11-1-1989

Persistent Flaws in Econometric Studies of the Deterrent Effect of the Death Penalty

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PERSISTENT FLAWS IN ECONOMETRIC STUDIES OF THE DETERRENT EFFECT OF THE DEATH PENALTY

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The research of Stephen Layson[1] gives criminologists a strong feeling of *deja vu*. Fifteen years ago, Isaac Ehrlich caught the criminological community by surprise with the dramatic finding that each execution could deter up to eight homicides.[2] At that time, most criminologists believed that deterrence research was fairly unequivocal—virtually none of it showed that the threat of execution discouraged potential murderers. The most unsettling feature of the initial aftermath of Ehrlich's findings was that most death penalty scholars found themselves ill-equipped to comprehend, much less evaluate, Ehrlich's sophisticated statistical approach. Thus, before more quantitatively capable critics could refute his work,[3] Ehrlich's conclusion had already influenced both popular and ju-

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1. Layson, *Homicide and Deterrence: A Reexamination of the United States Times-Series Evidence*, 52 S. ECON. J. 68 (1985).

2. *See* Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life and Death*, 65 AM. ECON. REV. 397, 398 (1975).

3. *See, e.g.*, 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); Barnett, *Crime and Capital Punishment: Some Recent Studies*, 6 J. CRIM. JUST. 291 (1978); Beyleveld, *Ehrlich's Analysis of Deterrence: Methodological Strategy and Ethics in Isaac Ehrlich's Research and Writing on the Death Penalty as a Deterrent*, 22 BRIT. J. CRIMINOLOGY 101 (1982); Bowers & Pierce, *The Illusion of Deterrence in Isaac Ehrlich's Research on Capital Punishment*, 85 YALE L.J. 187 (1975); Friedman, *The Use of Multiple Regression Analysis to Test for a Deterrent Effect of Capital Punishment: Prospects and Problems*, in 1 CRIMINOLOGY REV. Y.B. 61, 61-87 (S. Mesinger & E. Bittner eds. 1979); Hoenack & Weiler, *A Structural Model of Murder Behavior and the Criminal Justice System*, 70 AM. ECON. REV. 327 (1980); Hoenack, Kudrel & Sjoquist, *The Deterrent Effect of Capital Punishment: A Question of Identification*, 4 POL'Y ANALYSIS 491 (1978); Klein, Forst & Filatov, *The Deterrent Effect of Capital Punishment: An Assessment of the Estimates*, in DETERRENCE AND INCAPACITATION: ESTIMATING THE EFFECTS OF CRIMINAL SANCTIONS ON CRIME RATES 336 (A. Blumstein, J. Cohen & D. Nagin eds. 1978); Lempert, *The Effect of Executions on Homicide: A New Look in an Old Light*, 29

dicial thinking.[4]

Layson, a student of Ehrlich's,[5] now offers an even more dramatic conclusion—that every execution deters as many as eighteen homicides.[6] Unlike the divided public opinion on capital punishment in the mid-1970s, the current public mood overwhelmingly favors the use of the death penalty[7] and would embrace a finding such as this.

Prior to Ehrlich's work, the principal methodology used to search for possible deterrent effects of the death penalty was to compare homicide rates in abolitionist and retentionist jurisdictions or homicide rates in one jurisdiction before and after the introduction or abolition of the death penalty.[8]

Ehrlich's innovative work brought a different methodology, taken

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CRIME & DELINQ. 88 (1983); McGahey, *Dr. Ehrlich's Magic Bullet: Econometric Theory, Econometrics, and the Death Penalty,* 26 CRIME & DELINQ. 485 (1980); Passell, *The Deterrent Effect of the Death Penalty: A Statistical Test,* 28 STAN. L. REV. 61 (1975); Passell & Taylor, *The Deterrent Effect of Capital Punishment: Another View,* 67 AM. ECON. REV. 445 (1977); Peck, *The Deterrent Effect of Capital Punishment: Ehrlich and His Critics,* 85 YALE L.J. 359 (1976). For Ehrlich's responses to these criticisms, see Ehrlich, *On Positive Methodology, Ethics, and Polemics in Deterrence Research,* 22 BRIT. J. CRIMINOLOGY 124 (1982); Ehrlich, *The Deterrent Effect of Capital Punishment: Reply,* 67 AM. ECON. REV. 452 (1977); Ehrlich & Gibbons, *On the Measurement of the Deterrent Effect of Capital Punishment and the Theory of Deterrence,* 6 J. LEGAL STUD. 35 (1977); Ehrlich & Mark, *Fear of Deterrence: A Critical Evaluation of the "Report of the Panel on Research on Deterrent and Incapacitative Effects,"* 6 J. LEGAL STUD. 293 (1977); Ehrlich, *Deterrence: Evidence and Inference,* 85 YALE L.J. 209 (1975); Ehrlich, *Rejoinder,* 85 YALE L.J. 368 (1975); *see also* Ehrlich, *Capital Punishment and Deterrence: Some Further Thoughts and Additional Evidence,* 85 J. POL. ECON. 741 (1977).

4. For example, in 1972, the Supreme Court ruled that the death penalty as then applied was unconstitutional. Furman v. Georgia, 408 U.S. 238 (1972). In 1976, one year after Ehrlich's study was published, the Court held that statutes could be constructed so that capital punishment would be constitutionally permissible. Gregg v. Georgia, 428 U.S. 153 (1976). For a discussion of the role of deterrence research in *Gregg,* see Ellsworth, *Unpleasant Facts: The Supreme Court's Response to Empirical Research on Capital Punishment,* in CHALLENGING CAPITAL PUNISHMENT: LEGAL AND SOCIAL SCIENCE APPROACHES 177, 181-82 (K. Haas & J. Inciardi eds. 1988).

5. *Capital Punishment: Hearings on H.R. 2837 and H.R. 343 Before the Subcomm. on Criminal Justice of the House Comm. on the Judiciary,* 99th Cong., 1st and 2d Sess. 311, 314 (1985 & 1986) (testimony of Stephen Layson) [hereinafter *House Hearings*].

6. *See* Layson, *supra* note 1, at 80 ("the tradeoff of executions for murders is approximately -18.5"). In an even bolder assertion than Layson's, James Yunker concluded that each execution deters 156 homicides. *See* Yunker, *Is the Death Penalty a Deterrent to Homicide? Some Time Series Evidence,* 5 J. BEHAV. ECON. 45 (1976). This finding, however, has not been taken seriously in death penalty debates because of significant methodological flaws in his analysis. *See* Fox, *The Identification and Estimation of Deterrence: An Evaluation of Yunker's Model,* 6 J. BEHAV. ECON. 225 (1977); Sesnowitz & McKee, *On the Deterrent Effect of Capital Punishment,* 6 J. BEHAV. ECON. 217 (1977).

7. 244-45 THE GALLUP REPORT 10 (Jan.-Feb., 1986). Gallup's results indicate that 70% of the public supports the death penalty for murder, and 61% of the respondents believe that the death penalty acts as a deterrent. *See also infra* notes 51-58 and accompanying text.

8. *See, e.g.,* CAPITAL PUNISHMENT (T. Sellin ed. 1967).

from the field of economics, to bear on this question. He examined the effect of executions on national homicide rates between 1933-1969, holding constant certain demographic, socioeconomic, and criminal justice variables that were also expected to influence homicide rates. Layson's work used a similar methodology, but updated the Ehrlich analysis to 1977, examined different sets of explanatory variables, and attempted to improve the measure of homicide rates.

Policy-makers, as well as many criminologists and legal scholars, can be easily dazzled by the formidable equations which lend an air of authority to Ehrlich's and Layson's work. However, we must carefully inspect their work before we accept the conclusion that the death penalty works as a deterrent.

I. THE ECONOMETRIC APPROACH TO DETERRENCE

Given the extensive research devoted to the deterrence issue by notable scholars from Thorsten Sellin,[9] one of America's most respected criminologists, to Lawrence Klein,[10] Nobel Prize-winning economist, we would expect a clear answer to the question: Does the death penalty deter murder and, if so, to what extent? The elusiveness of the answer reflects the fact that no research design applicable to this question can provide definitive proof one way or the other. We can never say that we are absolutely positive the Loch Ness Monster does not exist; we can only say that if it exists, we have not yet found it.

The most powerful research design—the randomized experiment—may be used to study the effects of certain sanctions (e.g., income tax audits), but not executions. A state cannot experiment with the death penalty by executing all murderers from half of its counties, sentencing murderers from other counties to imprisonment, and comparing the subsequent homicide rates. As an alternative, some researchers have employed quasi-experimental designs, such as examining homicide rates before and after the introduction or abolition of a death penalty statute.[11] However, this type of experimental design is vulnerable to contamination by extraneous factors.

The econometric approach, used by Ehrlich and Layson among

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9. *See* T. SELLIN, THE DEATH PENALTY (1959) *reprinted in* MODEL PENAL CODE (Tentative Draft No. 9, May 8, 1959); CAPITAL PUNISHMENT (T. Sellin ed. 1967); T. SELLIN, THE PENALTY OF DEATH (1980).

10. *See* Klein, Forst & Filatov, *supra* note 3.

11. *See, e.g.,* Archer, Gartner & Beittel, *Homicide and the Death Penalty: A Cross-National Test of a Deterrence Hypothesis,* 74 J. CRIM. L. & CRIMINOLOGY 991 (1983); Fattah, *Canada's Successful Experience With the Abolition of the Death Penalty,* 25 CAN. J. CRIMINOLOGY 421 (1983).

others, instead attempts to identify a deterrent effect by measuring the statistical association between the risk of execution and the homicide rate. Whether the data are cross-sectional (e.g., cross-state variation in homicide and execution rates) or longitudinal (e.g., trends in rates of homicide and executions), the analyst strives to separate the effect of executions on homicide rates from the confounding effects of other variables.[12]

Layson, like Ehrlich, used three sanction variables in regression models explaining trends in the homicide rate.[13] Specifically, while controlling for a range of socioeconomic and demographic variables,[14] Layson estimated the association between the rate of homicide and (1) the likelihood of arrest for homicide, (2) the likelihood of conviction given an arrest for homicide, and, of greatest importance, (3) the likelihood of execution given a conviction for homicide.[15]

A correlation between the homicide rate and any one of the variables does not necessarily indicate causation; that the number of executions fell during the 1960s and 1970s while the homicide rate rose sharply does not mean that the former caused the latter. Recognizing that associations among variables may spuriously arise from extraneous factors or common causes, the econometrician will include in his or her model other important variables that reflect competing hypotheses for explaining trends in homicide rates. The inclusion of one, two, or even a dozen control variables does not guarantee the exclusion of spurious associations. To the contrary, many factors underlying the homicide rate may be omitted either because they are not seen as being critical[16] or, more commonly, because the data are unavailable.[17] Layson, in his analysis, considered as many as ten possible control variables;[18] however, the

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12. Fisher, *Multiple Regression in Legal Proceedings*, 80 COLUM. L. REV. 702, 730-35 (1980).

13. Layson, *supra* note 1, at 70-71.

14. *See infra* note 18.

15. Layson, *supra* note 1, at 70-71.

16. Fisher, *supra* note 12, at 730.

17. Klein, Forst, & Filitov, *supra* note 3, at 345.

18. The ten control variables used by Layson in his analysis were labor force participation, unemployment rate, per capita income, proportion of population non-white, proportion of population belonging to a religion, proportion of families with husband and wife present, and the population in the 14-24, 21-24, 25-29, and 21-29 age categories, all of which were expressed in natural logarithms. *See* Layson, *supra* note 1, at 70, table I. Layson's data set also included the percentage of the population that is male, government welfare payments, inflation, and median years of schooling by males aged 25 years and over. *See* S. Layson, Data Sources for "Homicide and Deterrence: A Reexamination of the United States Time Series Evidence" (June 20, 1985) (unpublished manuscript on file with the Loyola of Los Angeles Law Review).

validity of his findings are still in question because of omissions of both types.

For example, the increased availability of guns may affect the homicide rate both directly and indirectly.[19] The presence of a gun can stimulate aggression.[20] In addition, the use of a gun, as opposed to some other weapon, would be more likely to make an assault lethal.[21] Besides its greater lethality, the firearm distances the offender from his or her victim, making it easier, in a psychological sense, to take human life. Lacking physical contact, killing can be as "easy" as shooting at objects in a video game.

Cognizant of the biases that may result from excluding these important variables, Layson responds by including a time trend variable[22] as a proxy for the host of omitted variables. However, this solution is unsatisfactory. Although a linear time trend may adequately compensate for social, economic, or political factors which have increased or decreased linearly in rate, many critical factors have changed in a non-linear fashion and thus are poorly represented by a simple, straight-line trend in time.

Similarly, it is possible that the observed deterrent effect of executions may not suggest the impact of executions per se, but rather a more general trend underlying the rate of execution. Perhaps the homicide rate responds to the general "get tough" on crime posture. For example, had a measure of prison terms for homicide been available, the same deterrent effect may have shown up with this sanction variable. In short, capital punishment is but one way with which we sanction offenders, and its apparent deterrent effect may simply reflect the sensitivity of offenders to sanctions generally.

The factors and conditions underlying homicide trends are many and diverse; it is doubtful that the dozen that Layson considered span this range. Additionally, factors that influenced the homicide rate in the 1930s are likely to be different from those which affected that rate in the 1970s. One approach to this problem is to include the rate of other crimes as a control variable, since they react to many of the same causal

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19. Kleck, *Capital Punishment, Gun Ownership, and Homicide*, 84 AM. J. SOC. 882 (1979).

20. Berkowitz & LePage, *Weapons as Aggression-Eliciting Stimuli*, 7 J. PERSONALITY & SOC. PSYCHOLOGY 202, 206 (1967).

21. Kleck, *supra* note 19, at 893.

22. A time trend variable is an integer starting at 1 for the first annual observation and increasing by 1 for each additional year during the study period. Layson, for example, defined his time trend variable as "1" for 1933 through "45" for 1977. *See* Layson, *supra* note 1, at 70, table I. Time trend variables account for any linear trend that would have been captured by variables that were omitted or unavailable because of lack of data.

factors. Klein, Forst, and Filatov found, for example, that when the rate of crimes other than homicide was included as a control variable, the deterrent effect weakened and became insignificant.[23] Curiously, Layson did not use the data on other crimes, though the data were available and the results of Klein, Forst, and Filatov's work are certainly instructive.

The presence of substantial multicollinearity in the data complicates the task of finding the best specification, i.e., the best combination of variables to use in the model.[24] The variables are strongly intercorrelated, some over 0.95, which makes the coefficients of the model unreliable and highly dependent on the model specification.

To his credit, Layson performed a specification analysis of his results, estimating several alternative models.[25] However, his crude classification (whether coefficients were positive or negative and whether significant or not) obscured the instability of many of the coefficients between models. Given the instability, one cannot comfortably endorse any particular specification.

To be sure, econometric techniques are invaluable for many modeling and forecasting problems. For example, econometric methods have successfully forecasted the national crime rate.[26] In that case, whether the variables included in the model were the proper ones was secondary to whether the model forecasted accurately.

Econometric methods are tricky, however, when used to establish causal links through data analysis; a solid theoretical foundation is essential in order to develop and test causal models. With highly aggregated analyses such as Layson's, almost any specification will fit the data well. All four of Layson's alternative specifications[27] had R-squared val-

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23. Klein, Forst & Filatov, *supra* note 3, at 355-56.

24. Multicollinearity occurs when two or more predictors are so strongly intercorrelated that the regression procedure has difficulty separating the unique effects of each variable. As a result, whatever coefficients obtained are unstable or unreliable because they can change substantially with minor modifications in specification. The stability of the regression hyper-plane depends on the extent to which the predictor variables are orthogonal (uncorrelated) or oblique (correlated). As an analogy, consider a table top mounted on a single pole which is supported by a base of two crossed legs. If the legs are perpendicular (uncorrelated), the table is stable; if the legs are oblique (correlated), the table can wobble. Like the table top, the regression hyper-plane becomes more unstable as the legs (the variables) are increasingly correlated.

25. The specification analysis estimates homicide rates with several different combinations of predictor variables. Layson estimated the effect of five variables in 210 different equations, and the effects of 10 other variables in 84 different equations. *See* Layson, *supra* note 1, at 78, table V.

26. *See, e.g.,* J.A. FOX, FORECASTING CRIME DATA—AN ECONOMETRIC ANALYSIS (1978).

27. *See* Layson, *supra* note 1, at 76, table IV, equations 16-19.

ues over ninety-nine percent;[28] however, so would most any specification of ten variables, no matter how absurd.[29] In fact, one would be hard-pressed to find a ten-variable equation that does not fit well.

Economic models, which are grounded in well-developed macro-economic theory, are generally fairly large, involving multiple equations and numerous exogenous variables. In contrast, not only is our under-standing of the macro-dynamics of homicide minimal at best, but one simply cannot feel very secure about a single-equation model like Layson's having a dozen variables chosen somewhat arbitrarily.

Caution is always advised in interpreting coefficients and elasticities in causal terms. But when the issue is as critical as the death penalty, the results of econometric analysis may never be sufficiently unambiguous to recommend policy.

II. DATA QUALITY

Unlike psychologists and sociologists, for whom reliability, validity, and operationalization of measures are fundamental concerns, econo-mists seem to assume a much more relaxed position on this matter. In-deed, while the use of proxies for unavailable measures is customary in econometrics, a sociologist might be bitterly criticized for using inexact measures unless the measures can be shown to be reliable and valid in-dicators of the variable of interest.

It is to Layson's credit, therefore, that his primary objective in un-dertaking this research was to replicate earlier analyses using a more ap-propriate measure of homicide. Given the criticisms concerning the

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28. R-squared is "the percentage of variation in the dependent variable that is accounted for by all of the explanatory variables." Rubenfeld and Steiner, *Quantitive Methods in Anti-trust Litigation*, 46 LAW AND CONTEMP. PROBS. 69, 100 (1983). In other words, the higher the R-squared value is, the better we can account for changes in the dependent variable caused by the explanatory variables used. For example, if the R-squared of the change in the homi-cide rate is 99%, then 99% of the change can be accounted for by the variables which were used to determine what caused the change (such as access to handguns, prison terms, the death penalty, etc.).

29. In place of the five or six control variables used by Layson in his final specification (equations 16-19 in Layson's table IV), we simply use a fifth degree orthogonal polynomial as a control for the arrest, conviction, and execution sanction variables. (*See* Layson, *supra* note 1, for table IV, equations 16-19). The orthogonal polynomial is substantively meaningless, yet it is statistically desirable since the five terms are uncorrelated. While we do not suggest this specification to be theoretically relevant, it is certainly noteworthy that this "nonsense" specifi-cation (with R-squared of 0.994) fits the homicide variable nearly as well as Layson's best four equations.

coverage of the Uniform Crime Reports (UCR)[30] in its earlier years,[31] Layson chose Vital Statistics[32] as his source of homicide data.

Unfortunately, Layson did not employ the same scrutiny with regard to his other measures—particularly his sanction variables. To be blunt, the disposition data—persons charged and persons convicted—are of such poor quality that they are useless for research purposes. This data series was discontinued from the UCR program as of 1978 because of its poor quality.[33] In 1977, for example, only fourteen percent of the United States population was represented in reports by those agencies that submitted disposition data.[34] The quality of the disposition data is even worse for the early years of the UCR program in the 1930s.[35]

Moreover, Bowers and Pierce[36] show that the mix of jurisdictions that have supplied disposition data has been far from consistent over time, thus creating a serious bias in those time series.[37] Also, one may not reasonably assume that those agencies that did provide disposition data were representative of the United States as a whole. Thus, one cannot say what the probability of conviction and probability of execution really measure, if anything, when used as variables in a regression model. The use of this disposition data was a fatal flaw in Layson's findings, as it would be in any analysis that takes the disposition data seriously.

III. TIME PERIOD

One of the major criticisms of Ehrlich's research is that the deterrent effect disappears when data from the years following 1962 are removed from the analysis.[38] In other words, the negative association observed by Ehrlich had been produced during an era—beginning in 1963—when the homicide rate was rising sharply and executions were

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30. For a description of Uniform Crime Reports, see Wolfgang, *Uniform Crime Reports: A Critical Appraisal*, 111 U. PA. L. REV. 708, 708-17 (1963).

31. *See, e.g.*, Bowers & Pierce, *supra* note 3, at 188-89.

32. To count homicides, Layson used the following sources: For 1933-1936, U.S. DEP'T OF COMMERCE, BUREAU OF THE CENSUS, MORTALITY STATISTICS (1933-36); for 1937-1945, U.S. DEP'T OF COMMERCE, BUREAU OF THE CENSUS, VITAL STATISTICS OF THE UNITED STATES (1937-45); for 1946-1977, DEP'T OF HEALTH, EDUCATION, AND WELFARE, U.S. PUBLIC HEALTH SERVICE, VITAL STATISTICS OF THE UNITED STATES (1946-77). *See* Layson, *supra* note 18, at 1.

33. Perhaps this explains why the data series used in Layson's analysis ended with 1977.

34. 1977 F.B.I. UNIFORM CRIME REP. 281, table 56.

35. *See* Bowers & Pierce, *supra* note 3, at 188.

36. Bowers & Pierce, *supra* note 3.

37. *Id.* at 188-91. Disposition data were filed annually using a different form than that for crimes known. It was possible, therefore, for these data to be seriously flawed, while the monthly data on "crimes known" were adequate.

38. *See* Bowers & Pierce, *supra* note 3, at 197-98.

becoming exceptionally rare. Further, while advocates of the death penalty might view the 1960s wave of murder as "obviously" due to the lack of executions, all crimes, capital and non-capital, were on the rise during this time period.[39]

Layson anticipated this criticism and performed the Chow test[40] to determine if structural shifts existed over the time period he studied.[41] He failed to reject the hypothesis of no structural shifts.[42]

However, this hardly settles the issue. First, failing to reject an hypothesis does not mean that it is proven, only that it is retained. The power of the test may not be sufficient to detect a change, had there been one. Moreover, the Chow test is an overall test of the model based on degree of fit.[43] The degree of fit may not change significantly over time even though one or more regression coefficients do change.

In Layson's research, the key variable is the probability of execution given conviction. Figure 1 (see Appendix B) shows a scatterplot of homicide (Q) and the probability of execution (PE3), which reveals a V-

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39. Ehrlich's and Layson's work, as with most deterrence studies, used data that preceded the temporary moratorium on the death penalty between 1967 and 1977. While only a few studies have examined later data, those that have done so have continued to find no deterrent effect from the threat of execution.

The first examination of the deterrent effect of post-*Furman* executions searched for effects on the homicide rate from the first four post-*Furman* executions. McFarland, *Is Capital Punishment a Short-Term Deterrent to Homicide? A Study of the Effects of Four Recent American Executions*, 74 J. CRIM. L. & CRIMINOLOGY 1014 (1983). Homicides did decrease in Utah following the execution of Gary Gilmore in 1977, but McFarland shows this decline resulted from atypical inclement weather, not from the execution. *Id.* at 1030. After the other three executions (John Spenkelink, Florida, May 25, 1979; Jesse Bishop, Nevada, October 22, 1979; Steven Judy, Indiana, March 9, 1981), no corresponding decline in homicides occurred. *Id.* at 1021.

Bailey and Peterson looked at murders of police officers and police employees from 1973-1984, finding that rates were highest over the first four years, but have generally and steadily declined since. Bailey & Peterson, *Police Killings and Capital Punishment: The Post-*Furman *Period*, 25 CRIMINOLOGY 1 (1987). "Not for a single year was evidence found that police are safer in jurisdictions that provide for capital punishment. Nor did the analysis produce a single instance where higher levels of death sentences are associated significantly with lower rates of police killings." *Id.* at 22. Later, using both contiguous state matching and multiple regression to assess the relationship between capital punishment and homicide rates over the same time period, Peterson and Bailey similarly found no corresponding decline in homicides. Peterson & Bailey, *Murder and Capital Punishment in the Evolving Context of the Post-Furman Era*, 66 SOC. FORCES 774 (1988).

40. The Chow test, a procedure for testing the equality of two sets of regression coefficients, can be used for determining if a regression model estimated for one time period is different from that estimated for a second time period. Chow, *Tests of Equality Between Sets of Coefficients in Two Linear Regressions*, 28 ECONOMETRICA 591 (1960).

41. Layson, *supra* note 1, at 83.

42. *Id.*

43. Chow, *supra* note 40.

shaped pattern. Upon further examination, the positive slope corre-
sponds to the years prior to 1957, and the negative slope represents the
years since 1957.

Since the Chow test is a global test, it may not have been sufficiently
sensitive to this type of change. A more appropriate test would focus
only on the key execution variable. We have estimated a number of spec-
ifications of the model, adding in a dummy interaction term to measure
change in slope for the risk of execution (PE3). For most of the equa-
tions, the negative shift in slope after 1957 is significant, and for some,
the slope for executions prior to 1957 is not.

Adding this dummy interaction term into Layson's equations
reduces the magnitude and significance of the effect of execution risk for
the years 1936-1957, when executions were actually being performed
with some regularity. Specifically, as displayed in Table 1 (see Appendix
A), the regression coefficient for the risk of execution variable (PE3), esti-
mated by Layson as -0.103,[44] changes substantially to -0.064 when the
slope shift dummy is added. The change indicates that the association
between the risk of execution and the rate of homicide, holding the other
factors constant, is far from stable between the pre-1960 and post-1960
eras. Using this more sensitive and direct approach, we must question
Layson's confidence that structural shifts do not underlie his results.

Because Layson's specifications were plagued with problems of mul-
ticollinearity, we used orthogonal polynomials to reestimate his equa-
tions. The results show that Layson's pre-1957 effect of executions
changes sign and that the post-1957 dummy slopes contain the entire
negative association that Layson mistakingly interpreted as a deterrent
effect. Thus, much—if not most—of the negative association between
the risk of execution and the rate of homicide is a result of the post-1957
era when, coincidently, executions decreased while homicides increased,
and not necessarily because of each other (see Figure 2, Appendix B).

IV. NEGATIVE BIAS

Several potential problems arise because the number of homicides
appears in the numerator of the dependent variable (i.e., the homicide
rate Q) and in the denominators of the sanction variables (e.g.,
probability of execution PE3). As has been pointed out in regard to Ehr-
lich's research, measurement error in the number of homicides will pro-
duce an artificial negative association—or a negative bias—in the

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44. Layson, *supra* note 1, at 76, table IV, equation 16.

coefficient of execution risk.[45] For example, if a few homicides were randomly added in (or subtracted), the homicide ratio would increase (decrease) and the probability of execution would decrease (increase); therefore, the coefficient of the execution rate would shift negatively.

Layson used Vital Statistics to obtain data on the number of homicides, as UCR's poor coverage of homicides in the early years could produce bias.[46] Although it may be argued that the Vital Statistics measure of homicides is an improvement over the UCR measure, the Vital Statistics measure is not infallible. Indeed, suicides and accidental deaths can be improperly classified as homicides, and persons who are abducted and killed may be erroneously considered missing and therefore left out of the figures. In addition, the number of homicides committed by a multiple murderer will be reflected repeatedly in the denominator of the execution variable, biasing the coefficient downward. Of course, the multiple murderer cannot be executed multiple times.

In addition to these sources of error, homicide is improperly defined. In Layson's work, Q (the homicide rate) includes both capital and non-capital killings, as data that attempt to differentiate the two categories of homicide are non-existent. Whatever the deterrent effect of capital punishment is for capital homicide (premeditated murder), a large proportion of the reported homicides are manslaughters, which are generally not deterrable because they are either provoked or unpremeditated.[47]

As with measurement error, the inclusion of these non-capital crimes in the numerator of Q and the denominator of PE3 pushes the estimated deterrent effect of the death penalty in the negative direction.[48] We do not have data available at this point to estimate the model only for capital crimes. Still, any estimated deterrent effect should be adjusted for the inclusion of non-capital crimes.

V. AGGREGATION BIAS

The econometric approach is designed for modeling macro trends in society. Unfortunately, criminologists do not have a sound macro-theory

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45. *See, e.g.*, Klein, Forst & Filatov, *supra* note 3, at 347-49.

46. Layson, *supra* note 1, at 73.

47. Bailey, *Disaggregation in Deterrence and Death Penalty Research: The Case of Murder in Chicago*, 74 J. CRIM. L. & CRIMINOLOGY 827, 831-32 (1983).

48. This negative bias would not arise if the ratio of capital to non-capital homicides remained constant over time. This is most likely not the case, however. For example, in recent years the percentage of stranger-to-stranger homicides has been growing. This would suggest that the proportion of capital homicides may also be increasing. M. DIETZ, KILLING FOR PROFIT 3-12 (1988).

of homicide.[49] The theory of deterrence is, on the other hand, based on the thinking of individuals, i.e., micro-theory. The bias of measuring micro-relations with macro-data is well known, yet some observers tend to lose sight of this in the area of deterrence. To presume that a potential killer in Oregon may be deterred by an execution in Georgia is rather unreasonable. Nevertheless, it is an assumption implicit when inferring micro-level relationships from macro-level associations.

Not only does the use of nationally aggregated data ignore cross-state variation in the risk of execution, but it also ignores the fact that offenders have different perceptions and reactions to sanctions. Many potential murderers who would alter their behavior because of the risk of execution might also respond to changes in imprisonment rates or the average length of incarceration for murder. At the other extreme, those killers who perhaps are the most likely to be executed under current death penalty statutes, such as serial killers like Gacy, Bundy, and Buono, are the least deterrable because of their sociopathic character.[50] As to these cold-impassioned killers—the ones for whom the death sentence is perhaps tailored—little can be done to compensate for their lack of internal controls.

VI. MISINTERPRETATION OF LAYSON'S FINDINGS

The above problems aside, Layson's work has been misinterpreted and used to exaggerate the deterrent effect of the death penalty even beyond what Layson concluded. For example, in 1987, Bedau and Radelet documented 350 twentieth century cases in American jurisdictions in which innocent defendants were convicted of homicide or sentenced to death for rape.[51] Attorney General Edwin Meese requested that his staff prepare a response.[52] Stephen Markman (the author of the Justice Department response) and Paul Cassell expanded the response to include

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49. Brenner makes such an attempt. *See* ACHIEVING THE GOALS OF THE EMPLOYMENT ACT OF 1946—THIRTIETH ANNIVERSARY REVIEW, JOINT ECONOMIC COMMITTEE, 94TH CONG., 2D SESS., PAPER NO. 5 (Comm. Print 1976) (Brenner, *Estimating the Social Costs of National Economic Policy: Implications for Mental and Physical Health, and Criminal Aggression*); ESTIMATING THE EFFECTS OF ECONOMIC CHANGE ON NATIONAL HEALTH AND SOCIAL WELL-BEING, STUDY PREPARED FOR THE SUBCOMM. ON ECONOMIC GOALS AND INTERGOVERNMENTAL POLICY OF THE JOINT ECONOMIC COMM., 98TH CONG., 2D SESS. (Comm. Print 1984) (Brenner).

50. For a discussion of capital punishment in relation to serial killers, see J. LEVIN & J. A. FOX, MASS MURDER: AMERICA'S GROWING MENACE (1985).

51. Bedau & Radelet, *Miscarriages of Justice in Potentially Capital Cases*, 40 STAN. L. REV. 21 (1987).

52. Bedau & Radelet, *The Myth of Infallibility: A Reply to Markman and Cassell*, 41 STAN. L. REV. 161, 161 n.1 (1988).

Layson's work and published it in the Stanford Law Review.[53] The essence of the Markman and Cassell article is that the Bedau-Radelet study "gives excessive weight to the slight risk of erroneous executions," and that it fails to "consider the countervailing benefits of capital punishment."[54] In buttressing the latter point, Markman and Cassell use Layson's work to estimate that 125,000 murders have been deterred by capital punishment in America this century.[55]

The Markman and Cassell article provides a clear example of how research such as Layson's can be uncritically accepted—and even exaggerated—to support a preordained conclusion. Layson studied the period 1933-1977.[56] To arrive at their estimate of homicides deterred, Markman and Cassell used Layson's finding that each execution deterred eighteen homicides, and multiplied it by 7092, the number of executions in America during the first eighty-five years of this century.[57]

This extrapolation is preposterous. Even Layson admitted that the deterrent effect he claimed to have found existed only during the last fifteen years of the period he studied, and in eight of those years there were no executions. As Layson stated, "[i]f I exclude all the data [after] 1960, I do find that evidence for the deterrent effect of capital punishment is very weak" or even "nonexistent."[58]

VII. CONCLUSION

We have omitted other methodological issues, such as simultaneity and functional forms, from this review of Layson's paper. The problems discussed above are sufficiently formidable and noteworthy, and we do not wish to cloud the discussion of these significant concerns with what are perhaps statistical quibbles. We will, nevertheless, mention one minor issue as a way of concluding, because it represents the fundamental problem with this approach to studying deterrence.

Three competing functional forms for the model were considered by

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53. Markman & Cassell, *Protecting the Innocent: A Response to the Bedau-Radelet Study,* 41 STAN. L. REV. 121, 155 (1988).

54. *Id.* at 145.

55. *Id.* at 156.

56. While the full observation period in Layson's data spans 1933-1977, Layson only analyzed observations for 1936-1977 because of a three-year lag in the sanction variables.

57. Markman & Cassell, *supra* note 53, at 156 n.214. Later, Markman and Cassell multiply 125,000 by 0.25 to arrive at the conclusion that even if a 25% possibility exists that Layson's figures are correct, then 31,250 lives would have been saved by capital punishment between 1900-1985. *Id.* at 156 n.215. This logic is equivalent to supposing that if the chances of a nuclear bomb falling on New York City were 0.25, then we could plan on only a quarter of the population being killed if indeed a bomb were dropped.

58. *House Hearings, supra* note 5, at 316 (testimony of Stephen Layson).

Layson: linear, semi-log, and log-linear. Layson decided in favor of a log-linear form, not because any theory of deterrence dictates it, but because a statistical test—the Box-Cox analysis,[59] which is unrelated to criminological theory—endorses it.[60] Although we do not question the propriety of the test, we do question whether this test is the best way to approach such a weighty issue as measuring the deterrent effect of capital punishment.

The lack of macro-theory to guide model building, combined with a host of difficult methodological problems—measurement error, aggregation bias, temporal shifts, and causal ambiguity—makes the prospects bleak for econometric studies of deterrence. Brier and Fienberg, in their well-respected review of the econometric literature on deterrence, concluded that "little or no progress has been made during the past [ten] years in our understanding of the potential deterrent effects of punishment on crime."[61] We must conclude that Layson's findings add nothing to our understanding.

None of these criticisms should suggest any deficiency in Professor Layson's technical skill. Even the most expert econometrician could not derive reliable estimates of the deterrent effect of capital punishment from these data and with this approach. Indeed, the application of econometric techniques to deterrence is as arbitrary as the application of the death penalty,[62] and certainly offers nothing to guide policy.

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59. Box & Cox, *An Analysis of Transformations*, 26 J. ROYAL STATISTICAL SOC'Y, Ser. B, 211 (1964).

60. Layson, *supra* note 1, at 87.

61. Brier & Fienberg, *Recent Econometric Modeling of Crime and Punishment: Support for the Deterrence Hypothesis?* 4 EVALUATION REV. 147, 187 (1980).

62. *See, e.g.*, S. GROSS & R. MAURO, DEATH AND DISCRIMINATION: RACIAL DISPARITIES IN CAPITAL SENTENCING (1989).

APPENDIX A

TABLE 1

LEAST SQUARES ESTIMATES WITH SLOPE-SHIFT DUMMY

(t-values in parentheses)

Independent Variables[a]	Layson's Equation		
	Original Specification	Modified with Slope-Shift	Orthogonal Polynomials
Prob (Arrest) (PA3)	-1.57[b]	-1.429	-2.560
	(-4.57)[c]	(-5.05)	(-5.60)
Prob (Conv/Arrest) (PC3)	-0.436	-0.416	-0.463
	(-11.80)	(-11.29)	(-3.34)
Prob (Exec/Conv) (PE3)	-0.103	-0.064	0.037
	(-12.00)	(-2.24)	(0.87)
Slope-Shift (DUM×PE3)[d]		-0.046	-0.106
		(-1.59)	(-2.65)
Time Trend (TT)	-0.023	-0.029	
	(-3.33)	(-5.80)	
Percent 21-29 (A2129)	0.392	0.286	
	(4.44)	(3.01)	
Unemployment Rate (U)	-0.025	0.004	
	(-1.51)	(0.27)	
Labor Force Part (LFP)	-2.33	-2.309	
	(-7.48)	(-8.39)	
Income (Y)	0.297	0.438	
	(1.98)	(4.18)	
Percent Nonwhite (NW)	1.10	1.499	
	(1.91)	(3.13)	
First Degree Term (P1)			-0.59×10^{-3}
			(-0.14)
Second Degree Term (P2)			-0.70×10^{-4}
			(-0.24)
Third Degree Term (P3)			-0.66×10^{-6}
			(-0.09)
Fourth Degree Term (P4)			-0.24×10^{-5}
			(-4.20)
Fifth Degree Term (P5)			-0.40×10^{-7}
			(-0.40)
Constant (C)	6.22	4.899	10.302
Estimated Rho	-0.272	-0.805	-0.505
R-squared	0.996	0.998	0.994

[a] All variables, except for TT and P1 through P5, are in logs.
[b] Estimates are corrected for autocorrelation.
[c] Estimates with t-values over 2.0 are statistically significant.
[d] DUM×PE3 is zero for 1936-1956 and PE3 for 1957-1977.

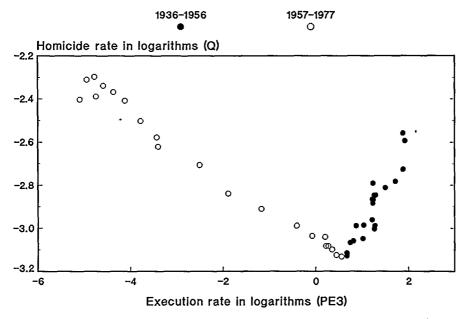APPENDIX B

FIGURE 1: SCATTERPLOT OF EXECUTION AND HOMICIDE RATES

FIGURE 2: TRENDS IN EXECUTION AND HOMICIDE RATES, 1936-77