than a price deflator for government expenditures, as we did.

We constructed two other variables differently than Ehrlich did. While "A" is described as the proportion of the residential population aged 14-24, Ehrlich's memorandum reveals that he used the number of 14- to 24-year olds in both the residential population and the armed forces overseas as a proportion of the total residential population. This is clearly inappropriate since youngsters in this age group overseas cannot contribute to the domestic homicide rate. The slightly greater standard deviation of our measure based exclusively on residential population figures undoubtedly reflects movements of this age group in and out of the country during the war years.

Secondly, "NW" is described as the proportion of nonwhites in the residential population, but Ehrlich's memorandum indicates that he took the number of nonwhites in the total population as a proportion of all those in the residential population. Moreover, he used annual estimates of the nonwhite population from the *Current Population Reports* for the 1960's (see Appendix, no. 1), instead of readjusted estimates based on the 1970 decennial censuses. Again, our measure, based exclusively on residential population figures and readjusted annual estimates, is a more accurate representation of the variable as originally defined.

For further evidence of comparability between the two data sets, note the values of corresponding correlation coefficients for the period 1934-1969:

	Ehrlich's Correlations	*Our Correlations*
PXQ_1 by $P^\circ a$	−.028	−.029
PXQ_1 by $P^\circ c\|a$	−.19	−.181
PXQ_1 by $\left(\frac{Q}{N}\right)^*$	.140	.133
PXQ_2 by $\left(\frac{Q}{N}\right)^*$	.085	.077
[illegible] $\left(\frac{Q}{N}\right)^*$	.096	.077

we have checked our data carefully, and we are satisfied that we have accurate measures of the variables.

With these data, we have reproduced Ehrlich's basic regression analysis. Table III[19] contains the estimated effects of six different measures of execution risk on the criminal homicide rate. The effects are represented by partial regression coefficients (or elasticities[20]), and their statistical significance is indicated by the ratio of these coefficients to their standard errors—the *t* values of the coefficients. (*t* values of more than 2.0 indicate statistically significant relationships between the dependent and independent variables.)[21]

The six measures of execution risk are alternative ways of representing the conditional probability of execution given conviction for murder. In five of the six measures, Ehrlich incorporates a delay between conviction and execution by dividing the number of executions in one year by the estimated number of convictions in the previous year.[22] In two cases,[23] he estimates execution risk at a given point in time in terms of the numbers of executions and convictions over a prior period of three or four years.[24]

19.

Table III

Estimated Effects of Execution Risk on the Criminal Homicide Rate

Six Alternative Measures of Execution Risk	*Effective Period*	*Partial Regression Coefficients*	*t Values*
PXQ_1	1935-1969	−.018	−.39
PXQ_2	1935-1969	−.068	−3.15
PXQ_{1-1}	1936-1969	−.023	−1.12
TXQ_1	1938-1969	−.059	−2.76
PDL_2	1939-1969	−.065	−3.45
PXQ_3	1935-1969	−.001	−.113

The definitions of these variables are contained in Ehrlich 1975, *supra* note 1, at 406-09.

20. When the execution and homicide variables are in logarithmic form, *see* pp. 199-200 *infra*, the partial regression coefficients indicate the elasticity of the homicide rate with respect to execution risk—that is, the percentage change in the homicide rate that can be expected from a one percent change in execution risk. Thus, an elasticity of −.068 (associated with PXQ_2) means that a one percent increase in this measure of execution risk can be expected to yield a decrease of .068 percent in the homicide rate.

21. A *t* value of 2.0 is generally taken as an indication of statistical significance because if the true value of the regression coefficient were actually zero, an estimated regression coefficient with a *t* value greater than 2.0 would only occur approximately five times out of 100. A *t* value greater than 2.0 is required for this level of statistical significance when the number of data observations exceeds the number of explanatory variables by five or less.

22. PXQ_2 does not incorporate such a delay.

23. TXQ_1 and PDL_2.

24. Two of Ehrlich's regression equations—PXQ_{1-1} and TXQ_1 in his Table 3, Ehrlich 1975, *supra* note 1, at 410—appear to be either misspecified or mislabeled in terms of the effective period of analysis. Given his data and analytic procedures, 1935 is the earliest possible beginning date for a regression analysis using any of the measures of execution

Table III shows a negative value for the regression coefficient associated with each of the six measures of execution risk. That is, the results of this initial regression analysis appear to indicate that, other things being equal, as the risk of execution among convicted offenders increases, the homicide rate decreases and, conversely, as execution risk declines, the homicide rate rises. For three of the six measures of execution risk, the estimated effect is at least twice its standard error, suggesting that the effect is not likely to have occurred by chance.

These results are similar to Ehrlich's.[25] He finds negative coefficients, ranging from −.039 to −.068, for the six execution measures. In four cases, his negative coefficients are statistically significant. In

tained prior to 1934 since values of many variables lagged one year are required by the reduced form equation. The modified first differences obtained in the second stage of the regression analysis by the Cochrane-Orcutt procedure cannot be obtained prior to 1935 since all exogenous and endogenous variables must be lagged one year. But with PXQ_{t-1} as the measure of execution risk (*see id.* at 406-09), the earliest starting date is 1936. The reduced form first stage equation requires that a lagged value of PXQ_{t-1} be used to estimate arrest and conviction rates, but the first legitimate value of PXQ_{t-1} is that for 1934, not for 1933, since the denominator of this measure incorporates values of homicides, arrests, and convictions lagged one year. This means that estimated arrest and conviction rates cannot be obtained before 1935 and that modified first differences cannot be estimated for periods beginning before 1936. Since Ehrlich gives 1935 as the beginning of the effective period, he may have used an erroneous (probably *zero*) value for PXQ_{t-1} in the first stage estimation procedure.

With TXQ_t as the measure of execution risk (*see id.* at 406-09), the earliest beginning point for the effective period should be 1938. Since values of TXQ_t depend on data from three prior years, the first values cannot be obtained before 1936; the first stage estimation of arrest and conviction rates with lagged TXQ_t cannot be made for years earlier than 1937; and hence modified first differences cannot be calculated for effective periods starting earlier than 1938. If, as Ehrlich indicates, 1937 is actually used as the beginning date of the effective period, lagged TXQ_t in the first stage equation will be an arbitrary (probably nonzero) value based on data from only two prior years (and a zero value for the third year).

PXQ_{t-1} and TXQ_t are used more extensively than any of the other execution measures in Ehrlich's regression analyses, and in virtually all cases the effective period of analysis begins one year too soon. The following equations in both of his papers would appear to be misspecified, and therefore improperly estimated, in terms of the effective period of analysis: equations 3 and 4 in Table 4 of Ehrlich 1973, *supra* note 1, at 53, and Table 3 of Ehrlich 1975, *supra* note 1, at 410; equations 2-5 in Table 5 of Ehrlich 1973, *supra* at 54, equations 1-6 in Table 6 of *id.* at 55, and Table 4 of Ehrlich 1975, *supra* at 410, and equations 3 and 4 in Table 7 of Ehrlich 1973, *supra* at 57.

We believe these equations are not simply mislabeled but are, in fact, improperly specified with respect to the effective period of analysis. With the data generated from information in Ehrlich's memorandum, *supra* note 15, we have estimated each of the above equations for its maximum proper effective period and for the apparently incorrect one indicated in Ehrlich's tabulations. In every case, we found that the results reported by Ehrlich correspond more closely with the estimates we have obtained for the incorrectly defined effective period. However, the resulting errors of estimation are relatively small in magnitude since they enter the second stage regression results through [illegible] in the first stage estimation of arrest and conviction rates.

25. [illegible] note 1, at [illegible].

addition, the relative strengths of the effects of arrest (P^*a), conviction ($P^*c|a$) and execution ($P^*e|c$) are the same as Ehrlich reports.[26] According to Ehrlich, "[t]he regression results regarding the effects of P^*a, $P^*c|a$, and $P^*e|c$ constitute perhaps the strongest findings of the empirical investigation. Not only do the signs of the elasticities associated with these variables conform to the general theoretical expectations, but their ranking, too, is consistent with the predictions . . . ."[27] Thus, by reproducing the rank order of effects among arrest, conviction, and execution rates, we have replicated an especially important aspect of his regression results.[28]

B. *Temporal Specification*

If the results of a time series regression analysis are a faithful representation of underlying causal processes, the values of the estimated coefficients will be independent of the specific time period chosen for the analysis. Thus, if the values of the coefficients associated with the various measures of execution risk change substantially when they are estimated for alternative time intervals, the negative values reported in Table III are not a reliable basis for inferring that capital punishment has a deterrent effect on murder.

Ehrlich addressed this issue by repeating the regression analysis for selected subperiods. He performed seven regressions in which varying numbers of years were removed from the beginning of the time series and two analyses in which three years were dropped from the end of the series. These alterations in the effective period of analysis do not appreciably change the elasticities associated with execution risk.[29] Ehrlich does, however, concede that the deterrent effects of arrest, conviction, and execution rates become weaker when as many as seven years are dropped from the recent end of the time series.[30]

We find that all empirical support for the deterrent effect of capital

26. *Id.*

27. *Id.* at 411.

28. Our results are comparable to Ehrlich's in other respects (*see* appendix II, *supra* note 2, for the full regression results). The elasticities associated with the alternative measures of execution risk are less in absolute magnitude than those associated with arrest and conviction rates, but relative to their standard errors, they tend to fall between those of the arrest and conviction measures. In addition, the signs of the elasticities associated with the remaining causal factors are the same as those obtained by Ehrlich.

29. Ehrlich 1975, *supra* note 1, at 410; Ehrlich 1973, *supra* note 1, at 56.

30. Ehrlich 1973, *supra* note 1, at 70.

punishment disappears when the five most recent years are removed from the time series that Ehrlich selected for analysis. Table IV[31] shows the estimated effects of execution risk on the criminal homicide rate for 10 periods with successively earlier ending dates. For the period ending in 1964, there are no statistically significant negative elasticities associated with the various measures of execution risk. For the period ending in 1963, the estimated elasticities have become positive in every case. Indeed, of the 24 coefficients reflecting the effects of execution risk for periods ending in 1963 and earlier, 20 are positive and only four are negative.

Furthermore, we find that the regression results are more adequate and consistent for the periods with earlier ending dates. The standard errors of the regressions are less, the F and R^2 statistics are consistently higher, and the Durbin Watson statistics are generally more acceptable for the periods ending in 1960 and 1963 than for those ending in 1966 and 1969.[32] In addition, the estimated coefficients for the other variables in the regression equations for the two shorter periods are

31.

Table IV

Estimated Effects of Execution Risk on the Criminal Homicide Rate for Effective Periods with Successively Earlier Ending Dates (Variables in Natural Logarithms) (t Values in Parentheses)

Ending Date of Effective Period	*Six Alternative Measures of Execution Risk*					
	PXQ_1	PXQ_2	PXQ_{1-1}	TXQ_1	PDL_1	$\bar{P}XQ_1$
1969	−.018	−.068	−.023	−.059	−.065	−.001
	(−.69)	(−3.15)	(−1.12)	(−2.70)	(−3.15)	(−.113)
1968	−.026	−.069	−.030	−.059	−.009	−.019
	(−.99)	(−3.50)	(−1.11)	(−2.76)	(−1.09)	(−1.36)
1967	−.041	−.064	−.061	−.061	−.068	−.060
	(−1.38)	(−3.65)	(−3.18)	(−3.61)	(−1.55)	(−1.98)
1966	−.020	−.055	−.053	−.050	−.056	−.013
	(−1.00)	(−3.79)	(−3.31)	(−2.88)	(−3.10)	(−1.59)
1965	−.016	−.041	.031	−.025	−.037	−.031
	(−.29)	(−1.51)	(−1.20)	(−.98)	(−1.53)	(−1.41)
1964	.028	−.021	−.017	−.009	−.013	.013
	(.91)	(−.70)	(−.58)	(−.31)	(−.40)	(.37)
1963	.057	.004	.004	.065	.018	.037
	(1.77)	(.08)	(.08)	(1.63)	(1.00)	(1.02)
1962	.052	−.030	−.021	.060	.021	.010
	(1.11)	(−.60)	(−.51)	(1.30)	(.35)	(.81)
1961	−.015	.011	.041	.086	.050	−.019
	(−.26)	(.67)	(.29)	(2.10)	(1.02)	(−.33)
1960	.013	.029	.009	.070	.067	.013
	(.21)	(.52)	(.25)	(1.72)	(1.36)	(.22)

32. See equations 1.1 to 1.4 in appendix B, supra note 2. These statistical measures are

generally closer in value than in the equations for the longer periods.[33]

Hence for the periods in which the model gives evidence of being more adequately specified, the regression analysis consistently shows a slightly positive—though not statistically significant—effect of execution risk on the homicide rate.

C. *Functional Form*

Seldom does an initial theoretical formulation, such as Ehrlich's economic model of the determinants of murder, unambiguously dictate the mathematical function which describes the true relationships among the variables. When the functional form is open to question or when the analyst wishes to establish the generality of his findings, he will typically examine regression results obtained under different assumptions about the form of the model.

Ehrlich assumes that the factors which determine the murder rate have a multiplicative effect. Adopting a standard regression technique, he uses logarithmic values of the variables, in order to transform this multiplicative relationship into an equivalent linear form suitable for regression analysis.[34] He reports that his regression results are not dependent on the specific assumptions he has made about the form of the relationships among the variables—that there is evidence of a deterrent effect even when he performs the regression analysis with the natural values of his variables (which corresponds to a linear rather than a multiplicative relationship among the variables).[35]

Using the natural values of these variables, we have re-estimated the coefficients shown in Table IV. We show these results in Table V.[36] There are, among the 60 estimates in Table V, more positive than negative coefficients associated with the various measures of execution risk. Only two of them, both positive, are statistically significant. The direction and size of the estimated coefficients do not appear to be systematically affected by the choice of time period. In other words, the last few years of the time series, which are apparently responsible for the evidence of a deterrent effect when logarithmic

33. Not only does the evidence for deterrence disappear, but Ehrlich's more general theoretical formulation is weakened by our findings. When we examine the results for periods ending in 1964 or earlier, we find only three instances out of 30 possible results which conform to his rank order predictions of the relative strengths of the deterrence variables. *See* pp. 196-97 *supra*; appendix B, *supra* note 2.

34. *See* J. Johnston, Econometric Methods 47-50 (2d ed. 1972); R. Wonnacott & T. Wonnacott, Econometrics 91-98 (1970).

35. Ehrlich 1975, *supra* note 1, at 412-13; Ehrlich 1973, *supra* note 1, at 36-37.

values are used, yield no such evidence with the natural values of the variables.

III. Sources of Ehrlich's Deterrence Evidence

We have seen that Ehrlich obtains evidence of a deterrent effect only by imposing highly restrictive conditions on his analysis. One might assume that this evidence of deterrence reflects either a strong deterrent effect operating exclusively in recent years or a more pervasive effect obscured by data inadequacies in the early years. We show instead that Ehrlich's evidence is strictly a statistical artifact, not the reflection of a deterrent effect over the entire period of analysis or the most recent subperiod.

in logarithms, the regression coefficients indicate the change in the homicide rate to be expected from a one unit change in execution risk. For example, a coefficient of −.00061 (for PXQ_2 in the period 1935-1969) means that a reduction in the number of executions from 10 to nine (per 100 convictions for murder), can be expected to increase the homicide rate from 5.00000 to 5.00061 (per 100,000 population), or to add 12 homicides for a population of 200 million.

Table V

Estimated Effects of Execution Risk on the Criminal Homicide Rate for Effective Periods with Successively Earlier Ending Dates (Variables in Natural Values) (t Values in Parentheses)

Ending Date of Effective Period	*Six Alternative Measures of Execution Risk**					
	PXQ_1	PXQ_2	PXQ_{1-1}	TXQ_1	PDL_1	PXQ_1
1969	.00008	−.00061	.00132	.00135	.00085	.00051
	(.05)	(−.73)	(1.02)	(.64)	(.43)	(.33
1968	.00038	−.00068	.00126	.00131	.00085	.00051
	(.25)	(−.79)	(.96)	(.61)	(.41)	(.31
1967	.00021	−.00051	.00029	.00106	.00039	.00016
	(.16)	(−.63)	(.56)	(.47)	(.18)	(.11
1966	−.00023	−.00010	−.00013	.00110	.00016	−.00016
	(−.20)	(−.52)	(−.17)	(.54)	(.22)	(−.38
1965	−.00027	−.00027	−.00038	.00121	.00087	−.00013
	(−.29)	(−.40)	(−.60)	(.81)	(.52)	(−.55
1964	.00009	−.00021	−.00031	.00101	.00123	−.00001
	(.11)	(−.37)	(−.57)	(.78)	(.78)	(−.01
1963	.00026	−.00019	−.00023	.00123	.00189	.00022
	(.29)	(−.28)	(−.37)	(.87)	(1.10)	(.21
1962	−.00030	−.00023	−.00032	.00058	.00120	−.00015
	(−.38)	(−.35)	(−.56)	(.53)	(.80)	(−.11
1961	−.00042	.00027	−.00003	.00117	.00216	−.00011
	(−.50)	(.15)	(−.05)	(1.35)	(2.00)	(−.68
1960	.00001	.00011	.00005	.00139	.00235	−.00013
	([illegible])	(.69)	(.09)	(1.30)	(2.17)	(−.20

A. *The Recent Years*

What is it about the middle and late 1960's which causes the execution variables to show negative effects on the homicide rate when they are in logarithmic but not in natural form? The answer lies in the opposing trends in the two variables and in the nature of the logarithmic transformation. The national homicide rate, as reported by the FBI, rose precipitously in the middle and late 1960's to levels well above those of the 1940's and 1950's. Indeed, between 1962 and 1969 the homicide rate rose almost 60 percent to a level exceeded only by the rate for 1933. At the same time, executions literally came to an end. Hence execution risk—the number of executions among those convicted of murder—took on extremely low values, approaching zero, in the middle and late 1960's.[37] A property of the logarithmic transformation is to emphasize variations at the lower range of a variable. For example, if execution risk is converted into logarithms, a difference between one and two executions per 1,000 convictions will be greater than a difference between 350 and 650 executions per 1,000 convictions. Consequently, the logarithmic transformation accentuates the decline in execution risk that occurred in the 1960's.

To show the effect of the logarithmic transformation on these low values of execution risk, we present in Table VI[38] the corresponding logarithmic and natural values of one of the six measures of execution risk for the years from 1960 through 1969. The natural values of execution risk have dropped from about one percent in

37. There have been no executions since 1967. In order to extend the effective period of analysis through 1969, Ehrlich had to generate nonzero execution rates for the years after 1967, since the logarithm of zero is not defined. He did this by supplying one nonexistent execution for 1968 and 1969 in the calculation of PXQ_1. Ehrlich 1975, *supra* note 1, at 409 n.h.

38. Table VI

Logarithmic and Natural Values of Execution Risk (PXQ_1) for Each Year 1960-1969

	Logarithms		*Natural Values*	
	The Absolute Value	*Standard Deviations from the Mean*	*The Absolute Value*	*Standard Deviations from the Mean*
1969	−3.823	−2.524	.022	−1.123
1968	−3.873	−2.554	.021	−1.121
1967	−3.131	−2.103	.044	−1.111
1966	−3.774	−2.494	.023	−1.123
1965	−1.742	−1.262	.175	−1.059
1964	−1.456	−1.077	.233	−1.034
1963	−.642	−.580	.526	−.911
1962	.171	−.084	1.191	−.630

the years 1960-1962 to less than .05 percent for the years 1966-1969. The values for the years after 1964 are all slightly more than one standard deviation below the mean for the entire period from 1933 to 1969. Putting execution risk in logarithmic form greatly accentuates the decline. The difference between 1960 and 1969 in logarithmic values (from .229 to −3.823) is more than three times the corresponding difference in natural values (from 1.257 to .022). In fact, for the period from 1966 to 1969, the logarithmic values of execution risk are all more than two standard deviations below their mean. Thus, by using logarithmic values of execution risk, Ehrlich gives considerably more weight in his regression analysis to the extremely low values of this variable after 1964.

Ehrlich has stated that the recent behavior of arrest and conviction rates as well as that of execution risk plays an important role in his regression results.[39] To examine the effect of using the logarithmic values of execution risk and the possibility that the logarithmic transformation of arrest and conviction rates may also influence the regression results, we present, in Table VII,[40] simple correlations of the arrest, conviction, and execution rates with the criminal homicide rate; these are shown for logarithmic and natural values of the variables and for time intervals with successively earlier ending dates from 1969 through 1960. The data for the recent years have an extraordinary effect on the correlation between the logarithms of execution and homicide rates; adding the last five years reduces the correlation from .836 to .123. In contrast, the recent data have much

39. Ehrlich 1975, *supra* note 1, at 70.

40. Table VII

Correlations of Arrest, Conviction, and Execution Rates with the Homicide Rate for Effective Periods with Successively Earlier Ending Dates (Separately for Logarithms and Natural Values of the Variables)

Effective Periods	*Arrest Rate* (P^*a) *Logarithms*	*Arrest Rate* (P^*a) *Natural Values*	*Conviction Rate* ($P^*c\|a$) *Logarithms*	*Conviction Rate* ($P^*c\|a$) *Natural Values*	*Execution Rate* (PQX_e) *Logarithms*	*Execution Rate* (PQX_e) *Natural Values*
1933-1969	−.809	−.832	−.501	−.516	.123	.553
1933-1968	−.801	−.818	−.488	−.496	.278	.644
1933-1967	−.793	−.809	−.492	−.497	.442	.720
1933-1966	−.791	−.808	−.496	−.499	.563	.758
1933-1965	−.792	−.809	−.496	−.499	.716	.773
1933-1964	−.795	−.813	−.494	−.497	.806	.772
1933-1963	−.806	−.821	−.488	−.491	.858	.765
1933-1962	−.820	−.837	−.495	−.500	.857	.750
1933-1961	−.819	−.845	−.505	−.511	.846	.741
[illegible]		−.831	−.510	−.520	[illegible]	[illegible]

less effect on the correlation between the natural values of execution and homicide rates; adding the last five years reduces the correlation only from .729 to .553.[41] These years have virtually no impact on the correlations, in either logarithmic or natural form, of arrest and conviction rates with the criminal homicide rates. Thus, Ehrlich's evidence of deterrence rests heavily on the relationship between the values of execution risk and homicide rates for the years after 1964. ✓

This conclusion might suggest that the use of the death penalty at the very low levels of execution risk in the middle and late 1960's had a deterrent effect strong enough to produce a measurable effect over the entire period when these recent years are combined with earlier years. We examine this possibility in Table VIII.[42] It shows annual changes in the homicide rate relative to national changes in the homicide rate for the period 1962 to 1968 among states that increased or decreased the number of executions imposed for murder. The relative changes in homicide rate are expressed in homicides per 100,000 people, and are obtained by subtracting the changes in the national homicide rate from changes in the individual states.

41. Note, in this connection, that Ehrlich's extension of the time series on execution risk from 1967 to 1969 by supplying executions for 1968 and 1969 in the calculation of PXQ_e, note 30 *supra*, reduces its correlation with the homicide rate in logarithmic values from .442 for 1933 to 1967 to .123 for 1933 to 1969 and in natural values from .720 to .553 for the same periods.

42. Table VIII

Annual Changes in Criminal Homicide Rate Among States Which Have Increased and Decreased Executions Relative to Annual Homicide Rate Changes in the Nation as a Whole for the Period 1962-1968

Annual Change	*States Which Have Decreased Executions*: *Relative Homicide Rate Change*	*Number of States*	*Population Proportion*	*States Which Have Increased Executions*: *Relative Rate Change Homicide*	*Number of States*	*Population Proportion*
1962-1963	−.10	11	.41	−.03	6	.17
1963-1964	.19	8	.33	−.33	3	.06
1964-1965	−.41	3	.13	.88	1	.02
1965-1966	−.93	4	.05	.50	1	.01
1966-1967	.62	1	.01	.09	2	.11
1967-1968	−.23	2	.11	—	—	—

States imposing executions for murder during this period were identified in Appendix A of W. BOWERS, EXECUTIONS IN AMERICA 200-401 (1974); criminal homicides annually by state were obtained from FBI, UNIFORM CRIME REPORTS FOR THE UNITED STATES (Table 4, 1962-1964; Table 4, 1965-1969); annual population estimates by state were taken from U.S. BUREAU OF THE CENSUS, DEP'T OF COMMERCE, CURRENT POPULATION REPORTS: POPULATION ESTIMATES AND PROJECTION (Ser. P-25, No. 460, Oct. 1971) (1962-1969). Changes in homicide rates for groups of states that increased and decreased executions are essentially the average of the annual changes in the states that comprise the group weighted [illegible]

Conclusion

We have shown that Ehrlich's findings are not a reliable basis for inferring the effects of capital punishment on the criminal homicide rate. Flaws in Ehrlich's data cast doubt on the ability to perform meaningful regression analysis. The analysis itself yields evidence of a deterrent effect only by relying on the unusual nature of the years after 1961 and on the logarithmic transformation of the data. When the analysis is performed for more appropriate periods, the hypothesis that the death penalty deters murders finds no support.

APPENDIX

Specific Data Sources Used in the Replication of Ehrlich's Regression Analysis

1. $\left(\frac{Q}{N}\right)^*$ *Criminal Homicide Rate* = Number of Criminal Homicides per year/annual civilian population.

   (a) Q = Annual number of murders and nonnegligent manslaughters from 1933 to 1969. Revised figures (1971) provided by the FBI.

   (b) N = Civilian population of the United States in 1,000's from 1933 to 1969. U.S. Bureau of the Census, Dep't of Commerce, Current Population Reports: Population Estimates and Projections Tables 3, 4 (Ser. P-25, No. 499, May 1973).

2. $P^{\circ}a$ *Clearance Rate for Criminal Homicide* = Fraction of murders and nonnegligent manslaughters cleared by arrest. FBI, Uniform Crime Reports for the United States (1933-1969).

3. $P^{\circ}c|a$ *Conviction Rate for Criminal Homicides* = Fraction of individuals found guilty as charged for murder and nonnegligent manslaughter. 1936-1969: *id.* After 1962 separate estimates are reported in the *Uniform Crime Reports* annual bulletin. Estimates from the time series which is continuous over the 1936 to 1969 period were chosen. These estimates were generally based on larger population bases. The value for 1961 is the average of the 1960 and 1962 estimates. 1933-1935: values 1933 to 1935 were obtained from Ehrlich (Ehrlich 1973, *supra* note 1, Figure 1).

4. $P^{\circ}e|c$ *Execution Risk for Criminal Homicide* (PXQ_1, PXQ_0, PXQ_{t-1}, TXQ_0, PDL_1, PXQ_1.) The measures of execution risk are all variations of the form $\frac{E}{C}$, where $C = Q \cdot P^{\circ}a \cdot P^{\circ}c|a$.

   (a) E = The number of executions for murder. U.S. Bureau of Prisons, Dep't of Justice, National Prisoner Statistics Bulletin Table 1 (No. 46, Aug. 1971).

   (b) C = Number of convictions for murder.

5. L *Labor Force Participation Rates:*

   $$\text{for 1940-1969 } L = \frac{CL}{(TN - TL + CL)}$$

   $$\text{for 1933-1939 } L = \frac{CL}{(N - P13)}$$

   (a) CL, TL = The civilian and total labor force in 1000's from 1933 to 1969. U.S. Bureau of Labor Statistics, Dep't of Labor, Employment and Earnings Table A-1 (Jan. 1971).

   (b) TN = Total noninstitutional population from 1940 to 1969. *Id.*

   (c) N = Annual civilian population as defined in 1(b).

   (d) P13 = Annual population 13 years old and under from 1933 to 1939. U.S. Bureau of the Census, Dep't of Commerce, Current Population Reports: Population Estimates and Projections (Ser. P-25, No. 311, July 1965).

6. U *Unemployment Rate of the Civilian Labor Force, 1933 to 1969.* Employment and Earnings Table A-1, *supra* 5(a).

7. A *Fraction of the Resident Population 14 to 24 Years of Age.*

   $$A = \frac{P1424}{RP}$$

   (a) P1424 = Number of persons 14 to 24 years of age in the resident population.

   1933-1939: Current Population Reports, *supra* 5(d).

   1940-1949: U.S. Bureau of the Census, Dep't of Commerce, Current Population Reports: Population Estimates and Projections Table 1 (Ser. P-25, No. 98, Aug. 1954).

   1950-1959: *id.* Table 1 (Ser. P-25, No. 310, June 1965).

   1960-1969: *id.* Table 2 (Ser. P-25, No. 519, Apr. 1974).

   (b) RP = Resident population in 1000's. References same as given in (a) for each of the respective time periods.

8. Y *Friedman's Estimate of Real Permanent Income per Capita.* The following equation was used to compute Y:

   $$Y_t = (.330)\,Y_{t-1} + (.226)\,Y_{t-2} + (.151)\,Y_{t-3} + (.106)\,Y_{t-4} + (.0\ )\,Y_{t-5} + (.049)\,Y_{t-6} + (.033)\,Y_{t-7} + (.023)\,Y_{t-8}$$

   (a) The weights in the above equation were obtained from E. Karni, The Value of Time and the Demand for Money, 1971, Table 33 (unpublished doctoral dissertation, Univ. of Chicago).

   (b) Y_{t-1} through Y_{t-8} are logarithmic estimates of per capita real national income for the years 1925 to 1969. The national income figures were provided by E. Karni. Population figures for per capita estimates were obtained from:

   1925-1929: Current Population Reports, *supra* 1(b), Table 1.

   1930-1968: U.S. Dep't of Commerce, Survey of Current Business Table 7.6 (July 1969).

   (c) The per capita income was measured in terms of 1929 dollars using a price deflator obtained from source (a).

9. NW *Percent of Nonwhite Residential Population.* References same as given in 7(a) for each of the respective time periods.

If execution risk had a deterrent effect, states with declining numbers of executions would show a relative increase in homicide rate, and states with rising execution levels would show a relative decrease in homicide rate. But Table VIII demonstrates that there is no such pattern in the years since 1962. Among states which decreased executions, the homicide rate rose more than the national figure for two of the periods[42] and less than the national figure for four. Among states that increased executions, the change in homicide rate was below the national change in one comparison, very nearly the same in one case, and actually above in three of the five comparisons.

Table VIII also shows that the use of capital punishment during this period was restricted increasingly to a small minority of states. After 1964, no more than five states imposed executions in a single year, none of them imposed more than one execution per year, and none imposed executions two years in a row. In this situation, the national homicide rate cannot be expected to reflect possible deterrent effects presumed to occur primarily in the jurisdictions that actively use the death penalty. Thus, apart from problems of temporal specification and functional form, it would have been more appropriate, in view of the progressively restricted use of capital punishment in the nation, for Ehrlich to have shortened the effective period of analysis by removing the years after 1963, when no more than 10 percent of the states imposed executions, than to have extended the period of analysis two years beyond the end of executions in the United States.

B. *The Early Years*

We have already described the unreliability of data for the 1930's. By reproducing his regression analysis for effective periods with later beginning dates, Ehrlich may have hoped to diminish the effects of measurement error in these early years. But he has thereby given greater weight to the years after 1961, which are responsible in the first place for his evidence of a deterrent effect.

To determine the effects of measurement error in the early years, we must first remove the idiosyncratic recent years, and then successively drop years from the beginning of the time series. Accordingly, we have performed regressions for periods with 1963 as the ending date and with successively later beginning dates from 1935 through

42. However, one of these two periods, 1966-1967, involves only one state, [illegible]

1940. The estimated coefficients for logarithmic and natural values of two measures of execution risk are shown in Table IX.[44]

We know from Tables IV and V that the elasticities associated with execution risk for effective periods ending in 1963 and earlier are more often positive than negative, though usually not statistically significant. In Table IX, the coefficients are again predominantly positive, and become even more so as years are successively dropped from the beginning of the time series. In fact, the effective periods beginning in 1938, 1939, and 1940 show positive effects for execution risk in all 12 cases. These positive coefficients are, of course, absurd from the viewpoint of the deterrence hypothesis, although they may not be altogether meaningless.[45] They do, however, indicate unambiguously that data inadequacies in the early years of the time series have not obscured deterrent effects of capital punishment. Indeed, by all indications there are no deterrent effects to obscure.

Table IX

Estimated Effects of Execution Risk on the Criminal Homicide Rate for Effective Periods Ending in 1963 With Successively Later Beginning Dates (Separately for Variables in Logged and Unlogged Form) (t Value in Parentheses)

Beginning Date of Effective Period	*Logarithms* PXQ_{t-1}	*Logarithms* $P\widehat{XQ}_t$	*Natural Values* PXQ_{t-1}	*Natural Values* $P\widehat{XQ}_t$
1935	--	.037	--	.000219
	--	(1.02)	--	(.21)
1936	.003	.031	-.000225	.000082
	(.08)	(.95)	(-.37)	(.08)
1937	.007	.038	-.000079	-.000001
	(.19)	(1.02)	(-.12)	(-.00)
1938	.038	.033	.001117	.000833
	(.91)	(.95)	(1.10)	(.61)
1939	.062	.061	.001858	.001772
	(1.79)	(1.73)	(1.35)	(1.20)
1940	.095	.056	.001581	.001818
	(1.95)	(1.56)	(1.03)	(1.19)

44. Table IX presents the effects of two measures of execution risk, PXQ_{t-1} and $P\widehat{XQ}_t$. PXQ_{t-1} is the measure used most frequently by Ehrlich, and $P\widehat{XQ}_t$, of all the measures, is least biased by measurement error since its one year lagged homicide rate is only one of 18 variables that figure in its estimation. Equations 13.1-16.3 of appendix B, *supra* note 2, present detailed regression results for selected periods beginning in 1936, 1938, and 1940.

45. The possibility that because of a "brutalizing effect," capital punishment may encourage rather than deter murders is beyond the scope of this paper but is considered in Bowers & Pierce, *supra* note 2. In particular, we discuss the meaning of the positive regression coefficients we obtained when we estimated the number of executions [illegible] execution risk.

11. XGOV *Per Capita Real Expenditures on All Governments in Millions of Dollars.*

$$XGOV = \frac{(FE + SLE\text{-}D)}{RP^{*}PD^{*}10}$$

(a) FE, SLE = Federal and state/local expenditure on all governments.
1933-1938: U.S. DEP'T OF COMMERCE, HISTORICAL STATISTICS OF THE U.S.: COLONIAL TIMES TO 1957 (1960).

1939-1965: U.S. DEP'T OF COMMERCE, THE NATIONAL INCOME AND PRODUCT ACCOUNTS OF THE UNITED STATES, 1929-1965 STATISTICAL TABLES (1966).

1966-1969: U.S. DEP'T OF COMMERCE, SURVEY OF CURRENT BUSINESS (July 1970).

(b) D = National defense expenditures.
1933-1938: U.S. DEP'T OF COMMERCE, HISTORICAL STATISTICS OF THE U.S.: COLONIAL TIMES TO 1957, *supra* (a).

1939-1952: (purchases of goods and services). NATIONAL INCOME AND PRODUCT ACCOUNTS OF THE UNITED STATES, *supra* (a), Tables 1.1.

1953-1969: (purchases of goods and services). SURVEY OF CURRENT BUSINESS, *supra* (a), Table 3.10.

(c) RP = Residential population as defined by 7(b).

(d) PD = Implicit price deflator for all governments.
1932-1965: NATIONAL INCOME AND PRODUCT ACCOUNTS OF THE UNITED STATES, *supra* (a), Table 8.4.

1966-1969: SURVEY OF CURRENT BUSINESS, *supra* (a).

12. $XPOL_{-1}$ *Per Capita Real Expenditures on Police Lagged One Year in Dollars.*

$$XPOL = \frac{POLE^{*}100{,}000}{RP^{*}PD} \text{ where}$$

(a) POLE = Total police expenditure.
1932-1966: 6 U.S. BUREAU OF THE CENSUS, DEP'T OF COMMERCE, 1967 CENSUS OF GOVERNMENTS, No. 5, at 36 (1969).

1967-1969: U.S. BUREAU OF THE CENSUS, GOVERNMENTAL FINANCES IN 1969-70, at 15 (1971).

(b) RP = Residential population as defined in 7(b).

(c) PD = Price deflator as defined in 11(d).

13. *Alternative Vital Statistics Estimates of Homicide.*
Q = Annual number of willful homicides (minus the annual number of executions after 1918, when the two mortality categories were combined,

1933-1936: U.S. BUREAU OF THE CENSUS, DEP'T OF COMMERCE, MORTALITY STATISTICS (1933-1936).

1937-1969: U.S. BUREAU OF THE CENSUS, DEP'T OF COMMERCE, VITAL STATISTICS OF THE UNITED STATES (1937-1945); U.S. PUBLIC HEALTH SERV., DEP'T OF HEALTH, EDUCATION, AND WELFARE, VITAL STATISTICS OF THE UNITED STATES (1946-1969).

Deterrence: Evidence and Inference*

Isaac Ehrlich†

Because of space limitations and the short time I have been given to prepare my response to the two critiques of my work published in this issue of the *Journal*, I shall confine myself principally to the critique by Bowers and Pierce.[1] I do that not because the paper by Baldus and Cole[2] does not warrant a detailed reply but because an elaborate response to the central issues they raise is contained in a study of mine now in progress which deals critically with published research by Sellin and others.[3] I choose to focus on the Bowers and Pierce piece also because, as I hope to show, their work largely misinterprets and misapplies the framework I have developed for testing the deterrence hypothesis. Addressing their work critically provides the opportunity to elaborate upon some pertinent aspects of my research, hopefully for the benefit of interested scholars.

The conclusions of my time series study of murder basically are two: (1) that previous research never adequately tested a set of direct and specific implications suggested by a general theory of deterrence and (2) that my empirical findings, while tentative and inconclusive by the very nature of observational statistics, are not inconsistent with rather sharp implications emanating from this theory, including the hypothesized deterrent effect of the conditional risk of execution. Bowers and Pierce tacitly accept the first conclusion and seek to evade the second, evidently on faulty grounds. In their efforts to obscure the empirical findings, they have selectively deleted observations, utilized an inferior regression specification, considered irrelevant variables and correlations, and revealed in the process misunderstanding of elementary statistical concepts, as I discuss in points III, IV, and V below. They do not provide evidence based on a systematic statistical analysis showing that capital punishment, or punishment in general, does not deter crime. Essentially, they make only the point that the observed deterrent effect of the risk of execution *can be* confounded

* This reply has been prepared in cooperation with Randall Mark.

† Associate Professor of Business Economics, University of Chicago; Research Associate, National Bureau of Economic Research.

1. Bowers & Pierce, *The Illusion of Deterrence in Isaac Ehrlich's Research on Capital Punishment*, 85 YALE L.J. 187 (1975).

2. Baldus & Cole, *A Comparison of the Work of Thorsten Sellin and Isaac Ehrlich* [illegible]

when insufficient regard is shown for proper methods of hypothesis testing. Indeed, my principal response to Bowers and Pierce is that they concern themselves only with making a point (*i.e.*, confounding a result) rather than testing a hypothesis. There is a fundamental difference between a systematic and statistically coherent test of all the ramifications of a general hypothesis and an exercise in search of that set of circumstances which—for purely technical reasons—may weaken the effect of a single variable within a comprehensive model. Bowers and Pierce do not analyze the effects of variables other than the conditional probability of execution—variables such as estimates of apprehension and conviction risks and unemployment and labor force participation rates. Moreover, they do not address themselves at all to the question of the optimal form of testing for the deterrence hypothesis, given the technical limitations of observational statistics.

I shall respond in some detail only to the substantive issues raised by Bowers and Pierce and to a few of the arguments advanced by Baldus and Cole. Also, I shall point out only some of their more serious errors. For expositional convenience, the discussion below is ordered as seven general points.

I. Corroborating Evidence from Bowers and Pierce

First and foremost, the Bowers and Pierce work, however inadvertently, has lent considerable strength to the case for the deterrent effect of capital punishment, because their application of the theory and econometric methods outlined in my paper over the entire period considered in my analysis produces results quite similar to my own. This is noteworthy for several reasons.

A. Their data set is not identical to the set of data that I utilized.[4] Their comments indicate that at least four variables have been constructed differently in their work.[5]

B. In addition, Bowers and Pierce do not accurately execute my

4. A detailed description of the data set that I utilized is contained in the memorandum, I. Ehrlich, The Deterrent Effect of Capital Punishment: A Question of Life and Death, American Economic Review (June, 1975): Sources of Data, May 1975 (on file with *Yale Law Journal*). Mr. Bowers, among others, received a draft of this memorandum, which noted some errata in the published paper, and which was completed in July 1975 by Randall Mark.

5. Bowers & Pierce, *supra* note 1, at 191 n.18. They allege that one of these differences, that concerning $XPOE_{t-1}$, is due to my having used an unspecific auxiliary analysis for estimating missing values of this variable. Their allegation is false: my memorandum on data sources, *supra* note 4, at 17-19, outlines in detail the auxiliary procedure utilized. In addition, Bowers and Pierce utilize different values of [illegible] than did I for the years 1933, 1934, and 1935, because they did not choose to apply the procedure I outlined for estimating these values. Their reference to "Ehrlich's conviction rates" for the [illegible] finding. Bowers & Pierce, *supra* at 191 n.15.

regression analysis in which two measures of execution risk are used to test the hypotheses of the model. Their results in connection with these measures of execution risk are likely to be inferior to my own because they omit an observation; therefore their results are based on fewer degrees of freedom.[6]

C. Furthermore, Bowers and Pierce may have used computational procedures different from those I utilized. That their regression results and coefficients of serial correlation always differ from mine is consistent with this view.[7]

Yet despite these differences, the regression results which Bowers and Pierce report are "similar to Ehrlich's."[8] More particularly, they confirm not only the apparent restraining effect of the conditional probability of execution on the murder rate but also my predicted ranking of estimates of elasticities of the murder rate with respect to the three deterrence variables—the probability of apprehension, the conditional probability of conviction, and the conditional probability of execution.[9] This is at once a confirmation of the strength of my approach and a corroboration of the basic findings. The results stand out in contrast to previous allegations that no evidence exists suggesting that the death penalty may have a restraining effect on the frequency of murder in the population. How Bowers and Pierce attempt to obscure these results is the subject of the three following points.

II. The Effects of Data Imperfections

Bowers and Pierce attempt to invalidate the results of my empirical investigation of the effects of deterrence variables (and these results

6. Bowers and Pierce suggest that all regressions of mine in which PXQ_{t-1} and PXQ_t are used as measures of the conditional probability of execution are erroneous. Bowers & Pierce, *supra* note 1, at 195 n.21. They argue that I mistakenly utilized PXQ_{t-1} and PXQ_t for the effective periods 1935-1969 and 1937-1969, respectively, whereas, they claim, the absence of data on PXQ_{t-1} in 1933 would require starting the effective period one year later in each case. They then speculate that an "erroneous (probably zero)" value of PXQ_{t-1} must have been inserted by the computer for 1933. *Id.* Contrary to that speculation, however, PXQ_{t-1} in 1933 was estimated for use in all regressions with PXQ_{t-1} and PXQ_t over the periods 1935-1969 and 1937-1969, respectively. The value of PXQ_{t-1} in 1933 is estimated as 7.16299 (percent). The estimate is based on published FBI data for the murder rate and for the murder clearance ratio in 1932 and on an estimated conviction risk in that year.

The degrees of freedom associated with a regression are the excess of data points over the number of regression coefficients estimated.

7. The calculations I reported were computed via the Econometric Software Package, into which R.C. Fair's three-stage procedure is integrated. *See* J. Cooper, Econometric Software Package (1973). Bowers and Pierce provide no documentation of their computational procedures.

8. Bowers & Pierce, *supra* note 1, at 196.

9. *Id.* at 197.

alone) by posing, in effect, the extreme argument that data limitations prior to the 1960's preclude any empirical test of the deterrence hypothesis.[10] I do not agree with this general conclusion, and, in addition, I find their analysis concerning particular variables to be superficial and not constructive.

With respect to the dependent variable, the murder rate, the figures I have used are based on the Federal Bureau of Investigation's revised estimates of annual total murders and nonnegligent manslaughters. For the purposes of the empirical investigation, the FBI data are conceptually superior to the homicide series published in the *Vital Statistics of the United States* because the FBI category is defined to include only willful felonious homicides. Law enforcement officials, not health officials, bear the responsibility and undergo training for distinguishing willful felonious homicides from other homicides.[11] Indeed, by definition, the homicide data of the *Vital Statistics* explicitly include justifiable homicides and are likely to include some negligent manslaughters as well.[12] Moreover, the revised homicide data of the FBI reflect that agency's unique opportunity to incorporate into its estimates whatever homicide data have been collected by health officials. In contrast, the homicide figures tabulated for the *Vital Statistics* are never revised after the cutoff date for data collection for a given year.[13] In addition, information on death certificates, on which the *Vital Statistics* figures are based, sometimes may reflect classification of deaths only by medical cause rather than by external cause (such as accident or homicide), especially in those instances when death from homicidal assaults occurs later than the time of assault.

Because the *Vital Statistics* category includes some nonfelonious homicides, one would expect its homicide counts to exceed the FBI estimates. Yet, in every year from 1939 through 1961, the more narrowly defined FBI revised estimates are higher, by more than 900 in some years.[14] These comparisons suggest the importance of the technical considerations cited above that may lead to a significant underestimation of the relevant number of criminal homicides in the *Vital Statistics*. Tests which I have been conducting with independent bodies of data, and which I hope to report in the near future, indicate, nevertheless, that the effects of the deterrence variables—including the conditional probability of execution—on the homicide rate as reported by the *Vital Statistics* are qualitatively the same as those found in my time series investigation.

With respect to the empirical measures of the apprehension and conviction risks, the critics raise the issue of whether the FBI data are "unrepresentative" in the earliest years of the sample period.[15] Yet I have reported results demonstrating that the basic findings of my investigation are observed even without the presence of these early years in the observation set.[16] Moreover, their inference that the conviction data in 1936 and 1937 are biased because of a particular pattern observed in 1974 is purely conjectural and not founded upon any systematic analysis. Imperfections in data notwithstanding, my qualitative results in connection with the effects of apprehension and conviction risks over my entire sample period have been observed by others using the same FBI sources.[17]

More basically, though, the critics' attempt to discredit the empirical investigation on the basis of data quality is self-defeating to their own case. As is well known, "errors of measurement," as they are termed in the econometrics literature, generally lead to underestimation of the true effects of an explanatory variable in a simple regression analysis when that variable is subject to random measurement imperfections.[18] If the true variables of interest were grossly misrepresented by their empirical counterparts, then the statistical implementation of the theory should have *failed* to demonstrate any of the effects theorized. In particular, only a remarkable coincidence could then explain the fact that, as predicted by my theory, the findings show that the probability

10. *See id.* at 187-92. This direct implication of their remarks notwithstanding, Bowers and Pierce subsequently claim that their regression results are "more adequate and consistent" when they examine only subperiods ending early in the 1960's. *Id.* at 198. Yet by their own analysis, data imperfections clearly are less important in the 1960's than in earlier periods. *Id.* at 190-92.

11. Although health officials are expected, when they can, to distinguish homicides from other violent "external" causes of death, namely accidents and suicides, they clearly experience much difficulty in drawing these distinctions. As late as 1970, for example, in which the *Vital Statistics* reports 16,848 homicides, a total of 5,381 violent deaths are classified as due to undetermined cause, *i.e.*, either accidental or purposeful. II U.S. Public Health Serv., Dep't of Health, Education, and Welfare, Vital Statistics of the United States 1970, at 256.

12. Homicides inflicted through legal intervention by police are included in the homicide count reported by the *Vital Statistics*. Only beginning in 1949 can such homicides be separated from the total.

13. This failure to revise leads to an undercount of homicides pertaining to a given year [illegible] occurring in that year that were not reported by the cutoff date.

14. Bowers and Pierce's statement, *supra* note 1, at 189, that the FBI estimates differ from those of *Vital Statistics* by only about three percent for the period after 1949 is quite misleading in view of the magnitude of the year to year differences.

15. Bowers & Pierce, *supra* note 1, at 190, *see id.* at 190-92.

16. *See, e.g.*, Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life and Death*, 65 Am. Econ. Rev. 397, 410 (1975) (equation 5 of Table 3 and equation 7 of Table 4).

17. *See* W. Vandaele, The Economics of Crime: An Econometric Investigation of Auto Theft in the United States, 1975 (unpublished doctoral dissertation, Univ. of Chicago).

of arrest (measured by the FBI's clearance ratios) had a proportionally larger impact on the murder rate than the conditional probability of conviction (derived from the FBI's statistics) and that the conditional probability of execution had the least effect. The FBI's reported figures on arrest and conviction in the 1930's surely were not tailored to my theoretical predictions 30-odd years later.

III. The Effects of Deleting Observations

Curiously, Bowers and Pierce's remarks in the beginning of Part II of their paper may convey the impression that I have artificially restricted the time span of my empirical investigation to test deterrence effects. The fact is that my empirical investigation was conducted for the longest time period for which necessary data were available at the time of my study. In contrast, Bowers and Pierce conduct the bulk of their analysis over arbitrarily restricted subperiods after deleting specific observations from the complete data set. Their assertion that "Ehrlich's regression model fits the data better without them"[19] is based upon an erroneous method of inference, as I point out in Part V below. Here I shall address the consequences of discarding the information provided by the observations that they choose to omit.

Selective elimination of a sufficient number of observations from a regression analysis is a virtually foolproof method for reversing any single result derived from an original sample. Imagine, for example, a regression line verifying a negative association between the quantity demanded of corn and the price of corn. Since a majority of the data points typically will not lie on the regression line, the selective exclusion of data points can easily turn a significant negative relationship into an insignificant negative association or even a positive one. Such exclusions are particularly disturbing when the entire sample is relatively small. Indeed, the elimination of data points relating to murder in the 1960's—accounting for 17 percent or more of the full sample which I investigated—amounts to, in practice, the selective, nonrandom exclusion of observations crucial to an efficient estimation of the effects of key deterrent variables.

A. Omitting observations from the 1960's drastically reduces the variability in estimates of the conditional probability of execution and in the modified rates of change of these estimates, which are the actual regressors in the analysis. Whereas the rates of change in this variable had been quite stable for the preceding two decades, the objective (measured) risk of execution declined quite sharply starting about

1960. The sharp movements in the rates of change of the conditional risk of execution are, of course, not my invention—they accurately reflect the objective trend in the *true* risk of execution in the 1960's.[20] In addition, murder clearance ratios, used as objective estimates of the probability of arrest for murder, also exhibit little variability over specific subperiods ending in the early 1960's. Thus, variability in arrest and execution risks is particularly small between the late 1930's and the early 1960's.

It is a well known principle that a minimum amount of variability is necessary to perform a regression analysis. Indeed, an *efficient* sample designed for the purpose of estimating via a regression format the partial effects of specific explanatory variables on a dependent variable is one which maximizes the range of variability in the regressors. Imagine, again, the attempt to confirm the negative association between prices and quantities demanded of corn. The attempt would fail if the subperiod selected by a researcher for his regression analysis is one in which corn prices or their rates of change, whichever are relevant, are relatively stable. The appropriate inference, however, is not that the theory of demand fails to explain movements in corn purchases but rather that the selected subperiod cannot be utilized to estimate the partial effect of corn prices on quantities demanded. The importance of such considerations is apparent in my time series analysis not only in connection with deterrence variables but also in

20. The fact that this trend concerns only 20 states that imposed executions in 1960 has no specific relevance in connection with the estimated deterrent effect of execution, contrary to the unsystematic discussion of Bowers and Pierce, *supra* note 1, at 201. Only a subset, and typically a minority, of states enforced the death penalty in any given year. Many fewer than 20 applied the death penalty in every year from 1933 to 1967. Moreover, aggregating over states which had executions and states which did not implies that the estimated elasticity of the murder rate with respect to execution risk is likely to be biased downward relative to the true elasticity. *See* Ehrlich, *supra* note 15, at 408.

The inferences drawn by Bowers and Pierce, *supra* note 1, at 201-04, concerning murder rate and execution changes in particular states during the 1960's, are based on a faulty methodology. Their analysis of yearly changes in murder rates and executions in specific states over the six-year period 1962-1967 fails to control for the different trends in apprehension and conviction risks or in any other relevant determinants of murder across these states. It considers the absolute number of executions rather than the theoretically relevant conditional risk of execution in each state. It is based on comparisons of contemporaneous changes in executions and murder rates, comparisons which are not capable of identifying the relevant causal relationships in specific cases. Furthermore, the overwhelming trend in all the states compared over this subperiod was toward a complete cessation of executions. The occasional deviations from that trend in each state in a single year cannot serve as a statistically meaningful basis for the classification of states into those with increasing or decreasing executions. These fundamental shortcomings in Bowers and Pierce's independent analysis pertaining to the 1960's further underscores the importance of applying the *full* econometric framework utilized in my study over as long a period as the data permit in order to isolate the relation to the effect of the unemployment rate on the frequency of murder. While the association between the latter two variables is found to be positive over the entire sample period, this association weakens substantially when the subperiod relating to the 1930's is excluded from the observation set. The significance of the 1930's in connection with movements in the unemployment rate is well known.

B. A related consideration is that the estimates of the objective risk of execution show a strong time trend over the subperiod ending in the early 1960's. Specifically, the graph plotting the logarithms of these estimates over the time period from the late 1930's to the early 1960's appears to be nearly a negatively sloped straight line. Over the same subperiod the murder rate in the United States also exhibits a continual, systematic negative trend. Because of the significant negative trend in estimates of both the frequency of murder and the conditional risk of execution, the estimated effect of the latter on the former over this specific subperiod may simply reflect the effect of pure time trend.

The differences between regression results based on the full sample and those based on subperiods ending in the early 1960's, including those reported by Bowers and Pierce in their appendix B,[21] are consistent with the arguments developed above. Not only does the effect of execution risk appear to become quite weak as data points are deleted, but the effect of apprehension risk also becomes weak due to lack of variability. As indicated above, the effect of unemployment is also sensitive to the deletion of specific subperiods. More importantly, the negative partial effect of the time trend on the murder rate, which was verified over the entire period considered in my study, becomes "insignificant"[22] in Bowers and Pierce's regression results for subperiods ending in the early 1960's, although over those specific subperiods the murder rate continually declined. Hence the effect of pure trend should have been found to be even more pronounced over these periods. The fact that the effects of both the risk of execution and time trend weaken suggests a high degree of competition or multicollinearity[23] between the two variables over subperiods ending in the early 1960's.

C. The absence of variability and the presence of multicollinearity are hardly unique to my study but occur frequently in time series regression analyses. The conventional remedy to such problems is to *extend* the sample size so that variability may be enhanced and the separate effects of highly related explanatory variables may be extricated and identified. I have pursued this procedure from the outset in my efforts to extend the sample size into the early 1930's and up to the late 1960's. Bowers and Pierce pursue just the reverse course. Following my own report of weak results obtained from subperiods ending in 1963, they go on to perform most of their independent estimations over subperiods in which little meaningful analysis can be conducted.

D. As for the unfounded claim that somehow "significant" deterrent effects of execution risk and other punishment variables are "present" only in a short span of time relating to the 1960's and are not to be found in earlier (or later) periods, I plan to demonstrate in the near future, through evidence based on state-wide data in earlier years, that the deterrent effects of certainty and severity of punishment—including punishment by execution—are not uniquely associated with a specific set of data points.[24]

IV. The Merits of the Logarithmic Format

In the empirical implementation of my general theory of participation in illegitimate activities,[25] I consistently have emphasized a logarithmic-linear specification of the relevant equations. However, as my following comments will show, while the logarithmic-linear specification appears to be preferable on analytical and experiential grounds, the qualitative results from my time series study do not depend exclusively on this functional form.

The logarithmic-linear form can be justified on practical grounds if the elasticities of a dependent variable with respect to a set of explanatory variables are assumed to be constant to the first order of approximation. A logarithmic-linear specification is a superior regression format when the magnitude of the errors in the data are thought to be proportional to the level of the variables that the data purport

21. Bowers & Pierce, Deterrence, Brutalization, or Nonsense, 1975, at 21-26 (unpublished manuscript, Center for Applied Social Research, Northeastern Univ.) (appendix B on file with *Yale Law Journal*) [hereinafter cited as Deterrence].

22. The term "significance" here is used heuristically only to indicate relatively low standard errors. Standard tests of significance do not apply in connection with autocorrelation correction estimation procedures.

24. Further independent evidence on this point is provided by J. Yunker, The Deterrent Effect of Capital Punishment: Comment, Oct. 1975 (unpublished manuscript, on file with *Yale Law Journal*). Yunker's analysis, conducted over a sample period extending through 1974, yields results indicating that the murder rate declines as the risk of execution rises. The statistical methodology he uses, however, is different from my own.

25. *See, e.g.*, Ehrlich, *Participation in Illegitimate Activities: An Economic Analysis*, in ECONOMICS OF CRIME AND PUNISHMENT 68-134 (G. Becker & W. Landes eds. 1974) [here-

to measure. It would be rather implausible, for example, to assume that the magnitude of underreported and misreported crime is independent of the level of reported crime. This assumption is implicitly invoked by use of the linear specification in the natural values of the variables, the functional form stressed by Bowers and Pierce.[26] More plausibly, one may assume that the magnitude of reporting errors is proportional to the level of the relevant statistics.[27] Furthermore, in the case of murder investigation, the dependent variable of interest is the rate of capital murder rather than the rate of criminal homicide, the actual measure utilized. Previous researchers have assumed that the capital murder rate and the total homicide rate are proportionally related.[28] It would be convenient at least, then, to use a logarithmic-linear specification which enables a direct estimation of elasticities. Moreover, similar considerations also apply to the observed probabilities of apprehension, conviction, and execution, each of which is based upon data relating to reported willful felonious homicides rather than the true level of capital murders. All these considerations suggest that the efficient functional form underlying the murder supply function is likely to be one that utilizes the logarithms of the dependent variable and the key independent variables. And indeed, "prior information" accumulated through my past work on crime has led me to emphasize the logarithmic-linear regression format because of its observed relative efficiency.

Bowers and Pierce could have tested statistically for the optimal functional specification of the estimated supply-of-offenses function. In particular, they could have examined the efficiency of the logarithmic-linear specification relative to a specification that is linear in the natural values of all variables. In research now in progress, I have conducted statistical tests of optimal transformations based on a likelihood ratio method. As I plan to demonstrate, the conclusion emerging from these tests is that the logarithmic-linear format not only is decisively superior to the format using the natural values of the variables but that the former generally cannot be rejected as the optimal form within the class of single-parameter power transformations. Bowers and Pierce's demonstration that their regression results tend to deteriorate when running the regressions with natural numbers shows only that they evade the question of which of the two transformations is more appropriate. Hence they not only prefer testing the deterrence

26. Bowers & Pierce, *supra* note 1, at 199.

27. For an elaboration of this point, see Ehrlich, *Illegitimate Activities*, *supra* note 25, at Section IV, Note 1 [illegible]

hypothesis for subperiods of sharply limited usefulness, but they also prefer an inferior regression format.

As I noted in my paper, the basic results from that study were found to be unaffected qualitatively by the choice of functional form. The regression equation I report below was performed with the antilogarithms (*i.e.*, the natural numbers) of the same set of variables used to derive the results reported in my published paper, and indicates that the qualitative deterrent effects of apprehension, conviction, and execution are not exclusively dependent on a specific functional form.[29] Although the specification in natural numbers appears to be clearly inefficient in view of prior information and other tests, time series regression estimates derived via that format in this equation nevertheless indicate the existence of the expected deterrent effects.[30]

V. Basic Statistical Errors in My Critics' Work

Part II of Bowers and Pierce's critique refers to "errors" in my analysis. The fact is that I have learned of no single error in either my theoretical analysis or the statistical methodology used to implement the theory. In contrast, the work by Bowers and Pierce is riddled with errors and demonstrations of misunderstanding of basic statistical principles. A few examples will illustrate.

A. The "R^2" statistic is essentially irrelevant in connection with a two-stage least squares regression analysis or the related three-round procedure used in my study. Yet, with no qualification whatever, Bowers and Pierce cite this statistic in conjunction with every estimated equation which they report.[31] More seriously, they use the "R^2" statistic as a basis for inference.

B. The "R^2" statistics, as well as standard errors of the regressions, computed for subsamples having different ending dates are relied on

29.
$$q^* = \underset{(3.806)}{.215} - \underset{(-3.771)}{.000091\Delta^*P^*c|a} - \underset{(-1.573)}{.000830\Delta^*P^*a} - \underset{(-3.021)}{.00365\Delta^*PXQ_1} - \underset{(-2.150)}{.238\Delta^*L}$$
$$+ \underset{(2.128)}{.117\Delta^*A} + \underset{(6.501)}{.000115\Delta^*Yp} + \underset{(1.462)}{.000269\Delta^*U} - \underset{(-6.857)}{.00372\Delta^*T.}$$

The estimated equation above pertains to the effective period 1935-1969. The murder rate is represented by q^*; the risks of apprehension, conviction, and execution are represented by P^*a, $P^*c|a$, and PXQ_1, respectively; other symbols can be interpreted from Table 2 of my published paper, Ehrlich, *supra* note 16, at 409, and the numbers in parentheses denote ratios of estimated regression coefficients to their standard errors. The estimate for $\hat{\rho}$ is $-.119$. Not only is the direction of the effect of every variable the same as is found with the logarithmic-linear specification, but the predicted ranking of the elasticities of the measures of the three deterrence variables also is observed.

30. J. Yunker, *supra* note 21, performs regressions with natural numbers only. His regression analysis also indicates the existence of a deterrent effect associated with the [illegible]

by Bowers and Pierce to draw the startling inference that regression results are "more adequate and consistent for the periods with earlier ending dates."[32] Their lack of regard for, indeed their apparent lack of awareness of, the diminishing degrees of freedom associated with smaller and smaller subsamples is astonishing. Even if the regression analysis were based on the ordinary, or classical, least squares procedure, comparison of R^2 statistics and standard errors of the regressions across subsamples with successively smaller degrees of freedom would be improper: as Bowers and Pierce surely know, when the number of degrees of freedom is zero, the R^2 statistic necessarily is unity. It should be noted that the deletion of observations from the full set of observations led Bowers and Pierce to estimate regression equations in which the number of observations is as low as 22 while the number of parameters estimated in the reduced-form regression analysis is 19. They apparently fail to recognize that estimated regression coefficients based on successively smaller degrees of freedom become increasingly imprecise. In particular, estimates based upon so few degrees of freedom as those Bowers and Pierce stress are unlikely to be adequate for rejecting the hypothesis of no deterrent effects.[33]

C. At the end of their article, Bowers and Pierce report results from regressions in which they replace the conditional probability of execution—the theoretically relevant variable—with the absolute number of executions.[34] A finding of a positive association between homicide rates and the number of executions is cited as evidence of the "brutalizing" effects of capital punishment. Bowers and Pierce do not recognize, however, that the positive association may be expected on purely technical grounds. Where there are no homicides there can be no convictions or executions. In contrast, when murder rates rise, with arrest, conviction, and execution risks constant or rising, the number of convictions and executions also would rise. A positive (or zero) association between criminal homicides and the number of executions thus is hardly surprising. Indeed, more than 20 years ago this relationship was noted by Schuessler, who recognized that this correlation does not constitute a test of the deterrence hypothesis.[35]

32. Bowers & Pierce, *supra* note 1, at 190.

33. Unfortunately, on this point, as others, Baldus and Cole, *supra* note 2, at 180 & n.35, 184-85, uncritically accept the inferences drawn by Bowers and Pierce and other critics and, thus, implicitly commit the same errors. Without providing any valid grounds for their assertions, Baldus and Cole even more emphatically assert their judgment that the subperiod analysis provides estimates somehow superior to those obtained from the full sample.

For a definition of the term "degrees of freedom," see note 6 *supra*.

34. Bowers & Pierce, *supra* note 1, at 205 n.45; Bowers & Pierce, Deterrence, *supra* note 21, at 24 [illegible].

35. [illegible], *The Deterrent Influence of the Death Penalty*, 284 Annals 59 (1952), as well as by Baldus and Cole, *supra* note 2, at 183 n.45, who also fail to recognize the technical association between homicide and execution rates. It might be noted that Schuessler, at 59-60, went on to attempt a test of the deterrence hypothesis by measuring the simple correlation coefficient between homicide rates and a measure of the risk of execution in 11 executing states over the period 1937-1949. Although he found the correlation coefficient to be —.26, indicating that the murder rate falls as execution risk [illegible], and although this negative association turns out to be significant statistically at the five percent level for a one-tail test, neither Bowers and Pierce nor Baldus and Cole mention this result.

D. Although I have focused in this discussion on errors contained in the Bowers and Pierce paper, I should point out that the Baldus and Cole paper also is seriously flawed. For example, Baldus and Cole criticize my study because "the second-stage regression does not hold fixed" the cluster of variables denoted as X_2 in Table 2 of my published work.[36] However, these variables serve in the role of "omitted exogenous variables" in the murder supply equation. The remark shows that Baldus and Cole do not understand the simultaneous equation estimation framework underlying my study. Exogenous variables such as those summarized under X_2 *must* not be held constant in estimating the structural murder supply function. If they were, the two-stage least squares procedure would become meaningless. Their effects, however, are integrated appropriately in the estimation procedure through their incorporation in the reduced form regression analysis. This error by Baldus and Cole betrays quite a fundamental misunderstanding of the methodology which they have undertaken to evaluate. Indeed, in their comparisons of my research with that of previous researchers, they do not note the prime benefit of utilizing the simultaneous equation framework—that it attempts to identify the direction of the causal relationship between the frequency of murder and the deterrence variables. The direction of that relationship is not self-evident. For example, in times of high murder rates, states may be more inclined to convict and execute offenders, or even reinstate the death penalty, than in times of low [illegible]r rates. The simple association between these variables thus may give the appearance that, for example, an increase in the risk of execution "leads" to an increase in the murder rate, even though the opposite is true. A simultaneous equation estimation framework is designed to identify causal relations and thus avoid potentially biased results.

VI. The Gap Between Evidence and Inference in Previous Research

Bowers and Pierce and, in greater detail, Baldus and Cole, rely heavily on previous research, especially that of Thorsten Sellin, as

36. [illegible] & Cole, *supra* note 2, at 182 n.41.

evidence against the deterrence hypothesis.[37] In this section I shall briefly comment on the research by Sellin and others concerning the deterrence hypothesis.

The principal shortcoming of Sellin's research and related work is that the approach taken and the methods applied do not permit a systematic test of the main implications of the general theory of deterrence, a theory which posits that potential offenders respond to incentives. The shortcoming is basic because the implications following from the *general* deterrence hypothesis are what Sellin was attempting to challenge empirically.[38] Yet his work neither develops nor tests the full range of implications following from the theory that he attempts to reject; nor is a competing theory developed and tested. In addition to implying that punishments in general, and executions in particular, may deter crime, the general deterrence hypothesis provides the testable expectation that punishments imposed only with relative infrequency will have less impact than those imposed with a high degree of certainty. The theory of deterrence predicts not only the direction of effects of, for example, equal percentage changes in the relevant apprehension, conviction, and execution risks, but also the relative order of magnitude of these effects. Although the basic premise of these and other testable implications is that, on balance, potential offenders respond to incentives, Sellin never devised an analytical framework for rejecting this general hypothesis. More significantly, to my knowledge Sellin never reported any parametric or nonparametric statistical tests that could justify his rather strong conclusions. In all, there exists a considerable gap between the limitedly useful evidence provided by Sellin and others following his methodology, and the rather emphatic inferences drawn.

Two examples may illustrate this conclusion. Sellin has compared homicide rates in "abolitionist" and "retentionist" states, as defined by the legal status of the death penalty. But Sellin never accounts for the extent of the actual enforcement of the penalty in retentionist states. Yet by his own analysis, the suggested relevant variable is the risk of execution, not merely its legal status, for "were [the death penalty] present in the law alone it would be completely robbed of its threat."[39] The point is far from being subtle. In a number of "retentionist" states whose homicide rates are compared to neighboring abolitionist states, the execution risk was negligible throughout the

37. Baldus & Cole, *supra* note 2, *passim;* Bowers & Pierce, Deterrence, *supra* note 21, at [illegible] n.1, 2 n.6, 19 n.21.

38. See, e.g., Sellin, *Capital Punishment*, 25 Fed. Probation 3, [illegible]

[illegible]

entire period investigated by Sellin. For example, in Vermont, New Hampshire, South Dakota, and Nebraska, the death penalty was imposed only rarely after 1920; Massachusetts had no executions after 1917. Not surprisingly, with no allowance for actual execution risks, Sellin's simple graphical comparisons of homicide rates across different states do not *appear*[40] to uncover considerable differences. But could such observations justify his inference: "The conclusion is inevitable that the presence of the death penalty—in law or practice—does not influence homicide death rates?"[41]

This deficiency is even more glaring in tests attempted by Bowers in another work to determine the effect of the moratorium on executions in the United States.[42] Following Sellin's methodology, Bowers compares the levels of murder rates in nine arbitrarily chosen mixes of neighboring states in the four years preceding and subsequent to the judicial moratorium. He reports similar patterns in most groups. However, the plain fact is that *none* of the states in eight of the nine groups had a single execution throughout the period. And in the ninth group, Bowers creates a dubious distinction between New York, classified as abolitionist, and New Jersey and Pennsylvania, classified as retentionist, although New York ceased all executions in 1963—the same year as New Jersey and one year after Pennsylvania. That such comparisons are used as a basis for inference about the deterrent effect of capital punishment taxes one's imagination.

A second general shortcoming characterizing previous research on this issue by Sellin and others is the absence of any systematic standardization of data so that the effect of execution risk can be isolated from the effects of other factors that not only may influence the murder rate but also are expected to be systematically related to the risk of execution. Clearly aware of the general problem, Sellin has emphasized the need to compare states that are "as alike as possible."[43] However, his assumption that neighboring states satisfy this prerequisite is unacceptable. Pairs of neighboring abolitionist and retentionist states, such as Illinois and Wisconsin, Michigan and Indiana, or Massachusetts and Rhode Island, differ in their economic and demographic characteristics, in their crime rates and law enforcement activity, and presumably also in their medical services available to

40. As indicated above, Sellin's observations are not supported by statistical inference tests.

41. Sellin, *Executions in the United States*, in Capital Punishment 138 (T. Sellin ed. 1967).

42. W. Bowers, Executions in America 137-63 (1974). Baldus & Cole, *supra* note 2, at [illegible] n.1, cite this work by Bowers as corroboration of Sellin's finding[illegible]

victims of aggravated assaults.[44] In addition, as my analysis has shown,[45] variation in the legal status of the death penalty occasionally may be a result, rather than a cause, of changes in murder rates and thus may give rise to an apparent positive association between the two variables. For these reasons, the true effect of the death penalty on the murder rate cannot readily be inferred from simple comparisons of the sort performed by Sellin and others.

VII. The Proper Measurement of the Restraining Effect of Capital Punishment

My critics allude to my having inappropriately or incompletely estimated the deterrent effect of capital punishment because I neglected the interdependencies among apprehension, conviction, and execution risks.[46] The fact is that I have stressed and integrated these interdependencies both in my theoretical analysis and in the empirical investigation.[47] Both pairs of authors suggest that the only proper means of estimating the deterrent effect of the death penalty is by allowing apprehension and conviction risks to "vary" with execution risk rather than by holding them "constant," thereby estimating the "total" effect of the penalty. In fact, I have treated empirically both the "partial" effect of execution risk—by controlling for the apprehension and conviction risks—and the observed "total" effect—by estimating the effect of execution risk through a reduced form regression analysis controlling only for exogenous and predetermined variables. The former procedure is without question the only proper way of verifying the hypothesized deterrent effect of execution risk, for the theory predicts that an increase in the conditional risk of execution at *given* levels of apprehension and conviction risks would have a deterrent effect on the incentive to commit murder. For the quite distinct purpose of evaluating the overall desirability of capital punishment as an instrument of policy, both estimation procedures, in principle, can provide useful guidance. Indeed, both are reported in my study. Although the "total" effects associated with a few measures of execution risk appear somewhat smaller than their estimated "partial" effects, the quantitative differences are generally small.

It should be pointed out, in this connection, that my critics misconstrue the theoretical predictions relating to the "total" deterrent impact of the risk of execution. While unwarranted movements in execution risk are expected to induce opposite movements in apprehension and conviction risks, the magnitudes of the resulting effects will not necessarily offset the effect of the initial change in execution risk. Moreover, the theory implies that no such compensatory movements[48] are expected when the initial shift in execution is viewed as *warranted*. For example, if there were universal agreement that reinstatement of capital punishment under specific conditions were socially optimal, then there is no compelling reason to expect that juries would be less inclined to convict offenders charged with capital crimes. Levels of apprehension and conviction risks could also be maintained through an appropriate allocation of resources to specific law enforcement activities. Thus estimates of "partial" effects of execution risk, as well as of apprehension and conviction risks, provide useful information from a policy viewpoint.

Concluding Remarks

The basic issue underlying my theoretical and empirical investigation of murder has not been merely the deterrent efficacy of the death penalty but the more general issue of offenders' responsiveness to incentives. My time series analysis of the trend of murder in the United

44. The perfunctory analysis of Baldus and Cole, *supra* note 2, at 177 n.30, on this point shows little appreciation of the issues involved. At Table III their list of data purportedly for one year is a mix of data drawn from two different years (1960 and 1966). Yet the issue on which they are leveling evidence is the extent to which Sellin "controlled for other factors" in each and every year of the 40-odd years considered in his studies. There still exists substantial variation in the variables Baldus and Cole compare within the five groupings of states they consider. They have not presented data for the sixth grouping considered by Sellin. The data sources they cite for the construction of their permanent income measures for the states are not capable of generating such statewide measures. Their inferences about the impact of specific variables on the murder rate in each state are not based on any systematic analysis. In three of their five cases, a retentionist state has the lowest homicide rate within a particular grouping—a fact they choose not to mention. By Baldus and Cole's analysis this fact might be viewed as evidence for deterrence. I am not suggesting, however, that such inference can be justified given the fragmentary nature of the analysis. More generally, I question the logical basis for preferring a statistical method that provides only indirect and incomplete control for specific variables expected to have an impact on the crime rate over a regression analysis attempting to control these variables directly.

45. Cf. Ehrlich, *supra* note 16, at 406.

[illegible] *supra* note 21, at 6 n.9; Baldus & Cole, *supra* note

48. In reference to these possible compensatory changes in apprehension and conviction risks, both Bowers & Pierce, Deterrence, *supra* note 21, at 6 n.9, and Baldus & Cole, *supra* note 2, at 182 n.42, cite an exercise in an unpublished paper by P. Passell & J. Taylor, The Deterrent Effect of Capital Punishment: Another View, Feb. 1975 (Discussion Paper No. 74-7509, Columbia Univ. Dep't of Economics) (on file with *Yale Law Journal*), as evidence that the "total" effect of execution risk on the murder rate is likely to be positive. Ironically, neither pair of authors apparently understands the exercise by Passell and Taylor which has nothing to do with the "compensatory" changes discussed above. In fact, I believe that the exercise by Passell and Taylor is internally inconsistent and irrelevant, but since their paper has not been published yet I

states should not be evaluated as an isolated experiment but rather as part of the more general research into offenders' behavior. Viewed in this more general context, the new research by myself and other social scientists in recent years has lent considerable support to the proposition that, in the aggregate, potential offenders respond to both negative and positive incentives. The growing body of new research by economists and sociologists has indicated the existence of deterrent effects of severity and certainty of punishment as well as other systematic regularities attributable to the effects of incentives.[19] These conclusions have been demonstrated in studies using data from different times and different places. My research on the deterrence effect of capital punishment has produced results compatible with this new evidence.

The bulk of my critics' analyses challenges neither the theoretical formulation of the deterrence hypothesis nor the statistical techniques used in the theory's empirical implementation. Neither Bowers and Pierce nor Baldus and Cole[20] present valid tests that reject the deterrence hypothesis. I hope that their work does not baffle the lawyer pondering the merits of using an economic approach to law or of using statistical techniques to study legal questions. The statistical methodology is quite useful and relevant when appropriately applied. Contrary to the inferences of Bowers and Pierce, their study does not present statistically meaningful evidence that the risk of execution has no deterrent effect, let alone that it has a "brutalizing" effect, on the frequency of murder in the population.

In this reply, I have elaborated upon some aspects of my research which, because of space limitations, have not been fully discussed in my published paper on the death penalty. Needless to say, my discussion here is not a substitute for the published paper. The paper discusses the analytical framework underlying the general deterrence hypothesis, which is the main issue of concern in my research. It also elaborates upon the methodology needed for meaningful tests of that hypothesis. The paper also stresses the limitations of the empirical investigation and the tentative nature of the findings. Indeed, as I stated, conclusions based on studies of historical data necessarily are qualified in view of the difficulties of measuring efficient empirical counterparts of relevant theoretical constructs and in view of intrinsic limitations of statistical inference. However, data imperfections are likely to work *against*, not in favor of, the theorized deterrent effect of punishment and the effects of incentives in general. It is thus remarkable that the evidence of my time series analysis and additional research repeatedly has proven not inconsistent with rather sharp predictions emanating from the deterrence hypothesis. I have not claimed, however, that my research settles the issue of the deterrent effect of capital punishment. Nor have I advocated the use of capital punishment. As I stressed in my paper, the issue of deterrence is but one of a myriad of issues relating to the efficiency and desirability of capital punishment as a social instrument for combatting crime. The study of the deterrent effect of capital punishment is of considerable independent importance in connection with the hypothesis that potential offenders on the whole respond to incentives. Research on this issue undoubtedly will benefit in the long run from legitimate attempts to use more efficient data and statistical techniques than those heretofore employed in studies of capital punishment.

19. *See, e.g.*, B. Fleisher, The Economics of Delinquency (1966); The Economics of Crime and Punishment (S. Rottenberg ed. 1973); A. Smigel-Leibowitz, Does Crime Pay? An Economic Analysis, 1965 (unpublished M.A. thesis; Columbia Univ.); W. Vandaele, *supra* note 17; J. Yunker, *supra* note 21; Carr-Hill & Stern, *An Econometric Model of the Supply and Control of Recorded Offenses in England and Wales*, 2 J. Pub. Econ. 289 (1973); Gibbs, *Crime, Punishment, and Deterrence*, 48 S. Soc. Sci. Q. 515 (1968); Maxwell, Phillips & Votey, *Crime, Youth, and the Labor Market*, 80 J. Pol. Econ. 491 (1972); Tittle, *Crime Rates and Legal Sanctions*, 16 Soc. Prob. 409 (1972). *See also* Ehrlich, *Illegitimate Activities*, *supra* note 25, and references therein. The preceding is only a partial list of relevant studies in this area.

20. Baldus and Cole do not present evidence of their own. They rely mainly upon Sellin's work as evidence, which I discuss briefly at pp. 221-24 *supra*. Their discussion in Section IV merely repeats other authors' inferences and judgments concerning [illegible] which may prove to be unjustified or premature. It is beyond

to organizations which, unlike the typical business corporation, do not seek profit.

In these pages we have distinguished between public service organizations—institutions which channel the largesse of some [illegible] in the interest of others—and mutual benefit organizations, w[illegible] in purpose is to allow individuals to pool their income in order [illegible] spend it more efficiently. Public service organizations do not produce income which can easily be assimilated to the "profit" produced by a business for the benefit of investors. Even if this inherent unsuitability of an income tax were disregarded by imposing an arbitrary concept of "income" on their financial activities, the burden of the tax would not reflect the ability to pay of the individual beneficiaries.

The activities of mutual benefit organizations that consist simply in the members' doing together what they could do separately without income tax consequences (such as buying food or operating social facilities) are not fit objects of taxation. But such organizations may also do business with nonmembers or invest in assets which produce income inuring to the benefit of their members. There is no reason to permit income of these types, which would be neither excludable nor deductible from taxable income in the hands of an individual, to escape taxation when acquired under the umbrella of an organization. Frequently, however, the investment income of mutual benefit organizations would not be taxed if it were imputed to (or realized in the first instance by) the group's members, because it is used to defray expenses which the individuals would be entitled to deduct. One example is the investment income of a labor union, used to pay expenses serving the occupational interests of its members; another is a trade association's investment income, used for business expenses or to reduce the members' dues and assessments. In both cases, if the investment income were imputed to (or realized by) the members, they would be entitled to offsetting deductions under § 162. This is why to tax such income would be in effect to penalize taxpayers for doing together what they could do separately without being taxed. In sum, mutual benefit organizations should be taxed only to the extent of their investment income if such income would not be immune from tax in the hands of their members, or to the extent they do business with nonmembers in a way that produces something akin to "profit."

Comments

The Deterrent Effect of Capital Punishment: Ehrlich and His Critics

Jon K. Peck†

Editors' Introduction

In the December issue, the *Journal* published a statistical debate on the deterrent effect of capital punishment between Professor Isaac Ehrlich and two sets of critics, Messrs. Baldus and Cole and Bowers and Pierce. Professor Ehrlich's original study of capital punishment, published last spring in the *American Economic Review*, used sophisticated statistical techniques to arrive at conclusions very different from earlier research in the field.[1] Using regression analysis and economic theory, Ehrlich formulated and tested a model of the determinants of the murder rate, and found a significant deterrent effect associated with the use of the death penalty in the United States over the period from 1935 to 1969. The earlier research of Thorsten Sellin and others had consistently found no evidence of a deterrent effect. The issue of deterrence—in particular the technical merits of the Ehrlich study—has been raised in cases pending before the Supreme Court challenging the constitutionality of capital punishment.[2]

In their critiques in the previous issue of the *Journal*, Baldus and Cole argue that Ehrlich's approach is less appropriate for testing the deterrent effect of capital punishment than are the less complex techniques used in

† Assistant Professor of Economics, Yale University.

1. Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life or Death*, 65 AM. ECON. REV. 397 (1975).

2. *See* Editors' Introduction, *Statistical Evidence on the Deterrent Effect of Capital Punishment*, 85 YALE L.J. 164-69 (1975). Fowler v. North Carolina, *cert. granted*, 419 U.S. 963 (1974), *argued*, 43 U.S.L.W. 3582 (U.S. Apr. 21, 1975), *restored for reargument*, 422 U.S. 1039 (1975), which involves Eighth and Fourteenth Amendment challenges to capital punishment imposed under a mandatory sentencing procedure, was argued last spring and held over for reargument this Term. The Court has not yet scheduled the reargument in *Fowler*, but it recently granted certiorari in five other death penalty cases and scheduled them for argument on March 30. 44 U.S.L.W. 3429 (U.S. Jan. 22, 1976): Proffitt v. State, 315 So. 2d 461 (Fla. 1975), *cert. granted sub nom.* Proffitt v. Florida; Gregg v. State, 233 Ga. 117, 210 S.E.2d 659 (1974), *cert. granted sub nom.* Gregg v. Georgia; State v. Roberts, 319 So. 2d 317 (La. 1975), *cert. granted sub nom.* Roberts v. Louisiana; State v. Woodson, 215 S.E.2d 607 (N.C. 1975), *cert. granted sub nom.* Woodson v. North Carolina; Jurek v. State, 522 S.W.2d 931 (Tex. Crim. App. 1975), *cert. granted sub nom.* Jurek v. Texas. For a discussion of the facts of each of these cases, see N.Y. Times, Jan.

earlier research;[c] Bowers and Pierce adopt Ehrlich's approach but argue that no evidence of a deterrent effect is found when his method is correctly applied.[d] Ehrlich's reply defends the findings of his original study and attacks the analysis of his critics.[e] In this issue, Professor Peck comments on the debate between Ehrlich and his critics, and Professor Ehrlich adds a brief Rejoinder.

In his study of the deterrent effect of capital punishment, Professor Ehrlich estimated a sophisticated econometric model of a type common in more traditional areas of economic analysis.[1] As in any empirical analysis, his econometric specification and choice of data require assumptions beyond those which may be derived from his theoretical analysis. These assumptions concern the functional form of the relationship between the murder rate and its determinants, the form of the remaining but unestimated equations in the overall model of the supply and (negative) demand for murder, the nature of the random disturbance term, the stability of the coefficients over time, the accuracy and appropriateness of the data, and the consequences of aggregating the behavior of individuals to the national level. The correctness of many of these assumptions depends on the correctness of others in this list. Ehrlich's assumptions have been challenged by Bowers and Pierce and by Baldus and Cole in articles in the previous issue of this *Journal*. In this Comment, I will discuss some of their criticisms of Ehrlich's analysis, suggest ways of resolving these disagreements, and point out some weaknesses of the paired-state or matching method—the approach which Baldus and Cole prefer to Ehrlich's regression technique. In addition, I will address briefly Ehrlich's reply to the two critiques of his work.[2]

I. Ehrlich's Critics

1. Both critiques address the issue of the proper functional form of the relationship between the murder rate and its determinants. It is claimed that evidence of a deterrent effect is found with the logarithmic form but not with the linear form, and that therefore Ehrlich's results depend critically on his choice of functional form.[3]

It is quite true that the incorrect use of a logarithmic form can cause the relatively small values for execution risk in the recent years in Ehrlich's sample to appear to be statistical aberrations which strongly influence the regression line. While the critiques point out the difficulty with using a logarithmic form for values of execution risk approaching zero,[4] there are also problems with using the linear form for these values.[5] The theoretical analysis is not much help in choosing the correct functional form, but the data are. Statistical tests applied to the data can determine the best form over a range of possibilities which includes both the linear and logarithmic forms.[6] Of course neither form is likely to be exactly right, but only an approximately correct shape for the function is needed.

The related question of whether the same model adequately covers both the earlier and later sample periods—whether there is a structural change in the underlying relationship over time—is also a testable proposition. Bowers and Pierce find that the sign of the estimated elasticity of the homicide rate with respect to execution risk changes when the most recent years are dropped from Ehrlich's time series. They reject the possibility that the effect of the death penalty changed in recent years and conclude that the negative association between execution risk and the homicide rate is merely a statistical artifact.[7] But neither Ehrlich nor these critics have rigorously tested for structural change over the sample period. One such test recently reported found evidence of a significant structural shift between the periods 1933-1962 and 1963-1969.[8] In other words, in the recent subperiod the murder rate may be determined by different factors or may be determined by the same factors in a different way. However, the evidence of what appears to be structural change may also be the result of an incorrect specification of the model for the entire time period.

c. Baldus & Cole, *A Comparison of the Work of Thorsten Sellin and Isaac Ehrlich on the Deterrent Effect of Capital Punishment*, 85 YALE L.J. 170 (1975).

d. Bowers & Pierce, *The Illusion of Deterrence in Isaac Ehrlich's Research on Capital Punishment*, 85 YALE L.J. 187 (1975).

e. Ehrlich, *Deterrence: Evidence and Inference*, 85 YALE L.J. 209 (1975) [hereinafter cited as *Ehrlich Reply*].

1. An econometric model consists of equations derived from economic theory ... [illegible] using regression analysis. *See* Editors' Introduction, *supra* ...

3. Baldus & Cole, *supra* note c, at 181; Bowers & Pierce, *supra* note d, at 199-200.

4. *See* Baldus & Cole, *supra* note c, at 175 & n.22; Bowers & Pierce, *supra* note d, at 201-02. For a general discussion of this problem, see Young & Young, *Estimation of Regressions Involving Logarithmic Transformations of Zero Values in the Dependent Variable*, 29 AM. STATISTICIAN 118-20 (1975).

5. The linear form uses the natural values of all variables, including execution risk, which can never be less than zero. When execution risk approaches zero, the disturbance ... cannot assume large negative values. This constraint causes the estimated regression coefficients to have a systematic error. *See* H. THEIL, PRINCIPLES OF ECONOMETRICS 628-32 (1971).

6. *See* Box & Cox, *An Analysis of Transformations*, 26 J. ROYAL STAT. SOC'Y 211-52 (1964).

7. Bowers & Pierce, *supra* note d, at 200.

8. P. Passell & J. Taylor, The Deterrent Effect of Capital Punishment: Another View, ... [illegible] Columbia Univ. Dep't of Economics (on file

2. Bowers and Pierce stress the inaccuracy in Ehrlich's crime data for the early years in his sample.[9] The seriousness of this problem is impossible to judge. However, the FBI procedure—retroactively readjusting the early data based on analysis of the effects of later increases in the coverage of the reporting network—is similar to the accepted Census Bureau method of adjusting economic time series data for the effects of seasonal variation by using adjustment factors computed in part from later data. The time series generated by the FBI procedure may be amenable to regression analysis.

The inaccuracy of the data is compounded by the inability of Bowers and Pierce to replicate Ehrlich's estimates.[10] I do not know who is correct, but the differences seem too large to attribute to small differences in the definition of the variables or to correlations among the explanatory variables.[11] Ehrlich is clearly right in suggesting that the large differences in the estimates of the serial correlation coefficient reported by him and by Bowers and Pierce raise the question of error in the computation of this and other more important coefficients.[12] Good standards of documentation require that authors spell out exactly their computational procedures and identify the computer programs used.[13]

3. As Baldus and Cole emphasize, the level of aggregation of the analysis has an important effect on the validity of the conclusions.[14] If there are substantial state or regional variations in behavior, and if arrest and punishment for homicide primarily involve the behavior of individuals and states, then estimation of the model ought to be done at the level of the state or, better, the individual. Nothing in Ehrlich's general approach precludes an application of his model to more disaggregated data.[15] However, aggregate analysis is frequently

9. Bowers & Pierce, *supra* note d, at 187-92.

10. *Compare id.* at 195 n.19 (Table III) *with* Ehrlich, *supra* note a, at 410 (Table 3), *see Ehrlich Reply*, *supra* note e, at 210-11.

11. For an explanation of why this problem—known as multicollinearity—may lead to large differences in estimated coefficients with small differences in the data sample, see R. Wonnacott & T. Wonnacott, Econometrics 59-63 (1970).

12. *Ehrlich Reply*, *supra* note e, at 211. The serial correlation coefficient measures the correlation between successive values of the disturbance term. If there are computational errors in the calculation of this coefficient, there will also be errors in the estimated elasticities for the explanatory variables.

13. In his reply, Ehrlich does identify the computational procedures he used, and they appear to be reliable. *Id.* at 211 n.7.

14. Baldus & Cole, *supra* note c, at 175-77.

15. Ehrlich asserts that his new unpublished research using statewide data supports the findings of his original study. *Ehrlich Reply*, *supra* note e, at 217. Since I have not seen this work, I cannot comment on it. However, a recently published study based [illegible] theoretical model similar to Ehrlich's found no evidence [illegible] [illegible] Deterrent Effect of the Death Penalty: A Statistical Te[illegible]

performed in conventional economic problems with reasonable effectiveness. Such an analysis will be free of a statistical bias if the additional variables which ought to be included at the disaggregate (*e.g.*, state) level of analysis are unrelated to the explanatory variables at the aggregate level. However, the omissions will reduce the ability of the aggregate model to predict the murder rate.

4. The most basic point raised by Ehrlich's critics is the challenge by Baldus and Cole to his use of economic theory and regression analysis rather than experimentally based statistical procedures. The above enumeration of the problems with Ehrlich's analysis might suggest that an econometric approach to this problem is hopeless and that the paired-state comparisons endorsed by Baldus and Cole should prevail.[16] But there are also difficulties with the latter approach, especially in a situation in which some relevant variables cannot be controlled or even measured.

Ehrlich begins with a careful economic analysis of the determinants of murder. Many researchers, including some economists, might object that his theory applies at best only to a small subset of homicides and that he has failed to identify and take into account some important causal factors. To the extent that these variables can be specified and measured, they can be tested and controlled for; to the extent they cannot be so identified, one must assume they are not related to the other explanatory variables. If this assumption is false, Ehrlich's results are biased. For some of the variables, such as the urban migration rate, which Baldus and Cole suggested should be included,[17] the assumption seems plausible; for others, such as the level of crimes against property, the assumption seems implausible.

Any analysis must proceed conditionally on the specification of the model. Ehrlich's econometric approach involves many assumptions, some of which are testable; if these assumptions are true or nearly true,[18] his method is able to use them to detect effects which might be too small to detect in a less fully specified model. No one claims that the deterrent effect of capital punishment is large, and it is consequently very important to bring the data to bear on the question as efficiently as possible. Ehrlich imposes a theoretical framework on his empirical analysis and necessarily makes more assumptions than are required in the paired-comparison analysis. If these assumptions

16. The paired-state or matching method compares homicide rates in neighboring abolitionist and retentionist jurisdictions which are as alike as possible with respect to [illegible] influences on the homicide rate. *See* Baldus & Cole, *supra* note c, at 171-72, 177.

17. *Id.* at 180.

18. *See, e.g.*, Fisher, *On The Cost of Approximate Specification in Simultaneous*

are wrong, his conclusions are wholly or partially invalid. But many of these assumptions are testable.[19]

The matching approach has its own set of difficulties. It imposes relatively little explicit structure on the problem and is perhaps less likely than the econometric approach to find effects which are weak. The fundamental problem, however, is that the data are not generated in a controlled experiment. In making matched pairwise comparisons, the choice of pairs is inevitably subjective. To ensure that states are matched on all relevant variables requires a theory just as detailed as in an econometric analysis. In a classical experimental procedure, the control states and treatment levels are assigned randomly to observations to ensure that the effects of omitted variables are not systematic. This is obviously impossible in a "social experiment" where the death penalty is the treatment variable. Even if states are correctly matched in terms of the averages of all relevant variables, other differences may be important. For example, of two states with the same average permanent income, one may have a much greater proportion of low income families than the other. If low income families were disproportionately responsible for homicides, the pairing of the two would be inappropriate.

Another difficulty is the problem of spillovers between states. If state *A* has and uses the death penalty and its paired state *B* does not, it is possible that potential murderers would migrate to state *B* to avoid the death penalty and would thereby cause state *B* to have a higher homicide rate. One could not extrapolate from this comparison to a situation where all states or no states used the death penalty, since in that situation no one could avoid the penalty by migrating to an abolitionist state. Put another way, a murderer might be determined to commit the crime, regardless of the penalty, but might choose to commit it in the state where the consequences are less costly to him. The results of this behavior would give the appearance of a deterrent effect when none is present.

A final difficulty is the possible response of punishment policies to homicide rates. For example, if high or rising homicide rates led states to institute the death penalty and low or declining rates led states to abolish the penalty, retentionist states would tend to have higher homicide rates. This relationship could cancel out a possible negative correlation which would be produced if the penalty were in fact a deterrent. The paired-comparison approach cannot adequately

19. The argument for assuming the validity of certain basic (and statistically untestable) economic propositions, in order to test more specific hypotheses is summarized in

separate these effects, and consequently could fail to yield evidence of an underlying deterrent relationship. On the other hand, the particular regression procedure used by Ehrlich specifically takes account of the possible response of punishment variables to the murder rate.[20]

Any properly executed statistical analysis must include careful diagnostic checking of the model and consideration of alternative models which might also be consistent with the data. When using aggregate time series data, a number of different specifications are usually in general accord with the evidence. Thus the choice among competing models must be based on underlying theoretical analysis and on such empirical clues as particular data points which do not fit the model, correlations among explanatory variables, and stability of the important results under alterations in untestable and weakly maintained assumptions. For the reader to understand and evaluate the author's conclusions, some summary statement of the author's examination of this evidence is vital.[21] Any sound regression analysis must provide such a statement. Diagnostic procedures may be less necessary for a paired-comparison analysis, which requires fewer assumptions, but the diagnostics should include, for example, an investigation of the sensitivity of the findings to the choice of pairings.

II. Ehrlich's Reply

1. In his original article, Ehrlich emphasized as support for his analysis that the ranking in order of magnitude of the estimated effects of his three deterrence variables conforms to his theoretical predictions.[22] In his reply, he emphasizes that Bowers and Pierce, in attempting to replicate his results, obtained the same ranking for these estimates.[23] However, he does not show that this ranking would be inconsistent with other theories of the relationship between capital punishment and the murder rate. In the absence of such a demonstration, this evidence does not provide the strong support he claims.

20. In a regression equation in which the dependent variable (here the murder rate) exerts a causal influence on an explanatory variable (here the fraction of those convicted of murder who are executed), the equation must be estimated as if embedded in a larger model which simultaneously determines both variables. Ehrlich used a procedure appropriate for simultaneous equation estimation. *See* Ehrlich, *supra* note a, at 406; *Ehrlich Reply*, *supra* note c, at 219 (use of three round regression procedure related to two-stage least squares).

21. In his reply, Ehrlich states that the results of diagnostic tests performed in the course of his research now in progress show that the logarithmic form is optimal for estimating his equation. *Ehrlich Reply*, *supra* note c, at 218 (likelihood ratio tests for optimal functional form within the class of single-parameter power transformations).

22. Ehrlich, *supra* note a, at 414-416.

2. Ehrlich argues that the presence of errors of measurement in a variable can only weaken the variable's estimated effect and hence that improvements in the data could only strengthen his conclusions.[24] Economists are nearly always forced to use imperfect data and yet have drawn many strong empirical conclusions from them. But only the most harmless sorts of measurement error unambiguously reduce the estimated effects of a variable. If measurement errors in a variable are correlated with each other over time or are systematically related to other variables in the analysis, reducing the measurement errors will not necessarily strengthen the estimated effect. For example, when crime rates are high, the police may feel increased pressure simply to arrest someone for a crime even if that person is subsequently released. In Ehrlich's model the probability of arrest of the murderer, which is measured by the percent of murders cleared by an arrest, would then contain a measurement error correlated with the homicide rate. Because of the possibility of nonrandom measurement errors, Ehrlich should not assume that the true effects of execution risk and the other deterrence variables are systematically larger than the estimated effects.

3. Ehrlich argues that Bowers and Pierce have selectively deleted observations in order to distort his empirical findings.[25] However, examining subsets of the data to see if they are mutually consistent is an important part of validating a regression analysis. It is particularly important to examine the recent data, which in Ehrlich's analysis are most affected by the logarithmic transformation and which are in any event most relevant for policy analysis. Ehrlich correctly points out that estimation from a subset will be inefficient because of the reduced variability in the sample,[26] but he overlooks the purpose for estimating over subperiods—checking the specification of the model. When he analogizes the subperiod estimations performed by Bowers and Pierce to the selective deletion of observations in a regression analysis of the relationship between corn prices and quantities of corn demanded, he misses the point. In the terms of his example, Bowers and Pierce are asking whether the nature of the demand for corn has changed since the explosion in food prices of a few years ago; they are not trying to reverse the slope of the regression line by eliminating particular data points that give the line a negative slope.

4. Ehrlich defends his use of the logarithmic form for estimating his equation, but he does not address the problem of how to treat very small or zero values in the variables, a problem which is central to the criticisms made by Bowers and Pierce.[27]

5. Ehrlich attacks the conclusion of Bowers and Pierce that the regression results are better for samples which omit the data from the 1960's.[28] While he correctly identifies statistical errors in their discussion, their basic finding—that whether the estimated effect of execution risk on the murder rate is positive or negative depends on the ending point of the sample period—casts doubt on the stability of Ehrlich's results.

None of the studies considered here can be said to have resolved the question whether the death penalty deters murder. The regression approach can be improved by better specification, better diagnostic testing and disaggregation. The analysis should consider time series data on states in ways that to some extent bring it closer to the paired-comparison method. Thus a resolution and synthesis of these approaches may be possible. Ehrlich concludes in his reply that no statistically meaningful evidence has been presented against his analysis and that his analytical framework withstands the criticisms raised against it. Without denying the usefulness of his approach, which appears to me potentially fruitful, I believe that his particular finding of a deterrent effect rests on as yet inadequately tested assumptions and on an incompletely validated model. I shall await with considerable interest his further contributions on the questions raised in this debate without, for the moment, concluding that he has established a statistically significant deterrent effect.

Finally, I would like to note that according to Ehrlich's equations, a one percent change in per capita income or labor force participation has a much greater effect on the homicide rate than does a one percent increase in the use of capital punishment.[29] Even if the deterrent effect of capital punishment were of statistical significance, it may be so small relative to other influences on the murder rate that it is of little practical significance.

24. *Id.* at 213, 227.

25. *Id.* at 209, 214-17.

26. [illegible] It should also be noted that there are alternative estimation pro-

27. *See* p. 361 *supra*. Ehrlich also asserts that using the natural rather than logarithmic values of the variables yields evidence of a deterrent effect. As independent support for this conclusion, he cites an unpublished paper by J. Yunker, The Deterrent Effect of Capital Punishment: Comment, Oct. 1975 (unpublished manuscript, on file with *Yale Law Journal*). I believe Ehrlich would reject the analysis in that paper on grounds similar to his criticism of Sellin's work. Further, Yunker's conclusion that "the real [deterrent] effect is at least five times the size of the effect estimated by Professor Ehrlich" is unmitigated conjecture. *Id.* at 17. The paper also appears to contain technical errors.

28. *Ehrlich Reply*, *supra* note c, at 219-20.

29. *See* Ehrlich, *supra* note 3, at 409-10 (Tables 2 & 3) (elasticities for labor force participation and per capita income substantially larger than for conditional probability of execution).

Rejoinder

Isaac Ehrlich†

I appreciate Professor Peck's undertaking to comment on my reply, published in the last issue of this *Journal,* to critics of my work on murder and capital punishment. I wish to address briefly only a few of his remarks.

Peck suggests that the results of my regression analysis concerning the ranking of estimated elasticities of the murder rate with respect to measures of apprehension, conviction, and execution risk could be consistent with alternative theories of crime. In principle, such a possibility never can be denied. I believe his criticism would have been more constructive had he identified what specific theories he has in mind. Unfortunately, he errs in stating that I have not examined this possibility myself. In my own study, I have addressed the issue of whether a theory of crime based upon incapacitating effects of punishment could produce the same ranking of elasticities observed in the study.[1]

I believe that Peck mistakenly ascribes to me an argument that I have not advanced. I have not argued that "errors of measurement" can *only* work against the hypothesized deterrent effect. My reference to the classical case of "errors of measurement" has been quite specific. Curiously, contrary to his own argument, Peck's example in which errors of measurement in apprehension risk are *positively* correlated with the murder rate[2] does, of course, further illustrate why "errors of measurement" are likely to work against the hypothesized deterrent effect of apprehension. My basic point has been that if the variables used in the empirical analysis grossly misrepresent the theoretical variables that they purport to measure, then there is no reason to expect findings consistent with the detailed theoretical predictions.

I am glad Peck agrees with my criticism of statistical errors committed by my critics in their inferences from regression analyses of

† Associate Professor of Business Economics, University of Chicago; Research Associate, National Bureau of Economic Research.

1. Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life or Death,* 65 Am. Econ. Rev. 397, 413-14 (1975).

2. Peck, *The Deterrent Effect of Capital Punishment: Ehrlich and His Critics,* 85 Yale L.J. 359, 366.

subperiods of the full sample.[3] I do not agree with him, however, that my critics' work has addressed the issue of the stability of the regression equation, since they report no tests of stability. Nor do any of the works cited by Peck provide valid tests of stability. While I could not address this technical issue directly in my reply, I did indicate what I believe are the basic reasons for the apparent weak results obtained from analysis of certain specific subperiods.

Many difficulties in research on criminal behavior should rightfully occupy the thoughts of interested scholars, and skepticism is the essence of scholarly work. I am grateful to the editors of the *Yale Law Journal* for allowing me to express my skepticism on some aspects of the work that has been conducted in this area and to elaborate upon my own research.

3. *Id.* at 367.

It may also be of some interest to note that the test of optimal transformations in connection with the form of the regression equation the Box and Cox procedure which Peck suggests as the way of determining the efficiency of the logarithmic-linear form is the same one to which I have already referred in my reply. *Compare id.* at 367 *with* Ehrlich, *Deterrence: Evidence and Inference,* 85 Yale L.J. 209, 218 (1975). Peck does not recognize, however, that the problem of treating zero values of variables can be overcome through an appropriate transformation.

V. Conclusion

This Article concludes this four-part series on oligopolistic pricing. As has been demonstrated, contrived oligopolistic pricing suits will create many allocative and distributional benefits, particularly when they are brought in industries in which D/QV and P/MC are higher than average. Therefore, I have no doubt about the desirability of prosecuting oligopolistic pricing under the Sherman Act whenever it can be proved that explicit anticompetitive threats or promises have been communicated. In fact, although any final conclusion about this issue must await further evidence about the cost of proving illegal oligopolistic pricing whenever verbal threats or promises cannot be proved directly, I suspect that it also would prove desirable to bring (explicitly or implicitly) contrived oligopolistic pricing suits under the Sherman Act on the basis of the kind of circumstantial evidence described in Part III—at least in industries in which D/QV is not too far below average. Indeed, I even think a very good case can be made for passing new legislation that would condemn natural as well as contrived oligopolistic pricing—or at least for trying to prepare the way for such legislation by attempting to educate the business community and the general public to recognize the harmful effects of this practice and, concomitantly, the desirability of its prohibition. In any case, I believe that it would be desirable to prohibit the kind of premature announcement of prices that facilitates natural oligopolistic pricing.[38] I hope that this Article has established not only these results but also the usefulness of applying a sophisticated welfare economics framework when trying to evaluate micro-economic policies.

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38. On the other hand, I doubt the desirability of prohibiting a seller from taking advantage of the fact that his rivals would not charge his customers conventional competitive prices even [illegible] would not respond to their behavior, and I am certain that one should not preclude firms [illegible] advantage of their own competitive advantages. In brief, I would oppose both such prohibitions because I suspect that they would tend to result in too little QV investment being made in a world in which oligopolistic pricing was prohibited and consumer surplus was abundant. *See* Clear *but, supra* note 11, at 37–38; Three Types, *supra* note 20.

The Deterrent Effect of the Death Penalty: A Statistical Test*

Peter Passell†

Any number of arguments may be brought to bear on the issue of capital punishment. One might, of course, oppose or favor the use of the death penalty on moral considerations alone. Equally reasonably, one's opinion might be shaped by how fairly the punishment is administered in practice; the penalty may be applied selectively[1] or reserved for a narrow range of crimes that resist precise legal definition.[2] For many people such arguments alone are not likely to be persuasive. Execution may be commonly viewed as a distasteful alternative to other forms of punishment, but an alternative that could be defended on pragmatic grounds, if the gains in crime prevention are sufficiently great. Thus, informed public opinion might in theory be strongly influenced by evidence of capital punishment's capacity to deter murder and other crimes. If the death penalty were shown conclusively to deter violent crime, many people with moral or legal reservations would accept the costs of the deterrent to obtain its benefits.

The deterrent effect of the death penalty is of particular interest today because courts and legislatures are in the process of redefining the circumstances under which convicted felons may be executed.[3] While the legal status of the death penalty could be determined by arguments unrelated to

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* The author is extremely grateful for the assistance of Lee Friedman, David Kennett, and John [illegible] in the preparation of this Article.

† B.A. 1966, Swarthmore College; Ph.D. 1970, Yale University; Assistant Professor of Economics, [illegible] University.

1. Indeed, the possibility of selective application was the rationale of Furman v. Georgia, 408 U.S. 238 (1972): "[W]e know that the discretion of judges and juries in imposing the death penalty enables the penalty to be selectively applied . . . ." *Id.* at 255. "[T]hese discretionary statutes are unconstitutional in their operation. They are pregnant with discrimination and discrimination is an ingredient not compatible with the idea of equal protection of the laws that is implicit in the ban on 'cruel and unusual' punishments." *Id.* at 256–57.

2. *See* C. Black, Capital Punishment: The Inevitability of Caprice and Mistake 19 (1974) ([illegible] of standards defining homicide).

3. Since the holding of *Furman* that statutes allowing discretionary application of the death penalty are unconstitutional, *see* note 1 *supra*, the state legislative response has been to enact statutes [illegible] imposing the death penalty for conviction of certain crimes. Thirty states have enacted [illegible] statutes, *see* Brief for Petitioner at Appendix A, Fowler v. North Carolina, No. 73-7031 (U.S. [illegible]).

[illegible] current death penalty litigation centers on *Fowler*, a convicted murderer's challenge to the North Carolina death penalty statute. The basis of the challenge is that given *Furman*'s proscription [illegible] applied statutes, statutes enumerating crimes for which the death penalty is mandatory [illegible] infirm because they do not eliminate the discretion inherent in arrest and prosecution, [illegible] charge measure determines the crime for which one is convicted and thus in turn whether the [illegible] death will be imposed. *See* Brief of Petitioner, *supra* at 16–101. Argument was heard on [illegible] 21, 1975, but a decision was postponed and reargument was scheduled, [illegible] deterrence,[4] to the extent that pragmatic concerns are influential[5] the issue could turn on current research in the area.

I. Isaac Ehrlich and His Critics

Opponents of capital punishment can point to a substantial body of empirical literature in psychology and criminology that finds little evidence of a deterrent effect.[6] Recently, however, an economist, Isaac Ehrlich, has completed what is claimed to be an objectively superior statistical test of capital punishment deterrence in the case of murder.[7] When applied to data collected for the United States over the past four decades, Ehrlich concludes that a deterrent effect is discernible.[8]

Prior to Ehrlich's study researchers attempted to test deterrence hypotheses by comparing murder rates in jurisdictions with capital punishment and those without,[9] or murder rates in the same jurisdiction before and after capital punishment was abolished.[10] Another approach has been to compare murder rates before and after death sentences were imposed, or before and after executions actually took place.[11] Under all three approaches, the goal has been to isolate the effect of the death penalty threat from other factors that might explain variations in homicide rates.

While matching culturally similar states or contiguous time periods in a single city or state may be a reasonably effective device for holding social and economic factors constant, it typically also means that very few observations will be available. This data scarcity makes rigorous statistical hypothesis testing very difficult. Hence, even were one to accept without qualification the validity of such research designs and the accuracy of the

4. See notes 1 & 3 supra.

5. While pragmatic arguments are traditionally grist only for the legislative mill, even the Supreme Court has discussed the deterrence question, although the legal issue before the Court was framed in terms of the constitutionality of discretionary application. See, e.g., Furman v. Georgia, 408 U.S. 238, 345-55 (1972) (Marshall J., concurring).

6. The principal contributor to this literature is Thorstein Sellin. See, e.g., T. Sellin, The Death Penalty: A Report for the Model Penal Code Project of the American Law Institute (1959); Capital Punishment (T. Sellin ed. 1967); Sellin, Capital Punishment, 25 Fed. Probation at 3 (Sept. 1961). See also W. Bowers, Executions in America (1974); P. Passell & J. Taylor, The Deterrent Effect of Capital Punishment: Another View, March 1975 (unpublished Columbia University Discussion Paper 74-7509) reprinted in Reply Brief for Petitioner, at App. E, Fowler v. North Carolina, No. 73-7031 (Oct. Term 1974); Bailey, Murder and the Death Penalty, 65 J. Crim. L. & C. 416 (1974).

7. Ehrlich, The Deterrent Effect of Capital Punishment, 65 Am. Econ. Rev. 397 (1975). See also I. Ehrlich, The Deterrent Effect of Capital Punishment: A Question of Life or Death, Nov. 1973 (National Bureau of Economic Research Working Paper No. 18) reprinted in Solicitor General's Brief for the United States as Amicus Curiae, Fowler v. North Carolina, No. 73-7031 (Oct. Term 1974).

8. See Ehrlich, The Deterrent Effect of Capital Punishment, 65 Am. Econ. Rev. 397, 415 (1975).

9. See T. Sellin, supra note 6, at 23-34; Capital Punishment, supra note 6; Sellin, supra note [illegible]

10. See T. Sellin, supra note 6, at 34-52; Sellin, supra note 6, at 5-6.

11. See, e.g., Dann, The Deterrent Effect of Capital Punishment in Friends Society Series Bull. No. 29 (1935); Graves, The Deterrent Effect of Capital Punishment in California, in The Death Penalty in America (H. Bedau ed. 1967); Savitz, A Study in Capital Punishment, 49 J. Crim. L.C. & P.S. 338 (1958).

data employed, their evidence against deterrence could not be considered conclusive.

Ehrlich's approach—following from recent theoretical contributions to the explanation of illegal behavior[12]—is very different. Rather than using the legality or simple frequency of the application of the death penalty (executions per murder) as a surrogate for the subjective threat of capital punishment, Ehrlich employs executions per homicide conviction. Instead of comparing similar jurisdictions or time periods as a device for holding other explanatory variables constant, he specifies an additional set of explanatory variables (income, unemployment, age structure, etc.) and then uses the statistical technique of multivariate regression to isolate the impact of the death penalty deterrence variable. Observation points consist of annual data for the entire United States covering the time period 1935 to 1969.

Ehrlich's conclusions are as different as his technique. While Sellin and others found no evidence of deterrence, Ehrlich discovers a statistically significant trade-off between the murder rate and executions per conviction. The trade-off implies that a 1.00 percent increase in the execution rate will reduce murders by about 0.06 percent.[13] This translates into the "eight lives saved per execution" figure boasted by the Justice Department[14] and reported in the press.[15]

Ehrlich's findings have been criticized on a number of grounds.[16] These critiques find that Ehrlich's results are extremely sensitive to the choice of the time period included in the regression analysis[17] as well as to the mathematical specification of the regression model[18] and note that Ehrlich assumes the causes of murder are the same in all parts of the United States.[19]

12. See, e.g., The Economics of Crime and Punishment (S. Rottenberg ed. 1973).

13. Ehrlich, supra note 8, at 414.

14. See Solicitor General's Brief for the United States as Amicus Curiae, Fowler v. North Carolina, No. 73-7031 (Oct. Term 1974).

15. See Washington Post, April 13, 1975, § A, at 1, col. 1; Los Angeles Times, May 5, 1975, at [illegible].

16. Two types of critical response have been forthcoming. First, economists have found fault with Ehrlich's model. See P. Passell & J. Taylor, supra note 6. Second, scholars have questioned the use of econometric models for death penalty research, preferring instead comparative studies such as those by Sellin. See D. Baldus & J. Cole, A Comparative Analysis of the Empirical Work of T. Sellin and I. Ehrlich as a Basis for Inferring a Causal Relationship Between Capital Punishment and the Murder Rate in the United States, May 1975 (unpublished paper on file at the Univ. of Iowa).

17. See P. Passell & J. Taylor, supra note 6, at 4c-5c.

18. See P. Passell & J. Taylor, supra note 6, at 6c-7c.

19. See D. Baldus & J. Cole, supra note 16, at 19. By the choice of time series observations for the United States as a whole, Ehrlich is, in certain respects, able to make best use of this data, in [illegible] respects not. For example, one might expect annual variations in U.S. unemployment rates to [illegible] more accurately differences in unemployment than variations in unemployment rates between [illegible] the same year.

On the other hand, the value of aggregate United States time series data for the purpose of [illegible] testing is limited by the need to make a number of assumptions. First, one must assume [illegible] structure of the system explaining murderers' behavior remains the same, or virtually the same, for the entire time period. Second, there must exist a national market for murder in the sense [illegible] change in some national aggregate (per capita income, unemployment, etc.) must have nearly [illegible] effect on murders no matter what the geographic location of the change. One extra execution

Ehrlich also is subject to criticism for his use of Federal Bureau of Investi-
gation crime data.[20] Furthermore, as Ehrlich has noted himself, the trade-
off derived between murders and executions depends upon holding constant
all other variables in the system, including arrest and conviction rates.[21]
Even a modest reduction in conviction rates associated with a rise in execu-
tion rates might reverse the impact of the deterrent; Ehrlich's work is there-
fore consistent with the conclusion that more executions would result in
more murders.[22]

Hence, from a conservative scholarly perspective, little has been demon-
strated to date about capital punishment deterrence. Opinions, pro or con,
must be derived from other sources, empirical or theoretical. Nonetheless,
some might argue that Ehrlich has made a prima facie case for the de-
terrent effect of the death penalty and therefore the burden shifts to the
opposition to find contradictory evidence. However one chooses to evaluate
Ehrlich's research and that which preceded it, the desirability of additional
tests of the deterrence hypothesis using different data is clear. This Article
presents such a test using multivariate regression analysis on cross-section
state data for the United States for the census years 1950 and 1960.

II. A Model of Murder

Consider the model

$$(1) \qquad q/n = q(p, s, z),$$

where murders per capita (q/n) are a function of the subjective proba-
bility of punishment (p), the severity of punishment for those convicted
(s), and a vector of socio-economic variables (z). At this level of gen-

must have an almost identical impact on total murders, whether the execution takes place in Nebraska or Louisiana.

This is more restrictive than the assumption of a similar structure in every state, since aggregation of similar non-linear systems could introduce errors on its own. Suppose for example, that doubling the number of executions from one to two would have the effect of cutting murder rates by ten percent in either Maine or Louisiana. Suppose further there are 500 murders each year in Louisiana and 100 in Maine. An increase of one execution in Louisiana would reduce total murders by 50, while an increase of one execution in Maine would deter only ten murders.

In addition, the use of time series data creates purely econometric problems, introducing additional complexity to the interpretation of estimated coefficients. While the value of any econometric technique employed is dependent on a pyramid of assumptions, (1) hypothesis testing with time series estimators is conditional on a larger set of these assumptions; (2) interpretation of estimates will be more dependent upon large sample properties.

20. While use of Uniform Crime Reports as a data base for any study is subject to criticism because the index contains only *reported* crimes, Ehrlich's study is particularly infirm because he used data from the early years of the index when, due to a smaller and less representative sample, it was [illegible] less accurate than it is today. *See* Staff Report to Nat'l Comm'n on the Causes & Prevention of Crimes of Violence (1969).

21. Ehrlich, *supra* note 8, at 415.

22. *See* part V *infra*.

erality the model differs little from crime models proposed by Becker[23]
and tested by Ehrlich.[24]

Specifying the model more fully,

$$(2) \qquad q/n = q(p, t, e, a, i, m),$$

where p = the perceived probability of punishment,
t = the perceived prison sentence for those not executed, given conviction for murder,
e = the perceived probability of execution given conviction,
a = a demographic adjustment for otherwise unaccounted-for variations between age groups in the propensity to commit murder,
i = an economic adjustment for otherwise-unaccounted-for variations between income groups in the propensity to commit murder,
m = an adjustment for the proportion of the population which has few family or social ties in the community.

A theoretical justification of equation (2) merits little space here because the ground is covered thoroughly elsewhere.[25] The variables p, t, e are all deterrence variables in the sense that each influences the expected loss to the murderer. Variable p is the product of two variables, the subjective probability of arrest, and the probability of conviction given arrest used by Ehrlich.[26]

A demographic variable a is included to increase the efficiency of the estimators of the principal deterrence variables, since young adults are believed more prone to commit violent crimes.[27] Similarly, some adjustment i

23. Becker, *Crime and Punishment: An Economic Approach*, 42 J. Pol. Econ. 169 (1968).

24. *See* Ehrlich, *supra* note 8; Ehrlich, *Participation in Illegitimate Activities: A Theoretical and Empirical Investigation*, 81 J. Pol. Econ. 521 (1973). Note, however, that this Article argues that there exists no purely economic explanation of murder in contrast to sociological or psychological explanations. Presumably, the features that distinguish economic models in this context are assumptions of rationality—attempted maximization of perceived self-interest. Some notion of utility maximization, some consistent relationship in which individuals are influenced by their perceived self-interest does lie behind the model proposed here. At the same time the model is sociological and psychological, since some of the environmental variables considered below could easily be fitted to sociological and psychological explanations of violence.

25. *See* Ehrlich, *supra* note 8; Ehrlich, *supra* note 24; Mathieson & Passell, *Homicide and Robbery in New York City*, 5 J. Legal Studies — (forthcoming 1976).

26. This Article and Ehrlich's study both use objective probabilities as estimates for subjective probabilities in the models. State-by-state data on arrest rates, needed to separate objective arrest and conviction probabilities, are not available. Ehrlich was able to separate the two probabilities because they are available, year-by-year, for the United States as a whole. Note, however, since our theory is on [illegible] variable, the subjective probability of execution given conviction, this lack of data makes [illegible] difference.

27. This might simply be because the "price" of crime is lower for them—they may lose less [illegible] by spending time in prison; their sentences may be less harsh; they may systematically perceive

for the income of the observed population might be justified, since poor people generally commit more crimes.[28] Ehrlich included such demographic variables in another cross-section study.[29]

None of these variables, however, accounts for the individuals who are undeterred by social sanctions against murder for other reasons such as the absence of strong family ties or lack of friends and acquaintances in the community. Including such non-economic variables may be crucial if the model is to generate useful estimates of the deterrent effects. If a relevant variable is excluded from the model to be tested, estimates of the coefficients of the variables that are included may be seriously biased, as may be estimates of the standard errors.[30] The problem is quite intuitive. For example, Ehrlich's failure to control for such variables as increased racial tension and private ownership of handguns during the 1960's[31] might have led to spurious results. The decreased use of the death penalty might explain the jump in the murder rate during the period, but so might factors that created the general rise in crime.[32]

III. The Data

Data that might be used to estimate models of deterrence are largely limited to statistics collected through the agencies of the Justice Department. This study uses cross-section state data for the continental United States in 1950 and 1960, with 41 observations for the former year, 44 for the latter.[33] Although cross-section data are not subject to some of the infirmities of time series data,[34] the former have problems of their own. Many institutional sources of murder rate variation[35] cannot be controlled for. In addition, some states have only a few observations of variables, increasing possible measurement error. Despite unavoidable difficulties, estimates based on the cross-section should provide valuable counterpoint to alternative deterrence hypothesis tests.

a lower probability of arrest and conviction. However, it is not necessary to accept this economic view to believe that age composition affects murder rates; it may simply be a sociological phenomenon.

28. Again such an adjustment could be justified in opportunity-cost terms, *see* note 27 *supra*, since poor people lose less by being imprisoned, or in socio-psychological terms if poor people are more prone to violence due to frustration, fatigue, and other factors.

29. *See* Ehrlich, *Participation in Illegitimate Activities: A Theoretical and Empirical Investigation*, 81 J. Pol. Econ. 521 (1973).

30. *See, e.g.*, J. Johnston, Econometric Methods (2d ed. 1972).

31. *See* Ehrlich, *supra* note 8, at 406–08, 414 n.15.

32. I am indebted to Brian Forst for this point.

33. In 1950 data were incomplete for: Georgia, Idaho, Michigan, North Dakota, Rhode Island, South Dakota, Vermont; in 1960: Idaho, New Jersey, North Dakota, Vermont.

34. *See* note 6, *supra*.

35. A model utilizing cross-section data shares with models dependent upon aggregate US data a hypothesized lack of homogeneity which is not captured by the included environmental variables (poverty, age distribution, migrant population). Therefore this study adds a dummy variable (D_s) in some estimated forms to allow for differences among ten Southern states (Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas, Virginia) and the rest of the country. $D_s = 0$, not Southern; $D_s = 1$, Southern.

For convenience, observed surrogates for the model variables are printed in upper case.

Q/N = murder and non-negligent manslaughter offenses per capita reported to the FBI.[36] This statistic is an inefficient estimator for state-to-state variations in q/n because: (1) some fraction of murders is not reported to local law enforcement agencies or is misclassified; (2) some local agencies do not report crime statistics to the FBI, thereby introducing sampling error into state data; (3) manslaughter, one component of Q, is not a capital crime, and is therefore unlikely to be affected by punishments for capital crimes. If such errors are random, however, Q/N will yield unbiased estimates of variations in q/n.

$P = C/Q$ = the ratio of state prison commitments for murder and manslaughter to reported murders and non-negligent manslaughters.[37] Two factors reduce the efficiency of P as an estimate of p. First, the commitment statistic for murder and manslaughter is not an accurate measure of convictions for murder and non-negligent manslaughter: the crime categories are defined differently and commitments may not take place in the same year as convictions. Second, variations in the objective probability of conviction may not accurately reflect variations in the subjective probability of conviction. Note once more, however, that only systematic errors will introduce bias into the estimation procedure.

T = the mean (1960)[38] or median (1951)[39] number of months spent in prison by convicted murderers released that year. T is an inefficient estimator of t for the same reasons that P is inefficient for p. Additional sources of inefficiency include: (1) for some states T is based on sentences served by only a few released prisoners; (2) time spent in prison may not constitute a homogeneous punishment across states, since some prison systems treat prisoners more harshly than others; (3) sentences served by prisoners released this year but convicted years earlier reflect both current and past attitudes toward sentencing.

36. Statistics on murders and non-negligent manslaughters per 100,000 of population are based on U.S. Dep't of Justice, Fed. Bureau of Investigation, Uniform Crime Reports: Crime in the U.S. (published annually) [hereinafter cited as UCR]. UCR 1960, at 38–52, Table 3; UCR 1959, at [illegible], Table 3; UCR 1956, at 89, Table 31; UCR 1950, at 90, Table 32; UCR 1949, at 94, Table 36; UCR 1946, at 97, Table 33.

37. Statistics on commitments to state prisons for murder and manslaughter are from U.S. Dep't of Justice, Bureau of Prisons, Characteristics of State Prisoners 50–51, Table A5 (1960); U.S. Dep't of Justice, Bureau of Prisons, Prisoners in State and Fed. Institutions 73–74, Tables 40, 41 (1950).

38. The source of data for this variable is U.S. Dep't of Justice, Bureau of Prisons, Prisoners Released from State and Fed. Institutions 15–18, Table 3 (1960).

39. The source of data for this variable is U.S. Dep't of Justice, Bureau of Prisons, Prisoners Released from State and Fed. Institutions 24–27, Table 5 (1951). (No data were available for 1950.)

$E_t = (X_{t-1} + \dots X_{t+2})/4C_t$, a four-year average of executions for murder divided by current convictions.[40] A four-year average is used (1) because of long lags between sentencing and execution, (2) because of the extremely small number of executions in any given year, (3) because it seems reasonable to assume some lags in the formation of subjective expectations of execution given conviction.

A = the percentage of the resident population between the ages of 15 and 24.[41] A serves as an estimate for a, which presumably is some weighted average of the age distribution of the population. The weights are unknown because the ratio of crimes among age groups attributable to otherwise omitted variables is unknown. Thus, the study uses the portion of the population in this high murder-prone age group.

I = the percentage of the family population below an arbitrary cash income poverty line (1950 = \$2000, 1960 = \$3000).[42] The use of the nominal dollar statistic clearly produces an inefficient estimate of i, but it is not apparent that the error will be systematic.

M = the ratio of net non-white migrants in the previous ten years to total population.[43] Where net migration is negative, M is set equal to zero. M is offered as an estimator for the percentage of the population that consists of the displaced and rural poor who have moved to urban areas. Net total migration would not serve this purpose, since much of the population movement of the 1940's and 1950's consisted of suburban expansion and middle-class migration to the West. The estimate is relatively crude since it does omit poor white migrants and intra-state rural-urban migrants.

IV. Ordinary Least Squares (OLS) Estimates

A. *The Deterrent Effect of the Conditional Probability of Execution*

For the equation

$$Q/N = Q(P, T, E, A, I, M) \tag{3}$$

we would by hypothesis expect $\partial Q/\partial P < 0$, $\partial Q/\partial T < 0$, $\partial Q/\partial A > 0$,

40. Between 1959 and 1962 statistics on executions for murder are based on U.S. Dep't of Justice, Bureau of Prisons, Nat'l Prisoner Statistics, Executions (published annually) [hereinafter cited as Exec]. Exec No. 23, Feb. 1960, at 5, Table 4; Exec No. 26, March 1961, at 5, Table 4; Exec No. 28, April 1962, at 5, Table 4; Exec No. 32, April 1963, at 5, Table 4.

Between 1949 and 1952 statistics on executions for murder are based on Exec No. 5, April 1951, at 4, Table 4; Exec No. 6, Sept. 1952, at 5, Table 4; Exec No. 8, April 1953, at 5, Table 4.

41. Data for this variable are taken from U.S. Bureau of the Census, General Characteristics of the Population, U.S. Summary 167, Table 59 (1960); *id.* at 112, Table 62 (1950).

42. The statistics on the percentage of families below the poverty line are based on U.S. Bureau of the Census, County and City Data Book [hereinafter cited as County]. For 1960 I was calculated to be equal to the percentage of families with incomes below \$3000 in 1959 as reported in County 1962 at 3, table 1, vol. 23. For 1950 I was calculated to be equal to the percentage of families with incomes below \$2000 in 1949 as reported in County 1952 at 3, table 1, col. 21. Price deflators by state were not available.

43. Statistics for net non-white migration during the previous ten years as a percentage of total population for both 1950 and 1960 were based on U.S. Bureau of the Census, Statistical Abstract of the U.S. 40, Table 35 (1961).

$\partial Q/\partial I > 0$, $\partial Q/\partial M > 0$.[44] If capital punishment serves as a deterrent, other factors held constant, we would expect $\partial Q/\partial E < 0$. In other words, for an estimate of a linear form for equation (3), the capital deterrence hypothesis would tend to be confirmed if the coefficient of E were negative, rejected if the coefficient of E were zero or positive.

Results. Ordinary least squares estimates of the constant term (α) and the coefficients (β)[45] for the simple linear form of equation (3) for 1950 and 1960 are shown as Table 1, equations (a1) and (a2) with t statistics given in parentheses.[46] With the exception of the execution variable coefficient (and the coefficient of A in [a1]), all the estimated parameters are of the hypothesized sign and are significantly different than zero at the 90 percent confidence level.[47] The estimated coefficient of E, however, takes on a perverse sign and is not significantly different than zero:[48] no capital punishment deterrent is in evidence. Note too that the statistical significance of the other parameter estimates and the percentage of variance explained is virtually unchanged when E is excluded from the regression; results are shown as equations (b1) and (b2).[49]

44. Readers not familiar with calculus should note that the symbol $\partial Q/\partial E$ represents the partial derivative of Q (the murder rate) with respect to E (the execution rate). A partial derivative is a mathematical expression of the relationship between changes in an independent variable (here E) and changes in a dependent variable (here Q), all other independent variables (*i.e.*, P, T, A, I, M) being held constant. The symbol $\partial Q/\partial E$ represents the change in the murder rate that would result from a small change in the execution rate. The hypothesis that $(\partial Q/\partial E) < 0$ means that the murder rate would be expected to move in the opposite direction from changes in the execution rate, *e.g.*, as the execution rate rises (falls) the murder rate would be expected to fall (rise).

45. The Greek letter α is used here to refer to the constant term to be estimated in a regression; β refers to the coefficient vector (one or more coefficients) to be estimated in a regression.

46. The numbers presented in Table 1 represent econometric estimates of the constant term and coefficients of the murder equation. These numbers thus represent estimates of the relationship between the murder rate (dependent variable) and the various independent variables hypothesized to influence it. Estimates with positive signs support the hypothesis of a direct relationship between the murder rate (Q) and the independent variable in whose column the estimate is listed, *e.g.*, as the independent variable rises (falls) the murder rate rises (falls). Conversely, estimates with negative signs support the hypothesis of an inverse relationship between the murder rate and the independent variable. Therefore, to verify the hypothesis that increases in the execution rate result in decreases in the murder rate, the numbers in the column marked "Execution" must have negative signs. The larger the absolute value of the estimates, the greater will be the magnitude of the indicated relationship; in other words, for a given change in the independent variable there will be a larger change in the murder rate. The t-statistics given in parentheses provide indications of the degree of confidence that each estimate is significantly different from that which would be obtained by chance.

47. From the perspective of hypothesis testing or prediction, the value of statistical estimates of coefficients depends upon one's confidence that errors in data or misspecification of the model do not strongly affect the result. Given certain technical assumptions about the data and the model, it is possible to estimate confidence intervals around point estimates, *i.e.* a range of values in which the true coefficient lies. Hence the assertion here that the estimated coefficients are significantly different from zero at the 90 percent confidence level means that the probability that the true value takes on the opposite sign is less than 10 percent. For a discussion of confidence intervals and statistical significance see [illegible], *supra* note 30, at 135-52.

48. Acceptance or rejection of an hypothesis (for example, "the coefficient of E is less than zero") depends upon the statistical standard one sets. By convention and common sense, it seems appropriate to reject the hypothesis that E is less than zero unless it is statistically significantly less than zero at [illegible] percent confidence level. For a discussion of hypothesis testing with statistical methods see [illegible], *Forecasting and Analysis with an Econometric Model*, 52 Am. Econ. Rev. 104 (1962).

49. Addition of the Southern dummy variable (D_s) to the basic form, as shown in Table 1, [illegible] (c1) and (c2), makes little difference. Estimates of the coefficient of D_s imply that, other things being equal, Southern states have about 2½ extra murders per hundred thousand population

TABLE 1

ORDINARY LEAST SQUARES ESTIMATES OF MURDER EQUATION

Equation/Year	R^2	Constant	P	T	Execution	A	I	M	D_S
a1 1950	.698	-9.48 (-1.27)	-3.51 (-3.12)	-.0161 (-2.11)	8.48 (1.04)	7.44 (1.48)	.204 (4.38)	1.72 (2.22)	
a2 1960	.833	-2.18 (-4.47)	-3.82 (-2.14)	-.0140 (-4.13)	2.98 (0.26)	1.86 (5.35)	.169 (5.08)	2.27 (4.43)	
b1 1950	.689	-9.14 (-1.23)	-3.63 (-3.24)	-.0163 (-2.14)		.731 (1.46)	.207 (4.43)	1.93 (2.59)	
b2 1960	.833	-22.14 (-4.77)	-3.79 (-2.16)	-.0140 (-4.18)		1.89 (5.72)	.169 (5.14)	2.32 (5.02)	
c1 1950	.720	-4.86 (-0.62)	-3.04 (-2.67)	-.016[illegible] (-2.18)	7.75 (0.96)	.496 (0.96)	.159 (2.97)	1.51 (1.97)	2.59 (1.60)
c2 1960	.360	-15.1 (-2.90)	-4.68 (-2.78)	-.0134 (-4.27)	4.87 (0.45)	1.45 (4.06)	.122 (3.41)	1.95 (3.97)	2.40 (2.66)

The coefficient estimates obtained in Table 1 are for the most part not surprising, since they largely confirm earlier cross-section regression results on murder rates.[30] The deterrent effect of the likelihood of punishment and the length of prison sentences is supported, as is the influence of environmental variables that meet common sense criteria. The contrast with Ehrlich's time series confirmation of the deterrent effect of capital punishment is, however, quite striking. This highlights the importance of testing the sensitivity of our linear cross-section estimates both for different formulations of the capital punishment variable and different mathematical forms of the variables in the linear least-squares fit.

B. *Legal or* De Facto *Existence of Capital Punishment*

Suppose potential murderers are not influenced in a simple linear fashion by the frequency of execution of convicted murderers, but by the mere presence of a capital punishment statute on the books. Or, suppose that the fact that influences potential murderers is *de facto* abolition: whether executions of convicted murderers actually take place. Some states in 1950 and 1960 had death penalty statutes, but records free of executions for many years.

To test these hypothethical forms of the deterrent, dummy variables D_{e1} and D_{e2} were substituted for E. For states that did not permit capital punishment in 1949–52, 1959–62, $D_{e1} = 0$; for states that did, $D_{e1} = 1$. For states in which no executions took place 1949–52, 1959–62, $D_{e2} = 0$; for states in which they did take place, $D_{e2} = 1$.[31] Hence the deterrent hypothesis would be confirmed by statistically significant estimates of the coefficient of D_{e1} and D_{e2} less than zero.

Results. Regressions showing the alternative specifications are shown in Table 2, equations (d1), (d2), (e1), (e2).[32] Either hypothetical form for the deterrence variable must be rejected. In none of the four cases do statistically significant estimates appear.[33]

...ally. Regressions with dummy variables for two other regions (New England, Northern industrial states) were also attempted, with and without the Southern dummy. Their coefficients, however, were statistically insignificant and they did not markedly change the estimates of the other coefficients. Note, too, that the inclusion of the Southern dummy does not increase the statistical significance of the execution variable.

30. *See* Ehrlich, *supra* note 29; Mathieson & Passell, *supra* note 25.

31. Information needed to determine the states in which the death penalty is not applicable for murder, and states in which the death penalty is not applied for murder is found in Exec, *supra* note [illegible]. D_{e1} for 1950 and D_{e2} for 1949–52 were obtained from Exec No. 12, April 1955, at 3–4, Table 3; D_{e1} for 1960 and D_{e2} for 1959–62 were obtained from Exec No. 32, April 1963, at 4, Table 3.

32. Note that the Southern dummy variable (D_s) is included in equations (d1), (d2), (e1), and (e2) because, as may be inferred from Table 1, the variable explained an additional portion of the variation in murder rates. When D_s is excluded, however, the results are much the same; neither the signs nor the statistical significance of the coefficient estimates for the execution dummies are altered.

33. At the 95 percent confidence level.

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...ly. Regressions with dummy variables for two other regions (New England, Northern industrial states) were also attempted, with and without the Southern dummy. Their coefficients, however, were statistically insignificant and they did not markedly change the estimates of the other coefficients. Note, too, that the inclusion of the Southern dummy does not increase the statistical significance of the execution variable.

50. *See* Ehrlich, *supra* note 29; Mathieson & Passell, *supra* note 25.

51. Information needed to determine the states in which the death penalty is not applicable for murder, and states in which the death penalty is not applied for murder is found in Essec, *supra* note 41. D_a for 1950 and D_e for 1949-52 were obtained from Essec No. 12, April 1955, at 3-5, Table 3; D_a for 1960 and D_e for 1959-62 were obtained from Essec No. 32, April 1963, at 4, Table 3.

52. Note that the Southern dummy variable (D_s) is included in equations (d1), (d2), (e1), and (e2) because, as may be inferred from Table 1, the variable explained an additional portion of the variation in murder rates. When D_s is excluded, however, the results are much the same; neither the sign nor the statistical significance of the coefficient estimates for the execution dummies are changed.

53. At the 95 percent confidence level.

TABLE 2

STABILITY OF ORDINARY LEAST SQUARES ESTIMATES

Equation/Year	R^2	Constant	P	T	Execution	A	I	M	D_S
d1 1950 (with E_{D1})	.714	-4.84 (-0.61)	-3.14 (-2.74)	-.0159 (-2.11)	.410 (.479)	.473 (.910)	.162 (2.98)	1.67 (2.22)	2.68 (1.64)
d2 1960 (with E_{D1})	.862	-15.1 (-2.99)	-4.84 (-2.85)	-.0140 (-4.34)	-.493 (-.73)	1.50 (4.40)	.121 (3.39)	2.05 (4.60)	2.30 (2.54)
e1 1950 (with E_{D2})	.715	-4.81 (-.61)	-3.01 (-2.58)	-.0158 (-2.10)	.548 (.59)	.470 (.91)	.157 (2.90)	1.71 (2.28)	2.73 (1.67)
e2 1960 (with E_{D2})	.861	-15.4 (-3.06)	-4.65 (-2.77)	-.0139 (-4.24)	-.256 (-.51)	1.50 (4.37)	.122 (3.40)	2.04 (4.56)	.237 (2.62)
f1 1950 (10th root)	.705	.791 (.471)	-.606 (-2.75)	-.359 (2.73)	.0832 (2.42)	.107 (.08)	.930 (2.39)	.0208 (.57)	
f2 1960 (10th root)	.597	-1.41 (-.74)	-.692 (-2.18)	-.103 (-2.49)	.105 (3.18)	1.99 (1.34)	.536 (1.85)	.00179 (.05)	

C. *Sensitivity to Alternative Mathematical Forms*

A standard alternative to the simple linear form for regressions $[Y = \alpha + \Sigma_r \beta_r X_r]$ where X here indicates the right-hand-side variables, is to estimate a form that is linear in the natural logarithm of the variables $[\ln Y = \ln\alpha + \Sigma_r \beta_r \ln X_r]$. Ehrlich used the log form exclusively. This is not possible for our cross-section sample because a large number of observations have $E = 0$, for which the log is undefined. Another possible method of testing the sensitivity of the result, and one which is arguably more general, is to estimate an equation linear in the nth power transformation of the actual variables $(0 < n \leq 1)$ $[Y^n = \alpha + \Sigma_r \beta_r (X_r)^n]$. For small values of n, this will approximate the log transformation; for $n = 1$, it is just the linear form shown above.

Results. Equations (f1) and (f2) show the parameter estimates for $n = 0.1$ (10th root). Similar results, incidentally, hold for $n = 0.5$ (square root). Note that $\hat{\beta}_E$ is greater than zero, while the signs and significance of the other variables are not radically changed. Perhaps the most striking aspect to (f1) and (f2) is that the estimates of $\hat{\beta}_E$ are not merely positive, but statistically significant.[54] However, this is not taken to mean that executions cause murders; a more reasonable explanation is that the equations are misspecified. It does suggest that some systematic relationship between murder rates and execution rates may exist. Perhaps high murder rates cause high execution rates. One aspect of the question, the simultaneous determination of Q/N and E, is explored in the next section. Another aspect, the possibility that executions in no way affect murder rates, but that murder rates affect execution rates, is explored in Section VI.

V. Two Stage Least Squares (TSLS) Estimates

A. *Value of TSLS Estimates*

It is well known that the ordinary least squares estimates derived in the previous section will be inconsistent if murder rates are determined simultaneously with one or more of the right-hand-side variables. Risking oversimplification, the estimated coefficients will be inconsistent if the direction of causality goes from left-hand-side to right-hand-side as well as the opposite. Ehrlich very explicitly develops a theoretical simultaneous system in which potential criminals respond to deterrents and society responds to criminal behavior by adjusting the magnitude of the deterrents.[55] Thus murder rates "feed back" on execution rates and conviction rates.

54. At the 95 percent confidence level.
55. *See* Ehrlich, *supra* note 8, at 405.

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A prima facie case can certainly be made for the use of alternative estimation procedures to OLS. It is very important to keep in perspective, however, the desirability of using OLS even when some degree of simultaneity is suspected. Virtually any equation to be estimated belongs in theory to a system of simultaneous equations with feedback mechanisms. Exogeneity (determination outside the system) is a relative, not an absolute, notion. The quantitative importance of the bias built into the OLS estimators will depend upon the specific characteristics of the system in question.

Consider equation (3). Surely A, I, and M are nearly perfectly exogenous, though one could argue that high murder rates might conceivably affect migration. The cases of the three deterrence variables are ambiguous. Prison sentences served by persons currently being released might be partly influenced by current crime rates, as more plausibly might execution rates and conviction rates. Hence, the importance of the feedback is unclear.

It is also useful to remember that the commonly used simultaneous equations procedures such as two stage least squares are designed to generate only consistent and asymptotically efficient (in the limited information sense) estimates of the model parameters. Stated less technically, this means that the estimators will have the desired properties only if the sample of observations is sufficiently large. There is no absolute definition of "large," but one might suspect that a sample as small as the one here is not very large. Finally, one should note that TSLS estimators of the endogenous variable coefficients may not have even asymptotically desirable properties if the endogenous variables are inherently bounded, and the variables often have observed values near the bounds. All of the potentially endogenous variables in our model are bounded at zero, there being no meaning for example to a negative execution or conviction rate. Approximately one-fourth of the observed values have $E = 0$. Alternative procedures to TSLS do exist, but they are computationally cumbersome and not currently available as computer algorithms.[56]

B. *Results*

The basic results for the murder equation are presented in Table 3. The exogenous variables included in the system but excluded in the structural equation are:

F = median nominal family income for the previous year,[57]

V = percentage of voting age population voting in national elections,[58]

56. *See* Amemiya, *Multivariate Regression and Simultaneous Equation Models When the Dependent Variables are Truncated Normal*, 42 ECONOMETRICA 999 (1974).

57. Median family income for the previous year, in both 1960 and 1950, in nominal dollars, is based on U.S. BUREAU OF THE CENSUS, STATISTICAL ABSTRACT OF THE U.S. 317, Table 449 (19[illegible]).

58. Data for 1960 for this variable are from U.S. BUREAU OF THE CENSUS, STATISTICAL ABSTRACT OF THE U.S. 375, Table 600 (1972). For 1950 the data are from U.S. BUREAU OF THE CENSUS, STATISTICAL ABSTRACT OF THE U.S. 114, 115, Tables 407, 411 (1956).

H = percentage of residents living in the same house for at least the specified time period,[59]

W = the percentage of the resident population which is white,[60]

U = unemployment rate,[61]

L = percentage of persons 14 and over in the civilian labor force,[62]

Q^* = fractional change in murder rates over the past four years.[63]

These variables were chosen because we believe that they would belong in other equations, were it possible to specify the entire system of relationships determining variations in the endogenous variables. Note that the choice of these excluded exogenous variables is potentially important to the estimation procedure. For example, many of Ehrlich's[64] results may have depended upon the inclusion of the previous year's murder rate in a system explaining this year's murder rate.

Equations (g1) and (g2) in Table 3 are identical in form to (c1) and (c2), except an instrument ($\hat{E}$) is substituted for E in the regression. In other words, only the murder rate and execution rate are considered endogenous to the system. Once again, on the basis of the estimates one must reject the hypothesis that executions deter murders. As in (c1) and (c2) the estimated execution coefficients are positive and insignificant. In fact, the TSLS procedure appears to have very little impact on the regression coefficients in general; the equation seems to be insensitive to estimation procedure as well as to mathematical form.

Equations (h1) and (h2) go a step further, preserving the specification of the model but assuming P, T, and E to be determined simultaneously with Q/N. Again, there are few surprises. The execution coefficient remains statistically indistinguishable from zero. The significance levels of the coefficients of $\hat{P}$ and $\hat{T}$ are somewhat lower, but this would be expected in any event since the TSLS estimation procedure reduces the efficiency of the estimators.

Another indication of the relative stability of the relationship is found in (j1) and (j2). Here again, as an example of the power transformation discussed above, the 10th roots of the right-hand-side variables are regressed against 10th roots of murder rates, with execution rates assumed endoge-

59. The data for 1960 are from U.S. BUREAU OF THE CENSUS, *supra* note 41, at 257, Table 112; 1950 data are from *id.* 1950, at 121, Table 70.

60. The data for 1950 and 1960 are from U.S. BUREAU OF THE CENSUS, STATISTICAL ABSTRACT OF THE U.S. 28, Table 30 (1972).

61. Data for the unemployment variable in 1960 are from COUNTY, *supra* note 42, 1962 at 4, Table 1, col. 35; 1950 data are from *id.* 1952 at 4, Table 1, cols. 32, 36.

62. 1960 data are from U.S. BUREAU OF THE CENSUS, *supra* note 41, at 264, Table 119; 1950 data are from COUNTY 1952, *supra* note 42, at 4, Table 1, cols. 32, 33.

63. [illegible] = ([illegible] − [illegible])/[illegible]; [illegible] = ([illegible] − [illegible])/[illegible]. [illegible] is from UCR 1950, at 90, Table 14; [illegible] is from UCR 1946, at 97, Table 33; [illegible] is from UCR 1960, at 18, 32, Table 3; [illegible] is from UCR 1956, at 89, Table 31.

64. *See* Ehrlich, *supra* note 50.

TABLE 3

TWO-STAGE LEAST SQUARES (TSLS) ESTIMATES OF MURDER EQUATION

Equation/Year	$\bar{R}^2$	Constant	P	T	Execution	A	I	M	D_S
g1 1950 ($\hat{E}$)	.720	-4.79 (-.61)	-3.06 (-2.67)	-.0162 (-2.18)	6.53 (.52)	.493 (.96)	.159 (2.97)	1.54 (1.92)	2.60 (1.60)
g2 1960 ($\hat{E}$)	.860	-14.9 (-2.61)	-4.71 (-2.76)	-.0134 (-4.27)	6.48 (.30)	1.44 (3.62)	.122 (3.41)	1.92 (3.20)	2.41 (2.65)
h1 1950 ($\hat{P}$, $\hat{T}$, $\hat{E}$)	.667	-6.29 (-.65)	-5.70 (-2.41)	-.0169 (-1.28)	12.94 (.88)	.704 (1.15)	.161 (2.71)	1.29 (1.37)	1.52 (.78)
h2 1960 ($\hat{P}$, $\hat{T}$, $\hat{E}$)	.818	-8.23 (-1.06)	-10.2 (-1.99)	-.0157 (-2.07)	-9.20 (-.34)	1.20 (2.46)	.0985 (2.26)	1.42 (1.84)	2.96 (2.40)
j1 1950 (10th root; $\hat{E}$)	.674	1.22 (.67)	-.508 (1.99)	-.349 (-2.52)	.148 (1.87)	-.0992 (-.07)	.722 (1.80)	-.00248 (-.05)	
j2 1960 (10th root; $\hat{E}$)	.417	.851 (.34)	-.689 (-1.80)	-.115 (-2.31)	.240 (3.47)	.336 (.17)	.426 (1.21)	-.0239 (-.50)	

rous. As in (f1) and (f2), the coefficients of $\hat{E}$ are positive and statistically significant.

VI. An Execution Equation

Sections IV and V provide evidence that capital punishment does not act as a deterrent. Among the regressions in Tables 1–3 there are no instances in which the execution variable is negative and statistically significant. There are, however, reasons to believe that execution rates and murder rates are not entirely independently determined. Certainly the major impetus for new capital punishment legislation has been public frustration with the explosive growth of murder rates in the past decade.

Perhaps a more testable hypothesis about the relationship is that execution rates—executions per conviction—are a positive function of murder rates because of the attitudes of prosecutors, juries and judges. Given the discretion built into the system at each level it is plausible that, other things being equal, more convicted murderers will be sentenced to death the higher the perceived murder rate.

A proper statistical evaluation of the explanation of variations in the execution rate between states is beyond the scope of this paper. Among other purely econometric problems, the execution rate is a limited dependent variable. As mentioned earlier, since E is frequently equal to the bound value, the estimated coefficients will be biased. It is possible and relevant, however, to explore the issue in a tentative fashion, ignoring these difficulties.

Only a few of a number of alternative regression formulations proposed are shown here. Equations (k1) and (k2) in Table 4 show the OLS estimators for E as a linear function of murder rates lagged by one year, $(Q/N)_{-1}$, and M, the non-white migration variable. Equations (m1) and (m2) are basically the same formulation, but are estimated using TSLS with current murder rates as an endogenous variable. Neither the (k) nor (m) equations provide much insight into the sources of variation in execution rates, though in all four equations the coefficient of M, as would be expected, is significantly positive.

The addition of I, the percentage of the population below the poverty line, and Q^*, the rate of change of murders in the previous four years (estimates not shown) adds no explanatory power. Nor—in seeming contradiction to conventional wisdom—does a Southern states dummy variable. If one eliminates executions for rape and burglary, the South shows no particular propensity to execute convicted criminals.

Equations (n1) and (n2) produce a more interesting result. The TSLS estimators on the 10th roots of the variables are significant and of the ap-