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FEAR OF DETERRENCE

A Critical Evaluation of the "Report of the panel on Research on Deterrent and Incapacitative Effects"

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I. INTRODUCTION

A PROFUSION of research in the last few years by economists and others on the deterrent effect of the criminal sanction and other determinants of criminal activity attests to the empirical fertility of the apparently "heretical" proposition that potential offenders respond to incentives. The reasons for the unpopularity of this proposition in modern criminology, despite its being a fundamental principle of legal and penal systems throughout history, are beyond the scope of this article. Suffice it to note that the spectre of the return to scientific prominence of the classical deterrence hypothesis, cavalierly rejected by criminologists for a century, has precipitated intense criticism in scholarly journals of the developing economic approach to crime, especially in reaction to a new study of the deterrent efficacy of the death penalty challenging the conclusions of earlier researchers. The latest criticism is the report of a "Panel on Research on Deterrent and Incapacitative Effects" (Panel) prepared under the auspices of the National Academy of Sciences.[1] The Panel decries, and addresses itself in the report to,[2] the absence of a comprehensive assessment of the validity of studies "purporting to demonstrate that the deterrent and incapacitative effects of criminal sanctions could produce significant and quantifiable benefits in terms of reduced crime rates."[3] The implications of the very existence of such a quasi-official board of review are briefly commented upon in Section III below. The content of this self-proclaimed "competent" and "balanced" review[4] warrants the careful scrutiny of the community of scholars concerned with the issues being investigated.

In spite of the Panel's expressions of the comprehensive nature of its mission and in spite of the multitude of studies on deterrence mentioned in its report-some having influenced public policy for years – only works pursuing the economic approach to crime that have developed evidence consistent with the deterrence hypothesis were singled out for comprehensive reanalyses of data. While the report offers no reasons for the Panel's allocation of resources in this way, one cannot escape the observation that evidence consistent with the deterrence hypothesis is scrutinized with an ardor not evident in connection with papers denying the deterrent efficacy of sanctions.

To be sure, the Panel's report recognizes at one point that the evidence amassed in recent studies on deterrence "certainly favors a proposition supporting deterrence more than it favors one asserting that deterrence is absent."[5] Yet the bulk of the Panel's report may lead the casual reader to believe that the now considerable and still burgeoning body of evidence consistent with the deterrence hypothesis and with the economic approach to crime is hardly more informative than the virtual vacuum of scientific evidence of only a decade ago.

Given the prominence of my research in the Report of the Panel,[6] it appears appropriate that I reply to the substance of the criticism. The response to the issues can be detailed to best advantage by dealing, in turn, with the two commissioned papers – by Klein, Forst, and Filatov[7] and by Fisher and Nagin[8] - which contain the substantive basis for much of the Panel's report. Section II will analyze these papers individually. Section III will then examine the conclusions of the Report with particular regard to some recommendations advanced concerning future research in this area.

Before examining the analytical issues in detail, a remark on the composition of the Panel seems appropriate. While the methodological advances in recent research on deterrence have, to a considerable extent, come from work by economists, and while studies following the economic approach are a major focus of the Panel's work, not a single practitioner of the economic approach to crime is to be found among the panel's interdisciplinary roster of members. In contrast, the panel does include scholars who have pursued approaches in criminology that are seriously challenged by the economic approach and whose past work exhibits considerable skepticism, if not philosophical hostility, toward the deterrence hypothesis.[9] These comments are not intended, of course, to impugn the intellectual integrity of these respected scholars, nor to question the desirability of having their views represented. However, the imbalanced composition of the Panel may be partly responsible for the shortcomings of its work and conclusions, which are elaborated in the following sections.

II. ANALYSIS OF TWO COMMISSIONED PAPERS

This section focuses on the papers by Klein, Forst, and Filatov and by Fisher and Nagin which are presented as supplementary materials to the Report of the Panel. These two papers provide the basis for the bulk of the substantive comments within the Report itself on the economic approach to crime and on my work in this area. The points discussed in response to each of these papers are not intended to be exhaustive. More detail on specific issues discussed in Part A of this section is contained in "Deterrence: Evidence and Inference."[10] in "The Deterrent Effect of Capital Punishment – A Reply,"[11] and in "On the Measurement of the Deterrent Effect of Capital Punishment and the Theory of Deterrence."[12] An additional, more positive "reply" is contained in a new study of independent cross-state data on murder variations in the United States for the years 1940 and 1950 which on the whole corroborates my previous analysis of the time series data on murder. This work is reported in "Capital Punishment and Deterrence: Some Further Thoughts and Additional Evidence."[13]

A. *Some Comments on the Klein-Forst-Filatov Paper*

1. Replication of the Results of My Time Series Study. The Klein-Forst-Filatov paper, focusing on my first study on the deterrent effect of capital punishment, provides a useful service in replicating to within rounding errors the basic results reported in my 1975 *American Economic Review* paper.[14] The only exception, in connection with one equation, seems to be due to a computational error on their part.[15]

2. "Missing Variables," Subperiod Sensitivity, and the Optimal Regression Format. The paper repeats arguments previously advanced by other critics in connection with alleged sensitivity of the results of my time series regression analysis to changes in the regression format and to the deletion of observations from the 1960s from the full sample underlying my investigation. Since I have responded to these arguments in detail elsewhere,[16] I shall not repeat the analysis of these issues. I would merely point out that the authors do not provide a constructive analysis of the issue of an optimal regression format since they do not report any tests for optimal transformations of relevant functional forms. Furthermore, the results of their simple linear transformation appear to be erroneous, as the discussion in note 15 indicates. Not only are the basic results found to be largely robust to transformations of functional form, but independent tests of transformations reported elsewhere[17] seem to confirm the relative superiority of the logarithmic specification emphasized in my previous work. I would also point out that truncating the sample at the early 1960s not only destroys precious degrees of freedom already in short supply – as I argued in an earlier paper and as these authors themselves emphasize[18] - but also restricts the analysis to a period over which key variables are highly trended with time and display little variability.[19] Let me stress, however, that the authors' claim at the conclusion of Section 2 of their paper that the results in connection with the effect of execution risk on murder are to be considered an artifact of spurious movements in murders and execution sin the 1960s is not substantiated by any of the analyses presented in the paper. If the estimated execution-risk effect on murder were simply artifactual, then such an effect should also have been estimated in regressions dealing with other crimes since they shared the upward trend of murders in the 1960s. This, in fact, is not the case.[20] The authors then go on to conjecture that the entire magnitude of the estimated effect of execution risk is due to the absence of a measure of imprisonment length[21] (which is cited in my 1975 *American Economic Review* article), but they leave this conjecture untested. However, my work with cross-sectional data from 1940 and 1950, which permit introduction of the terms of incarceration for murder in the regression in addition to measures of conviction and execution risks, reveals that inclusion of this variable in fact improves the results: while the absolute magnitudes of the estimated deterrent effects of the execution-risk measures are hardly affected, the standard errors of these estimates as well as of the estimated effect of the conviction-risk measure reduce markedly. Moreover, all three deterrence variables appear to exert significant effects in the expected direction. This corroborating, independent evidence for deterrence from 1940 and 1950 is illuminating also because it does not support the authors' further conjecture that my time series results are due essentially to "missing factors," such as the Vietnam War or the proliferation of guns in the 1960s:[22] neither factor can explain the cross-sectional results from earlier periods. Moreover, investigation of the cross-section demonstrates that when account is taken of interrelationships among murder and other crimes against the person, the efficiency of the estimated deterrent effects unambiguously improves.[23]

It should also be noted that the authors err in stating that the increase in criminal homicides after 1960 has been less significant than that for crimes against property.[24] The fact is that the rate of increase in robbery and other property crimes slightly exceeded that of murder between 1963 and 1969, but fell short of it between 1963 and 1973.[25] In any event, Klein *et al.'s* attempt to infer from this evidence that the reduction in execution risk cannot explain the rise in criminal homicide over that period is at best superficial. Movements in specific crimes are the result of many factors, including arrest, conviction risks, and other variables specific to a crime which do not change uniformly over time. Furthermore, Klein *et al.* ignore the fact that from the late 1930s to 1963 the murder rate in the U.S. has been continuously on the *decline* while the opposite trend is observed with other felonies. Against this background of conflicting long-term trends in murder and other crimes, the surge in murders since 1964, which seems to be well explained by the regression model, is even more significant.

3. Alleged Biases Due to Measurement Errors. The authors allege that the results of my time series analysis can also be explained by possible errors of measurement in estimates of the total number of murders as well as related deterrence variables. However, as the following discussion will attempt to show, their analysis bears little relevance to my own work and their contention is false. In the first place, a few important considerations indicate that the measurement-errors argument does not apply to my statistical analysis and is not sufficient to explain the consistency of findings with the theoretical predictions-findings that have been corroborated in other work as well. The authors' argument relates to the construction of an objective measure of the conditional risk of execution, termed PXQ_1, via the ratio of executions in (t + 1) to convictions in (t), with the latter estimated as a product of the number of murders Q^o (as reported by the FBI) and the reported risks of apprehension and conviction $P^o a$ and $P^o c|a$ Errors in Q^o, which appears both in the numerator of the dependent variable and the denominator of PXQ_1, might, of course, inject downward biases in the estimated coefficient associated with PXQ_1 and, consequently, in other regressors.[26] The authors' discussion does not stress, however, that this problem has been accounted for in my work so that the bulk of the reported estimates should in principle be free of biases due to such measurement errors. Specifically, my time series study uses six alternative estimates of conditional execution risk, four of which-PXQ_{1-1} TXQ_1, PDL_1, and $P\hat{X}Q_1$[27]-are not based on the product of current values of Q^0, $P^o a$, and $P^o c|a$. In the corresponding regressions, the relevant regressors are estimated either via the reduced form regression equation $\left(P\hat{X}Q_1\right)$ or through distributed lag functions and related instrumental variables (TXQ_1, PDL_1, PXQ_{1-1}) that do *not* include contemporaneous values of Q^0, $P^0 a$, and $P^o c|a$. Yet regression equations using PXQ_{1-1}, TXQ_1, PDL_l, and $P\hat{X}Q_1$ which always include $P\hat{a}$ and $\hat{P}c|a$ *(reduced form* estimates of $P^0 a$ and $P^o c|a$) generally show results similar to those achieved with PXQ_1 or PXQ_2 as measures of execution risk. Furthermore, were the execution-risk effect spurious, or biased upward, then the estimated deterrent effect associated with PXQ_1 should have been larger in absolute value than the estimated effects associated with these other four measures. In fact, however, just the reverse is observed: the PXQ_1 coefficient is the smallest in magnitude. For the subperiod ending in *1967,* for example, the estimated coefficient associated with $P\hat{X}Q_1$ is found to be - 0.081 with a standard error of *0.02*8-one of the highest estimates-although $P\hat{X}Q_1$ is free of bias due to negatively correlated errors.

A more peculiar aspect of the authors' analysis of measurement errors is their introduction of random errors to the FBI's measure of the total number of murders, Q, which appears in the numerator of the dependent variable and in the denominator of one of the conditional execution-risk measures, PXQ_1. This exercise seems irrelevant to my own work and is potentially misleading to the uninformed reader. For what Klein *et al.* seem to do, in fact,[28] is to set every observation of PXQ_1 equal to a *single* value – its mean – and then introduce random errors to Q "that average 2% of the magnitude of Q." Consequently, the variation in their constructed measure of PXQ_1 arises *entirely* from the induced errors. A regression of the murder rate on the constructed "PXQ_1" variable would produce a regression coefficient of approximately negative unity – regardless of the actual variance of the induced error – simply as an artifact of the exercise. The important point is that while I used alternative "expected values" of PXQ_1 estimated through appropriate "reduced form" regression equations, distributed lag functions, and related instruments that do *not* contain contemporaneous measures of Q in order to *eliminate*, in principle, a "spurious" negative correlation between the dependent variable and the risk of execution measure, Klein *et al.* apparently achieve the reverse. They seem to force a correlation between these variables that arises only from measurement errors.[29] Their exercise could not provide *any* clues as to the magnitude of any bias in my estimates of the deterrent effect of capital punishment.

Essentially the same criticism applies to the authors' experiment of introducing random errors in the FBI's measures of the probability of arrest and the conditional probability of conviction. P^0a and $P^o c|a$, appearing both in the denominator of PXQ_1 and as separate regressors. Their experiment is irrelevant. In all the reported regressions, I used as regressors reduced form estimates of these variables, $P\hat{a}$ and $P\hat{c}|a,$ as well as the various estimates of the conditional execution risk enumerated above that are not based on contemporaneous values of P^0a and $P^o c|a$. Therefore, the allegation that errors of measurement in P^0a and $P^o c|a$ alone account for the magnitude of the estimated execution-risk effect is totally misleading.

The paper consistently gives the impression that somehow deterrent effects estimated in my work are exaggerated in the direction of the hypothesized results when there are at least equally good reasons to expect the opposite. My analysis considers some such possibilities.[30] Perhaps the strongest evidence bearing on the errors problem in connection with the estimation of the execution-risk effect, however, is obtained through the analysis of independent cross-sectional data from 1940 and 1950.[31] These independent samples provide for measures of the conditional risk of execution that are *independent* of estimates of the number of crimes. Thus, estimates of the $P^0 e|c$ effects in the latter study are entirely free of any negative biases due to correlated errors. In fact, the estimates derived there corroborate the results of the time series analysis and provide estimates of the deterrent effect of execution risk that are appreciably larger than those obtained from the time series analysis.[32]

4. The "Effect" of All Crimes Against the Person on Murder. One of the major points of the Klein paper arises from the introduction of an index of other crimes, CR, and particularly, of violent crimes, VCR, among the set of variables explaining the murder rate. This results in elimination of virtually all the apparently significant relationships-not just the effect of execution risk noted by the authors-in the regression equation. Again, the relevance of this exercise either as a test of theory or as a constructive econometric device is questionable since there is no theoretical rationale for those other crimes to "cause" murder. Different crimes may largely be caused by common and highly correlated factors the effect of which would, of course, be obscured by the attempt to "explain" one crime by another. The estimation of the effects of deterrence variables specific to one crime by this method would be particularly inefficient when the other crime introduced as an explanatory variable can serve simply as a proxy for the first crime itself.

5. An Uncritical Adoption of an Erroneous Argument. The paper at Section 2.3 and in the conclusion at point 4 repeats an argument by Passell and Taylor[33] that erroneously interprets and misapplies implications of my model and empirical results for the relation between the absolute number of murders and executions. These writers purport to show that if my "supply-of-offenses" function is transformed so as to relate the number of murders to the number of executions with the number of arrests "held constant," then the results of my econometric investigation imply that an increase in the number of executions will lead to an *increase* in the number of murders. Klein *et al.* refer to this association between murder and execution levels as particularly informative.

The argument is patently paradoxical. Moreover, closer examination shows it to be intrinsically inconsistent and irrelevant to the proper analysis of deterrence. Not only is the transformation of the relevant supply-of-offenses function-relating murder to the *risks* of apprehension and execution-to a function that relates murder to the *number* of apprehensions and executions irrelevant for the estimation of the effects of the pertinent prices of murder on its frequency, it amounts to an erroneous application of price theory; it is equivalent to attempting to estimate the effect of the price of butter on the demand for bread by holding *sales* of bread rather than its price constant. Such an artificial estimation could indeed lead to the paradoxical result that an increase in the price of butter-a classical complement to bread-would lead to an *increase*, rather than the expected decrease, in the quantity of bread demanded. A more complete analysis of this point and related issues is offered in my paper with Gibbons.[34]

By implicitly adopting Passell and Taylor's analysis, Klein *et al.* commit a similar error.

6. The Role of Big Models. The paper at Section 2.3 alleges that I have not dealt with the crime and law enforcement systems underlying my 1975 *American Economic Review* article on murder.[35] This is incorrect. I have specified a simple simultaneous equation model of crime and law enforcement activities in a previous work[36] that is referred to in the above paper as underlying the time series investigation and that has since been extended for other applications. While space limitations precluded full elaboration in the *American Economic Review* paper, a more complete presentation is planned for future work.

It is true that I have not attempted to model a comprehensive general equilibrium system in which crime, the economy, and societal values and morals are jointly determined. The absence of such a model is mentioned repeatedly by Klein *et al.* as a fundamental limitation of my work. The desirability of a comprehensive model which provides maximum information on relations of interest is, of course, not a matter of dispute. However, a larger model size does not necessarily imply more pertinent information. Klein *et al.* do not specify a more comprehensive model or show how such a model would improve my results. Given the presently extant data, I believe it would be more productive to rely on partial equilibrium systems that could be applied against superior bodies of data for the United States and other countries, while seeking a more complete and refined implementation of structural relations concerning private and public defense against crime and the returns from alternative legitimate and illegitimate pursuits. The relevance of such a conventional model should not be underestimated just because it does not purport to bring together economics, sociology, and philosophy in an eclectic manner.

B. *Some Comments on the Fisher-Nagin Paper*

1. *The Identification Problem.* The identification problem should be a legitimate concern for all empirical researchers and especially students of deterrence. Their lack of appreciation of that problem in the past has been a crucial shortcoming in their research and an important source of erroneous conclusions about the deterrent or incapacitative effects of punishment. Our quarrel with the authors, therefore, is not with their exposition of the principles of identification, but rather with the shortcomings of their application of these principles to the theory underlying the economic approach to crime or to my empirical applications, and with their disregard for the consistency of the results with a set of detailed theoretical predictions.[37]

2. *My Model and the Authors' Exposition.* The basic thesis underlying my derivation of the simultaneous equation system of crime and law enforcement activity is that there is responsiveness to incentives on the part of both violators of the law and those who uphold and enforce it. Potential offenders on the whole are assumed to be deterred by the threat of punishment and encouraged by the prospect of differential illegitimate rewards. Potential victims, in turn, are

assumed to respond to the threat of victimization by allocating resources both privately and collectively to minimize the net losses from crime. In particular, the behavior of law enforcement agents is assumed to be compatible with social optimization: the minimization of the net social losses from crime, including the costs of combating crime. This framework gives rise to the classical problem of identification inherent in any market equilibrium involving interaction between demand and supply forces: whereas an increase in deterrence variable s due to exogenous factors is expected to decrease the supply of offenses, an exogenous upward shift in the supply-of-offenses function will, in turn, increase the demand for public law enforcement and the magnitudes of the deterrent and incapacitative instruments.[38] The formulation of the relevant simultaneous equation system also stresses an additional structural relation: the *production* of means of deterrence and incapacitation through the allocation of resources to law enforcement activities. This specification identifies another source of simultaneity that is unrelated to optimal social response: given the amount of resources allocated to law enforcement instruments (including prison space) the higher the rate of offenses, the lower the productivity of law enforcement activity because of the loading or "crowding" of the system.[39] While "crowding" may yield estimates biased toward large deterrent effects, increased demand for enforcement activities in response to exogenous increases in the frequency of offenses would bias these estimates in the opposite direction. For this reason, my empirical implementation focused on the estimation of some of these structural relations via the relevant simultaneous equation estimation techniques that, in principle, avoid any simultaneous equation biases.

Fisher and Nagin do not challenge directly the economic approach underlying this work. However, their discussion of the basic problem if identification in the studies on deterrence that they survey focuses on only one source of simultaneous equation bias-the one due to "crowding"-while virtually ignoring the more fundamental identification problem due to optimal social response in the demand for enforcement. That the authors are preoccupied with the possibility of "crowding" effects (although not systematically-see point 3 below) while making only fleeting reference to the opposing supply shift bias betrays an unjustifiably selective formulation of the problem. While "crowding effects" might be expected in the short run due to adjustment costs,[40] it is rather unlikely that such effects would dominate the association between crime and law enforcement instruments persistently and, in particular, the association reflected in cross-sectional work indicates a strong positive association between the frequency of offenses and expenditures on police activity across different states.[41]

An exclusive reliance on "crowding effects" as the source of the identification problem-the assertion that "crime may deter punishment" as much as punishment deters crime-amounts to reliance on a theory that has little in common with economic methodology. Such a theory is indeed identified in the authors' general introduction (Section I) and ascribed to Blumstein and Cohen and to Blumstein, Cohen, and Nagin.[42] These writers hypothesize that "society is willing to deliver only a limited amount of punishment." Thus, as "crime rates increase, a relatively constant level of punishment is maintained by adjusting the standards defining criminal behavior, or by reducing the probability of sanctions being imposed or the severity of sanctions, or by all of these."[43] If this were a realistic description of the operation of the legal and enforcement system, then it would indeed be likely that crime would deter punishment rather than vice versa.

However, Blumstein *et al.* raise a spectre entirely incompatible with both logic and empirical observation. The proposition that society wishes to deliver a constant amount of punishment-regardless of the costs and social benefits of punishment-is as implausible as the proposition that society wishes to suffer a constant level of criminal abuse at all times regardless of the opportunities to minimize the corresponding social losses. Moreover, as noted above, more crime is, in fact, associated with greater police expenditures. While Fisher and Nagin do not formally adopt this theory, their discussion of the identification problem and their own outline of the simultaneous equation system of crime and law enforcement activity lean heavily toward this very approach.

3. *The Unexplored Implications of Their Argument.* Having raised the crowding issue as the crux of their argument on the identification issue, the authors then fail to examine its implications in the context of the studies they examine or of any other evidence that might illuminate its relative importance. To understand this failure, it is important to keep I mind my actual treatment of the identification problem. My analysis produced estimates of both single-offense functions and subsets of supply-of-offenses functions relating to different crimes through use of two-stage least squares and "seemingly unrelated" simultaneous equation estimation techniques that deal systematically with the identification problem. The authors' specific criticism of identification restrictions applied by me is based on an erroneous description of my actual work,[44] and it ignores subsequent work by others that demonstrates the robustness of my estimates to changes in specification restrictions.[45] The criticism is, at best, inconsistent with Fisher's own reliance on "near identification" to estimate relationships of interest in applications of economic theory he himself pursued.[46]

Suppose for the sake of argument, however, that the estimates of deterrent effects derived through applications of the Ehrlich model did reflect the "deterrent effect of crime on punishment" rather than the deterrent effect of punishment on crime. Then a set of expectations follows that must be consistent with the magnitude of reported estimates across different crimes and different data sets. As the following comments show, the prevailing evidence is incompatible with Fisher and Nagin's basic assertions.

(a) The crowding effect cannot, by any plausible analysis, apply equally well to all crimes. Serious offenses, that is, those imparting the greatest social loss, would be least likely to be affected. If prison space were in relatively short supply with no "inventories" available-to use the primary illustration of Fisher and Nagin-then violent offenders would not be expected to be released when larcenists or offenders convicted of nonindex crimes occupy much of the overall prison space (as they do). If follows, then, that the estimated deterrent effects associated with the more serious index crimes would, by this argument, be substantially smaller in magnitude than those associated with larceny and auto theft. The evidence from my cross-sectional analyses of data on seven felonies in 1940, 1950, and 1960 reveals, however, no significant differences on average among the magnitudes of deterrent effects of probability and severity of punishment across crimes against person and property, or murder and assault relative to theft.

(b) A crowding bias would imply that estimated deterrent effects based on 1940 data should be much smaller than estimates based on data from 1960 and 1970 since crime levels were *declining* in the late 1930s, whereas in the late 1950s and through the 1960s there has been a dramatic upturn in all rime rates. My evidence does not, however, reveal substantial differences among comparable estimates of deterrent effects in different years. Moreover, subsequent studies by Robling and by Bartel[47] indicate that the estimated deterrent effects in 1970 are in fact *lower* in absolute magnitude than those I estimated for 1940, 1950, and 1960.

(c) Empirical evidence reveals a considerable degree of stability in estimates of the length of time served in state prisons by prisoners released in different years (T). Bureau of Prisons' statistics from 1940, 1951, 1960, and 1964 show relatively little variation in the median time served for specific felonies over the past few decades.[48] More important, it is doubtful that the estimated coefficients associated with T in various regression analyses would be subject to any appreciable simultaneous equation bias because T measures the time served by prisoners *released* during the sample year. This variable, then, can be considered largely free of a "crowding bias" for a given year's crime rate. Indeed, my recent cross-sectional study[49] strongly supports this interpretation. In this latter work, data on T from the early 1950s are used as measures of anticipated length of imprisonment in both 1950 and (for lack of data) 1940, with the results in both years being statistically indistinguishable.

Thus, none of these direct implications supports Fisher and Nagin's criticism.

4. *Inaccuracies, Misrepresentations, and Eclecticism Concerning Identification Restrictions.* Fisher and Nagin criticize me for the exclusion of certain "socioeconomic" variables as well as unemployment and labor participation rates from the supply of offenses as part of my identification restrictions. Their assertion is factually incorrect. Unemployment, labor force participation, and age composition have been consistently included in the supply-of-offenses function, along with probability and severity of punishment, income and income inequality, and the measure of racial composition.[50] The exclusion of the former subset of variables in some of the reported regressions was done not for the purpose if imposing identification restrictions but only when these variables proved to have no systematic relationship with the dependent variable of interest.

What Fisher and Nagin's discussion ignores is that the frequency of offenses relates to basic *objective opportunities* that the theory identifies as relevant: the "negative" incentives of prospective sanctions and the "positive" incentives of relative illegitimate earning opportunities. These are accounted for directly by measures of probability and severity of punishment, the level and distribution of income, unemployment, and labor force participation rates. I have argued for introduction of measures of age and racial composition essentially as "correctors" of imperfect measures of probability and severity of sanctions and of legitimate earning opportunities. This argument generally has been found compatible with the results of the empirical analysis. In contrast, I see no reason for inclusion of population size and density or of police expenditures[51] in the supply-of-offenses function. Since the basic relevance of these variables, under the economic approach, lies in their effect on magnitudes of deterrence variables-the direct "prices" affecting criminal activity-they need not have any independent effect on the frequency of offenses. Rather, they belong in other structural equations of the model. Fisher and Nagin's insistence on the introduction of police resources in the *supply-of-offenses equation* in addition to deterrence variables follows theoretical eclecticism that is entirely unclear. The empirical results of my work lend considerable support to introduction of police expenditures and population size and density in the *production function* relating to the probability of punishment and for their exclusion from the supply-of-offenses equation.

It is, of course, easy to label any identification restrictions as "arbitrary," especially when little regard is shown for the theoretical analysis underlying the model's construction. Fisher and Nagin fail to point out, however, that sensitivity analysis reported both in my work and in the Panel's own commissioned study by Vandaele[52] testify to the basic robustness of the results to alternative specifications.[53] The estimates of deterrent effects are little affected even when all of the demographic variables are included in the supply function and even when lagged crime rates are not included in the reduced form.[54] The authors' objection to the use of *any* lagged endogenous variables for purposes of identification because of presumed serial correlation, however, is based mainly on speculation and is itself quite arbitrary. They do not present direct evidence that would invalidate reliance on lagged police expenditures in the reduced form. Moreover, rejection of all lagged endogenous variables would invalidate an extraordinarily large number of respectable economic works. The use of such variables I hardly unique to econometric work on deterrence.

5. *The Authors' Disregard of Tests Indicating the Robustness of My Findings.* Aside from their neglect of the theoretical analysis underlying my econometric specification and the evidence reported on alternative specifications, Fisher and Nagin ignore relevant evidence provided by Vandaele.[55] His paper finds my estimates of deterrent effects to be highly robust with respect to various alterations in identification restrictions and other changes in specification.

Vandaele's work includes three types of modifications: (1) deleting variables from the reduced form equation in stages; (2) enlarging the supply-of-

offenses function to include all the reduced form variables except lagged crime rates and lagged police expenditures; and (3) using lagged police expenditures as the only variable excluded from the relevant supply function. In addition, Vandaele performed these modifications after omitting certain states from the observation set. All of these experiments led him to conclude that "within the available data, the negative relationship between the crime rate and the probability of imprisonment and between the crime rate and the time served are not spurious."[56]

The authors' total disregard of Vandaele's experiments is surprising because it addresses some of their critical comments directly.

6. *The Role of Theory in Evaluating Empirical Work.* The extent to which estimates of behavioral relationships derived through valid econometric techniques are subject to simultaneous equation biases *ultimately must be judged by examining the consistency of the results with detailed theoretical expectations* the relevant priors. Indeed, Fisher himself shares this perspective elsewhere: "[w]hile nobody can afford to ignore the possible existence of the identification problem, neither is it legitimate to assume its presence without consideration of the economics involved."[57] Regrettably, Fisher does not pursue systematically his own recommendation in connection with the evidence on deterrence. On theoretical grounds the critics do not-and cannot-deny the proposition that punishment deters crime more than they can deny the basic demand law that increases in price discourage the quantity demanded. Nor can the authors deny that a negative association between crime and punishment has indeed been established. Essentially, their criticism amounts only to *speculation* that the estimated deterrent and incapacitative effects *might be* overstated while it disregards important considerations[58] suggesting possible understatement of such effects. The strongest retort to the authors' speculations is provided by the array of empirical findings developed through analysis of different crimes, different cross-sectional statistics, and different time series data that appear highly consistent with detailed and even sharp theoretical predictions. The results, for example, of my 1960 cross-section analysis,[59] my time series study of murder,[60] and Vandaele's time series analysis of auto theft,[61] all based on simultaneous equation estimation techniques which also systematically treat serial correlations, are consistent with theoretical predictions concerning the signs of virtually every key coefficient.[62] More strikingly, the findings by and large support sharp theoretical implications concerning the *ranking* as well as the signs of the elasticities of particular deterrence variables. For example, my empirical investigation of the time series of murder reveals that, as predicted, the estimated elasticity of the murder rate with respect to the risk-of-apprehension measure exceeds that with respect to the conditional probability-of-conviction given charge, which, in turn, exceeds the elasticity with respect to alternative measures of the conditional risk of execution. Fisher and Nagin's discussion at Section IV.B of their paper, which conjectures a different ranking, indicates that they may not be aware of these sharp theoretical implications of the economic approach to crime. Furthermore, my recent study on murder and the death penalty[63] shows an overall compatibility between the cross-sectional and time

series[64] analyses that is not always present in even the most traditional areas of applied economics. It is ultimately this array of findings and its consistency with detailed and sharp theoretical predictions that militates profoundly against any presumption of appreciable simultaneous equation biases. In particular, it is inconceivable that all of these results can be explained away consistently as arising from the "crowding" effects stressed by Fisher and Nagin.

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Any empirical work of applied economics can be challenged by raising the spectre of the identification problem, the possibility of "favorable" errors of measurement, or by claiming that additional relevant variables are "missing," or that select observations should have been deleted. But mere speculation along such lines that is unaccompanied by systematic and supporting theoretical and empirical analyses superior to those pursued in a given study is of little value. The existing estimates of deterrent effects no doubt can stand improvement through positive and constructive research using superior data and more refined estimation techniques. The analyses of both Klein *et al.* and Fisher and Nagin fail to make a *prima facie* case, however, for the allegation that the existing evidence is necessarily biased in any particular direction.

III. THE WORK OF THE PANEL AND ITS IMPLICAITONS FOR FUTURE RESEARCH

A. *The Panel's Conclusions on Deterrence*

Despite its own recognition that the evidence from the empirical studies of the deterrence hypothesis clearly leans toward a proposition supporting deterrence,[65] the panel admits to a reluctance to "assert that the evidence warrants an affirmative conclusion regarding deterrence."[66] That results based on observation al statistics, as opposed to truly "controlled" experiments, cannot constitute a proof for any proposition is, of course, well recognized in statistical literature as well as in careful studies of the deterrence hypothesis. Hardly any empirical study in the behavioral sciences is immune to this basic limitation. However, the panel's reticence in properly recognizing the preponderance of the accumulated evidence apparently derives primarily from those reviewers' arguments addressed in the preceding section: possible measurement errors, "missing" variables, and an identification bias. Yet, as particularly points 2, 3, 4, 5 and 7 of Section II.A and points 1-6 in Section II.B have demonstrated, the arguments are variously sciolistic, selectively advanced, and, almost invariably, speculative.

In maintaining steadfast pessimism regarding the accumulated evidence, and even the prospects for future research,[67] the Panel ignores the most basic, albeit unhighlighted, conclusions arising from its commissioned reanalyses and survey. Most fundamentally, the surveys challenge neither the propriety of the theoretical structure underlying the economic approach to crime nor the general statistical methodology used to implement the theory against available data.

Moreover, the reanalyses replicate to a high degree previously published results and attest to numerical accuracy of the computations and of the data. In other words, the Panel's extensive investigations of studies pursuing the economic approach reveal no substantive errors in research that has applied, in the Panel's words, "complex"[68] scientific methods and that has, by implication, exhibited at the very least conventional care in derivations of the results.

Thus, the Panel's reservations toward the reported findings of apparent deterrent effects stem not from any mistakes uncovered or from any fundamental methodological disagreements but instead are founded upon various conjectures-a level of criticism quite different in kind. Indeed, the impression derived from the entire document of the Panel is that the authors were not so much interested in rational and objective evaluation of the empirical evidence on deterrence as they were intent on showing that evidence to be defective. While the specific interpretations of statistical findings may quite rightly become the object of scholarly dispute, none of the work of the panel and its commissioned papers attempts to provide a systematic and comprehensive alternative explanation for the amalgam of cross-sectional and time series evidence consistent with the deterrence hypothesis. It seems inappropriate that evidence consistent with a set of detailed behavioral propositions emanating from a theory that also have proven useful in explaining a variety of other expressions of human behavior is hardly given equal weight to a set of speculations and some ad hoc behavioral propositions which do not derive from logical principles of general applicability.[69]

Inescapably, a review of the sort undertaken by the Panel begs the question of who is to review the reviewers. On the one level, the panel must shoulder responsibility for the scholarship of the papers commissioned and, of course, those by its staff and members. The control brought to bear by the Panel as a scientific review board is open to serious questions when these papers present largely irrelevant exercises as substantive,[70] accept uncritically those findings denying deterrence while viewing those supporting deterrence as inherently suspicious,[71] cite patently erroneous exercises as informative,[72] and present published work incompletely and inaccurately,[73] while referring repeatedly to previous work by Panel members, staff, and contributors as particularly instructive.[74]

More directly, the independent discussion provided by the authors of the Report suffers from major methodological shortcomings. In the first place, the report does not exhibit a systematic comprehension of the theory underlying some of the reported tests of the deterrence hypothesis. The economic model of criminal behavior points out the inseparable connection among indicators of certainty and severity of sanctions as interdependent components of the basic "prices" of criminal activity. Yet the report considers separately "models" and the results of empirical studies relating exclusively to apprehension risk, conviction risk, and execution risk as if these were independent instruments of deterrence that can be separated theoretically or for the purposes of empirical implementation and statistical inference.[75] In particular, the artificial distinction maintained throughout the report between capital and noncapital sanctions is theoretically implausible and methodologically unsound. It leads, for example, to the implicit acceptance of the peculiar "finding" that potential murderers are deterred by an additional month's imprisonment but not by the threat of execution.[76] Not only does the report fail to stress the comprehensive nature of the deterrence hypothesis as formulated by both classical and contemporary economists, it also refers to studies that misapply the relevant analytical framework or use unacceptable empirical counterparts of theoretical structures, as providing particularly relevant evidence.[77] The report's arbitrary assertion that virtually all social and demographic variables, including expenditures on police and density of population, belong in the "supply-of-offenses" or "crime-generating" function, even when the relevant punishment risk and severity are accounted for,[178] is, at the least, inconsistent with the thrust of the economic approach which identifies components of positive and negative incentives as the basic determinants of participation in criminal activity.

It is further unclear what rules of reasoning were applied in justifying repeated assertions as to what constitutes "plausible" identification restrictions[79] or why the likelihood of simultaneity between crime rates and certainty of sanctions is lower at the earlier rather than later stages of the criminal justice process.[80] The reliance on speculative reasoning is also apparent from the fact that in analyzing potential identification biases allegedly voiding regression results[81] the report recognizes only one source of simultaneity relation-that due to the crowding or loading of the law enforcement system during times of high crime-while ignoring the more fundamental source of simultaneity stressed in the literature[82] due to optimal (upward) adjustment in deterrence instruments when crime rates rise because of exogenous factors,[83] and which could bias estimates derived through classical least squares techniques in a "perverse" direction. The report contains, in addition, a number of factual errors and inaccuracies.[84]

Furthermore, the Panel's discourse on potential biases is strikingly uneven. Only possibilities that estimated deterrent effects have been exaggerated are raised while cogent considerations (see, for example, point 3 of Section II.A and point 2 of Section II.B) that could suggest the opposite are simply ignored. While the Panel singles out potential simultaneous equation bias as "the most important factor"[85] behind its suspicion of the evidence *for* deterrence, it overlooks its own commissioned evidence[86] that indicates, in the case of the 1960 cross-sectional analysis, even more sizable deterrent effects than those reported by me. It is also noteworthy that although constantly mentioned (and one-sidedly) in connection with the empirical evidence on apprehension, conviction, and punishment risk, the simultaneous equation bias (the identification problem) vanishes entirely as an issue in the Report's discussion of the evidence pertaining to the deterrent effects of the death penalty.[87] As recognized in the recent literature on deterrence, simultaneous equation biases, if present, are likely to bias regression estimates of the effect of the risk of execution on murder toward positive or "perverse" values, because of

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the predicted greater tendency of juries, judges, and other law enforcement authorities to impose and implement the death penalty in times when the risk of murder in the population increases significantly. Evidently, the authors of the Report have confidence in the ability of researchers to overcome simultaneous equation biases in research on the deterrent effect of capital punishment, but not on deterrence in general.

Yet an even more fundamental issue of responsibility is the conduct of the Panel. While invoking the prestigious auspices of the National Academy of Sciences, the Panel has not insured an adequate representation of competing views and internal critical evaluations. Furthermore, the Report's authors convey the impression that they present innovative analysis while failing to credit the published literature for addressing all the basic issues that they put forward. Each of the principal themes which they explore-errors of measurement, incapacitative effects, and the problem of identification-have previously been developed theoretically and, given data exigencies, also treated systematically in my own work.[88]

B. *Recommendations by the Panel and Implications for Future Research*

The Panel's list of recommendations for future research is by and large too general and noncommittal to be useful to researchers interested in overcoming actual difficulties of measurement and estimation. While the dearth of concrete recommendations for the "correct" conduct of future research on deterrence is hardly likely to impede future innovative work, two portentous proposals do stand out which require comment. Both, if enacted, would abridge academic freedom. Because the Panel has been financed by the Law Enforcement Assistance Administration, quite probably the largest funding agent for research in the area of deterrence, the panel's proposals potentially have a far-reaching impact on resource allocations for research. Aside from the dangers posed to freedom of research, these recommendations serve ultimately more as a commentary upon the panel and perhaps its intentions than as a reflection of any substance contained within its report.

Is the Panel's considered judgment "available results of the analysis on capital punishment provide us with no useful evidence on the deterrent effect of capital punishment."[89] Not only do they indefensibly judge existing evidence as useless, but the evidence of future research as well, for the Panel recommends, in effect, that funding research in this area cease, as it is not likely to provide inputs "that will or should have much influence on policy makers."[90] The implication that funding cease for research on the deterrent effect of the death penalty, however ominous in itself, is also surprising, in view of the opinion of the Panel's own commissioned commentators on death penalty research, Klein *et al.*, who describe such an endeavor as "a fascinating area of research with much scholarly potential."[91]

The second recommendation of the Panel that would act to stifle academic inquiry is, simply put, the institutionalization of some version of itself. The Panel recommends a permanent, official Advisory "Board to "oversee developing research."[92] Such a board "would be responsible for reviewing results as they emerge in the research literature" and "provide objective assessment of the validity of the research. . . ."[93] Such interference by a group of "validators" carries the danger of impeding legitimate scientific inquiry by politicizing research. The difficulties with an official board of review for research in any area are manifold but certainly include the problems confronted unsuccessfully by this panel: who are the reviewers and how are they selected; how are resources within the Board allocated in selecting which developing research to evaluate; and what checks circumscribe the work of the reviewers to assure that theories and findings popular among the Board members are not encouraged a the expense of those not as fashionable? Indeed, the work and conduct of this Panel are, as we hope we have shown, compelling arguments against the Panel's recommendations.

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[1] The Panel's work has been largely ancillary to the work of the Committee on Research on Law Enforcement and Criminal Justice under the auspices of the Assembly on the Behavioral and Social Sciences of the National Academy of Sciences. However, the Committee did not participate in the selection of the members of the Panel nor in the selection of the commissioned authors and reviewers. Neither did the Committee guide the panel's work. Moreover, the Panel's report has not been submitted to the Committee nor has the Committee endorsed, approved, or otherwise accepted the Panel's report. The Committee has issued an independent report. Committee on Research on Law Enforcement and Criminal Justice, Understanding Crime – An Evaluation of the National Institute of Law Enforcement and Criminal Justice (Nat. Acad. Sci., Susan O. White & Samuel Krislov eds., 1977).

[2] Panel on Research on Deterrent and Incapacitative Effects, Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates (Nat. Acad. Sci., Alfred Blumstein, Jacqueline Cohen, & Daniel Nagin forthcoming 1978) hereinafter cited as Report. Aside from the actual report of the Panel, the volume includes commissioned papers. Although our general evaluation relates to the entire report, the substantive discussion focuses on the Panel's report on research on the deterrent effects of criminal sanctions.

[3] Report i.

[4] Report, introductory statement titled 'Notice'.

[5] Report 7.

[6] Two papers were commissioned by the Panel specifically for the purposes of "reanalysis" of my studies. See Walter Vandaele, Participation in Illegitimate Activities: I. Ehrlich Revisited (cited in tab. of contents, draft, as Sensitivity Analysis of Simultaneous Econometric Models); Lawrence R. Klein, Brian Forst, & Victor Filatov. The Deterrent Effect of Capital Punishment: An Assessment of the Estimates, in Report (hereinafter cited as Klein *et al*.). Both papers used in their reanalyses the original data provided to them by me.

[7] Klein *et al., supra* note 6.

[8] See Franklin M. Fisher & Daniel Nagin, On the Feasibility of Identifying the Crime Function in a Simultaneous Model of Crime and Sanctions, among the Report's commissioned papers.

[9] For example, Marvin E. Wolfgang testified in 1972 before Congress that "I personally believe the evidence is already sufficiently compelling to indicate that the death penalty fails to deter." Capital Punishment: Hearings Before Subcommittee No. 3 of the Committee on the Judiciary,

House of Representatives, 92nd Cong., 2d Sess. 170 (1972). F.E. Zimring previously has described research by Ehrlich as being among the "sophisticated . . .*dangerous* descendents" (emphasis added) of earlier works by contemporary economists on the economics of crime and punishment. Franklin E. Zimring & Gordon J. Hawkins, Deterrence: The Legal Threat in Crime Control 55 & n.97 (1973).

[10] Isaac Ehrlich, Deterrence: Evidence and Inference, 85 Yale L. J. 209 (1975).

[11] Isaac Ehrlich, The Deterrent Effect of Capital Punishment – Reply, 67 Am. Econ. Rev. 452 (1977).

[12] Isaac Ehrlich & Joel C. Gibbons, On the Measurement of the Deterrent Effect of Capital Punishment and the Theory of Deterrence, 6 J. Leg. Studies 35 (1977).

[13] Isaac Ehrlich, Capital Punishment and Deterrence: Some Further Thoughts and Additional Evidence, 85 J. Pol. Econ. 741 (1977).

[14] Klein *et al., supra* note 6, at § 3; Isaac Ehrlich, The Deterrent Effect of Capital Punishment: A Question of Life and Death, 65 Am. Econ. Rev. 397 (1975).

[15] Klein *et al.* § 3.3 assert an error in estimation of the reported "linear regression" in my paper, *supra* note 10, at 219 n.29 due to an alleged error in the selection of a value of $\hat{p}$ (the relevant first-order serial correlation coefficient) that constitutes an input into the three-round estimation procedure of Ray Fair used in my original Am. Econ. Rev. study, *supra* note 14. We have pursued this matter at length by thoroughly checking the algorithms of the ESP computation package utilized and by verifying the correctness of the procedure with Ray Fair who, as we later learned, is himself the author of the relevant subroutine in ESP. The fact is that all of the results reported in the Am. Econ. Rev. paper were derived by this same procedure. While Klein *et al.* assert that my estimate of $\hat{p}$ = -.119 does not globally minimize the standard error of the equation, the table below indicated that it does. Indeed, the value of $\hat{p}$ selected by Klein *et al*. ($\hat{p}$ = .243) produces a standard error of the relevant equation appreciably higher than the minimum.

Rho	Standard Error of equation
-.35	.002110
-.25	.002056
-.15	.002036
-.119	.002035
+.0	.002053
.10	.002080
.20	.002110
.25	.002125
.35	.002164

[16] See Isaac Ehrlich, *supra* note 10; *id, supra* note 11.

[17] Isaac Ehrlich, *supra* note 13.

[18] Isaac Ehrlich, *supra* note 10; Klein *et al.* § 3.1.

[19] This argument is elaborated in Isaac Ehrlich, *supra* note 11.

[20] *Id.* at 456.

[21] Klein *et al.* § 2.1.

[22] *Id.*

[23] See Isaac Ehrlich, *supra* note 13, at § II.B(4).

[24] Klein *et al.* § 2.1.

[25] See Isaac Ehrlich, *supra* note 11, at 455n.9; 1973 F.B.I. Uniform Crime Reports for the United States 59, tab.2.

[26] For a more complete analysis of the issue see Analysis of Measurement Errors, app. 1 to § IV, in Isaac Ehrlich, Participation in Illegitimate Activities: An Economic Analysis, in Essays in the Economics of Crime and Punishment 68, 127 (Gary S. Becker & William Landes eds. 1974).

[27] See Isaac Ehrlich, *supra* note 11, for the definition of these conditional execution-risk measures.

[28] Klein *et al.* § 2.2 n.12.

[29] The authors' incomplete description of this experiment suggests that they have actually introduced in the murder regression equation a reduced form estimate of this measure of PXQ_1, constructed to exhibit variability solely due to measurement errors, and obtained a regression coefficient of about negative unity. The entire procedure is puzzling because if the measurement error introduced is truly an independently and identically distributed random variable – uncorrelated with the set of exogenous and predetermined variables – then the reduced form estimate of their constructed PXQ_1 variable has an expectation of a constant equal to its mean value. The authors' reduced form estimate of PXQ_1 would then be only a proxy for the constant term in the second stage, exposing the regression analysis to multicollinearity and rendering the regression results meaningless. The authors do not report any sensitivity analysis in connection with their results.
[30] Isaac Ehrlich, *supra* note 11, at 408.
[31] See Isaac Ehrlich, *supra* note 13.
[32] It is remarkable that in their discussion of Passell's paper investigating cross-sectional data on murders and executions in the U.S. in the 1950s and 1960s (see Peter Passell, The Deterrent Effect of the Death Penalty: A Statistical Test, 28 Stan. L. Rev. 61 (1975)), Klein *et al.* state: "We find no serious problems with the Passell paper" (id. at § 1.3 n.6) which reports insignificant deterrent effects of execution-risk measures on murder using an allegedly similar methodology to that developed in my 1975 Am. Econ. Rev. paper, *supra* note 14. Not only do all of the authors' reservations in connection with biases due to errors of measurement, "missing variables," spurious effects, and the absence of a "big model" vanish entirely here, but Klein *et al.* are also admirably undisturbed by Passell's report that while both risk and length of imprisonment do exert deterrent effects on murder, execution risk does not. During the periods under his investigation, the length of imprisonment for murder averaged ten years, while the remaining life expectancy of a typical murder convict was 25 years. If execution is equated in severity to a genuine "imprisonment for life," Passell's argument amounts to the curious proposition that although 1 additional month of incarceration after 10 years of imprisonment for murder deters, 300 additional months of incarceration do not.
[33] See Peter Passell & John B. Taylor, The Deterrent Effect of Capital Punishment: Another View (unpublished discussion paper, No. 74-7509, Dep't of Econ. Colum. Univ. 1975).
[34] Isaac Ehrlich & Joel C. Gibbons, *supra* note 12.
[35] Isaac Ehrlich, *supra* note 14.
[36] See sources in note 37 *infra.*
[37] The authors' remarks concerning my work are addressed entirely to my cross-sectional investigations. See Isaac Ehrlich, Participation in Illegitimate Activities: An Economic Analysis (unpublished Ph.D. dissertation, Colum. Univ., 1970); Isaac Ehrlich, *supra* note 26. The authors advance no discussion of the identification issue in connection with my time series analysis of murder and the death penalty.
[38] For a formal derivation see Isaac Ehrlich, *supra* note 26.
[39] See *id.*
[40] See *id.* at 92.
[41] *Id.* at 99 n.42.
[42] See Alfred Blumstein & Jacqueline Cohen, A Theory of the Stability of Punishment, 64 J. Crim. L. & Criminol. 198 (1973); Alfred Blumstein, Jacqueline Cohen, & Daniel Nagin, The Dynamics of a Homeostatic Punishment Process, 67 J. Crim. L. & Criminol. 317 (1976).
[43] See Franklin M. Fisher & Daniel Nagin, *supra* note 8, at § I.
[44] See § II.B(4) *infra.*
[45] See § II.B(5) *infra.*
[46] See Franklin M. Fisher, A Priori Information and Time Series Analysis chs. 1, 5 (vol. 26 Contributions to Econ. Analysis, 1962).
[47] Steve Robling, The Economic Determinants of Criminal Behavior in the Supply of Offenses-New Empirical Evidence for 1970 (unpublished manuscript, Univ. of chi. Bus. Sch., 1974). Ann Bartel, Women and Crime: An Economic Analysis (unpublished manuscript, Colum. Univ. 1977).
[48] See Isaac Ehrlich *supra* note 26, at 89 n.30; *id, supra* note 14, at 407 n.11.
[49] See Isaac Ehrlich, *supra* note 13.
[50] See Isaac Ehrlich, *supra* note 26, at § IV, tab. 6.
[51] See Franklin M. Fisher & Daniel Nagin, *supra* note 8, at §§ II, IV.B.

[52] See § II.B(5) *infra*.
[53] Isaac Ehrlich, *supra* note 37; Walter Vandaele, *supra* note 6.
[54] Isaac Ehrlich, *supra* note 26, at 102 n.46.
[55] Walter Vandaele, *supra* note 6, examines the 1960 cross-sectional data, provided to him by Ehrlich, for the individual and aggregated offenses categories.
[56] *Id.* at 21.
[57] Franklin M. Fisher, *supra* note 46, at 94.
[58] See § II.B(2) *supra.*
[59] Isaac Ehrlich, *supra* note 26.
[60] Isaac Ehrlich, *supra* note 14.
[61] Walter Vandaele, The Economics of Crime: An Econometric Investigation of Auto Theft in the United States (unpublished Ph.D. dissertation, Univ. of Chi. 1975).
[62] For a selected list of other studies, see Isaac Ehrlich *supra* note 10, at 226 n.49.
[63] Isaac Ehrlich, *supra* note 13.
[64] Isaac Ehrlich, *supra* note 14.
[65] See textual quote for note 5 *supra*.
[66] Report 7.
[67] See § III.B *infra*.
[68] Report 19, 81.
[69] The report relies heavily on Blumstein *et al.*s theory of "stable punishment" referred to in § II.B(2) *supra* which explains the observed negative association between crime rates and sanctions on "increased tolerance for criminality . . .where crime is more common." Report 33. The proposition is not only inconsistent with the principle of optimization generally invoked in economic theory (see the discussion in § II.B(2) *supra*), it is also analogous to a theory that posits that the negative association between, say, the price of coffee and coffee purchases is due to the coffee suppliers' desire to obtain a constant "bite" (in dollar terms) from coffee drinkers, that is, when buyers demand more coffee, the manufacturers lower coffee prices to hold constant the total "punishment" they inflict on coffee drinkers.
[70] See § II.A(3) *supra.*
[71] See for example, Klein *et al.*, *supra* note 6, at § 1.4n.6, for their comments on the paper of Peter Passell, *supra* note 32.
[72] See § II.A(5) and Isaac Ehrlich & Joel C. Gibbons, *supra* note 12.
[73] See discussion in § II.B(4) *supra.*
[74] See the Report authors references to Blumstein & Cohen, at 33, 47 [*supra* note 42]; Blumstein, Cohen & Nagin, at 33 [*supra* note 42]; Blumstein & Nagin, at 57, 59 [The Deterrent Effect of Legal Sanctions on Draft Evasion, 28 Stan. L. Rev. 241 (1977)]; Blumstein & Larson, at 47 [Models of a Total Criminal Justice System (report No. P-480 Inst. for Def. Analyses, 1970)]; Nagin at 48, 50, 54, 56, 83 [General Deterrence: A Review and Critique of Empirical Evidence, in Report (commissioned paper)]. Among the Report's commissioned papers see references of Fisher & Nagin to the "Blumstein model"; Nagin's references, at § II to Brian E. Forst [Participation in Illegitimate Activities: Further Empirical Findings, 2 Policy Analysis 477 (1976)] and references, at § IV to Blumstein & Cohen [*supra* note 42]; Cohen's references to Blumstein & Cohen [*supra* note 42] and Blumstein, Cohen & Nagin [*supra* note 42].
[75] Report 40-64.
[76] Report 82-83 and sources cited in note 71 *supra.*
[77] For example, while presenting Forst's work (see Brian E. Forst, participation in Illegitimate Activities: Further Empirical Findings, 2 Policy Analysis 477 (1976) with aggregate cross-sectional data from 1970 as a basis for reluctance to draw more affirmative conclusions on deterrence (Report 56) the Report's authors fail to point out that Forst uses in his version of the supply-of-offenses function a measure of the "quality of the correction system" as a "deterrence variable" (!) along with twelve other sociological, economic, demographic, and "deterrence" variables, nine of which are found to be statistically insignificant. The introduction in the regression equations of so many contrived, irrelevant, and possibly collinear variables defies an efficient test of the role of relevant deterrence variables and indicators of other incentives. More important, Forst uses as a measure of the length of imprisonment the aggregate stock of prisoners divided by the aggregate number of imprisonments per year in state prisons-clearly an inefficient empirical counterpart since it is sensitive to differences across states in the rate of growth of prison populations, annual

changes in the probabilities of apprehension, conviction and imprisonment, the stability of the length of sentences served over long periods of time, and changes in the composition of prisoners by the type of crime committed. Indeed, Forst's attempt to measure the length of incarceration in this way is analogous to and suffers from the same deficiencies as an attempt to measure a population's longevity by its birth rate. Aside from an unacceptable specification, Forst's estimation procedure seems inefficient since he fails to account for heteroscedasticity reported by Isaac Ehrlich, *supra* note 26; *id. supra* note 13, and Walter Vandaele, *supra* note 6. Furthermore, Forst uses a simple linear regression format which he selects from alternative specifications on the basis of an erroneous criterion (the adjusted coefficient of determination rather than the appropriate maximized log likelihood function). In view of this defective formulation and analysis, Forst's failure to estimate significant deterrent effects is understandable. Indeed, a number of studies which use the same cross-sectional data from 1970, but which implement the economic model more carefully, report results that are qualitatively identical to those derived in Ehrlich's study, *supra* note 26 (for references, see note 47 *supra.*) Nevertheless, a staff member of the Panel extols Forst's study as "one of the most thorough analyses," see Daniel Nagin, *supra* note 74, at § II.

[78] Report 43, 60, 61. Also see discussion in § II.B(4) *supra.*

[79] See, for example, Report 43, 54, 60.

[80] Report, 6. At 52 of the Report the authors suggest as their rationale for this argument that "police. . .spend only a small portion of their time apprehending criminal suspects." One fails to see the logical connection between the actual portion of police time devoted to making apprehensions and their role in securing and maintaining given levels of apprehension threat or risk through the totality of their inputs into police efforts.

[81] See, for example, Report 32-33.

[82] See Gary S. Becker, Crime and Punishment: An Economic Approach, J. Pol. Econ. 169 (1968) and Isaac Ehrlich, *supra* note 37; *id., supra* note 14; *id., supra* note 13.

[83] See discussion in § II.B(2) *supra*.

[84] The Report grossly misrepresents Vandaele's actual work and conclusion in Walter Vandaele, *supra* note 6, at §§ 4.2, 4.3, in twice stating that in his reanalysis he "was unable to find evidence of a deterrent effect for crimes against the person," Report 55, 57. In fact, Vandaele's reanalyses generally report *larger* effects in connection with both crimes against person and property. At 59 the report errs in stating that "only one analysis (Orsagh (1973) has estimated the deterrent effect of the conviction risk in the context of a simultaneous model," ignoring work by Walter Vandaele, *supra* note 61, and Isaac Ehrlich, *supra* note 14. The Report's reference is to Thomas Orsagh, Crime, Sanctions, and Scientific Explanation, 64 Crim. L. & Criminol. 354 (1973).

[85] Report 62.

[86] Walter Vandaele, *supra* note 6, at § 4.2.

[87] Report 80-86.

[88] See Isaac Ehrlich, *supra* note 26; *id., supra* note 37.

[89] Report 85.

[90] *Id.* at 86. An explicit recommendation against *funding* additional research on capital punishment has actually appeared in a previous draft of the Panel's report. Its deletion from the final report does not, however, alter the practical implication of the Panel's recommendation since it is addressed to the Law Enforcement Assistance Administration of the U.S. Department of Justice (LEAA), the sponsor of the Panel's work and a major funding agency in the area of crime research.

[91] Klein *et al., supra* note 6, at conclusion.

[92] Report 116.

[93] *Id.* at 116-17.