88423

The Crime Prevention Effects of Arrest and Imprisonment: Evidence from Multiple Cross-Section Analyses

Thomas F. Pogue

Department of Economics
The University of Iowa
Iowa City, Iowa 52242

February 1983

This research was supported by a grant from the National Institute of Justice (79NI-AX-0015).

The Crime Prevention Effects of Arrest and Imprisonment: Evidence From Multiple Cross Section Analyses

Abstract

This study estimates parameters of a model of crime generation and control that is similar to the models employed in previous econometric studies of crime. However, we utilize data that are more comprehensive, in years of coverage and in sanction measures than the data of other studies. One weakness of previous studies is that they are based on only one or a few cross-section samples. Thus, we do not know the extent to which differences in their results reflect sampling variation rather than differences in models and estimation techniques. In contrast, with the large data set of this study, we can estimate parameters for the same models from a number of samples that differ in unit and/or year of observation and then test hypotheses regarding the stability of the observed relationships.

In addition, we hypothesize that individuals' decisions regarding crime depend on perceived sanction levels that depend in turn on sanction levels realized in past periods. Our econometric results are consistent with this view. Arrest and imprisonment sanctions have little contemporaneous effect on property crime rates, and the lagged effects, although larger, are statistically significant only in the case of robbery.

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 that account for a major share of observed variation in property crime rates. In particular, rising unemployment accounts for about 10 percent of the 1970-77 average increase in burglary rates and about 15 percent of the average increase in larceny rates. With respect to public policy, the positive and significant influence of unemployment on burglary and larceny rates suggests that we might as reasonably attempt to control those crimes by reducing unemployment as by increasing the risk and severity of criminal sanctions.

amlpog/2/22/83

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 how observed rates of criminal activity are influenced by sanction severity (the magnitude of the penalties imposed on detected offenders) and sanction risk (the probability that a penalty will be imposed).[1] Sanction severity has been measured by the time served by imprisoned offenders; sanction risks have been measured by arrest and/or imprisonment probabilities. The results of some studies have been interpreted as evidence that the threat of imprisonment does in fact deter 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 previous study has satisfactorily identified and estimated a cause-effect relationship between criminal sanctions and crime rates.[2]

The research reported in this paper is an attempt to take account of the criticisms registered by Blumstein and others and thereby obtain improved estimates of how crime rates are affected by the apprehension and punishment of persons charged with criminal activity. Like previous studies, this

---

[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 variation 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)."

study employs standard statistical techniques to estimate relationships between sanction levels and reported crime rates for particular geographic areas [states and Standard Metropolitan Statistical Areas -- SMSAs] and to assess the extent to which the estimated relationships can be plausibly interpreted as evidence that the sanctions have crime prevention effects.

However, the present study differs from others in two important respects. First, it employs data that are more comprehensive, in years of coverage and in measures of sanction levels, than the data of other studies. One weakness of previous studies is that they are based on only one or a few cross-section samples. Thus, we do not know the extent to which differences in their results reflect sampling variation rather than differences in models and estimation techniques. In contrast, the large data set of this study allows us to estimate parameters for the same models from a number of samples that differ in unit and/or year of observation. Hypotheses regarding the stability of observed relationships can be tested, and we can determine whether the effects of arrest and imprisonment have become weaker, as Forst (1976: 490) suggests.

Second, we allow for the possibility of lags in the relationship between crime rates and sanction levels. One reason for a lag is that 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. And, 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.

Section II presents a model of how crime rates are related to observed sanction levels. Estimation problems are discussed in section III. Section IV describes the data used and section V presents estimates of and tests hypotheses regarding the parameters of the model. The final sections summarize our findings and discuss their policy implications.

It should be emphasized that this study, like previous studies, provides estimates of the marginal effects of criminal sanctions; that is, it provides estimates of how differences or changes in sanction levels affect crime rates. Hence, these estimates may be useful in answering the question: would an increase (decrease) in either the certainty or the severity of the penalty for a particular crime reduce (increase) the rate at which that crime is committed? But they do not answer the question: Would crime rates be higher if no penalties were applied? In policy making the first question is the relevant one, since the public policy issue typically is whether to apply sanctions that are more or less severe or more or less certain, and not whether to eliminate sanctions entirely.

II. A MODEL OF CRIME GENERATION AND CONTROL

The economic theory of crime that provides the framework for the econometric analyses reported below 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 sanction levels, measured by arrest and imprisonment probabilities and sentences served, affect crime rates as they influence perceived sanction levels and hence the perceived costs of criminal behavior. Higher sanction levels deter crime if and to the extent that they increase these perceived costs.[3]

Crime rates may influence sanction levels and resource inputs as well as the converse. In particular, objective sanction levels depend on crime rates and the resources used to apply sanctions -- to arrest, convict and imprison. And, the amount of these resources may depend in turn on the rates of various types of criminal activities and the costs that the public imputes to such activities, as well as the public's perception of the effectiveness of sanctions in preventing crime.

[3]We do not present a theory of individual behavior that leads to this prediction 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).

For hypothesis testing and estimation, this theory of crime generation must be stated in equations that determine crime rates and sanction levels. Following previous studies, we use the log-linear functional form for these equations; hence, all variables, both dependent and independent, are logarithms.[4] The crime rate equations are of the form:

$$(1) \qquad C_{ijt} = a_0 + a_1 P^*_{ijt} + a_2 X_{jt} + U_{ijt}$$

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 SMSA) at time t, the probability of being arrested for committing a crime of type i in jurisdiction j at time t as that probability is perceived by potential offenders, and the socio-economic variables hypothesized to affect crime rates. The latter are discussed in section IV.

Perceived sanction levels may differ from the levels that are actually applied, and perceived levels may adjust only gradually to changes in actual levels. A plausible representation of this process is:

$$(2) \qquad P^*_{ijt} = b_1 P_{ijt-\Theta} + \dots + b_K P_{ijt-\Theta-K+1}$$

where the integer Θ is the number of periods that elapse before a change in the logarithm of the objective probability of arrest (P) has any effect on the perceived probability, K is the number of past values of the objective

[4]Linear equations were also estimated to determine the influence of equation form on results. In general, the estimated effects of sanctions are larger for the log-linear than for the linear equations.

probability that influence the perceived probability, and $\sum_{k=1}^{K} b_k = 1$ if with sufficient time for adjustment perceived sanction levels equal actual levels. If $\Theta = 0$ and $K = 1$, then $P^*_{ijt} = P_{ijt}$; perceived probabilities adjust immediately (within the current period) to changes in actual probabilities. However, we expect $\Theta > 0$ and $K > 1$ because perceived sanction levels seem likely to change slowly and to be dominated by information about sanction risks and severity that is drawn from the past experience and observations of potential offenders.

Substituting equation 2 into equation 1 gives

$$(3) \qquad C = a_0 + \alpha_1 P_{-\Theta} + \ldots + \alpha_K P_{-\Theta-K+1} + a_2 X + U$$

where $\alpha_k = a_1 b_k$, k=1,K, and all subscripts except those denoting time lags are omitted for notational simplicity. The relationship between measures of the objective probability of arrest and the crime rate, given by the values of α_k, thus 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 be independent of 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.

Another process by which perceived sanction levels may adjust to actual levels is

$$(2a) \qquad P^* - P^*_{-1} = \delta(P_{-1} - P^*_{-1}), \quad 0 \leq \delta < 1,$$

where, again, all subscripts except those that denote time lags are omitted. Together, equations 1 and 2a imply

$$(3a) \qquad C = \delta a_0 + a_1 \delta P_{-1} + a_2 X - a_2(1-\delta)X_{-1} + (1-\delta)C_{-1} + U-(1-\delta)U_{-1}$$

as an alternative representation of the crime function.[5]

Objective sanction levels are assumed to be determined by crime rates, resources used to apply sanctions, and exogenous variables that influence the sanction levels achieved with given resources; specifically,

$$(4) \qquad P_{ijt} = c_0 + c_1 E_{jt} + c_2 CT_{jt} + c_3 Y_{jt} + V_{ijt}$$

where V_{ijt} is an error term and P_{ijt}, E_{jt}, CT_{jt}, and Y_{jt} are, respectively, the logarithms of the clearance rate (or probability of arrest) for crimes of type i in locality j at time t, a measure of resources devoted to the apprehension of offenders by locality j at time t, an index of overall criminal activity, and exogenous variables that influence arrest probabilities.[6] The latter are discussed more fully in section IV. The hypothesis is that P_{ijt} is positively related to E_{jt} and negatively related to CT_{jt}.

---

[5]The process of equation 2a can be approximated by that of equation 2. Expanding 2a gives $P^* = \delta P_{-1} + \delta(1-\delta)P_{-2} + \delta(1-\delta)^2 P_{-3} + \ldots$. The limit of the sum of the coefficients is 1.0: $\sum_{k=1}^{K} \delta(1-\delta)^{k-1} \to 1$ as $K \to \infty$. Thus, P^* defined by equation 2a can be approximated by a series of past observed values of P such as that included in equations 2 and 3. The accuracy of the approximation increases with K, the number of past values of P included in the equation.

[6]Resources used to apprehend offenders have been 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_{jt} but the choice is not critical because alternative measures are highly correlated. CT_{jt} has been measured by the FBI crime index for the locality.

That is, the probability of clearing a crime by arrest increases as the total resources available for clearing a given set of crimes increases.[7]

Two other sanction variables were used in addition to the probability of arrest; namely, PI_{ijt}, the logarithm of the probability of imprisonment for committing a crime of type i in locality j at time t, and T_{ijt}, the logarithm of the median time served by persons imprisoned for committing crimes of type i.[8] Equations similar to equation 4 that determine sanction variables other than P_{ijt} are not made explicit in the interest of brevity and because our primary concern is the estimation of the crime function (equation 3).

Equations of the form of 3 and 4 determine crime rates and sanction levels for given allocations of resources to criminal justice activities and given values of the exogenous variables included in the sets X and Y.

[7]Sanction levels may of course depend on the character and use of criminal justice resources as well as their aggregate dollar magnitude. Unfortunately, detailed data on resource use is 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).

[8]The probabilities of arrest and imprisonment have in some studies been decomposed into the probability of arrest, the probability of conviction given arrest, and the probability of imprisonment given conviction. Our data do not permit such a division.

To complete the model, we may think of the allocation of resources to crime control being determined by each locality's efforts to minimize the expected costs of crime <u>and</u> crime control, subject to the constraints imposed by crime functions such as equation 3, sanction functions such as equation 4, and the locality's willingness to trade off other goods for crime control.[9]

III. ECONOMETRIC PROBLEMS AND PROCEDURES

Equation 3 is the basic form of the crime equations estimated in this study, although 3a was also estimated. Numerous previous studies have employed a similar equation form, but they have typically assumed that crime rates and sanction levels are determined simultaneously, i.e., that $\Theta = 0$ and $K = 1$.

Ordinary least squares (OLS) estimates of equation 3 will have the desirable statistical property of consistency only if the sanction variables are statistically independent of the error term, U. Unfortunately, such is not likely to be the case. If $\Theta = 0$, as most previous studies have assumed,

[9]We speak of decisions being made by the "locality" for convenience, recognizing that decisions are made by the locality's population through the political process of government. Factors that would presumably influence a community's choice between these costs (of crime and crime control) are income, tastes, availability of state and federal aid for law enforcement, the magnitudes of other demands on the public purse, etc.

then equation 3 includes the current value of P, and equations 3 and 4 imply that P and C are simultaneously determined.[10] In this case, the sanction levels achieved in a particular period depend on the crime rates of that period, and P and U are not statistically independent. In particular, when U is relatively large, C will be relatively large and P 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 deterrent effect.[11]

On the other hand, if $\Theta \geq 1$ and the error terms are not serially correlated, then the sanction variables in equation 3 will be statistically independent of the error term.[12] And, OLS estimates of the parameters of equation 3 will be consistent. 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

[10]Simultaneity arises because C is a component of CT, which is the sum of reported FBI index crimes.

[11]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 deterrence effects.

[12]From Equations 3 and 4, we see that $P_{-\Theta}$ depends on $C_{-\Theta}$ which in turn depends on $U_{-\Theta}$. Thus, $P_{-\Theta}$ is correlated with U if U and $U_{-\Theta}$ 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.

error term (U).[13] If so, OLS will yield inconsistent estimates of the parameters of the crime equations.

We can now see that regardless of the value of Θ, OLS estimates of equation 3 are unlikely to be consistent. Previous studies that have assumed $\Theta = 0$ have employed simultaneous equations techniques, primarily two-stage least squares (2SLS), in an attempt to obtain consistent estimates. Consistent estimates of the parameters of equation 3 can also be obtained by 2SLS when $\Theta \geq 1$.[14] In this case, estimated rather than observed values of the predetermined variables are used in the crime and sanction functions. To illustrate, the predetermined (lagged endogenous) variables in equation 3 are $P_{-\Theta}$, $P_{-\Theta-1}$, 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

[13]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 are: 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 influences 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.

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

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 in equation 3, and the estimates of the parameters of equation 3 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.[15]

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 there is no serial correlation in U, in which case the SUR technique yields the same estimates as the 2SLS technique.[16]

Equation 3a poses basically the same estimation problems as 3, since the error term, $U-(1-\delta)U_{-1}$, will not in general be independent of the

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

[16] 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).

regressors P_{-1} and C_{-1}. Hence, it was estimated by the same techniques as equation 3.

IV. DATA

In collecting and integrating data from a number of sources our central concern has been to obtain data for a set of states and a set of Standard Metropolitan Statistical Areas (SMSAs) that are consistent both across states (or SMSAs) at each point in time and across time for each state (or SMSA). The required data are variables that measure crime rates, sanction levels, and the socio-economic "climate" in each state (or SMSA). Exhibit 1 defines the exogenous 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 that were observed in each state (or SMSA) over 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 and 66 SMSAs for the years 1968-77. Clearance and crime rates are available for each crime category included in the FBI's 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 environmental variables included in the crime equations, the set X, should reflect factors other than the activities and policies of the criminal

EXHIBIT 1. Variable definitions[a]

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

Exhibit 1 (Continued)

16.	R	Reported robberies per 100,000 population for a particular year.[b]
17.	P	Proportion of reported burglaries, larcenies, or robberies cleared by arrest; an estimate of the probability of arrest.[d]
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.[e]
19.	T	Median time served in months by persons imprisoned for burglary, larceny, or robbery before their first release.[e]
20.	E	Police protection expenditures of state and local governments of the state, dollars per capita per fiscal year.[b]
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.[b]

[a] Variable definitions are the same for both states and SMSAs with the following exceptions: AID for an SMSA is state and federal aid to the local governments of the SMSA; similarly, GX, PTX, and E are for the local governments of the SMSA; data on PI and T are not available at the SMSA level.

[b] Available annually.

[c] Available only for 1971.

[d] Available annually from 1968.

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

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

justice system that affect individuals' willingness to engage in criminal activity. In particular, variables measuring the relative economic gains from legitimate and illegitimate activities 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 therefore have been included in X. Of these, variables 1-3 have been most frequently used in other studies. The variables INC, NW, POV, UN, and EDH are included in X in part because they are indicators of the relative gains from legal and illegal activities. However, some of these variables may also reflect influences on the reporting of crimes. In particular, INC and EDH may be positively related to reported crime rates if more highly educated persons and persons with higher incomes are more likely to report crimes and to support allocating resources for the operation of an accurate reporting system. PAR is a proxy for the

high crime risk portion of the juvenile population; hence, a positive association with crime rates is expected. Law enforcement efforts, crime reporting practices, and richness of targets may all vary between urban and rural areas. To control for interstate differences in crime rates that grow out of differences in urbanization, URB has been included in the offense

equations estimated from state data. URB may affect either actual or reported crime rates, or both. Finally, the rate of population change (DPOP) has been included to allow for the possibility that factors associated with population growth may influence crime rates.

The set Y denotes those environmental variables that affect the sanction levels achieved with given resources. Y 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. For example, Vandaele (1978) included NW and a measure of the percentage of population that is young (age 18-24) in this set. Thus, there may be uncertainty about whether some of the variables defined in Exhibit 1 should be included in Y. However, there is no apparent rationale for including UN, DPOP, MVR, AID, GX, and PTX.

The resources that a jurisdiction (state or locality) allocates to crime control activities will depend on the total resources at the jurisdiction's disposal and competing demands on those resources. As resources used by the public sector increase relative to resources available for both public and private sector use, the imputed cost of additional public sector resource use for crime control, or any other purpose, increases. Crime control budgets will thus tend to be directly related to a jurisdiction's resource base and inversely related to its use of that base to provide other public services. A jurisdiction that is resource poor will tend to tolerate higher crime rates than one that is rich--the poor district will be more willing than the rich to incur the costs of crime in order to forego the costs of crime control. Similarly, jurisdictions with relatively high competing public sector demands will be more willing to trade crime for crime control costs than jurisdictions with relatively low competing demands.

Variables measuring the total resources at the disposal of a state or SMSA are the per capita magnitudes of personal income (INC), federal aid (AID), and market value of property subject to property taxation (MVR). Competing public sector demands are measured by per capita state and local expenditures for purposes other than crime control (GX). Competing demands may also be influenced by DPOP and DEN. Jurisdictions with higher rates of population growth and/or lower population densities may incur higher per capita costs in providing given levels of public services. The influence of DPOP and DEN on spending for purposes other than crime control is measured by GX; thus, given GX, crime control spending should be positively related to DEN and DPOP. In sum, crime control budgets will tend to be positively related to INC, AID, MVR, DPOP, and DEN and negatively related to GX.

Public expenditures for crime control may also be related to the division of responsibility between state and local governments and their tax structures. The proportion of revenue collected by property taxes (PTX) reflects both the extent of reliance on property taxes and the relative importance of state and local governments in the collection of revenue.

Finally, resources allocated to crime control activities may depend on the public's tastes for crime control and other public services. Taste differences among jurisdictions may be related to differences in income and education (INC and EDH). In addition, the variable GX may reflect public sector institutions and willingness to use the public sector to provide services. That is, taste and institutional factors that lead to relatively high per capita spending for services other than police protection may also lead to relatively high spending for police protection. This latter effect

high per capita spending for services other than police protection may also lead to relatively high spending for police protection. This latter effect tends to offset the negative effect of competing demands that is also represented by GX.

V. ESTIMATED CRIME AND SANCTION EQUATIONS

This section reports the results of estimating the model of crime generation represented by equations 3 and 4. In the preceding section, we noted which exogenous variables may plausibly be excluded from the crime and sanction equations: DEN, MVR, AID, GX, and PTX from the crime equations and UN, DPOP, MVR, AID, GX, and PTX from the sanction equations. These _a priori_ restrictions are sufficient to identify both crime and sanction equations when $\theta = 0$.[17] Both equations are identified without these restrictions when $\theta \geq 1$. Hence, the identification issue, which has been the focus of much of the debate about the interpretation of previous findings, is resolved if we accept the argument that crime rates are little affected by current changes in sanctions.

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 and data, various data sets and assumptions about the values of θ and K were employed. When presenting estimates of equations 3 and 4, we focus in sequence on the questions of how

---

[17] It might be argued that DEN and MVR should be included in the crime equations. Doing so does not prevent identification, and neither does it significantly alter results. Those aspects of DEN that influence crimes may be reflected by URB.

crime rates appear to be affected by sanction levels, the resources allocated to criminal justice activities, and environmental variables.

A. EFFECTS OF SANCTION VARIABLES

In estimating equation 3, three measures of sanction levels have been used: 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, then we report the effects of including additional sanction measures.[18]

Table 1 presents estimates of equation 3 for the cases of K = 1 and either $\theta = 0$ or $\theta = 1$. These equations were estimated from the pooled data set of 384 observations (48 states and 8 years).[19] Equations of the same

[18] A crime is said to be cleared by arrest when a person is arrested for and charged with the crime. The clearance rate for a particular crime is the number of crimes cleared divided by the number of crimes known to the police. The clearance rate is therefore a measure of the probability of arrest--the probability that a person committing a crime of a particular type will be arrested for the crime.

[19] The equations of Table 1 were estimated under the restriction that the coefficients of the clearance rate and the environmental variables are stable over the 8 year period. This restriction is not significant in the case of burglary; that is, the hypothesis that these coefficients are stable over the 1970-77 period cannot be rejected at the 5 percent level. This hypothesis can be rejected in the cases of robbery and larceny. But even in these cases, 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 explanatory variables are as presented in Table 1.

form were also estimated for each crime and each year (1970-1977).[20] The coefficients of these individual year regressions are not reported because they are qualitatively similar to those presented in Table 1. In particular none of the clearance rate coefficients in the individual year regressions are statistically significant at the 5 percent level, although they are predominately negative.[21] And, the hypothesis that the clearance rate

[20] For each crime, the individual year regressions are of the form

$$C_{jt} = a_{0t} + \alpha_{1t} \hat{P}_{jt-\theta} + a_{2t} X_{jt} + U_{jt}$$

where $\theta = 0$ or 1; j = 1,..., 48; and t = 1970,..., 1977. All regressions were estimated by both 2SLS and SUR. To obtain the SUR estimates, the eight individual year regressions for each crime and each case (K = 1 and $\theta = 0$; K = 1 and $\theta = 1$) were treated as a system. Note that the equations reported in Table 1 are the result of applying the restrictions $\alpha_{170} = \alpha_{171} = \ldots = \alpha_{177}$ and $a_{270} = \ldots = a_{277}$ to the system of individual year regressions.

[21] 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.

TABLE 1. Equation 3 with K = 1 and Θ = 0 or Θ = 1, estimated by SUR from pooled state data, 1970–1977[a]

Explanatory variable	Burglary Θ=0	Burglary Θ=1	Larceny Θ=0	Larceny Θ=1	Robbery Θ=0	Robbery Θ=1
	Coefficients and (standard errors) when dependent variable is:					
$\hat{P}$	-.034 (.062)	--	-.100** (.047)	--	-.209** (.083)	--
$\hat{P}_{-1}$	--	-.047 (.056)	--	-.122*** (.040)	--	-.153* (.076)
INC	.150 (.145)	.150 (.171)	.382*** (.141)	.340** (.135)	.570* (.285)	.448 (.277)
POV	-.062 (.115)	-.071 (.127)	.043 (.107)	.020 (.106)	-.262 (.189)	-.266 (.193)
NW	-.033 (.040)	-.024 (.044)	.005 (.037)	.002 (.038)	.396*** (.062)	.378*** (.067)
UN	.081** (.038)	.095** (.041)	.086*** (.030)	.139*** (.027)	-.040 (.062)	.026 (.053)
PAR	.842*** (.189)	.860*** (.202)	.268 (.172)	.276 (.173)	.477 (.291)	.590* (.306)
EDH	.659*** (.241)	.717*** (.256)	.957*** (.225)	.984*** (.227)	.257 (.366)	.439 (.395)
DPOP	.842*** (.264)	.768*** (.254)	.697*** (.212)	.738*** (.219)	-.024 (.393)	.114 (.409)
URB	.944*** (.153)	.847*** (.162)	.382*** (.138)	.319** (.139)	2.033*** (.235)	2.056*** (.246)

[a]For each crime, an equation of the following form was estimated from the 384 observations of the pooled data sample:

$$C_{jt} = a_{0t} + \alpha_1\hat{P}_{jt-\theta} + a_2 X_{jt} + U_{jt}, \quad t = 1970, \ldots, 1977 \text{ and } j = 1, 48 \text{ states.}$$

Alaska and Hawaii were excluded to make the sample comparable to previous studies. Including all 50 states had little effect on the coefficients. The environmental variables included in the equation (the set X) were the first eight variables in Exhibit 1. $\hat{P}_t$ was estimated in the first-stage regressions with the explanatory variables being the first thirteen listed in Exhibit 1. For 1973-1977 the dependent variable in the larceny equations is total larceny; for 1970-72 it is larceny over $50. This difference in definition does not significantly affect coefficient estimates; the pooled data equations estimated from 1973-77 data are very similar to those reported in this table. In particular, the coefficients of $\hat{P}$ and $\hat{P}_{-1}$ are -.127 and -.066, respectively, in the larceny equations estimated from pooled 1973-77 data.

* 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.

coefficients are zero in every year cannot be rejected.[22]

We have suggested above that individuals' decisions regarding criminal activity may be influenced importantly if not primarily by past sanction levels. This view is supported by the finding that the coefficients of current clearance rates are never statistically significant when both current and lagged clearance rates are included in the crime equations. See Table 2, which reports results that are consistent with the hypothesis that Θ=1. If this hypothesis is accepted, the coefficients for current clearance rates in Table 1 are non-zero because current and past clearance rates are correlated, and not because crime rates are influenced by current clearance rates.[23]

The equations reported in Table 1 assume that K = 1. To show the effect of allowing for a longer lag in the adjustment of perceived to actual sanctions, Table 3 reports the coefficients of lagged clearance rates in

---

[22]The eight individual year regressions for each crime and each case were treated as a system of seemingly unrelated regressions and estimated with and without the restriction that $\alpha_{170} = \alpha_{171} = \ldots = \alpha_{177} = 0$. The hypothesis tested was that this restriction causes a statistically significant increase in the sum of squared residuals; in no case was the increase significant. The F statistics for this test are 1.79, .94, and 1.87 for burglary, larceny, and robbery, respectively, while the critical F for the 5 percent significance level and degrees of freedom of 8 and 304 is approximately 2.0.

[23]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, and are of similar magnitudes for larceny and robbery.

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

	Value of F-Statistic for: K=1	K=2	K=3	K=4
Burglary	1.94	1.94	1.97	.56
Larceny	1.11	1.58	.32	.33
Robbery	1.65	1.00	1.31	1.04
Critical F value[b]	2.10	2.10	2.10	2.10

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

$$C_{jt} = a_{0t} + \sum_{k=1}^{K} \alpha_{kt} \hat{P}_{jt-k+1} + a_{2t} X_{jt} + U_{jt}, \qquad t = 1972,\ldots,1977 \text{ and}$$

$$j = 1,48.$$

These regressions were estimated by the procedure described in note a of Table 1; values of $\hat{P}_{jt-k+1}$ were estimated in the first stage and the 6 cross-section regressions were treated as a system and estimated by the SUR technique.

[b]This is the value that the calculated F-statistic must exceed if the hypothesis being tested is to be rejected at the 5 percent significance level. 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.

Table 3. Coefficients of lagged clearance rates in equation 3, estimated for different values of K from pooled state data, 1972-1977[a]

Coefficients and (standard error) of lagged clearance rate in equation for:

Lagged clearance rate	Burglary K=1	Burglary K=4	Larceny K=1	Larceny K=4	Robbery K=1	Robbery K=4
$\hat{P}_{-1}$	-.033 (.094)	-.078 (.116)	-.118 (.082)	-.117 (.099)	-.127 (.123)	-.204 (.194)
$\hat{P}_{-2}$		-.181 (.115)		.075 (.085)		.103 (.192)
$\hat{P}_{-3}$		.034 (.142)		-.011 (.095)		-.140 (.221)
$\hat{P}_{-4}$		.125 (.117)		.021 (.067)		-.074 (.181)
Sum of coefficients	-.033	-.100	-.118	-.032	-.127	-.315

[a]Coefficients are for equation 3 with θ=1 and K=1 or K=4. The environmental variables included in these equations are the same as those included in equation 3 of Table 1--the first 8 variables of Exhibit 1. The clearance rate coefficients are 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, pooling of data to estimate clearance rate effects seems appropriate.

crime equations estimated for 1972-1977.[24] If perceived sanctions adjust with a lag to past values of actual sanctions, the sum of these clearance rate coefficients, $\sum_{k=1}^{K} \alpha_k$, should increase in absolute value with increases in K. Comparing the estimates for K=1 and K=4 in Table 3 shows that such is the case for burglary and robbery, but not for larceny. There is thus some evidence that perceived sanctions respond with a lag to actual sanctions. But the evidence is weak; the coefficients of $\hat{P}_{-2}$, $\hat{P}_{-3}$, and $\hat{P}_{-4}$ are not statistically significant, either individually or as a group, in any of the three equations. This finding can be interpreted in either of 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 values of P are so highly correlated over the periods t-1 through t-4 that the lag structure cannot be estimated. In either case, crime equations need include only one lagged value of P to allow adequately for the effect of arrest on offense rates.

Estimates of equation 3a, which imposes a particular lag structure, have similar implications. When equation 3a is estimated by SUR from the 1970-77 pooled data, the coefficient of C_{-1}, which estimates $1-\delta$, is slightly more than 1.0 in both the burglary and larceny equations; and, in each case, the hypothesis that $\delta > 0$ can be rejected at the 1 percent significance level. As δ approaches zero in equation 3a, the effect of the arrest

[24] Equations were also estimated for K=2 and K=3, but results were similar so those equations are not reported. With K=4 and Θ=1, regressions for 1970 and 1971 would require clearance rates for 1966 and 1967; but clearance rates are available only for 1968 and subsequent years. Hence, estimates were obtained for 1972-77 rather than 1970-77.

sanction on crime rates likewise approaches zero. In contrast, the estimate of $1-\delta$ in the robbery equation is .65 and the implied value of a_1 is -.337. Thus, the estimates of equation 3a, like those of equation 3, imply that arrest has a greater effect on robbery than on burglary and larceny.

The coefficients of equation 3 were also estimated from data on 66 SMSAs for the years 1974-1977, with results quite similar to those reported in Table 1. In particular, they show no statistically significant link between crime rates and clearance rates. In the interest of brevity, these results are not presented and discussed in detail.

The results reported in Tables 1-3 are from crime equations that include more exogenous variables than most 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, the additional five 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 that include only INC, NW, and POV as exogenous variables are misspecified, even though they may show somewhat larger crime prevention effects for arrest.[25]

2. <u>Probability of imprisonment and time served</u>. The results just presented provide at most weak evidence that clearing crimes by arrest

[25] For example, when the crime equations are estimated with only three exogenous variables [INC, POV, NW], the hypothesis that the coefficients of the clearance rates are zero in every cross-section year (1970-77) can be rejected for all three crimes. In contrast, this hypothesis cannot be rejected for any crime when the exogenous variable set also includes UN, PAR, EDH, DPOP, and URB (see note 22). Thus, 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.

affects crime rates. One possible explanation for this finding is that arrests typically do not lead to imprisonment and hence are relatively weak sanctions.[26] In this case, clearance rates would be satisfactory indicators of sanction levels only if they were highly correlated with imprisonment probabilities and measures of time served. Although there is a weak correlation, it is important to try to determine whether the effects of sanctions on crime rates are stronger when sanction levels are measured by imprisonment probability and time served as well as arrest probability.

Data on probability of imprisonment and time served are available only for 1960, 1964, and 1970. Thus, crime equations that include all three sanction variables, P, PI, and T, can be estimated only for 1970 and adjacent years. Table 4 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.

The effect of including only time served and probability of imprisonment as the sanction variables in the crime equation is shown by equation 3c

[26]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.

TABLE 4. Effects of imprisonment sanctions, equation 3 estimated by SUR, state data, 1970-71[a]

Coefficients and (standard errors) in equations for:

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

in Table 4. Equation 3c includes the same environmental variables as 3b, and thus differs from 3b only 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. Results are similar when sanction variables are either lagged 2 years or not lagged. In the latter case, the coefficients of PI are negative in all three equations (for burglary, robbery, and larceny), but never statistically significant at the 5 percent level. Despite the weakness of the effects of the sanction variables, the crime equations explain a relatively high percentage of the crime rate variation; R^2 is .80, .79, and .54 for 2SLS estimates of equation 3b for burglary, robbery, and larceny, respectively.

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 coefficients 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 for 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 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 4 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, as was the case with arrest, 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.

B. CRIME RATES AND RESOURCE INPUTS

The resources allocated to criminal justice activities may influence crime rates as they affect the sanction levels achieved by the criminal justice system. This relationship is readily seen by substituting equation 4 into equation 3, giving for K=1

$$C_{ijt} = a_0 + \alpha_1 c_0 + \alpha_1 c_1 E_{jt-\Theta} + \alpha_1 c_2 CT_{jt-\Theta} + \alpha_1 c_3 Y_{jt-\Theta} + a_2 X_{jt} + \alpha_1 V_{ijt-\Theta} + U_{ijt}. \quad (5)$$

Resources may also affect crime rates more directly. For example, the number and visability of police patrols may affect crime rates in a given area even if they do not affect arrest and/or imprisonment probabilities.

To test for the effects of resources on crime rates and sanction levels, equations 4 and 5 were estimated by 2SLS and SUR. Because police (and other criminal justice) expenditures are likely to depend in part on crime rates, they may not be statistically independent of the error terms in either the sanction or the crime equations. Total index crimes, CT, also will not be independent of these error terms because they include the crimes under study. Thus, in the first stage regressions, E and CT were regressed on the 13 exogenous variables of Exhibit 1 and the resulting equations were used to generate estimated values, $\hat{E}$ and $\hat{CT}$, that were then used as explanatory variables in the second stage and SUR regressions.

Representative results for clearance rate equations estimated in this manner are presented in Table 5. Without exception, the expenditure coefficients are not statistically significant, although they are all positive. Similar equations were estimated with the dependent variables being the 1970 probability of imprisonment for either burglary, larceny, or robbery; in all cases, the expenditure coefficients are negative rather than positive and frequently statistically significant.[27] Equations for both clearance rates and imprisonment probabilities were estimated with various sets of exogenous variables in an attempt to find specifications that would yield positive and significant expenditure coefficients; none were found. In most instances, the exogenous variables as a group have a statistically significant effect on sanction levels, even though their individual coefficients are often not significant. The negative coefficient of EDH probably reflects reporting differences. Given the number of clearances (a function of E) and actual crime rates, measured clearance rates will be inversely related to reporting accuracy, which is likely to be positively related to EDH.

The coefficients of police protection expenditures, $\hat{E}$, in equation 5 are not presented because none are statistically significant. And, of the 24 coefficients (one for each of three crime equations in each of eight years), 19 are positive rather than negative. Similarly, the coefficients

[27] In the equations for probability of imprisonment, resource inputs are measured by criminal justice system expenditures, which include expenditures on courts and corrections as well as police protection. The reason is that imprisonment involves courts and corrections as well as police, while arrest is primarily a police activity. As an empirical matter the distinction is not important, since criminal justice system and police protection expenditures are highly correlated.

Table 5. Sanction equations (equation 4) estimated by SUR from pooled state data, 1970-77[a]

Explanatory variable	Coefficients and (standard errors) when dependent variable is: burglary clearance rate	larceny clearance rate	robbery clearance rate
$\hat{E}$	.031 (.119)	.040 (.118)	.064 (.145)
$\hat{CT}$	-.124 (.091)	-.192* (.101)	-.198* (.115)
NW	-.049 (.038)	.035 (.030)	-.103** (.043)
EDH	-.691** (.292)	-.422* (.236)	-.747** (.326)
DEN	-.011 (.026)	-.028 (.021)	-.069** (.029)
URB	.085 (.209)	.253 (.169)	.110 (.236)

[a] Equations of the following form were estimated from the 384 observations of the 1970-77 pooled data sample:

$$P_{ijt} = c_{0t} + c_1\hat{E}_{jt} + c_2\hat{CT}_{jt} + c_3Y_{jt} + v_{ijt},$$

where i = burglary, larceny, or robbery; j = 1, 48 states; t = 1970, ··· , 1977.

of $\hat{E}$ in equation 5 estimated from the pooled data are positive, but not statistically significant. The coefficients of the environmental variables in crime equations of the form of equation 5 are virtually the same as those obtained with equation 3. Essentially the same results were obtained when police protection inputs were measured in physical units (number of full time equivalent police employees per ten thousand of population) rather than dollar amounts.

In short, our results provide no evidence of a statistically significant marginal relationship, either direct or indirect, between crime rates and criminal justice expenditures. Neither do they provide evidence that greater spending leads to higher probabilities of arrest and imprisonment. More specifically, the conclusion is that given the manner in which resources were used in the sample years, differences in the dollar amount of resources employed did not give rise to significant differences in sanction levels and crime rates. Of course, these results do not mean that states would be unable to increase sanction levels and reduce crime rates by using additional resources in a different manner than they were used in the sample years. However, they do suggest that one cannot argue for more criminal justice spending as a means of increasing sanction levels and decreasing crime rates without addressing the question of whether and how the uses of the additional resources would differ from past uses, and without explaining why such differences in use might be expected to be effective.

The preceding interpretation of our estimates of equations 4 and 5 is subject to one caveat. The errors with which crime rates are measured may be inversely correlated with police (or criminal justice) expenditures. If so, the estimated coefficient of $\hat{E}$ in equation 4 may be negatively biased and may therefore understate the extent to which higher expenditures lead to

higher clearance rates. Similarly, the coefficient of $\hat{E}$ in equation 5 may be positively biased, leading to an underestimate of the crime prevention effect of police expenditures. Although the magnitude of these biases is unknown, our estimation procedure yields estimates that are consistent (asymptotically unbiased) and our samples are large (300+ for the pooled data results). Therefore, it seems reasonable to proceed on the assumption that the estimates of the coefficients of $\hat{E}$ in equations 4 and 5 are subject to minimal bias, keeping in mind that to the extent that they are biased they may understate the effectiveness of expenditures in reducing crime rates and increasing sanction levels.

C. EFFECTS OF ENVIRONMENTAL VARIABLES

Preceding sections show that variables measuring sanction levels and resource use by the criminal justice system do not contribute significantly to the explanation of observed variation in crime rates. But a large share of this variation is explained by the crime equations. For example, the values of R^2 for the 1970-77 2SLS estimates of equation 3 with $\theta=0$ and $K=1$ average .78, .73, and .88 for burglary, larceny, and robbery, respectively. Thus, the environmental variables account for a large fraction of the observed variation in crime rates. Moreover, the coefficients of these variables are essentially the same for all specifications of how clearance rates enter into the crime equations (for all assumed values of θ and K).

Table 1 presents representative estimates of the effects of environmental variables. The coefficients of INC, UN, PAR, EDH, DPOP, and URB are predominately positive, with the positive coefficients frequently being statistically significant; the negative coefficients are never significant. The coefficients of NW are significant and positive for all 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.

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 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 1, 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.[28] However, omitting INC and NW resulted in statistically significant coefficients for PAR in all three equations, while

[28]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 that 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.

leaving other coefficients and the overall explanatory power of the equations essentially unchanged. This result suggests that the positive relationship between NW and 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. In contrast, the other environmental variables, with the possible exception of INC, can be affected by policy only in the long run, if at all.

The crime equations for both states and SMSAs were also estimated with the ratio of the juvenile population (persons of age 14-17) to the total population included as an explanatory variable. If juveniles are more likely than adults to engage in property crimes, this variable should contribute to the explanation of observed variation in crime rates; but it does not. Thus, our results do not support the frequently expressed view that the juvenile portion of the population in general contributes disproportionately to property crime rates. Stated differently, a disproportionate share of crimes may be committed by young persons, as popular wisdom alleges, but the very substantial variation in observed crime rates among states and SMSAs cannot be explained by this phenomenon. However, our results do suggest that a fraction of the juvenile population, namely young persons who live in one-parent households, commits relatively more crimes than other fractions. Also, whether youths of age 14-17 are in school does not appear to be an important factor in their criminal activity, apart from the fact that many who are not in school are also from one-parent households: when the fraction of the population that is of age 14-17 and not in school is included as an explanatory variable in crime equations, its coefficients are not statistically significant.

VI. POLICY IMPLICATIONS

We have to this point considered mainly the statistical significance of the coefficients of the sanction and environmental variables included in the crime equations. We now turn to the question of their quantitative significance--their ability to explain cross-section and time-series variation in observed crime rates--and their implications for criminal justice policy. The discussion centers on point estimates of the effects of changes in environmental and sanction variables. These estimates are of course subject to error, but they were obtained from large samples by techniques that provide consistent estimators. Thus, we can argue that the point estimates are "best guesses" about the effects of variables, keeping in mind that they are subject to error.

Table 6 presents estimates of the crime rate change associated with a change of one standard deviation in each of the explanatory variables. These estimates, which are based on the equations for K=1 and Θ=1 reported in Table 1, show the relative importance of the sanction and environmental variables in the explanation of cross-section variation in crime rates. Other equations estimated from both state and SMSA data have basically the same implications, three of which are especially noteworthy.

Table 6. Effects of varying explanatory variables by one standard deviation, 1970 state data[a]

Explanatory variable	Percentage change in crime rate when explanatory variable is increased by one standard deviation: Burglary	Larceny	Robbery
P_{-1}	-1.0	-2.8	-4.0
INC	2.3	5.2	6.8
POV	-2.9	.8	-10.8
NW	-2.3	.2	35.6
UN	2.1	3.1	.6
PAR	21.5	6.9	14.8
EDH	11.2	15.4	6.9
DPOP	7.8	7.5	1.2
URB	19.1	7.2	46.4

[a] Effects are estimated from the pooled data equation for K=1 and Θ=1 reported in Table 1.

First, despite their substantial interstate variation, clearance rates are a relatively minor source of cross-section variation in crime rates.

Second, states with relatively unfavorable socio-economic environments can expect relatively high crime rates even if they succeed in clearing a relatively large fraction of their crimes by arrest. For example, increasing the burglary clearance rate from its sample mean (.20) to its sample maximum (.38) would reduce burglaries by only 3.1 percent. The corresponding decreases for larceny and robbery are 9.4 and 10.9 percent, respectively. For all three crimes, these decreases fall short of the increases in crimes that would be associated with only one standard deviation increases in UN and PAR--23.6, 10.0, and 15.3 percent, for burglary, larceny, and robbery, respectively. Thus, states may be able to offset in part the effects of an unfavorable environment with relatively high clearance rates, but full offsets are not likely.[29]

Third, criminal activity may be limited by policies or measures that reduce unemployment and/or the frequency of one parent households as well as by traditional criminal justice activities. To illustrate, reducing unemployment by one standard deviation would be as effective in reducing burglaries as increasing the burglary clearance rate by about two standard deviations. Or, the same reduction in burglaries could be achieved by reducing PAR by .05 standard deviations -- by reducing PAR from 5.67 percent to 5.60 percent.

A similar picture emerges when we try to identify the forces responsible for the increase in crime rates over the sample period. For 1970-77, the mean percentage changes in burglary, larceny, and robbery rates were 36, 37, and 34, respectively. The pooled data equations allow separation of these average changes into autonomous components and components that are due to changes in the various explanatory variables. The equations for K=1 and Θ=1 in Table 1 imply that a substantial fraction of the average change in each crime rate is associated with changes in the environmental variables, while relatively little of the change can be attributed to changes in clearance rates. See Table 7. For each crime, the autonomous change, which is measured by the 1970-1977 change in the equation intercept, is smaller than the change associated with changes in environmental variables. And, in the case of robbery, the autonomous change is negative; factors not taken into account apparently operated to decrease robbery rates over this period.

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

We cannot, of course, identify changes in INC and URB as direct causes of changes in crime rates; instead, the appropriate inference is that factors associated with rising income and increasing urbanization appear to account for a significant fraction of the 1970-1977 growth in crime rates. In the case of unemployment, a direct cause-effect relationship is more plausible, especially when we note that UN has a stronger influence on burglary and larceny, which are likley 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.

The elasticities for UN of about .1 might seem too small to be of policy significance. However, because UN varied substantially over the sample years, it accounts for quantitatively significant shares, 10.3 and

Table 7. Sources of crime rate change, state data, 1970-77[a]

Source of change[b]	Average change in crime rate attributable to indicated source (in percent)		
	Burglary	Larceny	Robbery
Autonomous ($a_{077}-a_{070}$)	16.1	8.5[c]	-10.1
P_{-1}	.5	-.4	-.8
INC	9.3	20.9	27.6
UN	3.7	5.5	1.0
DPOP	-.2	-.2	-.03
URB	6.8	2.6	16.4
Mean percentage change in crime rate, 1970-1977.	36.2	36.9	34.1

[a]Table entries show the fraction of the average change in crime rates over the 1970-1977 period that can be attributed to the average changes in the explanatory variables. The estimates are based on the pooled data equations for K=1 and θ=1 reported in Table 1. The regression coefficients are estimated such that for a particular crime:

$$\overline{C}_t = a_{0t} + \alpha_1\overline{P}_{t-1} + a_2\overline{X}_t, \ t = 1970, \ldots, 1977$$

where $\overline{C}_t \equiv 1/48 \sum_{j=1}^{48} C_{jt}$ and similarly for $\overline{P}_t$ and $\overline{X}_t$.

Hence, $\overline{C}_{77} - \overline{C}_{70} = a_{077} - a_{070} + \alpha_1\left(\overline{P}_{76} - \overline{P}_{69}\right) + a_2\left(\overline{X}_{77} - \overline{X}_{70}\right)$

The table entries state the terms on the right hand side of this equation as percentages of the left hand side.

[b]The other environmental variables, EDH, PAR, POV, and NW are not included since data were available for only one year, 1970.

[c]Adjusted for the change in 1973 from larceny over $50 to total larceny. This change in definition caused a one-time 89 percent increase in the reported larceny series of the FBI.

14.8 percent, of the 1970-77 increases in burglary and larceny rates. Or, as another illustration, the burglary rate in Alabama increased by 10 percent from 1974 to 1975. At the same time, UN increased by about 41 percent. Hence, with an elasticity of about .1 for UN approximately 4 percentage points, or 40 percent, of the 10 percent increase in burglary can be attributed to increased unemployment. 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 must be absent for their families to qualify for AFDC (Aid to Families With Dependent Children).

Regardless of how we interpret the effects of the exogenous variables, it is clear that changes in clearance rates were not important sources of crime rate changes during this period, simply because clearance rates did not change much.[30] And, for larceny and robbery, the mean change in clearance rates was positive, giving rise to a decrease rather than an increase in crime rates. Even if the true clearance rate coefficients were larger (in absolute value) by two standard errors (i.e., -.158, -.202, and -.305 for burglary, larceny, and robbery), clearance rate changes would account for only small percentage changes in crime rates (1.7., -.6, and -1.6).

This apparently weak link between crime rates and sanction levels suggests that states and localities have very limited capabilities for controlling crime. This conclusion is greatly reinforced by our finding

[30]The average percentage changes in clearance rates for this 8 year period were -10.7, 3.0, and 5.2, for burglary, larceny, and robbery, respectively.

that arrest and imprisonment probabilities are little affected by the allocation of resources to police protection and other criminal justice activities. To illustrate, Table 5 above provides estimates of the effects of police protection expenditures on clearance rates. These estimates can be used in conjunction with the clearance rate coefficients of Table 1 to estimate the effects of changes in police expenditures on crime rates. From Table 5 we note that a one percent increase in per capita police expenditures, E, increases the burglary clearance rate by .031 percent, while (from Table 1) a one percent increase in the burglary clearance rate reduces crime rates by .047 percent. Hence, a one percent increase in E would directly decrease burglary rates by (.047)(.031) = .0015 percent; the corresponding decreases for larceny and robbery are (.122)(.040) = .0049 and (.153)(.064) = .0098 percent, respectively.[31]

These estimates imply a very small crime prevention effect from increases in police spending: doubling police spending would directly decrease each crime rate by less than .1 percent. In contrast, reducing the unemployment rate by about 10 percent, for example from the 1970 mean of 2 percent to 1.8 percent, would reduce burglary, larceny, and robbery rates by about 1.0,

[31]These are only the initial changes. These decreases in crime rates may lead to a decrease in total crime, which according to the sanction equations would lead to higher clearance rates. Higher clearance rates would lead in turn to lower crime rates, etc. Thus, the long run effect of a change in E on a given crime rate depends in part on its effects on all crime rates. We have not estimated the effects of E on all categories of crime. However, if the effects for other crimes are of the same order of magnitude as those estimated for burglary, larceny, and robbery, the long-run effects of a change in E would be only about 2 to 3 percent higher than the one-period effects. More important, the proportion by which one-period effects understate long-run effects should be the same for all explanatory variables, e.g., UN. Thus, we can guage the <u>relative</u> influence of, say, E and UN with estimates of either one-period or long-run effects.

1.4, and .3 percent, respectively. These estimates suggest that although reducing unemployment may not produce large decreases in crime rates, it may nevertheless be a more effective tool for crime control than increasing police expenditures.

Since the preceding 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 (burglary, 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.

VII. SUMMARY

This study estimated parameters of crime equations (supply-of- offense functions) similar to those estimated in numerous previous studies. On balance, our results provide at most weak evidence that property crime 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.

In contrast, 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 the same results with 1970 data; but when the specification is altered to include plausible exogenous variables, the apparent crime prevention effect vanishes. 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 the 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.

From a policy perspective our findings suggest that there is little to be gained in terms of reduced crime rates by devoting more resources to the arrest and imprisonment of offenders. This is so for two reasons. First is the apparently weak effect of sanction levels on crime rates. Second, evidence that objective sanction levels are affected by marginal differences in resources allocated to the application of those sanctions is so weak as

to be virtually non-existent. Thus, even if sanction levels do affect crime rates, it appears that we as a society cannot affect sanction levels simply by allocating more resources to the criminal justice system. More specifically, one cannot argue that more criminal justice spending would increase sanction levels without addressing the question of whether and how the uses of the additional resources would differ from past uses, and without explaining why such differences in use might be expected to increase sanction levels.

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 10 percent of the 1970-77 average increase in burglary rates and about 15 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.

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.

Fox, J.A. ed., (1981) Models in Quantitative Criminology. Academic Press, New York.

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.

Jones, E.T. (1974) "The Impact of Crime Rate Changes on Police Expenditures in American Cities." Criminology 11, 516-524.

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.

Swimmer, E. (1974) "The Relationship of Police and Crime: Some Methodological and Empirical Results." Criminology 12, 293-314.

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