Offender Expectations and Identification of Crime Supply Functions*

Thomas F. Pogue

University of Iowa

Abstract

Econometric studies of crime have typically assumed that crime rates and sanction levels are determined simultaneously, and to achieve identification they have assumed that particular exogenous variables affect sanction levels but not crime rates.

In this paper, we suggest that individuals' decisions on whether to participate in criminal activity depend on perceived sanction levels that depend in turn on sanction levels realized in past periods. With decisions made in this manner, crime rates and sanction levels are not determined simultaneously, and the identification problem in its usual form does not arise. Our econometric results are consistent with this view; arrest and imprisonment sanctions have little contemporaneous effect on burglary, larceny, and robbery rates. Lagged effects, although greater than contemporaneous effects, are statistically significant only in the case of robbery.

December 1982

*Thomas F. Pogue, Department of Economics, The University of Iowa, Iowa City, Iowa 52242. This research was supported by a grant from the National Institute of Justice (79NI-AX-0015)

I. INTRODUCTION

The crime prevention effects of society's criminal justice (law enforcement) policies are of continuing interest. Reflecting this interest, a number of studies have estimated crime supply functions that presumably show how the application of criminal sanctions affects observed crime rates.[1] The results of some studies have been interpreted as evidence that the threat of imprisonment deters crime. For examples, see Ehrlich (1973, 1975), Sjoquist (1973), and Vandaele (1978). However, this interpretation has been questioned, most notably by Blumstein (1978), who expresses considerable doubt that any study has satisfactorily identified and estimated a cause-effect relationship between criminal sanctions and crime rates.[2]

Most studies have assumed that crime rates and sanction levels are simultaneously determined, and to achieve identification of crime supply equations, they have assumed that particular exogenous or predetermined variables affect sanction levels but not crime rates. The validity of these identifying restrictions is a central issue in the on-going debate about the deterrence implications of existing empirical work.

In this paper, we suggest that individuals' decisions on whether to participate in criminal activity are not significantly affected by prevailing sanction levels. Instead, the decisions made during a given period are based on perceived sanction levels that depend in turn on actual sanction levels of past periods. With decisions made in this manner, crime rates and sanction levels are not determined simultaneously, and the identification problem in its usual form does not arise. But problems of obtaining consistent estimates remain. Section II discusses these matters in more detail. Section III describes the data used in this study; section IV presents estimates of the response of perceived to actual sanctions. The final section summarizes our findings and briefly explores their policy implications.

II. A MODEL WITH LAGGED SANCTION EFFECTS

Numerous econometric studies of crime have estimated equations of the form:

(1) $$C_{ijt} = a_0 + a_1 P_{ijt} + a_2 X_{jt} + U_{ijt}$$

where U_{ijt} is an error term and C_{ijt}, P_{ijt}, and X_{jt} are, respectively, the logarithms of the reported rate of criminal activity of type i in jurisdiction j (state or city) at time t, the clearance rate (probability of arrest) for crimes of type i in jurisdiction j at time t, and exogenous socio-economic variables hypothesized to affect crime rates.[3] Other sanction measures, such as probability of imprisonment and time served, can reasonably be included in crime equations, and previous studies have done so. But to simplify the following discussion, equation 1 includes only one measure.

Most studies have recognized that crime rates may influence sanction levels as well as the converse. Specifically, they have assumed that objective sanction levels depend on crime rates, resources used to apply sanctions, and exogenous variables that influence the sanction levels achieved with given resources:

(2) $$P_{ijt} = c_0 + c_1 E_{jt} + c_2 C_{ijt} + c_3 CT_{jt} + c_4 Y_{jt} + V_{ijt}$$

where V_{ijt} is an error term and E_{jt}, CT_{jt}, and Y_{jt} are, respectively, the logarithms of a measure of resources devoted to the apprehension of offenders in jurisdiction j at time t, an index of overall criminal activity, and exogenous variables that influence arrest probabilities. The usual hypothesis is that P_{ijt} is positively related to E_{jt} and negatively related to CT_{jt} and

C_{ijt}. That is, the probability of clearing a crime by arrest increases as the total resources available for clearing a given set of crimes increases.[4]

The economic theory of crime that leads to equation 1 assumes that criminal activity by a given set of individuals depends on their perceptions of the relative gains and costs of legal and illegal behavior. Objective (actual) sanction levels, measured by arrest and imprisonment probabilities and sentences served, affect crime rates as they influence perceived sanction levels and thereby influence perceived costs of criminal behavior. Thus, in estimating crime equations such as equation 1, previous studies have assumed that perceived and objective sanction levels are one and the same.

But it is likely instead that perceived sanction levels depend on objective sanction levels realized in previous periods.[5] If such is the case, crime equations should include measures of past as well as (or rather than) current sanction levels. That is, instead of equation 1:

(3) $$C_t = a_0 + \alpha_1 P_{t-\Theta} + \ldots + \alpha_K P_{t-\Theta-K+1} + a_2 X_t + U_t$$

where crime and state subscripts are ommitted for notational simplicity, the integer Θ is the number of periods that elapse before a change in the objective probability of arrest has any effect on the perceived probability, and K is the number of past values of the objective probability that influence the perceived probability. If $\Theta = 0$ and $K = 1$, then perceived probabilities adjust immediately (within the current period) to changes in actual probabilities, and equation 3 is identical to equation 1.

The relationship between the objective probability of arrest and the crime rate, given by the values of α_k, incorporates two effects: the effect of the objective on the perceived probability and the effect of the latter on the crime rate. If either of these effects is zero, crime rates will not depend on objective sanction levels. The total effect of a change in the probability of arrest is given by $\sum_{k=1}^{K} \alpha_k$, which is negative if arrest has a crime prevention effect.

The contemporaneous crime prevention effect of a criminal sanction, the effect of P_t on C_t in equations 1 and 3, is identified only if X_t*, the set of exogenous and predetermined variables that affects C_t, P_t, and/or E_t, includes one more variable than does X_t.[6] This necessary condition for identification is met if Y_t includes at least one variable that is not included in X_t; or if $X_t = Y_t$, the condition is met if E_t depends on at least one exogenous or predetermined variable that is not included in X_t.

However, if we accept the argument that the contemporaneous effects of sanctions on crime rates are minimal or non-existent, then $\Theta \geq 1$ and the identification problem does not arise because P_t and C_t are not simultaneously determined.

Regardless of the value assumed for Θ, problems may arise in the estimation of equation 3. Ordinary least squares (OLS) estimates will have the desirable statistical property of consistency only if the sanction variables are statistically independent of the error term. Unfortunately, such is not likely to be the case. If $\Theta = 0$, as most previous studies have assumed, equation 3 includes P_t, which is not statistically independent of U_t if the sanction levels achieved in a particular period depend on the crime rates of that period, as equation 2 implies. In particular, when U_t is relatively large, C_t will be relatively large and P_t will tend to be relatively low -- by the resource saturation hypothesis. The estimated coefficient of P may thus be negative even if the arrest sanction has no crime prevention effect.[7]

If $\Theta \geq 1$ and the error terms are not serially correlated, then the sanction variables in equation 3 will be statistically independent of its

error term, and OLS estimates of its parameters will be consistent.[8] However, such serial independence is unlikely and, consequently, the predetermined sanction variables ($P_{-\Theta}$, etc.) included in crime functions such as equation 3 may be correlated with the error term.[9] If so, OLS will yield inconsistent estimates of the parameters of the crime equations.

Consistent estimates can be obtained by other techniques. If $\Theta = 0$, two-stage least squares (2SLS) yields consistent estimates provided that the identifying restrictions are valid. Consistent estimates can also be obtained by 2SLS when $\Theta \geq 1$.[10] In this case, estimated rather than observed values of the predetermined variables are used in the crime equations. To illustrate, the predetermined (lagged endogenous) variables in equation 3 are $P_{t-\Theta}$, $P_{t-\Theta-1}$, etc. These sanction variables depend on and are therefore correlated with values of exogenous variables in periods prior to and including $t-\Theta$. With 2SLS, each of the predetermined sanction variables is regressed on these exogenous variables. The resulting regression equations are used to generate estimates of the sanction variables. Equation 3 is then estimated with the observed values of the sanction variables replaced by these estimated values. Since the estimated sanction variables are linear combinations of the exogenous variables, they are asymptotically independent of the error term, and the parameter estimates thus obtained are consistent. Moreover, 2SLS estimates are consistent even if the correlation of clearance rates with the error terms arises in part because of errors in the measurement of clearance rates.[11]

However, these estimates are not efficient if there is serial correlation -- if the error for a particular state in a particular year is correlated with the error for that state in other years. When data for more than one cross-section year are available, as is the case in the present study, more efficient estimates of the coefficients of equation 3 can be obtained with the seemingly unrelated regression (SUR) technique. The SUR technique takes account of any serial correlation in the error of each state. There is an efficiency gain from the use of SUR unless errors are serially uncorrelated, in which case the SUR technique yields the same estimates as 2SLS.[12]

An alternative approach to estimating crime functions such as equation 3 is to make an explicit assumption about the serial correlation of the errors. In general, the correlation may be M-order:

$$(4) \qquad U_{ijt} = \sum_{m=1}^{M} \rho_m U_{ijt-m} + e_{ijt}$$

where $E(e_{ijt}) = 0$, $E(e_{ijt})^2 = \sigma^2$, and $E(e_{ijt}e_{ijt'}) = 0$ if $t \neq t'$. In practice, serial correlation can often be represented accurately by a low order ($M = 1$ or $M = 2$) process; in the present study, a first order process ($M = 1$) appears to be adequate. Setting $M = 1$ in equation 4 and substituting for U_t in equation 3 yields after rearranging:

$$(5) \qquad C_t = (1-\rho_1)\, a_0 + \sum_{k=1}^{K} \alpha_k (P_{t-\Theta-k+1} - \rho_1 P_{t-\Theta-k}) + a_2 X_t - \rho_1 a_2 X_{t-1} + \rho_1 C_{t-1} + e_t.$$

If equation 4 with $M = 1$ correctly represents the structure of U_{ijt}, then the error term in equation 5 is not serially correlated. If, in addition, $\Theta \geq 1$, OLS estimates of equation 5 will be consistent because the equation includes only lagged values of the sanction variable, which are statistically independent of the error term e. However, if $\Theta = 0$, then the equation includes the current value of the sanction variable; this current value will be correlated with e, and OLS estimates of 5 will not be consistent.

III. DATA

In collecting and integrating data from a number of sources, our central concern has been to obtain data for a set of states that are consistent both

across states at each point in time and across time for each state. The required data are variables that measure crime rates, sanction levels, and the socio-economic "climate" in each state. Exhibit 1 defines the exogenous (environmental) variables and the variables measuring crime rates and sanction levels that were used in this study.

An ideal data set would include the values of these variables for each state and a substantial number of years. The data set used in this study approaches this ideal data panel in that it includes annual observations on crime rates, clearance rates, and socio-economic variables for each state for the years 1968-77.[13] Clearance and crime rates are available for each crime category included in the FBI crime index. However, the data are far from ideal in that measures of probability of imprisonment and time served are available for only 1960, 1964, and 1970 — for 45 states in 1960 and 1964 and 32 states in 1970. Thus, crime equations that include all three sanction variables (probability of arrest, probability of imprisonment, and time served) can be estimated only for 1970 and adjacent years.

The exogenous variables included in the crime equations, the set X, should reflect factors other than the activities and policies of the criminal justice system that affect individuals' willingness to engage in criminal activity. In particular, variables measuring the relative economic gains from legitimate and illegitimate activites are relevant if, as is widely thought to be the case, property crimes are motivated by prospective economic gains. In addition, variables that reflect differences in reporting practices are appropriately included in X because the dependent variables in our analyses are reported rather than actual crime rates. Variables 1-8 in Exhibit 1 meet one or both of these criteria and are therefore included in X. Of these, variables 1-3 have been used most frequently in other studies.

EXHIBIT 1. Variable definitions

Variable Number	Natural Logarithm of Variable Denoted by	Exogenous (environmental) variables
1.	INC	Per capita income, thousands of dollars.[a]
2.	POV	Percentage of families with income below the poverty level.[e]
3.	NW	Non-white persons as a percentage of total population.[e]
4.	UN	Unemployment rate (unemployed persons as a percentage of population).[a]
5.	PAR	Persons under 18 and not living with both parents as a percentage of total population.[e]
6.	DPOP	Increase in population over preceding 10 years: current population as a proportion of the population of 10 years earlier.[a]
7.	EDH	Percentage of persons 25 years and older who have completed 4 years of high school or more.[e]
8.	URB	Percentage of population living in urban areas.[a]
9.	DEN	Population density.[e]
10.	MVR	Estimated market value of real property, thousands of dollars per capita.[b]
11.	AID	Federal aid to state and local governments of the state, dollars per capita per fiscal year.[a]
12.	GX	Expenditures for purposes other than the criminal justice system, financed with revenue from own sources, state and local governments, dollars per capita per fiscal year.[a]
13.	PTX	Property tax revenue as a proportion of revenue from own sources, state and local governments.[a]
		Endogenous (dependent) variables
14.	B	Reported burglaries per 100,000 population for a particular year.[a]

Exhibit 1 (Continued)

15.	L	Reported larcenies per 100,000 population for a particular year.[a,f]
16.	R	Reported robberies per 100,000 population for a particular year.[a]
17.	P	Proportion of reported burglaries, larcenies, or robberies cleared by arrest; an estimate of the probability of arrest.[c]
18.	PI	Commitments to state prisons for burglary, larceny or robbery as a proportion of reported burglaries, larcenies, or robberies; an estimate of the probability of imprisonment.[d]
19.	T	Median time served in months by persons imprisoned for burglary, larceny, or robbery before their first release.[d]
20.	E	Police protection expenditures of state and local governments of the state, dollars per capita per fiscal year.
21.	CT	Federal Bureau of Investigation Crime Index for a particular year; the total number of index crimes per 100,000 population where index crimes are murder, forcible rape, robbery, aggravated assault, burglary, larceny, and auto theft.[a]

[a]Available annually.

[b]Available only for 1971.

[c]Available annually from 1968.

[d]Available only in 1960, 1964, and 1970. In 1970, available only for 32 states.

[e]Available only in census years (1970 and 1960).

[f]For 1970-1972, L includes only larcenies involving losses in excess of $50; for 1973-77, all larcenies are included.

The remaining five exogenous variables listed in Exhibit 1 are assumed to affect crime rates only indirectly, through their effects on the resources used to apply sanctions and/or the sanction levels achieved with given resources. The resources that a state allocates to its criminal justice activities will depend on the total resources at its disposal and the competing demands on those resources, for which MVR, AID, and GX are plausible measures. These resources may also depend on state-local tax structures and the division of responsibility between state and local governments, both of which are reflected by PTX. The sanction levels achieved with given resources, particularly clearance rates, may be related to the geographic dispersion of the population, DEN.

The exogenous variables that affect the sanction levels achieved with given resources, the set Y in equation 2, may include some of the same variables as X because factors that influence individuals' decisions to engage in criminal activity may also affect their support of the law enforcement activities that lead to the arrest, conviction, and imprisonment of persons charged with crimes. Thus, there may be uncertainty about which of the variables in Exhibit 1 should be included in Y. However, there is no apparent rationale for including UN, DPOP, MVR, AID, GX, and PTX. These exclusions plus the exclusion of MVR, AID, GX, PTX, and DEN from the set X assure that both crime and sanction equations (equations 2 and 3) are identified when $\Theta = 0$.

IV. ESTIMATED CRIME EQUATIONS

This section reports the results of estimating equations 3 and 5. Crime categories employed were larceny, burglary, and robbery. Annual data were used; hence, a period is one year. To determine the sensitivity of the

results to model specification, equations were estimated for several assumed values of Θ and K. Sanction levels have been measured by clearance rates (P), imprisonment rates (PI), and median time served (T). We report first the results of estimating crime equations with the only sanction measure being the clearance rate, and then show the effects of including additional sanction measures.

A. Effects of sanctions

Table 1 presents the clearance rate coefficients for equation 3 estimated from the pooled data set of 288 observations (48 states and 6 years) with $\Theta = 1$ and K = 1,2,3, or 4. Twelve (of thirty) coefficients are positive rather than negative, and with the exception of robbery in the case of K = 3, they are not statistically significant, either individually or as a group.[14] However, in each case, the sum of the coefficients, which estimates the long run elasticity of the crime rate with respect to changes in the clearance rate, is negative.

If perceived sanctions adjust with a lag to past values of actual sanctions, the sum of the clearance rate coefficients, $\sum_{k=1}^{K} \alpha_k$, should increase in absolute value with increases in K. Table 1 shows that such is not the case for larceny, but for burglary and robbery the sum of the clearance rate coefficients increases as K increases from 1 to 3 and then decreases as K increases to 4. There is thus some evidence that perceived sanctions respond with a lag to actual sanctions. But the evidence is weak; the lagged clearance rate coefficients are not statistically significant, either individually or as a group, except for robbery with K=3. This finding may be interpreted in two ways. Perceived sanctions may adjust rapidly to changes in actual sanctions; K is in fact equal to one. Or, perceived sanctions may adjust gradually, but the actual sanction values are so highly

TABLE 1. Coefficients of clearance rates in equation 3, estimated by SUR from pooled state data, 1972-1977[a].

Coefficient and (standard error) of lagged clearence rate in equation for:

Lagged clearance rate	Burglary				Larceny				Robbery			
	K=1	K=2	K=3	K=4	K=1	K=2	K=3	K=4	K=1	K=2	K=3	K=4
$\hat{P}_{-1}$	-.033 (.094)	-.076 (.100)	-.084 (.123)	-.078 (.116)	-.099 (.081)	-.083 (.103)	-.152 (.130)	-.247 (.164)	-.127 (.123)	-.289 (.178)	-.259 (.170)	-.204 (.194)
$\hat{P}_{-2}$		-.086 (.099)	-.163 (.142)	-.181 (.115)		.074 (.096)	.005 (.107)	.122 (.129)		.140 (.203)	.047 (.195)	.103 (.192)
$\hat{P}_{-3}$			.013 (.183)	.034 (.142)			.051 (.128)	.025 (.153)			-.430*** (.062)	-.140 (.221)
$\hat{P}_{-4}$				.125 (.117)				.022 (.126)				-.074 (.181)
Sum of Coefficients	-.033	-.162	-.234	-.100	-.099	-.009	-.096	-.078	-.127	-.149	-.642	-.315

[a]For each crime, the coefficients reported in this table are the estimated values of α_k in the following equations:

$$C_{jt} = a_{0t} + \sum_{k=1}^{K} \alpha_k \hat{P}_{jt-k} + a_{2t}X_{jt} + U_{jt},$$

where K = 1,2,3 or 4, t = 1972,...,1977, and j = 1,...,48 states.

Alaska and Hawaii were excluded to make the sample comparable to previous studies. The environmental variables included in the equation (the set X) were the first eight variables in Exhibit 1. The values of $\hat{P}_{jt-k}$ were estimated in the first stage regressions with the explanatory variables being the first thirteen in Exhibit 1. The six individual year regressions for each crime and each value of K were first estimated by 2SLS and then treated as a system to obtain the SUR estimates.

Note that the clearance rate coefficients, α_k, do not vary with time; they are thus constrained to equality across years, while all other coefficients are free to vary. Since the hypothesis that the clearance rate coefficients take on the same values in each of the 6 cross-section regressions (1972-77) cannot be rejected at the 5 percent significance level for any crime and any value of K, pooling of data to estimate clearance rate effects seems appropriate.

* indicates statistical significance at 10 percent but not 5 percent level.
** indicates statistical significance at 5 percent but not 1 percent level.
*** indicates statistical significance at 1 percent level.

correlated over the periods t-1 through t-4 that the lag structure cannot be estimated.[15]

Crime equations were also estimated for each crime and each year (1972-1977). The coefficients of these individual-year regressions are not reported because they are qualitatively similar to those presented in Table 1. In particular, the hypothesis that the clearance rate coefficients are zero in every year cannot be rejected except for robbery in the case of K = 3 (see Table 2). Even this exception is not strong evidence of a crime prevention effect, since 7 of 18 clearance rate coefficients (3 coeffients in each of 6 cross section years) are positive rather than negative; and only 2 of 18 coefficients are statistically significant, one positive and one negative.

The coefficients presented in Table 1 were estimated on the assumption that $\Theta = 1$—that crime rates are unaffected by current values of clearance rates. To test this assumption, crime equations were also estimated with $\Theta = 0$. For equations estimated with $\Theta = 0$ and K = 1, the case that most previous studies have considered, the hypothesis that the clearance rate coefficients are zero cannot be rejected for any crime (see the first column of Table 3). Also, when both current and lagged clearance rates are included in the crime equations, the coefficients of the current clearance rates are never statistically significant (see Table 3). These results are consistent with the hypothesis that $\Theta \geq 1$, and they suggest that the estimated coefficients of current clearance rates reflect the correlation of current with past clearance rates, rather than the influence of current clearance rates on current crime rates.

Table 4 reports the coefficients of lagged clearance and crime rates in equation 5 estimated from pooled data.[16] The long run clearance rate elasticities derived from these coefficients are shown in the bottom row; only

Table 2. Test statistics for the hypothesis that crime rates are unaffected by lagged values of clearance rates[a]

	Value of F-Statistic for:[b]			
	K=1	K=2	K=3	K=4
Burglary	.76	1.45	1.03	.78
Larceny	1.39	1.06	.68	.99
Robbery	1.47	1.09	2.04**	1.41*

[a]For a given value of K and a given crime, and hypothesis tested is that in the following regressions $\alpha_{kt} = 0$ for $k = 1,\ldots,K$ and $t = 1972,\ldots,1977$:

$$C_{jt} = a_{0t} \sum_{k=1}^{K} \alpha_{kt} \hat{P}_{jt-k} + a_{2t}X_{jt} + U_{jt},\ j = 1,\ldots,48.$$

That is, the six individual year regressions for each crime and each case were treated as a system of seemingly unrelated regressions and estimated with and without restricting the clearance rate coefficients to zero. The hypothesis tested is that this restriction causes a statistically significant increase in the sum of squared residuals.

[b]For K=1, 2, 3 and 4, numerator degrees of freedom are 6, 12, 18, and 24, respectively; denominator degrees of freedom are 228, 222, 216, and 210, respectively.

Table 3. Test statistics for the hypothesis that crime rates are unaffected by current values of clearance rates[a]

	Value of F-statistic[b] for:			
	K=1	K=2	K=3	K=4
Burglary	1.94*	1.94*	1.97*	.56
Larceny	.83	.13	.36	.58
Robbery	1.65	1.00	1.31	1.04

[a]For a given value of K and a given crime, the hypothesis tested is that $\alpha_{1t} = 0$ for $t = 1972,\ldots,1977$ in the following regressions:

$$C_{jt} = a_{0t} + \sum_{k=1}^{K} \alpha_{kt} \hat{P}_{jt-k+1} + a_{2t}X_{jt} + U_{jt},\ j = 1,\ldots,48.$$

[b]For all values of K the numerator degrees of freedom are 6; denominator degrees of freedom are 228, 222, 216, and 210 for K = 1, 2, 3, and 4, respectively.

the elasticity for robbery is negative. Similar results were obtained when equation 5 was estimated from data for individual years 1972-77: none of the 6 elasticities estimated for burglary are negative, while only 2 of 6 elasticities are negative for each of the other two crimes. For comparison, recall that the elasticities estimated from equation 3 and reported in Table 1 are negative for all crimes, but statistically significant only for robbery. Thus, estimates of both equation 3 and 5 suggest that the crime prevention effect of arrest is greatest for robbery, and weak to non-existent for larceny and burglary.

The crime equations estimated in this study include more exogenous variables than those estimated in previous studies. In particular, Ehrlich (1973) finds that only income, poverty, and non-white have statistically significant effects on crime rates. However, in this study, five additional variables [UN, PAR, EDH, DPOP, and URB] contribute significantly to the explained variation in crime rates; for each of the three crimes, the hypothesis that these variables have zero coefficients can be rejected at the one percent significance level. Thus, crime equations should be specified to include these variables unless it can be argued that there is no theoretical basis for expecting them to affect crime rates, which is not the case. Crime equations that include only INC, NW, and POV as exogenous variables are misspecified, even though they often show somewhat larger crime prevention effects for arrest.[17]

One possible explanation for the weak crime prevention effects reported above is that arrests are relatively weak sanctions because they typically do not lead to imprisonment.[18] Thus, it is important to determine whether estimated sanction effects are greater when sanction levels are measured by imprisonment probability and time served as well as arrest probability.

Table 4. Elasticities of crime rates with respect to clearance rates, equation 5[a]

Explanatory variable	coefficients and (standard errors) when dependent variable is: Burglary[c]	Larceny[c]	Robbery[c]
P_{-1}	-.0228	.0073	-.0947**
	(.0263)	(.0318)	(.0450)
P_{-2}	-.0412	-.0276	.1440**
	(.0302)	(.0374)	(.0558)
P_{-3}	.0361	.0254	-.1481***
	(.0331)	(.0375)	(.0532)
P_{-4}	.0363	.0350	.0503
	(.0305)	(.0309)	(.0492)
C_{-1}	.9275***	.8971***	.8751***
	(.0208)	(.0173)	(.0256)
Sum of clearance rate of coefficients	.0084	.0401	-.0485
Elasticity of the crime rate with respect to the clearance rate[b]	.116	.400	-.388

---

[a]The coefficients in this table are from the following equations estimated by SUR for each crime with $\Theta = 1$ and $K = 3$:

$$C_{jt} = (1-\rho_1)a_{0t} + \sum_{k=1}^{K} [\alpha_k P_{jt-\Theta-k+1} - \alpha_k \rho_1 P_{jt-\Theta-k}] + a_{2t}X_{jt} - a_{2t}\rho_1 X_{jt-1} + \rho_1 C_{jt-1} + e_{jt}$$

where $j = 1,\ldots,48$; $t = 1972,\ldots,1977$; and the set X consists of the first eight variables in Exhibit 1. Estimates were also made with $K = 1$, with results similar to those reported above.

[b]From equation 5, it is readily seen that the sum of the clearance rate coefficients estimates $\sum_{k=1}^{K} \alpha_k(1-\rho_1)$, while the coefficient of the lagged crime rate estimates ρ_1. Thus, the long-run elasticity of the crime rate with respect to the clearance rate, $\sum_{k=1}^{K} \alpha_k$, is estimated by the ratio:

$$\frac{\text{sum of clearance rate coefficients}}{1 - \text{crime rate coefficient}}$$

[c]In the case of burglary and larceny, the clearance rate coefficients are not statistically significant either individually or as a group. However, in the robbery equation the coefficients are significant as a group, and three of four are individually significant.

Since data on probability of imprisonment and time served are available only for 1960, 1964, and 1970, crime equations that include all three sanction variables, P, PI, and T, can be estimated only for 1970 and adjacent years. Table 5 presents the coefficients of the sanction variables obtained when PI and T are added to equation 3. These equations, labelled 3b, are the same as those reported above except that PI and T are included as explanatory variables and the estimates are based on 32 rather than 48 states. The coefficients are frequently positive rather than negative and never statistically significant. Hence, they provide virtually no evidence that crime rates are influenced at the margin by sanction variables.

Equation 3c includes only time served and probability of imprisonment as sanction variables and therefore differs from 3b by the exclusion of the clearance rate. Again, the effects of the sanction variables are frequently positive rather than negative and never statistically significant. Equations 3b and 3c assume $\Theta = 1$ — sanction variables are lagged one year. When the sanction variables are not lagged ($\Theta = 0$), the coefficients of PI are negative in all three equations (for burglary, robbery, and larceny), but never statistically significant at the 5 percent level.

The results for equations 3b and 3c stand in sharp contrast to those obtained by Ehrlich and others in their analyses of 1960 data. For example, Ehrlich's (1973, p. 550) SUR estimates of the coeffients of PI are -.624, -.358, and -1.112 for burglary, larceny, and robbery, respectively; the corresponding coefficients for T are -.996, -.654, and -.286. With the exception of the coefficients of T in the robbery and larceny equations, the coefficients estimated by Ehrlich are more than twice their standard errors.

Coefficients more similar to those obtained by Ehrlich can be obtained by restricting the set of exogenous variables included in the crime equations to

TABLE 5. Effects of imprisonment sanctions, equation 3 estimated by SUR, state data, 1970-71[a]

Sanction variable	Burglary 3b	Burglary 3c	Burglary 3d	Larceny 3b	Larceny 3c	Larceny 3d	Robbery 3b	Robbery 3c	Robbery 3d
Clearance	.59			.29			-.82		
rate ($\hat{P}$)	(.51)			(.62)			(1.17)		
Probabilty of	.01	.02	-.29**	-.09	-.10	-.31*	.16	.31	-1.07**
imprisonment ($\widehat{PI}$)	(.16)	(.14)	(.14)	(.25)	(.16)	(.17)	(.59)	(.58)	(.52)
Time	.06	.09	.20	.68	.44	.45	1.13	1.35	-.72
served ($\hat{T}$)	(.39)	(.32)	(.31)	(.76)	(.58)	(.32)	(1.12)	(1.08)	(.82)

[a]Variables 1-13 in Exhibit 1 were used as regressors in the first stage regressions that generated estimated values of the sanction variables. These estimated values were then used to obtain the 2SLS estimates of each equation. The SUR estimates differ from the 2SLS estimates only in that they take account of the covariance of the errors across crimes. The 2SLS estimates are qualitatively the same as the SUR estimates. In equations 3b and 3c, the dependent variables are the 1971 crime rates, and the sanction variables are those for 1970; θ is assumed to be 1. Both dependent and sanction variables for equation 3d are for 1970: $\theta = 0$. Equations 3b and 3c include variables 1-8 in Exhibit 1 as environmental variables; equation 3d includes only variables 1-3.

INC, POV, and NW--the variables included in the equations reported by Ehrlich. The results of thus replicating Ehrlich's analysis with 1970 data are presented in Table 5 as equation 3d. The coefficients of PI in equation 3d are of approximately the same magnitude as those obtained by Ehrlich with 1960 data. But the same cannot be said for the coefficients of time served; Ehrlich's results show statistically significant and negative effects for T, while ours do not.

Comparing the results for equations 3c and 3d shows that we estimate weaker crime prevention effects for imprisonment than previous studies, particularly Ehrlich's, primarily because we argue that the correctly specified crime equations should include environmental variables in addition to INC, POV, and NW, and not because we utilize data for a different year and a smaller set of states. Also, we have treated T as endogenous, while Ehrlich assumed it to be exogenous.

B. Effects of exogenous variables

Crime equations of the form of equations 3 and 5 account for a large fraction of the observed variation in burglary, larceny, and robbery rates. For example, the values of R^2 for the 1972-77 2SLS estimates of equation 3 with $\Theta = 1$ and $K = 4$ average .78, .68, and .88 for burglary, larceny, and robbery, respectively. The corresponding averages for equation 5 with $\Theta = 1$ and $K = 3$ are .985, .975, and .986. Although the arrest and imprisonment sanctions apparently account for relatively little of the observed variaton in the crime rates under study, the latter are not random phenomena. Instead, they are significantly related to a number of economic and demographic factors.

Table 6 presents representative estimates of the effects of these exogenous variables.[19] The coeffients of INC, UN, PAR, EDH, DPOP, and URB are

TABLE 6. Equation 5 with $\theta = 1$ and $K = 1$ or $K = 4$, estimated by SUR from pooled state data, 1972-1977[a]

Explanatory Variable	Coefficients and (standard errors) when dependent variable is: Burglary		Larceny		Robbery	
	K=1	K=4	K=1	K=4	K=1	K=4
$\hat{P}_{-1}$	-.046 (.075)	-.095 (.076)	-.073 (.055)	-.171* (.100)	-.206** (.093)	-.267* (.151)
$\hat{P}_{-2}$		-.103 (.075)		.093 (.086)		-.092 (.129)
$\hat{P}_{-3}$		.022 (.075)		-.015 (.114)		-.133 (.143)
$\hat{P}_{-4}$		.045 (.080)		-.039 (.079)		-.099 (.128)
INC	.302 (.233)	.024 (.152)	.552*** (.164)	.577** (.261)	.568 (.337)	.410 (.491)
POV	-.013 (.137)	-.101 (.135)	.108 (.114)	.134 (.138)	-.160 (.211)	.017 (.279)
NW	-.039 (.045)	.006 (.050)	.038 (.039)	.050 (.046)	.397*** (.072)	.409*** (.090)
UN	.093* (.050)	.103* (.040)	.130*** (.030)	.107* (.064)	.022 (.061)	-.003 (.093)
PAR	.923*** (.207)	.778*** (.233)	.154 (.184)	.083 (.219)	.517 (.326)	.396 (.396)
EDH	.791*** (.263)	.736** (.298)	1.031*** (.240)	1.134*** (.268)	.636 (.424)	.784 (.494)
DPOP	.672** (.278)	.660** (.305)	.666*** (.245)	.869*** (.303)	-.142 (.453)	-.105 (.985)
URB	.857*** (.168)	.786*** (.181)	.338*** (.152)	.273 (.174)	2.058*** (.267)	2.176*** (.312)

[a]This table reports equations of the following form, estimated from the pooled data sample of 288 observations:

$$C_{jt} = a_{0t} + \sum_{k=1}^{K} \alpha_k P_{jt-k} + a_2 X_{jt} + U_{jt}, \quad t = 1972, \ldots, 1977 \text{ and}$$

$j = 1$, 48 states. Equations were also estimated for each crime for $K = 2$ and $K = 3$, but they are not reported because they are quite similar to those reported.

predominately positive, with the positive coefficients frequently being statistically significant; the negative coefficients are never significant. The coefficients of NW are significant and positive only in the robbery equations. Although the coefficients of the environmental variables do vary in magnitude and sometimes sign across equation forms and crimes, as a group the variables are always statistically significant.[20]

Many of the variables are undoubtedly proxies for a number of factors that influence the actual commission of crimes and their reporting. To illustrate, the positive coefficient of EDH may reflect in part higher rates of reporting in states and localities with more highly educated populations. That such is the case is consistent with the finding that the coefficients of EDH in the burglary and larceny equations are typically larger than those in the robbery equations. More serious crimes (robbery) are less likely to go unreported, regardless of the willingness of the victims to become involved and regardless of the professionalism of law enforcement agencies.

The variables INC, NW, and PAR appear to be highly interrelated in their effects on crime rates. In Table 6, PAR is significant for burglary while INC and NW are not; for larceny, INC is significant while PAR and NW are not; for robbery, NW is significant while INC and PAR are not. The coefficients of POV are never significant, which is probably due to the high correlation (-.82) of INC and POV. Thus, whether these variables reflect the influence of income, race, broken homes, or some combination of the three is difficult to judge.[21] However, omitting INC and NW resulted in statistically significant coefficients for PAR in all three equations, while leaving other coefficients and the overall explanatory power of the equations essentially unchanged. This result suggests that the positive relationship between NW and the crime rates found in numerous previous studies was observed because NW is a fairly

good proxy for the frequency of broken homes (PAR) and not because race directly affects criminal activity. Regardless of whether this particular explanation of previous findings is accepted, the importance of NW is clearly diminished by allowing for the influence of other factors that may just as plausibly affect crime rates.

The effect of unemployment is of particular interest, partly because a link between unemployment and crime has been posited frequently but seldom observed, and partly because unemployment can be influenced by public policy in the short run as well as in the long run.[22] Note that UN has a stronger influence on burglary and larceny than on robbery, which is plausible since burglary and larceny are likely closer substitutes for employment than is robbery. Similarly, that larceny should be more strongly influenced by UN than burglary is plausible, since the latter involves unlawful entry, usually by force, while larceny includes theft of a sort, e.g., shoplifting, that is likely to appear less risky and more feasible to the inexperienced and reluctant offender.[23]

Adding to the significance of UN as a cause of crime is its probable interaction with PAR. That is, UN may affect crime rates indirectly as well as directly to the extent that it leads to the breakup of families, as it apparently does when fathers be must absent for their families to qualify for AFDC (Aid to Families With Dependent Children).

Although the elasticities for UN might seem to small too be of policy significance, rising unemployment over the 1970-77 sample period accounts for a non-trivial share of the increase in burglary and larceny rates. Specifically, from 1970 through 1977, the average (over 48 states) percentage increase in the unemployment rate was 39.4. Table 6 shows an elasticity of burglary with respect to UN of .103, which implies an average increase in the burglary rate of (.103) (39.4) = 4.06 percent for the 1970-77 period. The corresponding increase for larceny is (.130) (39.4) = 5.12 percent. The 1970-77 average increases in burglary and larceny rates were 36 and 37 percent, respectively. Thus, the increase in unemployment accounts about 11 percent of the average increase in the burglary rate and about 14 percent of the increase in the larceny rate.

Moreover, as a source of change in burglary and larceny rates, unemployment change was more important than clearance rate change. For the 1970-77 period, the average percentage changes in clearance rates were -10.7, 3.0, and 5.2 for burglary, larceny, and robbery, respectively. These changes imply average percentage changes of 2.5, -.3, and -3.3 in the three crime rates when the changes are calculated with the largest clearance rate elasticities reported in Table 1 (-.234, -.099, and -.642). These calculations suggest that falling clearance rates were not the source of the 1970-77 increases in burglary, larceny, and robbery rates. Indeed, rising clearance rates had the effect of reducing the average increase in larceny and robbery rates.[24]

V. SUMMARY

This study has estimated parameters of crime equations (supply-of-offense functions) similar to those estimated in numerous previous studies. However, in specifying crime equations, we have allowed for lags in the adjustment of perceived to actual sanctions. Such lags are plausible, given the channels by which potential offenders obtain information about actual sanction levels.

Our results suggest that there are indeed lags and that clearance rates have little contemporaneous effect on crime rates. Of course, this interpretation of our findings does not rule out an observed inverse

correlation of current crime rates and current clearance rates. Such a correlation could arise if relatively high crime rates are associated with an overload of the law enforcement system that reduces the resources that can be devoted to solving each crime. Also, even if current crime rates depend only on past clearance rates, they may nevertheless be correlated with current clearance rates if the latter are correlated with past clearance rates.

On balance, our results provide at most weak evidence that burglary, larceny, and robbery rates are influenced by marginal variations in the application of arrest and imprisonment sanctions. The word marginal in the preceding sentence deserves emphasis; we are not saying that crime rates would not increase if no sanctions were applied. Instead, the evidence suggests that observed differences in sanction levels account for little of the observed variation in crime rates.

However, it is possible to obtain statistically and quantitatively significant crime prevention effects by selecting a particular equation specification, e.g., one that includes relatively few exogenous variables, or a particular cross-section year. For example, Ehrlich (1973) and others in their analyses of 1960 data obtained statistically significant and negative estimates for the coefficients of variables measuring the risk and severity of imprisonment. Using the same model specification, we have obtained similar results with 1970 data; but when the specification is altered to include plausible exogenous variables, the estimated crime prevention effect is much weaker. Hence, we have reached a different conclusion than Ehrlich and others about the effect of imprisonment because we have employed a different and more appropriate specification of crime equations, and not because conditions have changed so that a crime prevention effect that was present and observed in 1960 was not present and could not be observed in 1970.

Although crime rates appear to be little affected by sanction levels, they certainly are not random phenomena. They are related to a number of exogenous economic and demographic variables. Moreover, these latter variables account for a major share of observed variation in crime rates. In particular, rising unemployment accounts for about 11 percent of the 1970-77 average increase in burglary rates and about 14 percent of the average increase in larceny rates.

With respect to public policy, the positive and significant coefficients for the unemployment rate in the burglary and larceny equations suggest that we might as reasonably attempt to control those crimes by reducing unemployment as by increasing the risk and severity of criminal sanctions.

FOOTNOTES

[1]For a summary of the methods and findings of many previous studies, see Blumstein (1978), pp. 30-47 and pp. 95-139. For a critique of Blumstein, see Ehrlich and Mark (1977).

[2]Blumstein (1978) deals with this question in great detail. The conclusion of the Panel on Research on Deterrent and Incapacitative Effects with respect to analyses of natural variaton in non-capital sanctions is that "The major challenge for future research is to estimate the magnitude of the effects of different sanctions on various crime types, an issue on which none of the evidence available thus far provides very useful guidance. Blumstein (1978: 7)."

[3]We do not present a theory of crime that leads to this equation because such theories are amply developed elsewhere. See, for example, Becker (1968) and Ehrlich (1973). These theories focus on deterrence, which is the "inhibiting effects of sanctions on the criminal activities of people other than the sanctioned offender [Blumstein (1978: 3)]." However, sanctions may influence crime rates through mechanisms other than deterrence. As a society applies sanctions against particular activities, it defines behavioral norms for its members; it signals that particular activities are wrong or antisocial. This influence is often labelled the educational effect of criminal sanctions. Also, imprisonment tends to reduce crime rates by incapacitating and rehabilitating offenders. But it has the opposite effect if it reduces legitimate opportunities of released offenders and adds to their criminal skills and contacts. The sum of all of these effects is the crime prevention or crime control effect of sanctions. While most empirical studies claim to estimate the deterrent effect of sanctions, they in fact estimate the crime prevention effect. For fuller discussion of the mechanisms by which sanctions may affect crime rates, see Cook (1977), Blumstein (1978), and Brier and Fienberg (1980).

[4]The number of FBI index crimes can be used to measure CT_{it}. E_{it} can be measured by total police protection expenditures and police protection employment and payrolls. These are the expenditure and employment concepts used by the Bureau of Census and the National Criminal Justice Information and Statistics Services in their publications; see U.S. Dept. of Commerce, Bureau of Census, Expenditure and Employment Data for the Criminal Justice System, 1976, GSS no. 85, SD-EE no. 11, January 1978. There is no obviously best way to measure E_{it} but the choice is not critical because available measures are highly correlated.

Sanction levels may of course depend on the character and use of criminal justice resources as well as their aggregate dollar value. Unfortunately, detailed data on resource use are not available on a systematic basis. For further discussion of how observed sanction levels may be influenced by crime rates and criminal justice system resource use, see Blumstein (1978: 30-35) and Vandaele (1978: 327-29 and 346-51).

[5]Statistics reflecting sanction risks and severity are published with some delay and not in a form that is typically available to potential offenders. Indeed, imprisonment probabilities are not published as such. Data required to calculate the probabilities, as well as data on time served, have not been

published since 1970, and then only for 33 states. Data on clearance rates are not published except in the form of national averages. Thus, it is not plausible that potential offenders base their decisions directly on published sanction data. Instead, they must rely on unofficial sources that provide piece-meal information about sanctions. They must form expectations about arrest and imprisonment probabilities and about sentences on the basis of their own experience (if they are offenders), the experience of offenders whom they know, newspaper and word-of-mouth accounts of the arrest, imprisonment, and sentencing of persons charged with crimes in their community, etc. In doing so, they may only gradually modify their subjective or perceived sanction levels in response to current information. This adaptive response is a plausible form of behavior when the new information being used to revise perceived sanction levels is of uncertain accuracy. With sufficient time to make observations, individuals' perceived sanction levels may, but need not, approach actual levels. Fisher and Nagin express a similar view in Blumstein (1978:388).

Although we are arguing that perceived sanction levels adjust to actual levels with a lag, models that allow for immediate adjustment are estimated and compared to those that assume a lag. Whether crime rates of a particular period are influenced by sanction levels of that period may of course depend on the length of the period. The likelihood that crime rates will be influenced by same-period sanction levels increases as the period of observation lengthens, e.g., from one month to one year to one decade.

[6]The term identification as used here refers to over-identification as well as exact identification. To achieve identification, previous studies have tried to justify the exclusion of particular variables from X_t. Whether these exclusions are in fact justified is a central issue in the debate about how the results of those studies are to be interpreted. Also, when the excluded variables are predetermined rather than exogenous, there is a problem of obtaining consistent estimates if the equation error is serially correlated, which is likely. For more discussion of both of these points see Fisher and Nagin in Blumstein (1978: 361-399).

[7]Blumstein (1978) and other critiques of econometric studies of deterrence have made this point. Ehrlich and Mark (1977) argue that although P and U may be jointly determined there is no reason to presume a negative bias in estimates of deterrent effects.

[8]From equations 2 and 3, we see that $P_{t-\theta}$ depends on $C_{t-\theta}$ which in turn depends on $U_{t-\theta}$. Thus, $P_{t-\theta}$ is correlated with U_t if U_t and $U_{t-\theta}$ are correlated. This serial correlation is the correlation of the error for a particular state in a particular year with the error for that same state in previous years.

[9]The error term in equation 3 includes the effects of omitted determinants of actual and reported crime rates, as well as random influences. Among the omitted factors may be: law enforcement institutions and practices that influence the reporting and classification of crimes and arrests; population characteristics (attitudes, traditions, religious and ethical beliefs, the proportion of the population with criminal experience) that influence the manner in which individuals behave in given circumstances; private self-

protection measures taken by individuals to reduce the likelihood of victimization; and unmeasured dimensions of the economic and social environment that condition potential offenders' decisions. Many of these omitted influences are likely to change gradually, if at all, from one year to the next. For example, the characteristics of a state's population change gradually; adverse economic conditions may persist for years within particular states or metropolitan areas; etc. To the extent that these omitted and temporally stable influneces vary among states, the errors for each state will be serially (temporally) correlated. To the extent that the omitted influences are the same for all states, they are simply included in the intercept term, a_0. See Blumstein (1978: 127-129; 382-385) for further discussion of why serial correlation may arise.

[10]For description and evaluation of this technique as applied in models of the sort estimated in ths study, see Malinvaud (1966: 471-472; 604-607) and the references cited therein.

[11]See Kmenta (1971: 307-322) for disucssion of the estimation problems and procedures for models with both errors in variables and errors in equation.

[12]For a discussion of the application of SUR to multiple cross sections see Smith and Fibiger (1972); for discussion of the technique see Kmenta (1971: 517ff) and Zellner (1962).

[13]Crime equations were also estimated from panel data for 66 large Standard Metropolitan Statistical Areas. Similar results were obtained from the state and SMSA data; in the interest of brevity, only the equations estimated from state data are presented in the following section.

[14]In this and subsequent discussion, a coefficient is termed statistically significant at the five percent level if the ratio of the coefficient to its standard error exceeds the critical value of t for the appropriate degrees of freedom at the .05 confidence level. For example, the degrees of freedom for equation 3 estimated for individual cross-section years is 38. The critical value of t for 38 degrees of freedom and the .05 confidence level is approximately 2.03; hence, a coefficient in equation 3 is termed statistically significant at the five percent level if it is at least 2.03 times as large as its standard error. Other significance levels, one percent and ten percent, are similarly defined. When a significance level is not explicitly stated, it will be understood to be the five percent level.

This procedure for determining statistical significance is not exactly correct when the coefficients are estimated by 2SLS or by 2SLS in combination with SUR. In these cases, the ratio of a coefficient to its standard error does not have the presumed t distribution. However, there is evidence that the t distribution can serve as a tolerable approximation of the true distribution; see Kmenta (1971: 584-585). Note also that the true distribution is asymptotically normal; thus, the test statistics may be fairly accurate for the pooled data cases, which consist of 300 and more observations.

[15]The serial correlation of P_t is fairly high. For burglary, the correlations of P_t with P_{t-1}, P_{t-2}, P_{t-3}, and P_{t-4} average .81, .75, .69,

and .69, respectively, calculated for the 48 contiguous states and the period 1968-77. Correlations for larceny and robbery are similar.

[16]Equation 5 allows only for first order serial correlation. Equations that allow for higher order correlation were not estimated because the resulting set of regressors is large and highly collinear. Also, the estimated residuals for equation 5 show little evidence of serial correlation.

[17]For example, when only three exogenous variables (INC, POV, and NW) are included in the equations, tests corresponding to those reported in Tables 2 and 3 support rejection of the hypothesis that clearance rate coefficients are zero. However, the conclusion to be drawn from these tests is not that clearance rates have a statistically significant effect on crime rates. Instead, it is that by inadequately controlling for the influence of other factors on crime rates (by omitting relevant exogenous variables), the estimated crime prevention effects of arrest can be increased.

[18]From 1968-1977, clearance rates averaged about .2, .19, and .35 for burglary, larceny, and robbery, respectively, while imprisonment probabilities for the same crimes averaged .011, .006, and .066 in 1970, the most recent year for which these data are available. While arrest typically does not lead to imprisonment, it does entail significant costs for those arrested. Aggregate data from the Uniform Crime Reports 1977, Table 56, p. 218, show that the proportion of those arrested who were charged with a crime (held for prosecution) was high -- .93, .90, and .94 for burglary, larceny, and robbery, respectively. Of those charged with burglary only 9 percent were acquited or dismissed; the percentages of acquitals and dismissals for larceny and robbery were 11 and 17, respectively.

[19]The equations of Table 6 were estimated under the restriction that the coefficients of the clearance rate and the environmental variables are stable over the 6 year period. This restriction is not significant in the case of burglary; that is, the hypothesis that these coefficients are stable over the 1972-77 period cannot be rejected at the 5 percent level. Although this hypothesis can be rejected in the cases of robbery and larceny, relaxing the restriction of coefficient stability does not alter our conclusions about how clearance rates and the environmental variables affect crime rates. The direction and average magnitudes of the effects of these variables are as presented in Table 6.

[20]In particular, the coefficients of the exogenous variables in equations with $\theta=0$ and $K=1$ are virtually the same as those reported in Table 6. Of course, this similarity is to be expected if little of the variation in crime rates is linked to clearance rate variation, as is the case.

[21]Factor analysis shows that 93 percent of the variation in INC, POV, NW, and PAR can be represented by two factors: one that is highly correlated with INC and POV (correlation coefficients of -.99 and .80), and one that is highly correlated (.95 and .56) with NW and PAR. Factor analysis also shows tha the other exogenous variables (DPOP, UN, URB, and EDH) are relatively independent in that each is highly correlated with a factor that is only weakly correlated with the other 7 exogenous variables.

[22]See Long and Witte (1980) for a thorough review of previous findings regarding the relationship between economic conditions and crime.

[23]The elasticities of UN in equation 5 are similar in magnitude and pattern to those for equation 3 in Table 6.

[24]Since these comments are based on equations that do not include measures of the risk and severity of imprisonment, the question arises whether diminished application of the imprisonment sanction caused some of the 1970-77 increase in crime rates. An unequivocal answer to this question is difficult because estimates of imprisonment probability and time served for individual crime categories are not available for years after 1970. But we do have information about the overall use of the imprisonment sanction. The prison population has grown slightly more rapidly than the number of crimes, implying a slight increase in the average time served per reported offense (from 8.7 days in 1970 to 9.5 days in 1977). Thus, the costs imposed on offenders by imprisonment, as measured by time served, clearly have not diminished in any overall or average sense. Of course, this fact does not rule out a decline in the average imprisonment costs imposed on those committing the crimes under study (burglarly, larceny, and robbery). However, since burglary, larceny, and robbery account for a large and stable share, 85 percent, of total index crimes, it seems unlikely that the average time served per reported crime would have increased slightly (as it did) if there had been a significant decrease in the average time served per burglary, larceny, or robbery. And, it seems reasonable that the 1970-77 increases in these crime rates were not to any significant degree a reflection of weaker imprisonment sanctions.

REFERENCES

Becker, Gary S. (1968) "Crime and Punishment: An Economic Approach." Journal of Political Economy 76 (March-April): 169-217.

Blumstein, Alfred, J. Cohen, and D. Nagin (eds.) (1978) Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates, Report of the Panel on Deterrent and Incapacitative Effects, Assembly of Behavioral and Social Sciences. Washington, D.C: National Academy of Sciences. Referred to as Blumstein (1978).

Brier, S. and S. Fienberg (1980) "Recent Econometric Modeling of Crime and Punishment: Support for the Deterrence Hypothesis?" Evaluation Review 4 (April): 147-191.

Cook, P.J. (1977) "Punishment and Crime: A Critique of Current Findings Concerning the Preventative Effects of Punishment." Law and Contemporary Problems 41 (Winter): 164-204.

Cook, P.J. (1979) "The Clearance Rate as a Measure of Criminal Justice System Effectiveness." Journal of Public Economics 11:135-42.

Ehrlich, Isaac and Randall Mark (1977) "Fear of Deterrence: A Critical Evaluation of the Report of the Panel on Research on Deterrent and Incapacitative Effects." Journal of Legal Studies 6 (June): 293-316.

Ehrlich, Isaac (1973) "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation." Journal of Political Economy 81 (May-June): 521-565.

Ehrlich, Isaac (1975) "The Deterrent Effect of Capital Punishment: A Question of Life and Death." American Economic Review 65 (June): 397-417.

Forst, Brian E. (1976) "Participation in Illegitimate Activities: Further Empirical Findings." Policy Analysis 2 (Summer): 477-491.

Freeman, R.B. (1982) "Crime and the Labor Market," NBER Working Paper No. 1031. Cambridge: National Bureau of Economic Research.

Heineke, J.M. (1978) "The Supply of Legal and Illegal Activity: An Econometric Model." Technical Report ESCD-1-78, Center for Econometric Studies of Crime and the Criminal Justice System, Hoover Institution, Stanford University.

Kmenta, J. (1971) Elements of Econometrics. New York: Macmillan.

Long, S.K. and A.D. Witte (1980) "Current Economic Trends; Implications for Crime and Criminal Justice." Mimeo., University of North Carolina (June).

Malinvaud, E. (1966) Statistical Methods of Econometrics. Chicago: Rand McNally.

Mathur, V.K. (1978) "Economics of Crime: An Investigation of the Deterrent Hypothesis for Urban Areas." Review of Economics and Statistics 60 (August): 459-66.

McPheters, Lee and William Stronge (1974) "Law Enforcement Expenditures and Urban Crime." National Tax Journal 27 (December): 633-43.

Morris, D. and L. Tweeten (1971) "The Cost of Controlling Crime: A Study in Economics of City Life." The Annals of Regional Science 5 (June): 33-49.

Naroff, Joel, Daryl Hellman and David Skinner (1980) "The Boston Experience: Estimates of the Impact of Crimes on Property Values." Growth and Change 11 (October): 24-30.

Nagin, D. (1978) "Crime Rates, Sanction Levels, and Constraints on Prison Population." Law and Society 12 (Spring): 341-366.

Pogue, Thomas F. (1975) "Effect of Police Expenditures on Crime Rates: Some Evidence." Public Finance Quarterly 3 (January): 14-41.

Sjoquist, David (1973) "Property Crime and Economic Behavior: Some Empirical Results." American Economic Review 63 (June): 436-466.

Smith, V. Kerry and William W. Fibiger (1972) "An Approach for Efficient Estimation of State and Local Government Expenditure Determinants." Applied Econmics 4: 101-123.

Vandaele, W. (1978) "An Econometric Model of Auto Theft in the United States," in J.M. Heineke (ed.) Economic Models of Criminal Behavior. Amsterdam: North Holland.

Wolpin, K.I. (1980) "A Time-Series Cross-Section Analysis of International Variation in Crime and Punishment." Review of Economics and Statistics 62 (August): 417-23.

Zellner, A. (1962) "An Efficient Method of Estimating Seemingly Unrelated Regressions and Tests for Aggregation Bias." Journal of the American Statistical Association 57 (June): 348-368.

END