FINAL REPORT

THE CRIME-CONTROL EFFECT OF ARREST AND INCARCERATION: A CRIMINOMETRIC APPROACH

by

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PREFACE

This draft has been prepared for the purpose of review by the National Institute of Justice.

ABSTRACT

In this Report, a theoretically rigorous deterrence model is developed. In its philosophy, structure and choice of variables, the model builds upon and extends past deterrence research. In the empirical implementation of this model UCR Index offense rates are related to four sanctions instruments: the probability of being arrested, the probability of being incarcerated, the length of prison sentence, and the length of post-prison probation. These variables are treated, theoretically and empirically, as part of an interacting system of equations. The empirical analysis provides measures of the crime-control impact of these sanctions on individual UCR offense rates. It is argued that the principal contribution to crime-control derives from deterrence, and that incapacitation's impact on offense rates is minimal. The empirical analysis also provides, as subsidiary results, an explanation for variation in arrest and incarceration probabilities and in the length of incarceration.

The statistical model incorporates most of the demographic and socioeconomic control variables which have been shown in the deterrence literature to maintain a consistent and theoretically plausible association with offense rates. The empirical analysis uses aggregate (judicial-district-level) cross-sectional data, as well as data relating to individual UCR offenders newly admitted to prison. The data are drawn from Georgia and North Carolina and relate to the years 1978 and 1979, respectively. Conventional econometric procedures are used to estimate the model's parameters.

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CHAPTER 1

THE RESEARCH CONTEXT

A. INTRODUCTION

The thesis that the certainty, severity, and celerity of sanctions act as a deterrent to criminal behavior has had a long and varied history. Its first formal presentation is usually credited to Cesare Becarria (1767). In recent years, the deterrence doctrine has been shown to belong to a more general theory, predicated on rational, voluntary, individual choice. Becker (1968) provided the initial micro-theoretic foundation. His formulation, later extended by Ehrlich (1973), Block and Heineke (1975), Heineke (1978), and others, has for its basis the brute assumption that potential offenders and victims behave as though they are rational, and that they strive to maximize their own individual wellbeing.

From the rational choice model of Becker and Ehrlich one may readily deduce the existence of a deterrent effect from the imposition of sanctions, as well as an economic status relation to criminal activity. In the Becker-Ehrlich model, it is assumed that the individual maximizes his wellbeing by maximizing his wealth. Wealth is defined as a composite of both assets and income, and is made to depend upon both present values of assets and income as well as anticipated future values. One maximizes wealth by allocating one's time between legitimate and illegitimate activities. Leisure is assumed to be constant. It follows, therefore, that an increase in time spent in illegitimate activity must be at the expense of time spent

in legitimate activity. The model also assumes: (i) that legitimate and illegitimate work are substitutable (explicit consideration is not given to the moral/ethical value, irksomeness, etc. of illegitimate work vis à vis legitimate work); (ii) that the returns to both activities are positive; (iii) that the returns to one activity do not affect the returns to the other; (iv) that illegitimate returns are stochastic (Bernoulli-distributed), depending upon the probability of being sanctioned, whereas legitimate returns are non-stochastic; (v) that sanctions can be expressed in monetary values and are otherwise unrestricted; (vi) that the potential offender's estimates of his returns to legitimate and illegitimate activity and of the cost of sanctions are monotonically related to their objective values; and (vii) that persons with more wealth are more willing to undertake risk.

From these assumptions it can be shown that a decrease in legitimate returns, an increase in illegitimate returns, a reduction in the probability of being sanctioned, or an increase in the severity of the sanction received will induce a transfer into illegitimate activity and, therefore, an increase in the crime rate. The results derived from this model are unambiguous: sanctions deter criminal activity.

The Becker-Ehrlich model has been widely applied. Indeed, implicitly or explicitly, most of the studies to be reviewed in the next section of this chapter are premised upon, or presume to test, this version of the rational choice model.

Recently, the Becker-Ehrlich model has been subjected to critical reanalysis (Block and Heineke, 1975; Block and Lind, 1975; Heineke, 1978). It has been shown that when some of the assumptions underlying the Becker-Ehrlich model are changed, the behavioral implications emanating from the model are no longer unambiguous. The direction taken by the newer theory involves the relaxation of the assumptions: (i) that legitimate and illegitimate work are devoid of moral content, are equally irksome, etc.; (ii) that all activity and all sanctions may be reduced to monetary equivalents; (iii) that one's wealth may be reduced to zero through the imposition of sanctions; and (iv) that leisure time is a constant,

The more general theory that has evolved from the work of Block, Lind, and Heineke asserts that one cannot predict a priori whether a decrease in returns to legitimate activity, an increase in returns to illegitimate activity, a reduction in the probability of being sanctioned, or a reduction in the severity of the sanction will induce an increase in illegitimate activity. Most particularly, the Block-Heineke model removes the theoretical presumption that sanctions deter criminal activity. In the context of the less restrictive assumptions of their model, theory becomes agnostic.

Generally speaking, theoretical indeterminacy adds complexity to empirical analysis. This would be especially true if empirical analysis supported the view that no relation exists between sanctions and offense rates. Assuming that one's statistical procedures are

impeccable, that the data are fully trustworthy, and that the many problems attending empirical investigation of the deterrence issue, such as those enumerated by Orsagh (1979), have been successfully overcome, the absence of a statistical relation would have to be ascribed to one of three mutually exclusive possibilities: (i) A relation exists but, due to chance variation, the analysis fails to detect the relation. (ii) The theory is, in fact, incorrect. (iii) The theory is correct, but the configuration of empirical values for the model's parameters negates the existence of a relation. In this instance, the evidence is "correct" and the empirical conclusion would be sustained under repeated sampling from this particular environment.

If the empiricist concludes from his evidence that the theory is incorrect; but, in fact, and unknown to him, the first of the three propositions is true, he commits an "alpha error." One reduces alpha risk by increasing the size of one's sample or by conducting more studies. Normally, one would become increasingly confident that the theory is incorrect if, under repeated sampling, one were to fail to discover an empirical association between the two variables under consideration. However, when one's theory yields indeterminate results, disbelief in the first proposition does not automatically require belief in the second. Rather, more testing may be demanded, testing conducted, in this instance, in environments in which the model's parameters may assume different values.

Suppose, on the other hand, that empirical investigation provides strong evidence to support the existence of a relation between the two variables -- the situation that typifies the deterrence issue, as we shall show. Suppose, also, that one's statistical procedures and data are both faultless. In this case, we would be led to choose between two propositions: (i) There is, in fact, no relation between the two variables. The observed relation is spurious, and arises from chance variation. (ii) The theory is correct, at least with reference to the environment from which the sample was drawn.

If the empiricist concludes from his evidence that the theory is correct; but, in fact, and unknown to him, the first of the two propositions is true, he commits a "beta error." Since one's risk of committing a beta error diminishes with increasing sample size, i.e. by conducting more studies, a statistical relation that continues to manifest itself through repeated sampling (more studies) would cause one to become increasingly confident that, in fact, a true relation exists. However, this conclusion would only be applicable to the environment from which the sample was drawn. Generalization beyond the tested environment would be wholly inappropriate.

Therefore, ceteris paribus, more data will always be preferred. It permits a reduction in alpha (or beta) risk with no necessary attendant increase in beta (or alpha) risk, thereby increasing the likelihood that the empiricist will make a correct decision with respect to the validity of the null hypothesis, concerning sanctions and the offense rate. But beyond this obvious advantage, the indeterminacy that characterizes the more general theoretical model must certainly enhance our interest in, heighten the value of, and supply

additional justification for, further empirical investigation of the deterrence issue. It is gratifying, therefore, to note that enormous research effort has been devoted to the question of the efficacy of sanctions as a deterrent to criminal activity, particularly in recent years. To illustrate: Beyleveld (1980) provides an up-dated, extraordinarily complete, and exceptionally well annotated bibliography of, and extensive commentary upon, deterrence studies published in English between January 1946 and December 1978. He lists 568 items in all. Of these, 419 (or 74 percent) have appeared since 1970.

Most deterrence studies have been concerned with the effects of legal sanctions. The sanctions that derive from the response of private individuals and agencies, and that have their impact on the offender's career, his present and future earnings, and his status with family, friends, and the wider community have received little attention. Moreover, the extensive work which has focused on legal sanctions has been directed to a consideration of its certainty and severity dimensions. The celerity dimension has been almost totally neglected. For example, Beyleveld's (1980) bibliography contains just four references, all of which utilize perceptual data derived from small samples of individuals. Indeed, the scope of deterrence research is even more limited, having dealt almost exclusively with five sanctions: the probabilities of arrest, conviction, incarceration, and execution, and the length of prison sentence. Finally, quantitative analysis has been largely, though by no means exclusively, confined to the effect of these sanctions on UCR Index offenses.

In the following section of this chapter, we shall review the more relevant literature that relates legal sanctions to Index offenses.

B. REVIEW OF THE LITERATURE

In this section of the report we shall summarize the results of recent studies of the deterrent effect of legal sanctions on the crime rate. In developing our review we were guided by the following criteria: The studies must be quantitative, must have been published since 1970, must relate to one or more of the seven Index offenses,[1] must relate to the certainty or severity of sanctions as measured by the arrest or incarceration rate, or to the length of incarceration, and must be methodologically sophisticated, in the sense defined below. The subset of deterrence studies formed by the intersection of these conditions is important in three respects: First, for evaluation purposes, the subset is manageably small. Second, in its theoretical and methodological orientation, as well as in its subject matter, this subset is in the mainstream of current empirical research. And, third, this subset is most immediately concerned with the issues and concerns motivating this report.

Most deterrence studies fall far short of meeting the foregoing

[1]Homicide (including non-negligent manslaughter), rape, assault, robbery, burglary, larceny, and motor vehicle theft. For brevity, the latter shall often be referred to as auto theft.

selection criteria. On the other hand, our subset would have included many additional studies had the criteria been slightly modified. These marginal studies deserve some attention. By identifying these excluded, almost relevant studies, we provide, by indirection, a clearer definition of the scope and boundaries of this review and of this report. Therefore, it is useful to note the following particular exclusions from our review:

(i) Studies that encompass broader crime aggregates, such as those that deal with all felony offenses (Orsagh, 1973; Tittle and Rowe, 1974; Phillips and Votey, 1975; Carr-Hill and Stern, 1979;[2] et al., or even broader aggregates, such as that of Witte, 1980).

(ii) Studies concerned with particular non-UCR offenses. As recent examples, we have the quantitative analyses of tax evasion (Mason and Calvin, 1978), shoplifting (Kraut, 1976), illegal drug use (Burkett and Jensen, 1975), and driving under the influence (Zabor, 1976; Johnson, et al.,1976).

(iii) Studies that measure deterrence variables indirectly -- for example, the use of law enforcement expenditure to represent the risk of being sanctioned (Greenwood and Wadycki, 1973; McPheters and Stronge, 1974).

[2]Burglary offenses are also analyzed by Carr-Hill and Stern, and are included in our tables.

(iv). Studies that are exclusively concerned with the probability of being executed -- for example, the reanalysis of the Ehrlich (1975) study by Bowers and Pierce (1975). However, some studies -- for example, Ehrlich (1975) and Passell and Taylor (1977) -- address the death penalty issue, but also provide evidence concerning the deterrent effect of other sanctions on the homicide rate. This evidence will be reported below.

(v) Studies that use neither control variables nor a multiple equation system to develop their estimates. (Most of these use simple correlation analysis.) Included among these studies are the Gibbs's article (1968) that initiated the modern deterrence controversy, as well as studies by Gray and Martin (1969), Bean and Cushing (1971), Logan (1972), Tittle (1969), and Chiricos and Waldo (1970).[3] These have been excluded because later analyses (Ehrlich, 1973; Black and Orsagh, 1978;

[3]Bean and Cushing (1971) do utilize control variables in their analysis -- specifically, a regional North/South dummy variable which operates by itself and in interaction with the sanctions variables. We regard this advance in model design as quite minor. In comparison to the more technically sophisticated analyses reported below, their work is clearly outdated.

The panel models of Logan (1975) and Greenberg, et al. (1979) include no control variables, but their estimates are derived from a system of simultaneously determined crime-sanctions relations.

Vandaele, 1978b; and others), using the same 1960 (and 1950) cross-sectional data, improve upon the former studies by introducing control variables to neutralize the confounding effects of omitted non-sanctions factors on the crime rate. Moreover, the latter studies explicitly account for the possibility that crime and sanctions are interdependent. In our view, these technically more advanced analyses render the earlier work obsolete.

Our summary of the empirical evidence concerning the linkage between UCR offenses and sanctions is presented in the following four tables. The observations in these tables are derived from the coefficients of particular sanctions variables, and refer to their signs and their orders of magnitude. Each observation consists of an alphabetic character, which may or may not be preceded by

The alphabetic character indicates the sign of the coefficient and whether or not it is "significant." The alphabetic character is derived from the ratio of the coefficient to its standard error.[4] The following tabulation relates this ratio, t, to the character appearing in the table:

[4]In ordinary least squares this ratio is, of course, Student's t-statistic. In two-stage least squares, the ratio approaches t asymtotically as the sample becomes large.

Sign of the Ratio, t			
Positive		Negative	
Value	Character	Value	Character
$t \geq 3.0$	vp	$t \leq -3.0$	v
$3.0 > t \geq 2.0$	sp	$-3.0 < t \leq -2.0$	s
$2.0 > t > 0.0$	p	$-2.0 < t < 0.0$	n

We shall refer to s as "significant" and v as "very significant." However, because the number of degrees of freedom used to estimate the coefficients varies from study to study, and because the small sampling distribution of t is not known when the coefficients are estimated for some of the simultaneous equations procedures, it is not possible to assign a particular, precise level of significance to s and v. Hence, "significant" and "very significant" should be viewed as very crude approximations to levels of significance in the neighborhood of 0.05 and 0.01. Alternatively, the reader might think of the alphabetic characters as a simple rank ordering of t values from "very positive" to "very negative."

The numerical value preceding the alphabetic character indicates the number of coefficients associated with the particular alphabetic character. (A blank preceding the character indicates a single observation.) For example, according to Table 1.1, the relation between homicide offenses and arrest rates was tested by Study No. 8 (Ehrlich, 1975). He estimated the arrest rate coefficient using fifteen different specifications of his basic model. Nine of the coefficients were simply negative, five were negative and significant, and one was very significant.

The data in these tables must be interpreted with care. While signs of coefficients and significance levels (in the sense used here) provide a useful basis for summarizing and evaluating the mass of data reported in these studies, a mechanistic, face-value acceptance of these data is likely to engender serious misinterpretation. Three interpretative issues are of considerable importance, and deserve particular attention:

(i) The models used in these studies vary greatly in structure and specificity, and defy simple, meaningful categorization. Some empiricists presuppose a single crime/sanction relation, in which the crime rate is alleged to depend upon one or more sanctions, as well as certain control variables. Other empiricists presuppose the existence of multiple relations, and develop models that are supposed to account for the simultaneous interaction of offenders and the criminal justice system. Here, the crime rate is a function of one or more sanctions and one or more of these sanctions are, themselves, assumed to be a function of the crime rate. While the evidence emanating from the more sophisticated, simultaneous-equation models appears to carry more authority, it is not clear, in principle, that this should be so, particularly when the evidence is based on small samples (Johnston, 1963: 275-295; Christ, 1966: 464-481; Namboodiri, et al., 1975: 517). Nonetheless, it is useful to dichotomize studies by model structure -- single equation (SE) vs. simultaneous equations (SIM) -- just as it is expedient to exclude from our survey those studies that do not employ control variables in their model.

(ii) Many of the coefficients reported in the literature are derived from the same basic data set. The extent to which these coefficients can be treated as independent observations raises complex theoretical and empirical issues whose analysis is beyond the scope of this report. However, intuition suggests that the greater the difference in model specification, the more independent the test is likely to be. Measuring "differences in model specification" is, itself, an extremely complex problem. We propose to beg the issue, and adopt a simple, but probably fairly reasonable working principle; viz., that differences in model specification existing between any two studies is likely to be greater than differences existing within a particular study. (The raison d'etre of yet another study is, after all, that it is different from its precedessors.) Thus, in Table 1.1, the agreement of the homicide coefficients of Study 8 with those of Study 9 is viewed as more significant than the agreement of the coefficients of Study 8 among themselves.

(iii) Table 1 provides no evaluation of other dimensions of research quality: whether the appropriate variables are included in, and excluded from, each of the equations of the model, whether the correct functional form has been adopted, whether the model is robust with respect to changes in model specification, whether the choice of statistical proxies for the model's theoretical variables was sound, whether the data actually measure what they purport to measure, whether the results of the analysis are presented with the care, circumspection,

and qualification that the model and data necessitate -- these and other important qualitative considerations are necessarily absent from the tables. For a full evaluation of the empirical evidence, one must return to the literature itself. In the tabular data reported below, aside from criteria relating to model structure and independence of data sets, each study's results are treated as being of equal importance.

A final comment: In the survey presented below, attention is directed to the general pattern of coefficients associated with specific sanctions. Variations within or between subsets of these coefficients shall not be analyzed. In particular, we shall not compare the coefficients associated with one offense with those of another. While such comparisons would be of considerable interest, permitting one to evaluate the hypothesis that deterrence is more effective for some offenses than for others, the evidence presented below is too meagre to warrant such comparison. As the reader shall see, empirical research has been very uneven in its coverage of UCR offenses. Attention has been directed largely to homicide and, secondarily, to one or another of the Index offense aggregates. Relatively little evidence is available concerning the effect of legal sanctions on the other individual Index offenses, certainly not enough to warrant the application of inferential statistical analysis.

1. Arrest Rates

Our literature review includes fourteen studies which examine the relation between UCR offense rates and arrest rates -- the latter defined as the ratio of the number of arrests or clearances to the number of crimes known to the police. The data from these studies is summarized in Table 1.1. All told, these studies report 198 individual arrest coefficients. The distribution of these coefficients, by sign and significance level, is given in the following tabulation. As the reader can see, the overwhelming proportion of coefficients --

Sign and Significance Level of Arrest Coefficients

Value of Coefficient	Distribution	
	Number	Percentage[a]
Positive	23	12
Very significant	1	0.5
Significant	0	0.0
Not significant	22	11.1
Negative	175	88
Not significant	83	42
Significant	29	14
Very significant	63	32
Total	198	100

[a]Detail may not add to total or subtotal due to rounding.

88 percent -- are negative.[5] Moreover, approximately one-third of all

[5]This observation is reinforced when it is realized that most of the positive coefficients derive from just two studies -- those using the "panel model" procedure. This imbalance suggests the possibility that the panel procedure may be biased toward positive values.

TABLE 1.1

EFFECT OF ARREST[a] RATES ON UCR OFFENSE RATES:
SUMMARY OF RECENT QUANTITATIVE EVIDENCE[b]

				Offense									
No.	Study Author	Data Base	Methodology	Homicide	Rape	Assault	Robbery	Burglary	Larceny	Auto	All Violent[c]	All Property[c]	All Offenses
1	Knorr (1979)	Regions & states, 1950, 1960 pool	SE	p, 3n, 3s, v	-	-	-	-	-	-	-	-	-
2	Sjoquist (1973)	Cities, 1960	SE	-	-	-	-	-	-	-	-	8v[d]	-
3	Bartel (1979)	States, 1970	SE	-	-	-	-	-	n, v	-	3n, 4s	n, 3s, 5v[e]	-
			SIM	-	-	-	-	-	-	-	2n	2n, v	-
4	Greenberg, et al. (1979)	Cities, 1964-1970	SIM[f]	p, 2n	2p, n	p, 2n	3n	3n	2p, n	p, 2n	-	-	2p, n
5	Logan (1975)	States, 1964-1968	SIM[f]	2p, 2n	4n	4n	p, 3n	3p, n	3p, n	4n	-	-	p, 3n
6	Avio & Clark (1978)	Canadian Districts, 1971	SIM	-	-	-	3n, 4s 2v	9v	9v	-	-	-	-
7	Carr-Hill & Stern (1979)	U.K. Districts	SIM	-	-	-	-	vp, 5n, s	-	-	-	-	-
8	Ehrlich (1975)	U.S., 1933-1969	SIM	9n, 5s, v	-	-	-	-	-	-	-	-	-

Table 1.1 (continued)

No.	Study Author	Data Base	Methodology	Homicide	Rape	Assault	Robbery	Burglary	Larceny	Auto	All Violent[c]	All Property[c]	All Offenses
				Offense									
9	Passell & Taylor (1977)	U.S., 1933-1969	SIM	6n, s	-	-	-	-	-	-	-	-	-
10	Brier & Fienberg (1980)	U.S., 1933-1969	SIM	p, 2n	-	-	-	-	-	-	-	-	-
11	Klein, et al. (1978)	U.S., 1933-1969	SIM	p, 4n	-	-	-	-	-	-	-	-	-
12	Vandaele[d] (1978a)	U.S., 1933-1969	SIM	-	-	-	-	-	-	s, v	-	-	-
13	Fox[g] (1978)	U.S., 1950-1974	SE	-	-	-	-	-	-	-	3s, 13v	4s, 12v	-
			SIM	-	-	-	-	-	-	-	n	n	-
14	Orsagh[h] (1981)	U.S., 1951-1977	SE	-	-	-	-	-	-	-	-	-	2n
			SIM	-	-	-	-	-	-	-	-	-	n

[a]Defined as the ratio of the number of arrests or clearances relative to the number of crimes known to the police.

[b]The cell entries are based on the ratio of the variable's coefficient to its standard error. The ratio, t, was assigned the symbol p, n, s, etc. according to the following rule:

$t \le -3.0$:	v	$t > 3.0$:	vp
$-3.0 < t \le -2.0$:	s	$3.0 > t \ge 2.0$:	sp
$-2.0 < t < 0.0$:	n	$2.0 > t \ge 0.0$:	p

[c]Robbery is excluded from All Violent offenses and included in All Property offenses except where indicated.

[d]Excludes motor vehicle theft.

[e]Two models in this group exclude larceny.

[f]Panel model

[g]Robbery included in All Violent and excluded from All Property category.

[h]Uses aggregate arrests less those resulting in incarceration relative to crimes known to the police.

coefficients are both negative and statistically very significant.

As we indicated above, many of these coefficients were generated from models that are very similar in structure and estimating procedure. Hence, the foregoing tabulation exaggerates -- and perhaps grossly exaggerates -- the true number of independent tests of the deterrence hypothesis. Hence, the application of a formal statistical test to these data cannot be justified. As a partial correction for this "near redundancy," we propose to treat all coefficients pertaining to one study and one offense as a single observation. For example, Sjoquist's eight homicide coefficients and Greenberg's three homicide coefficients each will be reduced to a single observation. In effect, this procedure computes an unweighted mean of the coefficients occurring within the intersection of an offense and an individual study. More precisely, sample coefficient b_{ij} is assumed to be related to the true coefficient, β_j by the equation $b_{ij} = \beta_j + \varepsilon_i$, wherein ε_i is a random variable with zero expectation and finite variance and j refers to the intersection of a particular offense and particular study. The b_{ij} values are obtained by testing one or more variants of a basic model, using one or more estimating procedures. The mean of b_{ij}, then, is intended to express the "average" performance of that basic model, applied to a particular data set.

To quantify the data of Table 1.1, we shall ignore significance levels and shall simply assign the values plus and minus one to each respective positive and negative coefficient. (An alternative procedure, which would have permitted a more powerful statistical

test than that employed below, would have had the coefficients ranked across the spectrum from positive to negative value, by order of significance.) Using our simple sign procedure, the values are summed, then divided by the number of coefficients contained within the observation. As an example, we note that the value assigned to Knorr's (1979) homicide study is -.75. We also note that the value of an observation is bounded by plus and minus one.

We shall test the one-sided null hypothesis that there is either no association or that there is a positive association between the arrest rate and the offense rate. The alternative hypothesis is that the relation is negative. The mean and standard deviation obtained from the sample data are -.618 and .518, respectively. The _t_-statistic, estimated with 32 degrees of freedom, is -6.85, and is, of course, highly significant. If these 33 observations are independent, we may conclude that a negative association exists between arrest and offense rates -- at least within the environments from which these data have been drawn. If, in addition, the incapacitation effect associated with arrest is negligible, we would regard these results as very strong evidence that arrest acts as a deterrent for UCR offenders.

2. Incarceration rates

Our literature survey includes twelve studies which examine the relation between UCR offense rates and the incarceration rate -- the latter defined as the ratio of the number of prison admissions for a particular UCR offense to the number of these offenses known to the police. The data relating to the incarceration rate coefficients are summarized in Table 1.2. Three of the studies cited in the table used more than one data set. We have chosen to treat these individual data sets as independent events, thereby increasing the effective number of studies to sixteen. The data of Table 1.2 refer to 442 individual incarceration coefficients. The distribution of these coefficients, by sign and significance level, is presented in the following tabulation:

Sign and Significance Level of Incarceration Coefficients

Value of Coefficient	Distribution Number	Percentage[a]
Positive	24	5
Very significant	0	0
Significant	0	0
Not significant	24	5
Negative	418	95
Not significant	240	54
Significant	89	20
Very significant	89	20
Total	442	100

[a]Detail may not add to total or subtotal due to rounding.

Almost all of the coefficients -- fully 95 percent -- are negative. Over half of all coefficients are both negative and very significant.

TABLE 1.2

EFFECT OF INCARCERATION RATES[a] ON UCR OFFENSE RATES: SUMMARY OF RECENT QUANTITATIVE EVIDENCE[b]

No.	Study Author		Data Base	Methodology	Homicide	Rape	Assault	Robbery	Burglary	Larceny	Auto	All Violent[c]	All Property[c]	All Offenses
1	Ehrlich (1973)	(a)	States 1940 & 1950	SE	s	-	2s	2s	2s	2s	-	2s	2s	-
		(b)	States 1960	SE SIM	s s,v	s 2v	s 2v	s 2v	s 2v	s 2s	s 2v	s v	s v	-
2	Ehrlich (1977)	(a)	States 1940	SE	2s,11v	-	-	-	-	-	-	-	-	-
		(b)	States 1950	SE SIM	3n,2s, 10v 2v	-	-	v 2v	v 2v	-	-	-	-	-
		(c)	States 1940, 1950 pool	SE	8v	-	-	-	-	-	-	-	-	-
3	Black & Orsagh (1978)	(a)	States 1950	SE SIM	7s 2p,n	-	-	-	-	-	-	-	-	-
		(b)	States 1960	SE SIM	7s s,v	-	-	-	-	-	-	-	-	-
4	Vandaele (1978b)		States 1960	SE SIM	4s 3p,16n, 7s	4v 8n,s, 17v	4v 6p,n, 16v	4v 3n,6s 17v	4v 4s,22v	2n,2s 19n,6s, v	4v 4n,22v	2v 3n,6s, 15v	2v 24v	4v 2n,24v
5	Nagin (1978b)		States 1960	SIM	-	-	-	-	-	-	-	-	-	5p,2n, s
6	Loftin (1980)		States 1960	SE	5n	-	-	-	-	-	-	-	-	-
7	Forst, (1977)		States 1960-70 difference	SE SIM	7n n,v	- -	- -	- -	- -	- -	- -	- -	- -	- -

Table 1.2 (Continued)

No.	Study Author	Data Base	Methodology	Homicide	Rape	Assault	Robbery	Burglary	Larceny	Auto	All Violent[c]	All Property[c]	All Offenses
8	Forst, (1976)	States 1970	SIM	-	-	-	-	-	-	-	-	-	n
9	Wadycki & Balkin (1979)	States 1970	SIM	-	-	-	-	-	-	-	-	-	s,v
10	Brier & Fienberg (1980)	States 1970	SIM	-	-	-	-	-	-	-	-	-	3p
11	Bartel (1979)	States 1970	SE	-	-	-	-	-	2n	-	3p,3n	3n,5s, v	-
			SIM	-	-	-	-	-	-	-	2p	2s	-
12	Orsagh (1981)	U.S. 1951-1977	SE	-	-	-	-	-	-	-	-	-	2n
			SIM	-	-	-	-	-	-	-	-	-	n

[a]Defined as the ratio of number of first admissions for a particular UCR offense to number of these offenses known to the police.

[b]See footnote b, Table 1.1.

[c]See footnote c, Table 1.1.

As the reader will note, however, these results are dominated by Vandaele's data in Study no. 4 which account for over half of the values appearing in the table. Although the pattern of coefficients obtained when this study's results are omitted remains much the same, the study's dominance points up the hazard involved in interpreting data that are not altogether independent. The evaluation technique outlined in the preceding section of this report is specifically designed to neutralize this kind of dominance. By application of this technique, which yields 43 synthetic observations, we substantially reduce this study's influence on the overall analysis. From the sample we obtain a mean, standard deviation, and t-statistic of -.871, .400, and -14.4, respectively. If we accept the assumption that these 43 observations are independent events, we may assert the existence of a relation between incarceration rates and UCR offense rates -- at least within the environments from which the sample data were derived. Assuming, in addition, that the incapacitation effect is negligible, this assertion is tantamount to the conclusion that incarceration acts as a deterrent to UCR offenders.

3. Length of Incarceration

Our literature survey includes fourteen studies which examine the relation between UCR offense rates and the length of incarceration --

the latter defined as the average time served by UCR offenders.[6] Some studies involve more than one data set. Each of the individual data sets are treated separately, resulting in an additional four "studies." The pertinent coefficient data are presented in Table 1.3. These data refer to a total of 487 coefficients. The distribution of these coefficients, by sign and level of significance, is given in the following tabulation:

Sign and Significance Level of Sentence Length Coefficients

Value of Coefficient	Distribution: Number	Distribution: Percentage
Positive	47	10
Very significant	0	0
Significant	0	0
Not significant	47	10
Negative	440	90
Not significant	236	48
Significant	102	21
Very significant	102	21
Total	487	100

As was true of the two preceding sanctions, we observe that the overwhelming proportion -- 90 percent -- of all coefficients are negative. Moreover, one fifth of all coefficients are both negative and very significant. It should also be observed that, as was true of

[6] Except for Avio and Clark, time served is measured ex post: it is time actually served to first release. Avio and Clark use an ex ante measure; viz., the time that a newly incarcerated offender can expect to serve.

TABLE 1.3

EFFECT OF SENTENCE LENGTH[a] ON UCR OFFENSE RATES:
SUMMARY OF RECENT QUANTITATIVE EVIDENCE[b]

	Study		Data Base	Methodology	Homicide	Rape	Assault	Robbery	Burglary	Larceny	Auto	All Violent[c]	All Property[c]	All Offenses
					Offense									
1	Ehrlich (1973)	(a)	States 1940 & 1950	SE	s	-	2s	n,s	2s	n,s	-	2s	s	-
		(b)	States 1960	SE	n	n	n	n	s	s	n	s	s	-
				SIM	2n	2s	2s	2n	2v	2n	2n	-	v	v
2	Ehrlich (1977)	(a)	States 1940	SE	6n,5s, 2v	-	-	-	-	-	-	-	-	-
		(b)	States 1950	SE	3n,6s, 6v	-	s	s	-	-	-	-	-	-
				SIM	s,v	-	2s	2s	-	-	-	-	-	-
		(c)	States 1940, 1950, pooled	SE	2n,6v	-	-	-	-	-	-	-	-	-
3	Black and Orsagh (1978)	(a)	States 1950	SE	2p,4n, s	-	-	-	-	-	-	-	-	-
				SIM	2p	-	-	-	-	-	-	-	-	-
		(b)	States 1960	SE	n,6s	-	-	-	-	-	-	-	-	-
				SIM	3s	-	-	-	-	-	-	-	-	-
4	Vandaele (1978b)		States 1960	SE	3n,s	p,3n	4n	p,3n	s,3v	3n,s	4n	2s,v	3v	s,3v
				SIM	11p,14n, s	p,19n, 6s	6p,3n, 14s	23n, 3s	6s, 20v	24n, 2s	25n, v	10n,3s, 11v	n, 23v	3n,6s, 17v
5	Nagin (1978b)		States 1960	SIM	-	-	-	-	-	-	-	-	-	6p,2n
6	Loftin (1980)		States 1960	SE	p,3n, s	-	-	-	-	-	-	-	-	-
7	Sjoquist (1973)		Cities 1960	SE	-	-	-	-	-	-	-	-	-	9n,s[d]
8	Forst (1977)		States 1960-70 difference	SE	p	-	-	-	-	-	-	-	-	-
				SIM	p	-	-	-	-	-	-	-	-	-

TABLE 1.3 (Continued)

	Study	Data Base	Methodology	Offense: Homicide	Rape	Assault	Robbery	Burglary	Larceny	Auto	All Violent[c]	All Property[c]	All Offenses
9	Forst (1976)	States 1970	SIM	-	-	-	-	-	-	-	-	-	p
10	Wadycki and Balkin (1979)	States 1970	SIM	-	-	-	-	-	-	-	-	-	s,v
11	Bartel (1979)	States 1970	SE SIM	- -	- -	- -	- -	- -	- -	- -	6n 2n	p,7n[e] n,s	- -
12	Brier & Fienberg (1980)	States 1970	SIM	-	-	-	-	-	-	-	-	-	p,2n
13	Avio and Clark (1978)	Canada districts 1971	SIM	-	-	-	9n	2p,7n	9p	-	-	-	-
14	Swimmer[f] (1974)	Cities 1960	SE SIM	n 2n	n 2n	s n,s	n 2n	s 2s	n 2n	n n	- -	- -	- -

[a]Defined by Avio and Clark as sentence received upon admission to prison; by all others as time served to first release.

[b]See Footnote b, Table 1.1.

[c]See footnote c, Table 1.1.

[d]See footnote d, Table 1.1.

[e]See footnote e, Table 1.1.

[f]The sanction is expected sentence length; i.e., the probability of being sanctioned times the average sentence served to first release.

incarceration rate data, fully half of the coefficients derive from one study, that of Vandaele. Again, we apply the weighting scheme described above, treat the individual coefficients within the intersection of one offense and one study (using the expanded definition of the latter) as equally valid, unbiased estimators of the true coefficient for that study and that offense, and obtain thereby a sample of 55 synthetic observations. The mean, standard deviation, and t-statistic derived from this sample are -.781, .526, and -11.0, respectively. Assuming that the observations represent independent events, we are led to reject the null hypothesis of no association between UCR offense rates and the length of incarceration. Assuming, in addition, that the incapacitation effect ascribable to incarceration is negligible, we would conclude that the length of prison sentence acts as a deterrent to UCR offenders.

4. Conditional Probability of Incarceration

Our literature survey includes ten studies which examine the relation between UCR offense rates and the conditional probability of incarceration -- the latter defined as the ratio of the number of prison admissions for a particular UCR offense to the number of arrests or clearances for that offense.[7] The data representing the coefficients of

[7]The Avio and Clark study uses the number of convictions, rather than the number of incarcerations in the numerator of the ratio.

this sanction variable are presented in Table 1.4. These data involve 104 individual conditional probability coefficients. The distribution of these coefficients, by sign and significance level, is presented in the following tabulation:

Sign and Significance Level for Conditional Probability Coefficients

Value of Coefficient	Distribution Number	Distribution Percentage
Positive	29	28
Very significant	0	0
Significant	0	0
Not significant	29	28
Negative	75	72
Not significant	41	39
Significant	13	13
Very significant	21	20
Total	104	100

Three-quarters of the coefficients are negative, and about a fifth of all coefficients are both negative and very significant. By application of the evaluation technique described above, these 104 coefficients were reduced to 14 synthetic observations. These observations, in turn, yield a mean, standard deviation, and t-statistic of -.496, .692, and -2.68, respectively. If one adopts the one-sided null hypothesis, and a 0.01 level of significance, one is just able to reject the null hypothesis (prob. $(t \leq -2.650) = .01$); d.f. = 13). Thus, the evidence suggests that an association exists between the conditional probability

TABLE 1.4

EFFECT OF INCARCERATION/ARREST RATIO[a] ON UCR OFFENSE RATES:

SUMMARY OF RECENT QUANTITATIVE EVIDENCE[b]

No.	Study Author	Data Base	Method-logy	Homicide	Rape	Assault	Robbery	Burglary	Larceny	Auto	All Violent[c]	All Property[c]	All Offenses
1	Knorr (1979)	Regions, States 1950, 1960 pool	SE	6p,n	-	-	-	-	-	-	-	-	-
2	Sjoquist (1973)	Cities 1960	SE	-	-	-	-	-	-	-	-	2n[d]	-
3	Bartel (1979)	States 1970	SE	-	-	-	-	-	2n	-	3p,3n	3n,5s, v[e]	-
			SIM	-	-	-	-	-	-	-	2p	2s	-
4	Avio & Clark[f] (1978)	Canadian districts 1971	SIM	-	-	-	9n	2p,7n	9p	-	-	-	-
5	Carr-Hill & Stern (1979)	U.K., districts	SIM	-	-	-	-	4p,3n	-	-	-	-	-
6	Ehrlich (1975)	U.S., 1933-1969	SIM	n,s 13v	-	-	-	-	-	-	-	-	-
7	Passell & Taylor (1977)	U.S. 1933-1969	SIM	n,2s, 4v	-	-	-	-	-	-	-	-	-
8	Klein, et.al. (1978)	U.S. 1933-1969	SIM	n,3s, v	-	-	-	-	-	-	-	-	-
9	Brier & Fienberg (1980)	U.S. 1933-1969	SIM	3p,8n, v	-	-	-	-	-	-	-	-	-
10	Vandaele (1978a)	U.S. 1933-1969	SIM	-	-	-	-	-	-	v	-	-	-

[a]Defined as the number of incarcerations relative to the number of arrests or clearances.

[b]See footnote b, Table 1.1.

[c]See footnote c, Table 1.1.

[d]See footnote d, Table 1.1.

[e]See footnote e, Table 1.1.

[f]The data refer to the ratio of convictions to charges. While the denominator should approximate the number of arrests, the numerator is more encompassing than the number of incarcerations.

of incarceration and the UCR offense rate, but the strength of this evidence is substantially weaker than that relating to the other three sanctions variables.

5. Summary and Conclusions Concerning the Deterrence Literature

All four sanctions have been shown to be inversely related to one or another of the seven Index offenses. The degree of association is so strong, at least with respect to the first three sanctions, that chance variation can be ruled out as a source of the inverse relation. Assuming that the incapacitation effect does not have a significant effect on this relation,[8] and that the observations used in the statistical tests reported above are statistically independent, the logical inference from this survey and analysis would be that these sanctions -- certainly the first three -- act as a deterrent to UCR offenders.

Despite the strength of the evidence, the hypothesis that these sanctions deter is subject to several important qualifications:

(i) The statistical test that was used to establish the existence of an inverse relation is but one of many possible tests that could have been conducted. Its selection was based on compromise. On the one hand, a more restrictive assumption concerning the statistical independence

[8] We shall argue below that there is good reason to believe that the incapacitation effect is, indeed, negligible.

of the coefficients could have been adopted. For example, instead of assuming that the individual property offense coefficients were independent of each other, one might have broadened the definition of the synthetic observation to include all coefficients within the intersection of a particular study and all property offenses. As another example, one might have combined all studies using a common data set into a single observation -- e.g., all studies based on 1960 state data. Had one or another of these more restrictive assumptions been adopted, the foregoing statistical tests would have been conducted with substantially fewer degrees of freedom, and the statistical significance of the inverse relation might not have been established.

On the other hand, one might have adopted an alternative procedure for quantifying the coefficient data that underlie Tables 1.1 to 1.4. Among many possibilities, one might have ranked the coefficients from most positive to most negative, or one might have simply used the t-statistic associated with each coefficient as an index. Our subjective impression is that these more discriminating alternatives would have generated stronger support for the deterrence hypothesis than the dichotomous valuation procedure actually used.

The objective of this literature survey was not to provide a thorough, comprehensive statistical analysis of past research findings, however. Rather, it was simply to show that this body of research can be interpreted, with good justification, as providing strong evidence that the sanctions of arrest and incarceration exert a deterrent force on individuals contemplating the commission of an Index offense.

(ii) Most of the sample data were drawn from the United States, and refer to the years 1950 to 1970 and to large geographical units -- cities, states, or the entire United States. In view of our early cautionary remarks concerning the conclusions that may, and may not, be drawn from the more general theoretical model, we must leave open the possibility that analyses similar to those which have been reviewed, but which use data drawn from different environments -- e.g., from smaller geographical units, such as counties, situated within one of these larger units, or from a sample of inner cities -- might not yield results that support the deterrence hypothesis.

(iii) Fisher and Nagin (1978) have argued that some of the studies which we have reviewed have failed to solve the identification problem even though they resort to multiple-equation systems in deriving their empirical coefficients. According to the authors, the failure comes about because important variables were excluded from the equation used to explain the offense rate, and has, as its consequence, the potential for imparting serious bias in the estimated coefficients. In principle, if one's theory is correct and complete, this omission-of-variables problem would not arise. The fact that the problem does arise can be attributed in large measure to the practical difficulty in empirical research of finding appropriate statistical surrogates for one's theoretical variables. Extremely important in this regard is the omission of informal sanctions and the incapacitation effect from the studies that have been reviewed. The magnitude of the bias introduced

by this omission is not known, but could be substantial.

(iv) Nagin (1978b) has shown that the use of crime rates and sanctions <u>rates</u>, coupled with large measurement error in the number of crimes reported, can produce a substantial, but spurious, inverse relation between crime rates and sanctions. This criticism is particularly directed at the studies appearing in Tables 1.1 and 1.2.

(v) Every one of the studies reviewed can be faulted for measurement error ascribable to the use of inappropriate statistical measures. To illustrate:

(a) Strictly speaking, a proper test of the deterrence hypothesis requires estimates of the level of sanctions perceived by potential offenders (Gibbs, 1975). Because these perceptual data do not exist, "objective" sanctions have been used in the studies reviewed. Moreover, the objective sanctions are obtained from a subset of the population of actual offenders and may not be representative of the sanctions that would have been imposed on those offenders who successfully avoided being sanctioned, or the sanctions that would have been imposed on those potential offenders who opted not to commit an offense.

(b) A proper test of the deterrent effect of incarceration requires an estimate of the offense rate for those who are eligible for incarceration and a probability of being sanctioned which applies only to that population of eligibles. Relating the probability of imprisonment -- an adult sanction -- to a combined juvenile and adult offense rate violates this requirement. Given the present extraordinary contribution of juveniles to the crime rate, this violation is especially significant.

(c) A proper test of the deterrent effect of the length of incarceration requires that one estimate the length of incarceration that potential offenders expect to receive. Instead, the studies which we have reviewed, with the exception of Avio and Clark, use estimators which refer to past experience, viz. the length of incarceration experienced by offenders just released from prison.

(d) It is essential that one neutralize, or correct for, the effect of the offender's economic status and of his expected rate of return to illegitimate activity. Accordingly, one requires indicators that closely approximate the status of potential offenders and the illegitimate returns available to them. The more careful deterrence studies recognize this requirement; but, data deficiencies being what they are, these studies seem compelled to rely on broad aggregates such as the overall male unemployment rate and median family income. Neither variable may approximate the economic status experienced by, or the returns available to, potential offenders.

(e) At the basis of all deterrence research -- natural variation, experimental, or what have you -- is the crucial, but altogether unrealistic premise that one can detect or observe a non-event. In particular, we presume to observe, admittedly indirectly, a potential offender who decides not to offend because the expected consequence of offending is worse than the consequence of not offending (Gibbs, 1975).

One might easily extend this list of deficiencies in the research literature. The motivation for presenting this litany, however, is not to provide a complete enumeration, but to identify the more important and troublesome issues in the research. Nor do we pretend that we can repair all of these deficiencies. Gibb's philosophical point is fundamental and unanswerable. Nagin's systematic measurement error may be avoided by resorting to models that use numbers of offenses, arrests, etc. rather than rates; but that strategy has its own difficulties, as we shall argue below. However, we do believe that some of the difficulties cited above can be resolved through the conjunction of better modelling and a better data base. The present research is motivated by the desire to produce that better model and to exploit a rich data base that has only recently become available for empirical research.

C. RESEARCH OBJECTIVES

In this research we propose to develop a theoretical model which explains offense rates as part of an interacting system of relations. The model is specifically designed to test for the existence of a deterrent effect. The model considers the seven Uniform Crime Report Index offenses and four deterrence instruments: (i) the probability of being arrested, given that the arrestee is not incarcerated, (ii) the probability of being incarcerated, (iii) the length of incarceration, and (iv) the length of post-prison probation.

The empirical version of the theoretical model provides another test of the deterrence hypothesis. By developing and analyzing another, independent sample of observations on UCR offenses and UCR offenders, the test augments our knowledge of the crime/sanction relation. Moreover, the test will be directed at all seven of the Index offenses. As our literature survey revealed, most prior research has been concerned either with homicide or with one or another of the offense aggregates. Relatively little information is available concerning the deterrent effect of sanctions on the other Index offenses.

The data utilized in this research have the advantage of being drawn from a different environment than that used by the studies reviewed above. The population is that of an individual state, the unit of observation is a judicial district. Judicial districts are smaller, on the average, and likely to be more homogeneous than the observational units used in most studies. Hence, this research extends the range of environments within which the deterrence hypothesis has been tested.

The model which is developed and estimated in this report builds upon and extends past deterrence research. In so doing, every effort is made to incorporate into the model those demographic and socioeconomic control variables which have been shown to maintain a consistent and theoretically plausible association with offense rates. By so doing, we hope to minimize the likelihood of producing seriously biased regression results through the omission of some crucial "factor X."

The empirical model has three desirable design characteristics:

(i) The data available for this research permit time served for individual UCR offenses to be explained by a combination of individual offender and community-level characteristics. Because of their unusual detail, these data possess the special advantage that the explanatory system can be fashioned to test the hypothesis that sentence length is, itself, dependent on the offense rate. We believe that this treatment of sentence length, as an endogenous variable, embedded in a large criminal justice model, is unique.

(ii) Because the model uses within-state, cross-sectional data, prison capacity is a constant. Thus, we avoid Nagin's (1978a) trenchant criticism of past deterrence research, and meet his requirement that one explicitly account for, or neutralize, the effects of this variable on the deterrence estimators.

(iii) One of our data sets permits an evaluation of the judiciary's split-sentencing option. Two analyses are of particular interest. First, it is possible to compare the effectiveness of in-prison time served as a crime-control instrument with that of post-release probation time. Second, it is possible to measure the court-established trade-off (marginal rate of substitution) between these two sanctions and to do this by offense type.

Finally, the empirical model strives to minimize systematic error and random measurement error, thereby minimizing bias and estimator variance, through the use of statistical surrogates that more nearly approximate their theoretical counterparts. We believe that the general quality of the data used in this study is at least as good as that used in other deterrence research. Many of our statistical variables are derived from the same sources used in these other studies and are, therefore, of comparable quality. The data that emanate from less conventional sources tend to be better, in part because they are based on individual observations. With individual observations, we are able to develop sanctions estimates for very small geographical regions, to develop a superior, ex ante sentence length measure, to endogenize the sentence length variable, to develop a quite different proxy for legitimate employment opportunities available to potential offenders, and, finally, to develop a unique measure for evaluating the effect of incapacitation on the offense rate.

CHAPTER 2

THE THEORETICAL MODEL

A. NOTATION

In the following, lower case names refer to observations on individual incarcerated offenders, upper case to regional averages of offender and other data.

Symbol	Theoretical Variable	Statistical Counterpart
CRM, CRM_{-1}, CRM_W	The offense rate, its value lagged, and an offense aggregate weighted by severity of the individual offenses	Per capita number of offenses known to the police
AR	Probability of being arrested, given that an offense was committed	Arrests relative to crimes known to the police
AR_I (AR_{NI})	Probability of being arrested and incarcerated (of being arrested and not incarcerated), given that an offense was committed	Incarcerations (arrests less incarcerations) relative to crimes known to the police
PI	Probability of being incarcerated given that one has been arrested	Incarcerations relative to arrests
SL, PBTN, sl, pbtn	The severity of the sanction of incarceration: length of incarceration (SL) and length of post-release probation (PBTN)	SL: Expected term of incarceration to be served by adults at time of admission. PBTN: Expected term of probation upon release from incarceration
P15-29, P15-19, NW, EXCON	Population subsets having differentially greater "tastes" for criminal activity and/or receiving differential treatment by the CJS	P15-29, P15-19: Proportion of the population of age 15-29 and 15-19, respectively. NW: Proportion of non-whites in population EXCON: Offenders released from incarceration relative to the non-incarcerated population

Symbol	Theoretical Variable	Possible Statistical Counterpart
EMPLOY	Labor force status indicator	Offender-based employment index based on scores such as: 3: Employed full-time 2: Employed part-time or has been unemployed for a short time 1: Unemployed for long time or has never worked (but is capable of working)
INCOME	Index of illegitimate income opportunities and of community's ability to buy law enforcement services	Per capita income of families and unrelated individuals
COP	Public security services	Per capita number of full-time equivalent police and sheriffs, sworn and civilian
REV	Indicator of a community's ability to buy crime prevention services	Per capita local government revenues
SIZE	An index of the size of the community in which the typical individual resides	The index for the ith observation is: $SIZE_i = \sum^n (c_{ij}p_{ij})/\sum^n p_{ij}$, wherein c, the size of the community, is weighted by p, the number of persons living in that community, there being n communities in all in a given district
score, SCORE	Index of severity of present offense(s)	Sum of severity for all offenses for which offender was sentenced to present incarceration (uses Georgia Dept. of Correction scores)
prior, PRIOR	Index of severity of past offense(s)	Same as above, but for prior offenses; or number of prior convictions
age, sex, nw	Demographic attributes of incarcerated offenders	Offender's age, sex, and race

Symbol	Theoretical Variable	Possible Statistical Counterpart
MV	Index of demand for police services for traffic supervision	Per capita number of motor vehicle registrations

B. THE BASIC THEORETICAL MODEL

$$CRM = CRM(AR_{NI}, AR_I, SL, PBTN, SIZE, P15\text{-}29, NW, EMPLOY, INCOME) \quad (2.1)$$

$$AR = AR_{NI} + AR_I \quad (2.2)$$

$$AR = AR(CRM, COP, P15\text{-}19, NW, SIZE) \quad (2.3)$$

$$AR_I = AR(CRM, COP, P15\text{-}19, NW) \quad (2.4)$$

$$SL = SL(CRM, AR_I, score, prior, age, sex, nw, pbtn) \quad (2.5)$$

$$COP = COP(CRM_w, REV, INCOME, MV) \quad (2.6)$$

The model focuses on offense rates rather than numbers of offenses. From a theoretical point of view, the choice between rates and numbers is a matter of indifference. From a statistical viewpoint, however, rates are preferred. A model whose variables are expressed in numbers, rather than rates, is likely to encounter serious problems of multicollinearity. (The number of offenses, of course, may be derived from the model by simply multiplying the offense rate by its corresponding population aggregate.)

1. The Principal Equation

The first equation of the model explains the offense rate. Embedded in the equation are four explanatory variables whose function it is to assess the existence of, and magnitude of, the deterrent effect of legal sanctions.

This equation is, therefore, of central interest for this study. The three deterrence instruments discussed above -- the probability of arrest and incarceration and the length of incarceration -- are represented in the model by AR_{NI}, AR_I, and SL. One of the data sets to be used provides information on a fourth deterrence variable, post-incarceration probation. PBTN appears in the model to appraise the significance of this component of the sentence variable. Thus, sentence severity will be treated as a two-dimensional variable at times.

The rational choice model justifies the inclusion of the nonwhite and youth variables in the equation. Nonwhites -- practically equivalent to blacks in this study -- and young persons have lower incomes and, therefore, experience greater relative gains from successful criminal activity; while, on the other hand, they experience a smaller loss in earnings from being apprehended and sanctioned because of unsuccessful criminal activity. The social class hypothesis (Miller, 1958; Bancroft, 1968) provides additional support for the introduction of the nonwhite variable, and Mertonian strain theory for both the nonwhite and youth variables (Cloward and Ohlin, 1960). Empirical evidence overwhelmingly supports the contention that younger persons are more criminogenic, at least with respect to Index offenses (Sutherland and Cressey, 1978: 124-130),[1] and offers strong support for the existence of higher black offense rates (as evidentiary examples we offer: NCJISS, 1977 for burglary; Vandaele, 1978a for auto-theft; and, more generally, Ehrlich, 1973; Elliot and Ageton, 1980; Hindelang, 1978; Orsagh, 1981; Renshaw, et al., 1978). However, studies which consider the

[1]The peak age for arrests for the seven Index offenses is between 15 and 20 (Greenwood, et al., 1980).

confounding influence of socioeconomic status, age, and urbanization sometimes report no statistically significant "race" effect (Bartel, 1979; Swimmer, 1974; Wadycki and Balkin, 1980).

The rational choice model has also been used to justify the inclusion of an employment variable in the crime equation (Becker, 1968; Ehrlich, 1973; Sjoquist, 1973). However, theoretical extensions of the rational choice model by Block and Heineke (1975) and Heineke (1978) have shown that the employment-crime relation is indeterminate; that, for example, improved employment opportunity may simply induce a transfer from leisure to legitimate activity with no corresponding reduction in criminal activity. The empirical evidence concerning the unemployment-crime relation is equally inconclusive. (See the surveys of this literature by Braithwaite, 1978; Gillespie, 1975; and Orsagh, 1981). Indeed, the evidence is so confused that the latter is led to conclude that "the effect of unemployment on crime rates is minimal at best."

Community size is included because of the well-known, often reported association between urbanization and five of the Index offenses (the exceptions are rape and, possibly, homicide). Explanations for this phenomenon emphasize variables that are largely sociological in nature: "extensive conflicts of norms and values, rapid social change, increased mobility of the population, emphasis on material goods and individualism and an increase in the use of formal rather than informal social controls." (Clinard and Abbott, 1973: 85) In addition, Boggs (1965) suggests that urbanization may be a determinant of the extent of exposure of offenders and victims to each other or, in the context of the rationality model, of the search costs associated with criminal activity.

The basic equation also contains a theoretical variable to represent the expected gains from crime. (Search costs would, of course, be a component of this variable.) Earlier versions of the rational choice model deduced the existence of a positive relation between offenses and expected gains: where the potential rewards from illegitimate activity are higher, potential offenders will be more likely to commit a criminal act. The driving force is economic gain. Hence crimes against property (including robbery) are to be explained by this variable. Its linkage to rape, assault, and homicide would be tenuous at best, notwithstanding the fact that some homicide is motivated by a desire for economic gain. Heineke (1978) has shown, however, that there is not even a necessary linkage between economic gain and property crime if one adopts a more general and more realistic model of the criminal choice. In the more general model, neither the rewards from legitimate labor nor the economic payoff from crime unambiguously influences the criminal choice.

Empiricists are equally ambiguous in their interpretation of the "gains" variable. Gains are usually indexed by per capita income, or an analogous variable such as the manufacturing wage rate. Although many empiricists have chosen such a variable to represent the potential payoff from crime (Reynolds, 1971; Ehrlich, 1973, 1975a; McPheters and Stronge, 1974; Forst, 1976, 1977), these writers have not demonstrated that their variable measures what it pretends to measure. It is difficult to see, for example, how the highly aggregative, average income measure found in these models

can represent the atypical economic circumstances that very likely characterize potential offenders, or can be relevant for their economic calculus (Orsagh, 1981). More significantly, many empiricists have interpreted these same measures as reflecting not the attractiveness of criminal activity -- more income, more crime -- but as a reflection of the unattractiveness of legitimate activity -- less income, more crime (Fleisher, 1966; Weicher, 1970; Grieson, 1972; Sjoquist, 1973; Beasely and Antunes, 1974; Swimmer, 1975, Witte, 1980).

The ambiguity in the empirical representation of the gains variable has deeper implications for the rational choice model. Income undoubtedly covaries with important sociological determinants of the crime rate. For example -- and this is simply proferred as one example -- it is probably true that the pace of socioeconomic change is greater where per capita income (or the wage rate) is higher. This is certainly exemplified in the contrast between urban and rural environments. The ultimate consequence of rapid change is, of course, a diminished propensity to conform to traditional, lawful standards of behavior on the part of those experiencing such change. Conversely, one anticipates a diminution in the extent and intensity of the informal response to deviant behavior, emanating from family, neighborhood, and community institutions, by lawful members of the society who, themselves, have experienced such change. Thus, according to this view, per capita income covaries with the "taste" for deviant and criminal behavior across communities, and also with the informal societal response to deviant and criminal behavior. Consequently, the influence of these sociological factors on the crime rate will find its expression in a positive relation between income and the crime rate. In short, income is deficient as a gains variable because it captures too many confounding forces within its net.

Nevertheless, the fact remains that the income variable, whatever it measures, is often found to be statistically significantly related to the offense rate and, therefore, appears to qualify as a bona fide independent variable. We feel compelled to treat it as such. But how shall it be interpreted? If EMPLOY and NW perform as expected, they ought to provide indices of legitimate income opportunities. If, in addition, the coefficient of INCOME is significantly positive, we could then accept Ehrlich's (1973) view that INCOME represents the potential returns to illegitimate activity. However, we insert, and insist upon, the important caveat that in such an eventuality, we shall not have demonstrated this to be true; that, indeed, variation in the potential rewards to criminal activity may have little to do with the crime rate; that, in fact, a positive coefficient for the INCOME variable may just as likely signify that the foregoing sociological variables have had a significant effect on the crime rate.

2. Variations in Equation (2.1)

The foregoing variables comprise and define the basic equation. Together with Equation (2)-(6), they constitute the basic model. We propose to examine three variants of the basic model, derived from three modifications of Equation (1). These are:

Variant One: The Effect of Incapacitation

To appraise the hypothesis that imprisonment reduces the crime rate by incapacitating potential offenders, we shall introduce the variable EXCON into the basic equation. EXCON is defined as the ratio of the number of offenders recently reintegrated into the general population relative to the number of persons in the general population. If these ex-offenders are similar in their criminal proclivities to persons still incarcerated, the coefficient of EXCON should approximate the offense rate that would be obtained for those offenders still incarcerated, were they released into the general population. Two measures of the incapacitation effect may then be obtained: (i) The coefficient provides a direct measure in itself. It implies that, by virtue of their imprisonment, these persons were prevented from committing the entire number of offenses given by the coefficient. (ii) An alternative measure would be to subtract from this coefficient the coefficient of the offense rate ascribable to the general population. The resulting measure would be an indicator of incapacitation's "marginal" contribution to the crime rate. It would provide an alternative interpretation of the reduction in the offense rate ascribable to incarceration.

Because the quantitative evidence to date agrees "that the present incapacitation effect of prison is minimal" (Cohen, 1978), one does not expect either measure to indicate the existence of a substantial incapacitation effect. Nor does one expect the introduction of EXCON into the basic equation to have a significant effect on the estimates relating to the deterrent effect of sanctions.

Variant Two: The Conditional Probability of Incarceration

Following Ehrlich (1973), most writers who have used arrest and incarceration data have defined the probabilities of arrest and incarceration as, respectively, the ratio of the number of arrests to number of offenses and the ratio of the number of incarcerations to the number of arrests. While this formulation may possess theoretical and mathematical appeal, it cannot claim empirical validity. The question is, how does a potential offender actually view the risk of being sanctioned? Does he think: "If I do this, I'll either get away with it, get caught and get off lightly, or get caught and go to prison." Or does he think: "If I do this, I might get caught. If I get caught, I might go to prison." The latter thought process presumes much more sophisticated reasoning on the part of the potential offender. He cannot simply allocate probabilities (1/3, 1/3, 1/3; 80, 20, zero; etc.) as he implicitly does in the former process. Rather, he has to consider the risk of getting caught (i.e., arrested), with its probability -- 50, 50; 80, 20; etc. -- and then he must consider the risk of going to prison, assuming that he has been caught -- 50, 50; 70, 30; or what have you. We find it difficult to believe that potential offenders engage in such a process. We find it particularly difficult to believe that potential offenders would think in marginalist terms, as the last of the calculations requires. Nevertheless, because we cannot, ourselves, demonstrate that our view of reality is more correct, and because we cannot reject the possibility that this alternative view has validity, we propose to consider a variant of the basic model in which AR and PI are substituted for AR_{NI} and AR_I.

Variant Three: Sanctions With A Distributed Lag Effect

The basic model assumes that this year's crime rate is only influenced by this year's sanctions, not those of prior years. An alternative assumption is that sanctions exercise their effect on the crime rate over a period of years. In particular, letting S signify one or more of the sanctions under consideration, we might specify a general linear relation of the form:

$$CRM_t = \beta S_t + \beta_1 S_{t-1} + \beta_2 S_{t-2} + \cdots \qquad (2.7)$$

Becoming less general, let us assume that sanctions have a distributed lag effect. That is, assume that an immediate sanction has a greater effect on the crime rate than a sanction meted out some time ago. To be more specific, let us assume that the present effect of a sanction meted out i periods ago is:

$$\beta_i = \beta\lambda^i \quad , \qquad 0 < \lambda < 1 \qquad (2.8)$$

Now let Equation (8) be placed in (7), forming an equation, E*. Let E* then be lagged one period and then be multiplied through by λ. Let the resulting equation then be subtracted from E*. When this is accomplished, one obtains the quite simple expression

$$CRM_t = \beta S_t + \lambda CRM_{t-1} \qquad (2.9)$$

Thus, the lagged value of the offense rate appears in Equation(9) -- and could appear in Equation (1) -- as a direct result of the distributed lag assumption. This variant of the basic model consists, therefore, of the addition of CRM, lagged one year, to the set of regressors in the principal equation.

3. The Model's Other Equations

The research focuses on Equation (1). Equations (2)-(6) appear in the model to assure the empirical identifiability of the principal equation. They simply provide the instrumental variables and the wider context that are necessary to assure the consistency of the empirical estimates to be derived for Equation (1). Because the individual specification and estimation of these equations are peripheral to the estimation of Equation (1) and to the evaluation of the deterrence hypothesis, discussion of their role in the model may be confined to the following brief remarks.

Equations (2.2)-(2.4)

These equations are concerned with the probability of being legally sanctioned. Equation (2) simply expresses the fact that aggregate arrests are decomposed into those that result in incarceration and those that do not. An explanation for aggregate arrests is provided by Equation (3). COP appears in the aggregate arrests equation because of the assumption that

the marginal output of law enforcement is positive: with more police, more offenders may be identified, located, and apprehended, and more arrests will be made. CRM appears in the equation because of the presumption that the (positive) marginal productivity to law enforcement activity (captured by the COP variable) diminishes as "output" increases. Diminishing marginal productivity implies that an increase in the crime rate results in a reduced probability of being sanctioned, assuming that the level of law enforcement effort remains constant. Empirical justification for the inclusion of CRM and COP in the equation may be found in Ehrlich (1973), and Forst (1976). P15-19 and minority status appear in the equation to express the hypothesis that these two variables influence the likelihood of one's being arrested (Orsagh, 1981). SIZE permits one to test the hypothesis that the larger the community, the more difficult it is to identify and apprehend an offender. Thus, it is expected that SIZE and AR will vary inversely.

Equation (2.5)

This equation explains the term of incarceration received by a newly convicted defendant. Except for CRM and AR_I, the variables in the equation relate to individual, not regional-level, observations. Most of the variables receive theoretical and empirical support in the very large "sentencing variation" literature. This evidence overwhelmingly supports the contention that the severity of the offense (score) and the offender's prior criminal history (prior) are important determinants of the length of sentence received. Sex, race, and age are also frequently cited as determinants of sentence length. Because data relating to the length of post-incarceration probation happen to exist, we are offered a unique opportunity to appraise the practice of split sentencing. Specifically, one of the data sets available permit an appraisal of the tradeoff (the marginal rate of substitution) between incarceration time and post-release probation time. The expectation is that, <u>ceteris</u> <u>paribus</u>, where pbtn is higher, SL will be lower.

The equation indicates that the sentences meted out to individual offenders are also determined by the crime rate and incarceration rate prevailing within the region (court district) of conviction. The mechanism through which these variables are presumed to affect sentence length is complex, involving the balancing of costs imposed upon the victims of crime with the costs associated with incarcerating convicted offenders. The following brief development, which is based on the rational choice model, is used to indicate under what conditions the coefficients of CRM and AR_I may be expected to be negative.

Notation

TC	:	Total societal costs
c_v, c_p	:	Average cost to victims of crime and for maintenance of prisons Average costs are assumed to be constant.
CRM	:	The offense rate
sl	:	Mean sentence length
ADD	:	Number of admissions to prison for convictions related to CRM
STK	:	Total prison population

The Sentence Length Model

Societal costs derive from two sources: the costs incurred by the victims of crime, and the costs borne by the members of society to maintain a prison system. That is,

$$TC = c_v \cdot CRM + c_p\ STK. \tag{2.10}$$

In a steady state, the inmate population would be a function of the annual flow of inmates into the prison and the average length of sentence served. More precisely,

$$STK = ADD \cdot SL. \tag{2.11}$$

We assume that sentence length has a deterrent and, possibly, an incapacitative effect. Thus,

$$CRM = CRM(SL), \qquad CRM'_{SL} < 0. \tag{2.12}$$

We assume that the effectiveness of sentence length in reducing crime is subject to diminishing returns -- adding one year to a normal ten-year sentence, for example, has less deterrent impact than adding one year to a two-year sentence -- and that the deterrent effect is independent of the crime rate itself. More precisely,

$$CRM''_{SL} > 0; \qquad \frac{\partial CRM'_{SL}}{\partial CRM} = 0. \tag{2.13}$$

We assume that an increase in crime leads to an increase in the number of persons arrested, convicted and sent to prison; i.e.

$$ADD = ADD(CRM), \qquad ADD'_{CRM} > 0. \tag{2.14}$$

We assume that the marginal productivity associated with arresting, convicting, and incarcerating offenders diminishes with increases in the offense rate, and that ADD is independent of SL:

$$ADD''_{CRM} < 0, \qquad \frac{\partial ADD'_{CRM}}{\partial SL} = 0. \tag{2.15}$$

Finally, we assume that a relation exists between the crime rate and sentence length in the sense that the court varies sentence length in response to the crime rate. The direction of this effect is positive or negative, depending on judicial policy and available resources.

$$SL = SL(CRM), \qquad SL'_{CRM} \gtrless 0. \tag{2.16}$$

The rational choice model assumes that society attempts to minimize cost with respect to sentence length. Hence, we have

$$G = \frac{\partial TC}{\partial SL} = c_v\ CRM'_{SL} + c_p[ADD'_{CRM} \cdot CRM'_{SL} \cdot SL + ADD] = 0. \tag{2.17}$$

We assume that a finite minimum sentence, greater than zero, exists, and has for its solution the values SL_0, TC_0, etc.

The effect of an increase in the crime rate on sentence length, given that the system has the equilibrium values SL_0, TC_0, etc. is given by

$$\frac{\partial SL}{\partial CRM} = -\frac{c_p[ADD''_{CRM} \cdot CRM'_{SL} \cdot SL + ADD'_{CRM} \cdot CRM'_{SL} \cdot SL'_{CRM} + ADD'_{CRM}]}{CRM''_{SL}[c_v + c_p \cdot ADD'_{CRM} \cdot SL] + 2c_p \cdot ADD'_{CRM} \cdot CRM'_{SL}}. \tag{2.18}$$

As the model is now specified, the sign of Equation (2.18) is indeterminate. To anticipate the empirical results reported below, we note that the sign will be negative -- sentences will be shorter where crime rates are higher -- if both numerator and denominator are positive. The numerator will be positive if $SL'_{CRM} < 0$, as would happen if heavy caseloads forced more concessions from the court. The denominator will be positive if the first expression (which is positive) exceeds the second expression (which is negative).

The effect of an increase in prison admissions on sentence length is given by

$$\frac{\partial SL}{\partial ADD} = -\frac{c_p}{CRM''_{SL}[c_v + c_p \cdot ADD'_{CRM} \cdot SL] + 2c_p \cdot ADD'_{CRM} \cdot CRM'_{SL}} \tag{2.19}$$

Equation (2.19) will have a negative sign if the denominator is positive, i.e., if the first term is greater than the second.

Equation (2.6)

Expenditure for law enforcement services is hypothesized to vary directly with the number of crimes committed, with the value of property to be protected, with the community's ability to purchase these services, and with the demand for services other than crime prevention. Willingness and ability to pay and the value of property at risk are assumed to depend upon the community's present and past income level and on local government revenues. Reasonably good proxies for these variables are per capita income and per capita government revenue. We assume that the demand for protection -- willingness to pay -- also varies directly with the degree of potential harm that would be forthcoming from an offense. Accordingly, the offense variable weights offenses by a seriousness-of-offense score. Finally, an index of vehicular traffic is added to the equation as a proxy for the demand for police services for traffic supervision.

CHAPTER 3

THE EMPIRICAL MODEL: GENERAL CONSIDERATIONS

This chapter is concerned with general issues concerning the development, presentation, and interpretation of the empirical results that will be presented in later chapters. The issues to be discussed are (i) the specification of the empirical model, (ii) the nature and sources of the data, (iii) the representativeness of the Georgia and North Carolina data samples, (iv) the procedures to be used to obtain empirical estimates for the model's coefficients, and (v) the format in which these estimates will be presented.

A. MODEL SPECIFICATION

The endogenous/exogenous relations existing among the variables appearing in the empirical model are as follows:

Endogenous Variable	Variables Related to Primary Endogenous Variable: Other Current Endogenous	Lagged Endogenous	Exogenous
CRM	AR_{NI}, AR_I (AR, PI)[a]	CRM_{-1}[b]	PBTN, P15-29, NW, INCOME, SIZE, EMPLOY, EXCON[c]
AR, AR_I, PI	CRM, COP	--	P15-19, NW, SIZE, INCOME
SL	CRM, AR_I	--	score, prior, age, sex, nw, pbtn
COP	CRM_W	--	MV, REV, INCOME

[a] Substitutes for AR_{NI}, AR_I in the model's second variant.

[b] Introduced to form the model's third variant.

[c] Introduced to form the model's first variant.

In its structure and choice of variables, the model builds on the work of those who have used simultaneous estimation procedures, beginning with the work of Ehrlich (1973).

It should be clear from the foregoing description of the theoretical model and from the above tabulation that the theoretical model's crime equations are properly identified, and that the choice of excluded variables satisfies the criteria set down by Fisher and Nagin (1978). We are especially confident in this respect because (i) of Vandaele's (1978b) finding that Ehrlich's model, which resembles ours in many respects, is quite insensitive to major variation in model specification;[1] and because (ii) our own results, reported below, are relatively insensitive to variations in model-specification.

Several comments are in order concerning the model's empirical specification:

(i) For many of the model's theoretical variables there exist two or more alternative statistical measures. The following examples illustrate the variety of possibilities available: Sentence length may be measured using either expected sentence length at time of admission, time to be served to first consideration for parole, maximum possible sentence length, minimum possible sentence length, or actual time served by those just released from incarceration. It is possible to

[1]The Ehrlich model is not insensitive to the introduction of a prison capacity variable (Nagin, 1978b). However, our model is based on intrastate, cross-sectional data. Hence, the capacity variable is held constant.

index illegitimate income opportunities by using family, individual, or family and individual income data, or by more offense-specific indices such as per capita number of motor vehicle registrations (for motor vehicle theft), by per capita number of commercial establishments and residences (for burglary), etc. The employment index and the index of the severity of the offense(s) occasioning the present incarceration were each constructed using one out of a very large number of reasonable, alternative weighting systems.

Even if one were to reduce the choice of indices to just two alternatives in each instance, one would still be faced with a very large number of combinations of the basic model that would require estimation. Because the resource costs associated with estimating so many models would be prohibitively high, and because it would be extremely difficult to comprehend and make sense of the mass of coefficients which would be generated if these models were, in fact, estimated, we have confined ourselves to the use of a single index in each instance, an index that, we hope, closely approximates the variable appearing in the theoretical model.

(ii) In an early formulation of the sentence length equation (Equation 2.5), we hypothesized that the court would be influenced by the offender's marital status, IQ, education, occupation, and employment status. Early regression runs indicated very clearly that none of these variables was statistically related to sentence length. Furthermore, their presence in, or absence from, the empirical equation had virtually no effect on the other coefficients in the equation. Consequently, these variables were deleted from the model.

(iii) The basic model and two of its variants were estimated for the seven Index offenses and also for two offense aggregates: All Violent offenses and All Property offenses. We chose to include robbery in the former and to exclude it from the latter because robbery does involve violence, or the threat of violence, and because this categorization is the one conventionally used in criminal justice. We are aware of no theory that proscribes this categorization and of no empirical evidence that shows that robbers are more like other UCR property offenders than they are like other UCR violent offenders. Nevertheless, we recognize that robbery is, prima facia, an offense motivated by pecuniary considerations, that many econometric studies include robbery in the property offense aggregate, and that our decision to include robbery in All Violent offenses was, essentially, arbitrary. Therefore, in the early stages of the empirical work, we estimated several regressions using the alternative aggregate measures. As one might expect, the addition of robbery to a particular crime aggregate affected that aggregate in much the same way that adding observations to an existing data set affects the mean of that data set. Specifically, if R, P1, and P2 represent the coefficients of robbery and of property offenses, with and without robbery, respectively, then P1 can be explained as a linear combination of R and the P2 aggregate; viz., $P1=(1-\alpha)P2 + \alpha R$ where α is the ratio of the number of robbery observations to the combined number of homicide, rape, assault, and robbery observations. In the

North Carolina and Georgia data sets used, the value of α with respect to property offenses and property arrests is in the order of 2-7 percent; that for violent offenses and violent arrests 12-35 percent. Thus, because robbery is small relative to the other property offenses, we expected, and found, that the alternative property measures yielded very similar results. The same was true for violent offenses when the robbery coefficients were similar to the aggregate that excluded robbery. In the other instances, robbery's effect on the larger violent crime aggregate was that which the relation given above would have led one to expect. Thus, the choice of crime aggregate is immaterial with respect to property offenses, but occasionally has a significant, predictable effect on the violent offense aggregate, an effect that the reader may infer for himself.

(iv) The effect on the offense rate of the incapacitation of convicted offenders shall only be reported for Georgia, for the two crime aggregates, and for the basic model. A wide choice of statistical measures were available as proxies for the theoretical concept, EXCON. We rejected the matching of released offenders to offenses by narrow offense categories -- for example, the use of released robbery inmates as the variable in the robbery equation -- because we believe that the rate of crossover in offense categories is significant.[2] Hence, it

[2] In their study of adolescent delinquents Wolfgang, Figlio, and Sellin (1972: 244-255) show that an offender is just as likely to switch crime types as commit another offense of the same type. In their study of habitual felons, Petersilia, Greenwood, and Lavin (1978: 19-21) found that the habitual felon "did not specialize in a certain type of crime but switched crime types frequently." (p. vii)

would seem that a broader aggregate would be more appropriate. In our initial regression runs we used the broadest aggregate, i.e., all persons released within a particular time period. The empirical results that were derived using this aggregate were so inconclusive -- almost half of the coefficients of EXCON were of perverse (negative) sign, and the coefficients were never large in magnitude, whether the equation was estimated for individual offenses or for the offense aggregates -- that we were discouraged from examining the association of offense rates with alternative subsets of the population of prison releasees.

B. DATA FOR THE MODEL

The model uses aggregate cross-sectional data for the states of Georgia and North Carolina. Except where otherwise noted, the data refer to the year 1978 for Georgia and to 1979 for North Carolina. The year 1978 was chosen for Georgia because it was the latest year available for OBSCIS data at the time this research project began. The year 1979 was chosen for North Carolina because the 1978 data set lacked information on key variables -- in particular, employment data for North Carolina for the individual offender.

Some of the data were obtained as observations on individual incarcerated offenders. Other data were obtained as data aggregates, usually reported at the county-level of aggregation. Except for the sentence length equation (Equation 2.5), the unit of observation for

estimation purposes is the judicial district, which comprises one or more counties. The judicial district is the elemental unit to which the probability and length of incarceration variables relate. The Georgia sample contains 42 districts, the North Carolina sample, 30 districts. The judicial district was chosen for the unit of observation for these equations because the alternative, smaller geographical unit, the county, often contained too few observations to permit meaningful statistical analysis.

Equation (2.5) is based on observations of individual incarcerated UCR offenders. The smallest number of observations is for the offense of rape for the state of Georgia (n=91). For one offense categories, the sample size exceeds 3000. Table 3.1 indicates the size of sample by state and type of data.

Table 3.1

SAMPLE SIZE BY TYPE OF DATA AND STATE

Unit of Observation	Type of Data	Sample Size	
		Georgia	North Carolina
Judicial district	Upper case variables in the theoretical model of Chapter 2	42	30
Individual inmates	Incarcerations for		
	Homicide	324	393
	Rape	91	104
	Assault	334	535
	Robbery	685	679
	Burglary	1457	1440
	Larceny	613	1613
	Auto	209	188
	All Violent	1434	1711
	All Property	2279	3241

Most of the empirical variables derive from conventional data sources: CRM, COP, and AR are obtained from state police information network source agencies; REV from the biennial Census of Governments, and SIZE, P15-29, P15-19, NW, and INCOME from Census of Population documents. Not all data can be obtained for the specific year desired. For example, the age distribution of the population is based on the 1970 census, its racial composition on an extrapolation of 1960-1970 trends. Since the model utilizes cross-sectional data, relative values, not absolute values, are relevant. It may be safely assumed that the former are reasonably stable over the time intervals considered here. Moreover, the data that are likely to be most out-dated are exogenous variables, are of minor importance for policy-making; and, therefore, are not crucial to the analysis.

The sanctions variables (AR, AR_{NI}, AR_{I}, SL, PBTN, PI), the offender release variable (EXCON), the criminal history variables (score, prior), and the demographic and socioeconomic variables (EMPLOY, sex, nw, age) were obtained from the departments of correction of Georgia and North Carolina, and were derived from their Offender-Based State Corrections Information System (OBSCIS) data sets. These data sets constitute a unique and very important source of criminal justice data, permitting estimation of an Ehrlich-type econometric model at a smaller level of aggregation than has hitherto been possible. Moreover, the OBSCIS data make possible:

(i) Estimates of <u>both</u> the probability of incarceration and of the length of incarceration for a large data set disaggregated to the state judicial district level. Moreover, sentence length can now be given its appropriate measure. Instead of being defined as the mean length of sentence served by those just released from prison, which all studies except that of Avio and Clark (1978) have used, it can be measured as the sentence that is expected to be served by those who have just been consigned to prison. The distinction is particularly important since, at the present time, sentences appear to be undergoing significant change. Note, also, the OBSCIS provides sanctions estimates for very specific offense categories.

(ii) Estimates of the number of offenders newly released from prison, under conditional and unconditional release, at the judicial district level, by specific offenses, thereby enabling one to estimate the incapacitation effect using a very different statistical procedure.

(iii) More accurate estimates of the potential offender's economic status, thereby permitting a very different estimate of the effect of unemployment on the crime rate. (OBSCIS provides employment data for offenders at the time of their arrest.)

The availability of such rich and precise statistical detail for deterrence research is unprecendented.

C. ESTIMATION PROCEDURE

The presence of current endogenous variables as independent variables in a regression equation implies that the equations are interdependent and that ordinary least squares estimating procedures can produce seriously biased estimates of the model's coefficients (Orsagh, 1973). Standard procedures exist for dealing with this bias. The most common approach, and that to be adopted in this project, is to estimate the coefficients by means of two-stage least squares (TSLS). Alternative approaches exist, such as three-state least squares and full information, maximum likelihood. But there is no compelling theoretical reason for choosing one of these instead of TSLS. We have, therefore, adopted the most common procedure employed in the econometric literature.

The results derived from estimating the model will be used to evaluate the theoretical model. These results also permit an appraisal of the appropriateness of the chosen statistical surrogates. The presence of coefficients that are not statistically significant, or that have perverse signs are often a signal that the theory is deficient, that the model has been improperly specified, or that inappropriate surrogates have been used to represent the model's theoretical arguments. An important objective in developing the empirical model and in presenting and interpreting the results of the empirical effort will be to reconcile potential conflicts between our theory and the empirical evidence. That is, our objective is to produce an empirical model whose results, when correctly interpreted, do not grossly and irreconcilably contradict theory.

Because of time constraints, we have not developed a formal analysis of the residuals of the regression equations. Such analysis would provide a useful commentary on the model's specification -- whether functions to the natural numbers are "better" than log-log or log-linear transformations, whether the principal equation is, indeed, identified, whether there is evidence of heteroskedasticity, etc. Such analysis is, of course, essential to a full evaluation of the empirical model. Until this analysis is conducted, the empirical results must be viewed as provisional. Accordingly, in the reportage that follows, we shall focus on the general patterns formed by the signs of the coefficients and shall judge the coefficients and their standard errors in terms of rough orders of magnitude. We do not believe it advisable to impute greater reliability and precision to these estimates until the extended residual analysis is completed.

D. CRIME AND CRIMINAL JUSTICE RATIOS FOR GEORGIA AND NORTH CAROLINA

Tables 3.2 and 3.3 present summary offense rate and sanctions data for Georgia for 1978 and for North Carolina for 1979. Table 3.4 presents comparable data for the United States for two of these ratios. (Data for the other ratios are not available for recent years.) Although the years do not exactly correspond, they are close enough to allow meaningful comparisons. The data show that the offense and arrest rates for Georgia are comparable to those prevailing in the nation at large. Georgia's homicide and rape rates are higher than the national average, but its larceny and motor vehicle theft are below the average. Because of the latter two individual offense rates, its rate for all property offenses is also below average. Except for the very high and puzzling value for assault, Georgia's arrests rates are remarkably similar to those of the United States at large.

Thus, the evidence of Tables 3.2 and 3.4 suggests that Georgia's crime rates and the response of its police agencies to the crime rate approximate that of the nation as a whole. While we do not infer from these data that Georgia is a microcosm of the United States, we do believe that the results that have been obtained from the Georgia sample have some applicability beyond the State of Georgia.

Except for homicide and assault, offense rates in North Carolina are below the national average. We suspect that the exceptional assault rate is a statistical artifact. We note two facts: (i) North Carolina's ratio of robbery to assault rates is very low compared to the United States' and Georgian ratios, and (ii) that North Carolina's offense rates are lower than Georgia's except for assault. We suspect that, to a significant extent, discretion determines whether some offenses are classified as assault rather than as robbery, and that in North Carolina discretion is biased heavily toward defining these offenses as assaults. If our surmise is correct, the aberration disappears. Certainly, when robbery and assault are combined, all of North Carolina's offense rates are lower than Georgia's, and all but its homicide rate are lower than those for the United States.

TABLE 3.2

CRIME AND CRIMINAL JUSTICE RATIOS: GEORGIA, 1978[a]

Offense	Offenses Per 10,000 Population (CRM) (1)	Arrests per Offense (AR) (2)	Non-Incarceration Arrests per Offense (AR_{NI}) (3)	Incarcerations Per Offense (AR_I) (4)	Sentence Length (SL) (5)	Post-Prison Probation (PBTN) (6)
Homicide	1.4	.94	.40	.44	8.1	0.8
Rape	3.8	.43	.38	.047	6.5	1.6
Assault	26	.60	.58	.025	2.0	1.4
Robbery	17	.32	.24	.081	4.2	1.2
Burglary	148	.14	.12	.019	2.1	1.3
Larceny	245	.17	.16	.005	1.3	1.0
Auto	35	.15	.14	.012	1.5	0.8
All Violent	48	.50	.44	.06	4.8	1.2
All Property	428	.16	.15	.010	1.8	1.2

[a]The ratios appearing in columns (1) through (6) are defined more completely in the notation section accompanying the presentation of the model, presented in Chapter 2.

Sources:
Offenses and arrests: Georgia State Crime Commission, Statistical Analysis Center, (May 1980); Georgia Crime Information Center (computer printout and other agency-supplied data).
Incarcerations and Columns (5) and (6) from Georgia Department of Corrections, OBSCIS tapes.
Column (3) = Column (2)-Column (4). (Discrepancies are due to rounding.)

TABLE 3.3

CRIME AND CRIMINAL JUSTICE RATIOS: NORTH CAROLINA, 1979[a]

Offense	Offenses per 10,000 Population	Arrests per Offense (AR)	Non-Incarceration Arrests per Offense (AR_{NI})	Incarcerations per Offense (AR_I)	Sentence Length (SL)
	(1)	(2)	(3)	(4)	(5)
Homicide	1.1	1.06	.40	.67	13.4
Rape	2.1	.57	.47	.093	16.1
Assault	34	.76	.73	.029	2.7
Robbery	7.8	.48	.32	.16	9.3
Burglary	131	.20	.18	.020	3.5
Larceny	242	.19	.18	.012	1.9
Auto	23	.20	.18	.015	1.9
All Violent	45	.71	.64	.070	8.6
All Property	396	.19	.18	.015	2.6

[a]See footnote, Table 3.1

Source: Offenses and arrests from North Carolina, Department of Justice, Police Information Network (1979).
: Incarcerations and Sentence Length from North Carolina, Department of Correction. OBSCIS tapes.
: Column (3) = Column (2)-Column (4). (Discrepancies are due to rounding.)

TABLE 3.4

CRIME AND CRIMINAL JUSTICE RATIOS: UNITED STATES, 1978

Offense	Offenses per 10,000 Population	Arrests per Offense
Homicide	.90	1.02
Rape	3.1	.44
Assault	26	.49
Robbery	19	.36
Burglary	142	.16
Larceny	274	.19
Auto	46	.16
All Violent	49	.44
All Property	462	.18

Source: Federal Bureau of Investigation, Uniform Crime Reports, 1978. Table 24 and p. 25.

Except for larceny, North Carolina's arrest rates are higher than the nation's. The differences are particularly evident for violent offenses. The contrast between North Carolina and Georgia is also significant. North Carolina's arrest rates are higher than Georgia's for every offense category. Its incarceration rate -- column (4) -- is also substantially higher, averaging seventeen percent higher for violent offenses and fifty percent higher for property offenses.

The pattern of higher sanctions levels in North Carolina is repeated in the sentence length data. Sentences for the violent offenses, homicide, rape, and robbery, are especially noteworthy in this respect, with inmates in North Carolina expected to serve about twice as many years for the latter two offenses as inmates in Georgia. Of course, some of the recorded variation in sentence length, and in the other sanctions, as well, is likely to be artifactual. Concepts are not always equivalent, definitions are not completely uniform, between the two states. For example, "expected sentence length" is developed in each state from a formula adapted to the laws and practices of that state's criminal justice system. It may be that North Carolina's estimates systematically overstate, and Georgia's understate, the actual sentence to be served.

An alternative, but not mutually exclusive, hypothesis is that Georgia's police agencies over-report Index offenses relative to North Carolina. Were this to be true, and were the additional recorded offenses to be less serious, Georgia would be shown to have higher offense rates, lower arrest and incarceration rates, and shorter prison sentences, which is precisely what one does find in these data. The point of these examples is to suggest that, until conceptual and mensuration issues such as these are resolved, these data, alone, should not be used to assert the existence of higher sanctions levels in North Carolina. Leisure permitting, these issues might, with profit, be explored. For the purposes of this report, however, it is sufficient, and reassuring, to note that, despite these potentially very serious problems, the general pattern of results reported for North Carolina closely resembles that for Georgia.

E. FORMAT OF THE PRESENTATION OF THE EMPIRICAL RESULTS

The equations were estimated as linear functions to the natural numbers. To facilitate comparisons among variables having different units of measure, all coefficients were transformed into elasticities. The elasticity was estimated at each variable's mean value, and is defined as follows: if the coefficient of variable Z has an elasticity of β, then a one percent increase in Z, in the neighborhood of Z's mean, is associated with a β percent change in the dependent variable, all other variables in the equation being held constant.

The equations were estimated with the inclusion of an intercept term. The intercept values shall not be reported, however, since they have little interpretive value for this research.

To evaluate the statistical significance of the elasticities reported below, each elasticity is accompanied by the ratio of its coefficient to its standard error. In the OLS procedure, this ratio defines the _t_-statistic. In the TSLS procedure, the ratio approaches _t_ as the sample size becomes very large. We shall, for convenience, refer to all of these ratios as _t_-statistics.

Estimates will be presented for each of the seven Index offenses and for the violent and property offense aggregates. Note that the data relating to the latter two offense categories derive from the pool of individual observations, not from a simple mean of the individual offense coefficients. These violent and property offense data are, therefore, weighted averages of the individual offense data, wherein the weights are the numbers of observations per offense. Hence, it follows that the coefficients of, say, violent offenses may differ substantially from a simple unweighted mean of the individual coefficients for homicide rape, assault, and robbery. (The data for PBTN in Table 4.5 nicely illustrate this point.)

The next chapter provides empirical estimates for the model using the Georgia data set. The following chapter provides estimates using the North Carolina data set.

CHAPTER 4

EMPIRICAL RESULTS FOR GEORGIA

In this chapter we wish to present the results of our effort to estimate the model developed in Chapter 2 with data derived from the state of Georgia. Equations (1), (3), (4), and (5) have been estimated for nine offense categories. Since two estimation procedures were used, there are eighteen regression equations associated with each of the enumerated equations. Moreover, Equation (1) was also estimated in three variants, described in Chapter 2. We begin by presenting the results obtained for the principal equation, estimated within the context of the basic model. We then present the results obtained for the principal equation within the context of the three variants of the basic model. The results relating to the other equations of the system are then presented.

A. RESULTS FOR THE PRINCIPAL EQUATION WITHIN THE BASIC MODEL

The following three sections are specifically concerned with the effect of the four sanctions instruments on the UCR offense rates. The last section describes the effect on these offense rates of the other variables in this equation: SIZE, NW, EMPLOY, etc.

1. The Overall Pattern

Analysis shall proceed from the general to the specific. At the most general level, we ask whether the four sanctions, AR_{NI}, AR_I, SL, and PBTN

CONTINUED

1 OF 3

have a deterrent effect on the seven individual UCR offenses under consideration. To answer the question, we assume (1) that the sample space consists of the joint distribution of the four sanctions and seven offenses; and (2) that the twenty-eight elements in the sample space are independent events. We shall evaluate the one-sided hypothesis that sanctions have no effect on the offense rate against the alternative hypothesis that they have a deterrent effect. The null hypothesis is tested using the 28 OLS coefficients reported in Table 4.1, and is also tested using the corresponding TSLS data reported in Table 4.2. (The OLS and TSLS data sets have not been pooled because it is not likely that they are independent of each other.) The probability distribution of the mean of these 28 observations will form a _t_ distribution. Assuming that sanctions have neither a deterrent nor an incapacitation effect, the expected value of _t_ will be zero or positive. The sample data are summarized for the two estimation procedures in the following tabulation:

Procedure	Mean	Std. Deviation	t
OLS	-.050	.181	-1.47
TSLS	-.036	.215	-.90

$t_{.05}$ (27 d.f.) = -1.70

The results reported in this tabulation are inconclusive. The configuration of coefficients derived from the OLS estimation procedure provides

TABLE 4.1

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE: ELASTICITIES AND ASYMTOTIC t VALUES: GEORGIA, 1978, ORDINARY LEAST SQUARES PROCEDURE

Eqn.	Depend. Var.	AR_{NI}	AR_I	SL	PBTN	SIZE	P15-29	RACE	EMPLOY	INC
		Independent Variables								
(1)	Homicide	-.26 (3.78)	-.23 (3.53)	.44 (1.63)	-.02 (.43)	.11 (3.29)	-.47 (.92)	.14 (.85)	1.92 (1.28)	-.45 (1.12)
(2)	Rape	-.13 (1.48)	-.17 (1.89)	.25 (2.74)	.03 (.76)	.14 (3.88)	1.02 (1.83)	.36 (2.11)	-1.11 (.69)	1.28 (3.10)
(3)	Assault	-.52 (3.79)	-.07 (1.66)	-.20 (1.15)	-.04 (.58)	.08 (1.96)	.99 (1.53)	.02 (.10)	-2.23 (1.30)	.63 (1.22)
(4)	Robbery	-.08 (.69)	-.00 (.04)	.11 (.71)	-.01 (.13)	.51 (14.24)	.82 (1.55)	.22 (1.29)	-1.11 (.73)	1.31 (3.22)
(5)	Burglary	-.19 (2.28)	-.19 (3.76)	.15 (1.25)	-.01 (.27)	.08 (4.54)	1.07 (4.00)	.17 (1.98)	-1.72 (2.27)	.96 (4.10)
(6)	Larceny	-.14 (.91)	-.14 (2.12)	.03 (.27)	-.02 (.37)	.07 (3.12)	1.93 (5.56)	.31 (2.59)	-2.04 (1.85)	1.35 (4.58)
(7)	Auto	-.04 (.54)	-.06 (1.38)	.20 (2.27)	-.10 (2.79)	.16 (6.91)	.78 (2.48)	-.17 (1.64)	-1.92 (1.99)	1.15 (4.04)
(8)	All Violent	-.45 (3.33)	-.15 (2.11)	.16 (.71)	.02 (.30)	.18 (5.44)	.56 (1.04)	.09 (.51)	-1.77 (1.21)	.61 (1.46)
(9)	All Property	-.15 (1.27)	-.16 (3.12)	.11 (.75)	-.03 (.57)	.08 (4.20)	1.51 (5.24)	.21 (2.24)	-2.03 (2.35)	1.18 (4.84)

TABLE 4.2

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE: ELASTICITIES AND ASYMTOTIC t VALUES: GEORGIA, 1978, TWO STAGE LEAST SQUARES

Eqn.	Depend. Var.	AR_{NI}	AR_I	SL	PBTN	SIZE	P15-29	RACE	EMPLOY	INC
(1)	Homicide	.11	-.19	-.15	-.11	.10	.09	.17	.54	-.26
		(.36)	(.87)	(.15)	(1.24)	(1.94)	(.11)	(.67)	(.19)	(.39)
(2)	Rape	.32	-.23	.79	.01	.15	.82	.33	-.87	1.26
		(1.01)	(1.21)	(3.49)	(.20)	(4.16)	(1.26)	(1.91)	(.45)	(2.91)
(3)	Assault	-1.70	.08	-.08	-.24	.15	-.67	-.98	-3.55	-.36
		(1.27)	(.58)	(.10)	(1.13)	(2.00)	(.43)	(.92)	(1.43)	(.32)
(4)	Robbery	-.09	-.04	-.08	-.04	.50	.76	.28	-.83	1.29
		(.34)	(.12)	(.25)	(.32)	(9.28)	(1.06)	(.75)	(.52)	(3.04)
(5)	Burglary	-.14	-.17	.20	-.01	.08	1.12	.16	-1.77	1.01
		(.44)	(1.07)	(.63)	(.08)	(3.73)	(2.61)	(1.43)	(1.70)	(2.24)
(6)	Larceny	-.36	-.04	-.04	-.01	.07	1.93	.28	-2.87	1.34
		(.53)	(.14)	(.12)	(.12)	(2.17)	(4.04)	(1.76)	(1.29)	(1.94)
(7)	Auto	-.20	-.19	.07	-.02	.13	.37	.02	-1.52	1.33
		(.80)	(1.14)	(.20)	(.30)	(2.77)	(.70)	(.09)	(.97)	(2.19)
(8)	All Violent	-.30	-.30	-.23	-.04	.19	.59	.07	-1.06	.39
		(.52)	(1.52)	(.37)	(.35)	(4.10)	(.51)	(.18)	(.55)	(.48)
(9)	All Property	-.63	-.41	-.53	-.06	.07	1.00	.30	-1.73	.62
		(.88)	(1.70)	(.68)	(.88)	(1.39)	(1.55)	(1.94)	(1.42)	(.95)

stronger support for the deterrence hypothesis than does the TSLS estimation procedure, but in neither case is one justified in rejecting the null hypothesis. Thus, one may not assert the existence of a general deterrent effect operating across these four sanctions instruments. If a statistically significant deterrent effect exists, it is likely to reside in one or more of these instruments, but not in all of them.

2. Differences Among Sanctions

An inspection of the pattern of coefficients and of t-statistics suggests that the four sanctions may have very different effects on the offense rate. To evaluate this hypothesis, the data were subjected to an analysis of variance. The results appear in Table 4.3.

TABLE 4.3

ANALYSIS OF VARIANCE OF COEFFICIENTS RELATING TO SEVEN UCR OFFENSES AND FOUR SANCTIONS INSTRUMENTS: OLS AND TSLS PROCEDURES

	Sum of Squares	df	Mean Square	F ratio
OLS				
Across Means	.440	3	.147	$F = \frac{.147}{.018} = 8.02$
Within	.439	24	.018	
Total	.879	27		
TSLS				
Across Means	.558	3	.186	$F = \frac{.186}{.140} = 1.33$
Within	3.353	24	.140	
Total	3.911	27		

$F_{.95}(3,24) = 3.01$; $F_{.99}(3,24) = 4.72$

The evidence from Table 4.3 is somewhat inconclusive. On the other hand, the OLS data strongly support the inference that the four sanctions have significantly different effects on the offense rate; while, on the other hand, the TSLS evidence is consistent with the hypothesis that these sanctions have similar effects. Nonetheless, these results and a casual inspection of Tables 4.1 and 4.2 will probably incline the reader to support the view that, most likely, the rank order of the four sanctions with respect to their effectiveness as a deterrent is: AR_I, AR_{NI}, PBTN, SL. Their relative effectiveness can be best displayed, perhaps, by the following tabulation, in which the actual means of the coefficients, based on the seven UCR offenses, are presented:

Means for Seven Offenses	Sanctions AR_{NI}	AR_I	SL	PBTN
OLS Elasticities	-.19**	-.12***	.14	-.02*
TSLS Elasticities	-.29	-.11***	.10	-.06**

***, **, *: Significant at the 1, 5, and 10 percent significance levels, respectively, with six degrees of freedom.

One sees that the mean value of AR_I's coefficients is negative, large, and highly significant, using either procedure. The means of AR_{NI} and PBTN are also negative and generate approximately equal, but lower, significance levels, with AR_{NI}'s coefficients tending to be the larger. On the other hand, the means of SL's coefficients are unexpectedly positive. The means in this tabulation are, of course, unweighted. The weighted means reported for All Violent and All Property offense categories tell a somewhat different story. While the OLS coefficients reported in Table 4.1 are also positive, SL's TSLS coefficients for All Violent and All Property offenses, given in Table 4.2, are negative, though these, too, are not statistically significant.

3. <u>Sanction-Specific Analysis</u>

<u>The Deterrent Effect of the Risk of Incarceration</u>

The evidence presented in the foregoing tabulation provides very strong support for the contention that, overall, the risk of incarceration has a deterrent effect on UCR offenders: the unweighted mean of the coefficients is statistically highly significant using either estimation procedure, all seven OLS coefficients and six of the seven TSLS coefficients are negative, and the individual TSLS violent and property offense coefficients are, themselves, statistically significant.

Finally, the pattern displayed by the coefficients of the individual offenses, as well as the summary data reported for violent and property offenses, supports the hypothesis that violent and property offenders are approximately equally responsive to the threat of punishment.

<u>The Deterrent Effect of Other Arrest Outcomes</u>

The evidence concerning the effect of arrests whose outcome does not result in imprisonment suggests that this variable also has a deterrent

effect on UCR offenders, but this conclusion is slightly less persuasive than that relating to the probability of imprisonment. All seven OLS coefficients are negative, and their mean is statistically significant at the five percent level. However, two of the seven TSLS coefficients are positive, and the mean of this set of coefficients is only significant at the ten percent level.

The pattern of coefficients for this variable suggests an interesting hypothesis. We note, first of all, that the positive coefficients relate to the two offenses -- homicide and rape -- that carry the largest mean sentence length and also have the largest expected sentence (see Table 3.2). The hypothesis that we advance is this: Suppose that one is asked to choose between a legitimate and a criminal act and that the latter involves two potential outcomes. Let the consequence of one of the outcomes become increasingly important relative to the other. Beyond some threshold value, the consequential outcome will assume a dominant role in decision-making, and the inconsequential outcome will no longer affect one's choice between the legitimate and the criminal act. For example, suppose one contemplates robbing a bank. If one is really at the margin between committing and not committing the robbery, one is dealing in gains and losses of a relatively high magnitude. Under these circumstances, it is not likely that an increase or decrease in the probability of receiving a citation for double-parking while engaged in the robbery will influence the robbery decision. By the same token, those marginal offenders, consciously or subconsciously engaged in homicide or rape decision, are not likely to be influenced in their decision by variations in the risk of an arrest that would cause them little more than a minor inconvenience.

The Deterrent Effect of Post-Prison Probation

Post-prison probation appears to have a deterrent effect on UCR offenders but, like non-incarceration arrests, this conclusion carries slightly less weight than that pertaining to the probability of incarceration. Except for rape, PBTN's coefficients are negative, and the means of the OLS and TSLS sets of coefficients are significant at the five and ten percent levels, respectively. Finally, we note that the magnitude of PBTN's coefficients appears to be significantly lower than that of AR_{NI} and of AR_I.

The Deterrent Effect of Length of Incarceration

The foregoing evidence provides no support for the contention that lengthening the term of incarceration has a deterrent effect on UCR offenders. The TSLS results are clearly inconclusive -- three of the seven coefficients are positive. The OLS results are more troubling: six of the seven coefficients derived by this procedure are positive. If, indeed, offenders are not deterred by longer prison sentences, we should have expected somewhat less positive coefficients. The results that have been obtained are disappointingly perverse. We interpret them as a signal that something may be wrong with the data or with this study's statistical design.

We suspect that one source of the difficulty resides in our procedure for measuring sentence length. The rational choice model asserts that some potential offenders will be deterred from committing an offense if the cost of committing that offense increases. The cost referred to in the model is

unit cost. Unfortunately, SL does not measure unit cost in those instances in which an offender is incarcerated for more than one offense. In the statistical model, SL is defined as the number of years to be served, whether it be for one offense or for many, rather than the number of years to be served per offense, as the theoretical model requires. Thus, increases in SL will reflect, in part, increases in the number of offenses committed. SL and the dependent variable are made to covary, in part, by definition. Consequently, SL's coefficient is biased toward positive values.

A second source of positive bias in the sentence length coefficient exists. We will show below that in-prison time and post-prison probation time were treated as substitutes by the court. Thus, while one offender may have received a two-year prison sentence, another may have received a one-year prison sentence coupled with three years of subsequent probation. This being so, the measured effect of SL on the offense rate, CRM, assuming that Equation (2.1) is represented by a linear function, is given by

$$\frac{\partial CRM}{\partial SL} = b_{SL} + b_{PBTN} \cdot \frac{\partial PBTN}{\partial SL}, \qquad (4.1)$$

where the b's are the true coefficients of the two severity-of-sanctions variables. We believe that the absolute value of the right hand partial derivative is substantially greater than one (see the results relating to sentence length reported below). If b_{SL} and b_{PBTN} are both negative (e.g., if a deterrent effect exists for each sanction), the second term on the right, which represents the bias in the estimate of SL's effect, will be positive. If, in addition, the two true coefficients are approximately equal, the measured effect of SL on CRM could be large, positive, and statistically significant.[1]

Thus, our procedure for estimating SL provides two potentially serious sources of positive bias, and may explain the existence of these counter-intuitive, positive coefficients.

4. Other Variables in the Principal Equation

The other five variables in the principal equation are of peripheral interest to this study. Hence, discussion of their contribution to an explanation of the offense rate may be limited to the following brief remarks.

SIZE

Of all the explanatory variables appearing in the principal equation, including the sanctions variables themselves, the size of community is most consistently statistically significant and of relatively large magnitude. There is no question but that, *ceteris paribus*, larger communities have higher

[1] By contrast, while PBTN's measured effect also has a positive bias derived from the first term in the expression,

$$\frac{\partial CRM}{\partial PBTN} = b_{SL} \frac{\partial SL}{\partial PBTN} + b_{PBTN}, \qquad (4.2)$$

an approximate equality of the two coefficients could preserve a small, statistically significant, negative measured effect, since the offsetting positive bias term will tend to be small.

offense rates. This generalization holds for each of the seven individual offenses, and for All Violent and All Property offenses as well.

P15-29 and NW

The population within the ages of 15 and 29 and the non-white population appear to have a greater propensity to engage in property crime -- especially burglary and larceny -- than is true of the rest of the population. It is not certain from these data that these two population subsets are more predisposed to violence, although the evidence does lean in that direction, and hence would incline one, as a best guess, to reject the null hypothesis of no relation for these offenses as well.

EMPLOY and INCOME

The signs of the coefficients of EMPLOY and of INCOME are mostly consistent with the hypothesis that relate employment status and the income variable to the offense rate. Hence, this evidence supports the conclusion that such a relation exists. Because the relation appears to hold just as well for rape and assault as it does for crimes having an economic motivation, one cannot go much beyond an inference that the relation exists. In particular, it would be inappropriate to infer that EMPLOY demonstrates that economic need is a cause of crime, or that INCOME demonstrates that crime is motivated by opportunities for illegitimate income. The data are equally consistent with other interpretations. For example, EMPLOY could as well reflect the degree of distaste for legitimate work among potential offenders, while INCOME might reflect the extent to which informal controls deriving from family, neighborhood, and other social institutions are inoperative. Obviously, if EMPLOY serves primarily as an index of the community's "taste" for legitimate work, or if INCOME serves as an index of social disorganization, one's etiological story would assume a quite different cast.

B. ALTERNATIVE SPECIFICATION OF THE PRINCIPAL EQUATION

Three variants of the basic equation have been examined and are reported upon below. Specifically:

(1) The model has been modified to test for the existence of an incapacitation effect. As indicated above, the coefficients of AR_I and SL in the principal equation measure a combined deterrent and incapacitation effect. To "net out" the incapacitation effect, the variable EXCON has been added to the basic equation.

(2) The model has been reestimated using, as alternative measures of the probability of being sanctioned, aggregate arrests and the conditional probability of incarceration, given that an arrest has occurred.

(3) The basic model presumes that this year's sanctions influence this year's crime rate and not that of subsequent years. The model has been reestimated on the alternative assumption that sanctions exercise their effect on the crime rate over a period of years.

1. Variant One: The Incapacitation Effect

The Georgia data were used to evaluate the hypothesis that incapacitation significantly reduces the offense rate. The hypothesis was evaluated by introducing the variable, EXCON, into the equation where EXCON is defined as the time-weighted number of persons discharged from prison relative to all persons in the population.[2] EXCON will be an acceptable measure of the incapacitation effect, in the first sense of the expression given in Chapter 2, if the criminal propensities of those just released from prison approximate those of persons still incarcerated, for in that case the offense rate ascribed to those just released would be equal to the offense rate of those not yet released. That is, the number of offenses committed by those just released would measure the number of crimes prevented by the incapacitation of those not yet released.

The measure, EXCON, is subject to two potentially important sources of bias:

(1) If release occurs on the average after (or before) the age of maximum criminality, the coefficient of EXCON will overstate (understate) the incapacitation effect. However, because the majority of releasees serve relatively short sentences -- less than five years in Georgia, on the average, for violent offenses and less than two years for property offenses -- the difference in age-specific criminality is not likely to be all that great relative to the age of maximum criminality.

[2]The index is more completely defined in Appendix A.

(2) The incapacitation effect is based on the counterfactual concept of the number of offenses that would have been committed had the incarcerated population been free to move about in the general population. Shaw and McKay (1942) have argued that ex-offenders tend to locate in relatively small geographical areas, and are likely to transmit their deviant values to others within such areas. If the authors are correct in their argument, the concept of incapacitation, may need to be broadened to include the number of offenses that would have been committed by free persons who become infected by the values of those incapacitated offenders who could have been released from prison.

However one may wish to define the incapacitation effect, its empirical estimation would be rendered considerably more difficult if Shaw and McKay are correct. If ex-offenders do locate themselves among the criminal population; then whether or not they infect others with their deviant values, their own recidivistic offenses will covary statistically with the offenses of the more general, but, by hypothesis, more criminal population. Regression analysis would not be able to distinguish the separate effects of these two groups on the crime rate. Accordingly, EXCON will reflect the combined contribution of these two groups to the crime rate; and, therefore, will exaggerate the incapacitation effect.

Thus, EXCON's coefficient may be biased toward or against the incapacitation effect. A priori, it is not possible to say which bias, if either, is consequential. But with these caveats in mind, we now observe the effect of introducing EXCON into the principal equation as an explanatory variable. The coefficients of the reestimated principal equation are reported below in Table 4.4. The coefficients only relate to All Violent and All Property offense categories. As was indicated in Chapter 3, we do not believe it is meaningful to relate EXCON to the separate UCR offenses.

The data of Table 4.4 do not support the contention that incapacitation significantly reduces the offense rate. If there were an incapacitation effect, one would expect to find a positive relation between the offense rate and EXCON. The coefficients for All Violent offenses are positive, as hypothesized, but they are of very small magnitude and are not statistically significant. The property offense coefficients, on the other hand, have the wrong signs. Perhaps more significant for this research is the fact that none of the other coefficients in any of the four equations is materially changed by the addition of EXCON to the principal equation. Indeed, to two decimal places, most coefficients are identical to those reported for the basic model. Therefore, we conclude that the estimates of the deterrent effect presented in the basic model are not biased in favor of the deterrence hypothesis by the omission of an incapacitation variable. The effect of incapacitation on the crime rate is, at best, minimal.

2. Variant Two: The Conditional Probability of Incarceration

In this version of the basic model AR and PI are substituted for AR_{NI} and AR_I. The coefficients derived from this version of the empirical model are reported in Tables 4.5 and 4.6. As indicated above, AR and PI are derived from a conceptual framework that views the criminal choice somewhat differently with respect to the risk of being sanctioned. Thus,

TABLE 4.4
VARIANT ONE OF THE BASIC EQUATION:
THE INCAPACITATION EFFECT:
GEORGIA, OLS AND TSLS PROCEDURES

VARIABLE	All Violent Offenses				All Property Offenses			
	OLS		TSLS		OLS		TSLS	
	Coef.	t	Coef.	t	Coef.	t	Coef.	t
AR_{NI}	-.44	3.16	-.31	.65	-.16	1.22	-.66	1.14
AR_{I}	-.15	2.03	-.31	1.90	-.16	3.07	-.32	2.54
SL	.15	.68	-.25	.40	.11	.73	-.55	.85
PBTN	.03	.35	-.04	.34	-.04	.57	-.10	1.18
SIZE	.19	5.36	.19	4.58	.08	4.04	.05	1.18
P14-29	.54	.98	.52	.59	1.52	5.10	1.25	3.46
NW	.09	.47	.05	.15	.21	2.18	.34	2.14
EMPLOY	-1.64	1.05	-.86	.44	-2.09	2.18	-2.74	1.69
INCOME	.60	1.40	.34	.52	1.18	4.76	.86	2.64
EXCON	.02	.25	.02	.20	-.01	.15	-.09	.99

TABLE 4.5

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE, USING AGGREGATE ARRESTS AND THE CONDITIONAL PROBABILITY OF INCARCERATION: ELASTICITIES AND ASYMTOTIC t VALUES: GEORGIA, 1978, ORDINARY LEAST SQUARES PROCEDURE

Eqn.	Depend. Var.	Independent Variables								
		SL	PBTN	AR	PI	SIZE	P15-29	RACE	EMPLOY	INC
(1)	Homicide	.42 (1.56)	-.02 (.48)	-.49 (5.29)	-.09 (.72)	.11 (3.34)	-.43 (.83)	.14 (.88)	2.17 (1.40)	-.46 (1.15)
(2)	Rape	.19 (2.27)	.00 (.09)	-.17 (1.64)	-.07 (1.03)	.14 (3.78)	1.10 (1.91)	.31 (1.76)	-1.30 (.75)	1.26 (2.83)
(3)	Assault	-.21 (1.19)	-.05 (.65)	-.59 (4.07)	-.02 (.28)	.08 (1.88)	1.05 (1.57)	.03 (.11)	-2.28 (1.30)	.68 (1.31)
(4)	Robbery	.11 (.72)	-.01 (.11)	-.06 (1.12)	.05 (.40)	.51 (14.23)	.85 (1.61)	.22 (1.31)	-1.25 (.81)	1.34 (3.16)
(5)	Burglary	.13 (1.15)	-.02 (.40)	-.40 (3.58)	-.17 (3.43)	.08 (4.78)	1.05 (4.07)	.16 (1.91)	-1.67 (2.28)	.87 (3.74)
(6)	Larceny	.03 (.23)	-.04 (.65)	-.32 (2.21)	-.19 (2.34)	.08 (3.49)	1.85 (5.40)	.28 (2.38)	-1.75 (1.58)	1.20 (3.92)
(7)	Auto	.22 (2.60)	-.09 (2.80)	-.11 (1.90)	-.09 (1.76)	.16 (7.20)	.84 (2.76)	-.18 (1.77)	-1.70 (1.80)	1.04 (3.54)
(8)	All Violent	.17 (.79)	.03 (.37)	-.58 (3.82)	-.07 (.70)	.18 (5.38)	.63 (1.16)	.09 (.51)	-1.77 (1.18)	.62 (1.45)
(9)	All Property	.09 (.65)	-.04 (.67)	-.35 (2.80)	-.19 (3.24)	.09 (4.52)	1.44 (5.14)	.19 (2.12)	-1.96 (2.35)	1.04 (4.21)

TABLE 4.6

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE, USING AGGREGATE ARRESTS AND THE CONDITIONAL PROBABILITY OF INCARCERATION: ELASTICITIES AND ASYMTOTIC t VALUES: GEORGIA, 1978, TWO STAGE LEAST SQUARES

Eqn. Depend. Var.	SL	PBTN	AR	PI	SIZE	P15-29	RACE	EMPLOY	INC
(1) Homocide	-.33 (.35)	-.10 (1.14)	-.32 (.70)	-.52 (1.31)	.11 (2.10)	.16 (.19)	.14 (.57)	2.95 (.91)	-.39 (.57)
(2) Rape	.85 (3.43)	-.03 (.61)	.34 (.93)	-.15 (1.18)	.16 (4.11)	.77 (1.16)	.23 (1.31)	-.79 (.37)	1.18 (2.45)
(3) Assault	.17 (.24)	-.27 (1.46)	-1.77 (1.57)	.36 (1.46)	.17 (2.28)	-1.82 (.96)	-1.17 (1.21)	-4.82 (1.83)	-.51 (.47)
(4) Robbery	-.05 (.16)	-.03 (.23)	-.09 (.29)	.06 (.15)	.50 (9.54)	.82 (.97)	.26 (.75)	-1.04 (.48)	1.34 (2.50)
(5) Burlgary	.26 (.84)	-.01 (.14)	-.35 (.74)	-.11 (.78)	.08 (3.76)	1.10 (2.44)	.14 (1.28)	-1.93 (1.90)	.97 (1.88)
(6) Larceny	-.00 (.01)	.00 (.03)	-.35 (.47)	.03 (.10)	.06 (1.68)	1.97 (3.64)	.29 (1.68)	-3.14 (1.34)	1.44 (1.63)
(7) Auto	.65 (1.37)	-.07 (.86)	-.40 (1.53)	-.55 (1.92)	.20 (3.28)	.56 (1.34)	-.09 (.45)	1.37 (.59)	-.12 (.12)
(8) All Violent	-.07 (.12)	-.02 (.16)	-.64 (.92)	-.27 (1.14)	.19 (4.06)	.72 (.63)	.04 (.09)	-1.00 (.48)	.33 (.37)
(9) All Property	-.99 (1.07)	-.08 (1.08)	-1.55 (1.47)	-.47 (2.03)	.05 (1.05)	.60 (.79)	.32 (2.08)	-1.75 (1.46)	.09 (.10)

the coefficients of the first two variables cannot be compared, one to one, with the coefficients of the latter two variables. Nevertheless, both formulations were structured to address the same question; and, when their variables are taken as a pair, they permit a test of the same deterrence hypothesis.

It is apparent from a comparison of the data presented in Tables 4.5 and 4.6 with the data derived from the basic model (presented in Tables 4.1 and 4.2), that the reformulation of the basic model produces almost no substantive change. The conclusions that have been advanced concerning the deterrent effects of the risk of arrest and incarceration and of the severity of the legal sanction are not materially affected by the new data; i.e., support for the deterrence hypothesis is in no way diminished by these data. In addition, the conclusions that relate to the other variables in the principal equation stand approximately as before. The only changes of consequence are found in the TSLS procedure and concern the coefficients of the Auto equation: specifically, the coefficients of NW, EMPLOY, and INCOME reverse signs.

Thus, we conclude that the choice of variables to express the probability of being sanctioned does not alter one's conclusion concerning the efficacy of legal sanctions.

3. Variant Three: The Distributed Lag Assumption

In this variant of the basic model, the lagged value of the dependent variable appears as a regressor in the principal equation. Whether the estimated coefficients of this equation are unbiased or not

depends upon the essentially unknown behavior of the equation's disturbance term (Theil, 1971: 261). The following development illustrates the problem and the critical assumption that must be made concerning the disturbance term. (Note that some assumption must be made about its behavior.)

Let the stochastic relation between the offense rate, CRM, and the level of sanctions, S, be given by

$$CRM_{kt} = \beta S_{kt} + \beta_1 S_{k.t-1} + \beta_2 S_{k.t-2} + \ldots + \epsilon_{kt}, \qquad (4.3)$$

where k and t refer to a particular region and a particular time period, respectively.

We adopt the conventional assumptions:

$$S, \epsilon \quad \text{are independent,} \qquad (4.4)$$

$$E(\epsilon) = 0 \text{ for all } k, t, \qquad (4.5)$$

$$E(\epsilon'_{kt}\epsilon_{js}) = \sigma^2 \text{ for } k = j \text{ and } t = s, \text{ and} \qquad (4.6)$$

$$E(\epsilon'_{kt}\epsilon_{js}) = 0 \text{ otherwise.} \qquad (4.7)$$

In words, the disturbance is assumed to have a constant variance across regions and time, and the ϵ_{kt} disturbance is not related cross-sectionally or longitudinally to any other disturbance term.

Using the Koyck transformation described in Equations (2.7)-(2.9), we obtain

$$CRM_t = \beta S_t + \lambda CRM_{t-1} + \zeta_t \qquad (4.8)$$

wherein the k subscript is suppressed for convenience of notation, and

$$\zeta_t = \varepsilon_t - \lambda\varepsilon_{t-1}. \tag{4.9}$$

The sample is drawn across k regions at time t, but uses the CRM values from t-1. Let b and r represent the OLS estimators of the true coefficients β and λ. The expected values of the estimators, b and r, can be shown to be related to β and λ by

$$E\binom{b}{r} = \binom{\beta}{\lambda} + E(X^1X)^{-1} \begin{pmatrix} E(S_t\zeta_t) \\ E(CRM_{t-1}\zeta_t) \end{pmatrix}, \tag{4.10}$$

where the summation is over the k regions, and X is the k*2 matrix of the S, CRM_{-1} observations. Thus, b and r will be unbiased estimators of β and λ if both $E(S_t\zeta_t)$ and $E(CRM_{t-1}\zeta_t)$ equal zero. The former is true by virtue of (4.4) and (4.9). If no restrictions are placed on the distribution of ζ, we have from Equation (4.3) and (4.4)

$$E(CRM_{t-1}\zeta_t) = E\left[(\beta S_{t-1} + \beta_1 S_{t-2} + \beta_2 S_{t-3} + \ldots + \varepsilon_{t-1})(\zeta_t)\right] = E\left[\varepsilon_{t-1}\zeta_t\right]. \tag{4.11}$$

The last expression may then be transformed, using (4.9), (4.6), and (4.7):

$$E(\varepsilon_{t-1}\zeta_t) = E\left[(\varepsilon_{t-1})(\varepsilon_t - \lambda\varepsilon_{t-1})\right] = -\lambda\sigma^2 . \tag{4.12}$$

Thus, if no restrictions are placed in the disturbance, ζ , the estimators b and r are shown to be biased. Suppose, instead, that we assume that ζ is, itself a random variable with

$$E(\zeta_t\zeta_s) = 0, \; t \neq s. \tag{4.13}$$

Then, using (4.9) to expand the last expression in (4.11), we obtain

$$E(\lambda\zeta_t\zeta_{t-1} + \lambda^2\zeta_t\zeta_{t-2} + \lambda^3\zeta_t\zeta_{t-3} + \ldots) = 0. \tag{4.14}$$

In this case, the estimators are unbiased. Thus, using this alternative and quite reasonable assumption, we might conclude that OLS procedures produce unbiased regression coefficients.

Tables 4.7 and 4.8 present our regression results for this variant of the basic model. The best generalization that can be made from these data is that they diminish the strength with which conclusions concerning the components of the deterrence hypothesis may be advanced, but that the conclusions, themselves, need not be modified or withdrawn. The differences between the results of the basic model and those of its third variant are not dramatic. On the average, the significance levels of the sanctions variables diminish, as one would expect, since the sanctions instruments covary with CRM_{-1}. In addition, at the TSLS level, some changes in sign occur that diminish the overall strength of the hypothesis that AR_{NI} and AR_I have a deterrent impact. On the other hand, the conclusions reached concerning the other variables in the principal equation remain wholly intact. The most drastic difference concerns the values of the coefficients in the TSLS homicide equation, and here the observed differences may be described as minor variation in the neighborhood of a central tendency value of zero.

TABLE 4.7

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE, ASSUMING SANCTIONS HAVE A DISTRIBUTED LAG: ELASTICITIES AND ASYMTOTIC t VALUES: GEORGIA, 1978, ORDINARY LEAST SQUARES

Eqn.	Depend. Var.	AR_{NI}	AR_I	SL	PBTN	CRM_{-1}	SIZE	P15-29	NW	EMPLOY	INC
		Independent Variables									
(1)	Homicide	-.22 (3.64)	-.23 (4.02)	.23 (.94)	-.04 (.93)	.52 (3.36)	.10 (3.30)	-.47 (1.05)	-.09 (.60)	2.88 (2.16)	-.53 (1.53)
(2)	Rape	-.10 (1.23)	-.13 (1.53)	.19 (2.07)	.02 (.65)	.40 (2.17)	.13 (3.97)	.33 (.54)	.15 (.81)	.20 (.12)	.66 (1.36)
(3)	Assault	-.38 (3.38)	-.04 (1.10)	-.09 (.67)	-.07 (1.24)	.66 (4.76)	.07 (2.25)	.13 (.25)	-.26 (1.41)	-.69 (.51)	-.03 (.07)
(4)	Robbery	-.07 (.61)	-.00 (.02)	.10 (.63)	-.01 (.16)	.15 (1.13)	.48 (10.52)	.61 (1.10)	.13 (.66)	-.74 (.48)	.98 (1.96)
(5)	Burglary	-.07 (1.08)	-.09 (1.91)	.06 (.71)	.01 (.16)	.50 (4.88)	.08 (6.10)	.49 (2.08)	.08 (1.14)	-.95 (1.58)	.51 (2.54)
(6)	Larceny	-.06 (.62)	-.05 (1.08)	-.05 (.59)	-.01 (.17)	.66 (7.05)	.08 (5.42)	.43 (1.43)	.01 (.08)	-.56 (.76)	.30 (1.26)
(7)	Auto	-.00 (.05)	-.05 (1.78)	.11 (1.76)	-.06 (2.50)	.49 (6.24)	.13 (8.24)	.37 (1.64)	-.09 (1.25)	-1.26 (1.91)	.43 (1.91)
(8)	All Violent	-.34 (2.89)	-.11 (1.71)	.12 (.64)	-.02 (.26)	.51 (3.53)	.15 (4.84)	.00 (.00)	-.14 (.80)	-.58 (.45)	.03 (.06)
(9)	All Property	-.04 (.50)	-.07 (1.66)	.05 (.46)	.00 (.11)	.57 (5.72)	.08 (6.11)	.53 (2.00)	.05 (.67)	-.81 (1.25)	.45 (2.11)

TABLE 4.8

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE, ASSUMING SANCTIONS HAVE A DISTRIBUTED LAG: LEASTICITIES AND ASYMTOTIC t VALUES: GEORGIA, 1978, TWO STAGE LEAST SQUARES

Eqn.	Depend. Var.	AR_{NI}	AR_{I}	SL	PBTN	CRM_{-1}	SIZE	P15-29	RACE	EMPLOY	INC
(1)	Homicide	.20	-.26	.21	-.10	.64	.10	-.10	-.12	1.69	-.60
		(.73)	(1.46)	(.28)	(1.37)	(2.76)	(2.24)	(.15)	(.53)	(.72)	(1.14)
(2)	Rape	.32	-.18	.73	.00	.18	.15	.52	.23	-.46	1.00
		(1.07)	(.98)	(3.03)	(.06)	(.81)	(4.38)	(.85)	(1.15)	(.25)	(1.91)
(3)	Assault	-.37	-.02	.17	-.08	.71	.09	-.23	-.34	-1.17	-.28
		(.35)	(.18)	(.39)	(.55)	(2.05)	(1.39)	(.22)	(.50)	(.58)	(.39)
(4)	Robbery	-.09	.00	-.05	-.02	.16	.47	.61	.14	-.53	.95
		(.36)	(.00)	(.18)	(.25)	(.98)	(9.80)	(.99)	(.39)	(.33)	(1.71)
(5)	Burglary	-.03	.03	.18	.02	.61	.09	.44	.04	-1.12	.55
		(.13)	(.19)	(.81)	(.47)	(3.23)	(5.57)	(1.74)	(.46)	(1.51)	(2.37)
(6)	Larceny	.01	-.05	-.04	-.01	.67	.08	.43	.00	-.35	.32
		(.03)	(.42)	(.23)	(.12)	(4.84)	(4.53)	(1.28)	(.01)	(.25)	(1.13)
(7)	Auto	-.11	-.03	.07	-.05	.47	.12	.33	-.05	-1.71	.54
		(.67)	(.22)	(.34)	(.97)	(4.59)	(3.90)	(.94)	(.32)	(1.62)	(1.50)
(8)	All Violent	-.00	-.10	-.07	-.05	.62	.13	.36	-.01	-.28	.13
		(.01)	(.49)	(.15)	(.48)	(2.47)	(2.78)	(.46)	(.05)	(.17)	(.21)
(9)	All Property	.14	.07	.40	.02	.72	.10	.47	-.04	-.80	.46
		(.28)	(.26)	(.67)	(.33)	(2.14)	(3.32)	(1.55)	(.22)	(.85)	(2.02)

The first-order correlation coefficients between CRM_{-1} and the other regressors in the principal equation are sometimes quite high. The more extreme values occur with the TSLS procedure and are as follows:

Equation	Other Regressor	Correlation Coefficient
Robbery	SIZE	.83
Burglary	AR_I	-.83
Auto	INCOME	.83
All Property	AR_I	-.82

The coefficients of AR_I reverse sign, becoming positive in the burglary and All Property offense equations, when the lagged offense rate is introduced into the basic model. This result is not plausible if CRM_{-1} simply spreads out the sanctions effect over a period of years. The Robbery equation is always dominated by SIZE. The problem is compounded when, in addition, a correlation coefficient of .83 exists between SIZE and ROB_{-1}. It then becomes exceedingly difficult through regression analysis to distinguish the separate effect of the sanctions variables on the offense rate. Thus, we believe that the distributed lag model tends to understate the significance of the true relation between sanctions and the offense rate.

C. THE MARGINAL IMPACT OF THE RISK OF INCARCERATION

We have produced four directly comparable estimators of the deterrent effect of incarceration on UCR offenders. These are derived from the basic model and from the variant of the basic model in which it is assumed that sanctions operate with a distributed lag. If we view these four coefficients as equally valid estimators of the true coefficient of AR_I, we may develop a range of values that reflects the marginal effect of the risk of incarceration on UCR offense rates. Table 4.9 provides estimates of the number of offenses and incarcerations that occurred in Georgia in 1978, as well as the range of the AR_I coefficients, expressed, as always, in elasticity form. We have transformed reported offenses into actual offenses, using national victimization reporting rates. Then, by application of the simple computational formula given in the footnotes to the table, we have derived estimates of the marginal impact of incarceration on actual offense rates. These estimates appear in column (5).

These estimates indicate the reduction in the number of actual offenses that would have occurred if, ceteris paribus, one additional person were to be incarcerated and were to receive a combined prison and post-prison probation sentence equal to the mean value for that person's particular offense.

The range of values appearing in column (5) is extremely wide for some offense classes, attesting to the critical importance of one's choice of model and estimating procedure, and forcefully arguing against imputing great

TABLE 4.9

REDUCTION IN OFFENSES ASSOCIATED WITH ONE ADDITIONAL INCARCERATION: GEORGIA, 1978

Offense	Number of Reported Offenses (1000s) (1)	Number of Incarcerations (2)	Reporting Rate[a] (percent) (3)	Range of Coefficients[b] (4)	Reduction in Number of Offenses[c] (5)
Homicide	0.73	324	95	.19-.26	.45-.62
Rape	1.93	91	56	.13-.23	4.9-8.7
Assault	13.4	334	45	0-.07	0-6.2
Robbery	8.45	685	60	0-.04	0-.8
Burglary	75.0	1457	55	0-.19	0-17.8
Larceny	124.9	613	27	0-.14	0-106
Auto	18.1	209	71	.03-.19	3.7-23
All Violent	24.5	1434	64	.10-.30	2.7-8.0
All Property	218.0	2279	49	0-.41	0-80

[a]Based on criminal victimization data for the United States in 1975 (Federal Bureau of Investigation, 1977). The homicide value is a pure guess. The robbery (Burglary) rate is a weighted mean of commercial establishment and individual (household) rates.

[b]The values are derived from Tables 4.1, 4.2, 4.7 and 4.8. Positive coefficients were assigned a zero value. All other values in this column are negative.

[c]Column (5) is derived from 100,000 * Col. (1) * Col. (4) divided by Col. (2) * Col. (3).

precision or reliability to these (or to other) estimates of the deterrent effect. Nevertheless, despite these wide ranges, some useful information may be derived from the data. First, we observe, not surprisingly, that the marginal impact of incarceration is probably lower for violent offenses than it is for property offenses. Second, the marginal impact of incarceration for property offenses may be very high, indeed. Note, for example, the extreme value for larceny. Third, the data permit some rough estimates of the marginal productivity to be derived from a reallocation of law enforcement effort, and offer the possibility that a reallocation of criminal justice resources would be in the public interest. The following development touches, very briefly, on this latter issue.

In Table 4.10 we present the Sellin-Wolfgang severity-of-offense scores for UCR offenses, and based on their index, two indices of the "social saving" associated with an increase in the risk of incarceration. (The Sellin-Wolfgang index has been scaled so as to equal 100 for homicide.) The gross social saving index is derived by simply multiplying the values of column (5) of Table 4.9 by those of column (1) of Table 4.10. The index thus assumes that one incarceration reduces the social cost of a victimization by the value of the offense score given by the Sellin-Wolfgang index. The second social saving index accounts for a part of the social cost incurred by the criminal justice system as a result of increasing the rate of incarceration. It assumes that social cost is proportional to the expected length of sentence received by offenders for a particular offense, and is derived by dividing column (2) of Table 4.10 by the sentence length data of Table 3.2. This index explicitly, though imperfectly, accounts for the fact that the incarceration of a murderer demands a greater social outlay than the incarceration of a larcenist. Hence, the second index defines

Table 4.10

RELATIVE SOCIAL BENEFIT TO BE DERIVED THROUGH AN INCREASE IN THE INCARCERATION RATE

Offense	Sellin-Wolfgang Index[a]	Social Saving	
		Gross	Net
	(1)	(2)	(3)
Homicide	100.0	45-62	6-8
Rape	31.7	166-296	26-45
Assault	22.8	0-141	0-71
Robbery	12.0	0-10	0-2
Burglary	9.5	0-170	0-81
Larceny	6.0	0-635	0-489
Auto	9.9	163-229	108-152
All Violent	22.1	60-177	12-37
All Property	7.5	0-603	0-335

[a]From Sellin and Wolfgang (1964), with their index scaled to Homicide = 100.

social saving as a reduction in the social cost of victimization per year of time served under the new incarceration. It is important to note that the latter social saving concept assumes that the costs of increasing the probability of incarceration are the same across offense classes -- i.e., that it would cost just as much to arrest, charge, convict, and incarcerate an additional murderer as it would an additional larcenist. This proposition is patently false, of course. For remedy, one could devise a more comprehensive index, one that would combine sentence length with these other criminal justice system costs; but, in the absence of a sound theoretical underpinning, and without considerable detailed attention to various cost items, we doubt that this additional exercise would be productive.

Crude though these approximations are, they carry some important implications, and raise some important questions. While the wide ranges of the social saving measure relating to individual offenses considerably limit the inferences to be drawn from these data, one may, at least, observe that:

(i) The relative "pay-off" associated with increasing the incarceration rate for homicide and robbery, using either social saving concept, appears to be considerably less than that for some other offenses. When one considers that it costs more to apprehend, convict, and incarcerate homicide and robbery offenders, the relative pay-off, in terms of the net social saving concept, would be even less than that given in column (3).

(ii) It seems likely that resources would be better spent incarcerating more rapists and motor vehicle thieves. When criminal justice costs are accounted for, the differential advantage with respect to rape is considerably reduced, but does not disappear.

(iii) It is intriguing to note the potentially very substantial social benefit to be derived from increasing the rate of incarceration of convicted larcenists relative to the rate for violent offenders. If this group of offenders is as sensitive to incarceration as some regressions indicate, the relatively short prison sentences meted out to such offenders become, in cost/benefit terms, an exceedingly powerful instrument for social policy.

To conclude this brief section, two comments are in order: First, the foregoing estimates are meant to be suggestive. They are not quantitative measures of social benefit in the usual sense conveyed by numerical data. Better theoretical and empirical modeling, specifically addressed to the social benefit issue, could have provided more precise, quantifiable estimates, and would have been developed at this place had time and resource limitations not intervened. Second, the social saving concept refers only to the trade-off among UCR offenses at present overall levels of criminal justice expenditure. It does not address the broader question concerning the optimum allocation of criminal justice resources -- e.g. should more resources be allocated to police vis à vis corrections functions -- or of the yet broader questions concerning the optimum allocation of resources to criminal justice uses vis à vis other social uses. Thus, we may conjecture that resources would be better allocated if fewer robbers were incarcerated and, in their stead, more rapists were incarcerated, but we have no way of knowing from these data whether or not the aggregate level of incarceration ought to increase or decrease. Conceivably, incarceration rates for both robbery and rape ought to increase; though, if that were true, we might wish that the former increased less than the latter.

D. OTHER EQUATIONS IN THE BASIC MODEL

1. Sentence Length

The data presented in Tables 4.11 and 4.12 are designed to explain variations in sentence length across jurisdictions. The data are consistent with the sentencing variation literature in supporting the contention that the most important determinant of the length of prison sentence is the seriousness of the offense (or offenses) for which conviction was obtained. For every UCR offense category, and for both estimating procedures, the coefficient of score is positive and statistically significant. The sentencing variation literature also leads one to expect that another important determinant of length of prison sentence would be the offender's prior criminal history. Our data clearly support this view. All of the coefficients of prior are positive, and only those for larceny and motor vehicle theft are not statistically significant. Note, also, that the difference in magnitude between the coefficients of score vis à vis prior is so large that no formal statistical test is required to conclude that prior's effect on sentence length is smaller than that of score.

Did the Georgia courts discriminate among offenders on the basis of the offender's race, sex, or age? There is in these data a suggestion that they did. It appears that blacks were treated more leniently for violent offenses -- almost certainly for homicide -- and may have been treated somewhat more harshly for property offenses. (The latter observation is highly

TABLE 4.11

DETERMINANTS OF LENGTH OF PRISON SENTENCE: GEORGIA, 1978: OLS PROCEDURE

Dependent Variable	Independent Variables							
	nw	sex	age	score	prior	pbtn	CRM[a]	AR_I[a]
(1) Homicide	-.34 (3.59)	-.24 (2.86)	-.13 (1.82)	.40 (6.22)	.03 (1.85)	-.07 (7.55)	-.13 (3.84)	-.04 (1.33)
(2) Rape	-.05 (.27)	- -	-.11 (.52)	.29 (2.45)	.10 (2.90)	-.11 (3.75)	-.28 (2.73)	-.07 (.65)
(3) Assault	-.21 (1.35)	-.22 (1.41)	-.02 (.16)	.70 (7.34)	.08 (3.23)	-.12 (4.09)	-.18 (3.21)	-.07 (1.59)
(4) Robbery	.11 (1.25)	-.24 (1.81)	.17 (1.68)	.23 (3.41)	.10 (6.48)	-.05 (3.96)	-.15 (3.91)	-.00 (.12)
(5) Burglary	.08 (.94)	-.28 (1.21)	.09 (.81)	.57 (7.95)	.13 (7.11)	.00 (.28)	-.21 (3.81)	-.04 (.85)
(6) Larceny	-.08 (.66)	-.14 (1.20)	.44 (3.65)	.52 (8.41)	.03 (1.26)	-.09 (4.32)	-.39 (5.21)	-.17 (3.33)
(7) Auto	.05 (.35)	.51 (1.47)	.24 (1.47)	.86 (5.04)	.01 (.31)	-.16 (4.92)	-.25 (2.60)	-.14 (1.61)
(8) All Violent	-.07 (1.05)	-.12 (1.54)	.17 (3.07)	.61 (14.18)	.07 (6.29)	-.10 (9.93)	-.17 (6.51)	-.04 (1.74)
(9) All Property	.05 (.77)	-.19 (1.78)	.13 (1.64)	.62 (12.34)	.10 (6.52)	-.01 (.89)	-.25 (5.62)	-.07 (2.00)

TABLE 4.12

DETERMINANTS OF LENGTH OF PRISON SENTENCE:
GEORGIA, 1978: TSLS PROCEDURE

Dependent Variable	Independent Variables							
	nw	sex	age	score	prior	pbtn	CRM^a	AR_I^a
(1) Homicide	-.34	-.26	-.11	.38	.02	-.08	-.17	-.15
	(3.59)	(3.08)	(1.62)	(5.75)	(1.68)	(7.79)	(4.13)	(2.18)
(2) Rape	-.02	-	-.10	.29	.10	-.11	-.32	-.08
	(.11)	-	(.50)	(2.47)	(2.97)	(3.79)	(2.69)	(.53)
(3) Assault	-.20	-.23	-.01	.70	.08	-.12	-.20	-.06
	(1.26)	(1.45)	(.12)	(7.34)	(3.28)	(4.14)	(3.07)	(.79)
(4) Robbery	.12	-.24	.18	.23	.10	-.05	-.17	-.03
	(1.34)	(1.82)	(1.72)	(3.36)	(6.44)	(4.01)	(3.49)	(.51)
(5) Burglary	.12	-.27	.12	.57	.13	.00	-.31	-.17
	(1.36)	(1.16)	(1.09)	(7.87)	(6.89)	(.16)	(4.78)	(2.68)
(6) Larceny	-.06	-.15	.44	.51	.03	-.09	-.50	-.28
	(.47)	(1.31)	(3.68)	(8.30)	(1.20)	(4.49)	(5.61)	(3.68)
(7) Auto	.16	.63	.22	.87	.02	-.15	-.40	-.36
	(1.10)	(1.83)	(1.38)	(5.14)	(.64)	(4.94)	(3.56)	(2.86)
(8) All Violent	-.06	-.12	.17	.61	.07	-.10	-.19	-.02
	(.89)	(1.57)	(3.02)	(14.20)	(6.45)	(9.91)	(5.84)	(.58)
(9) All Property	.09	-.19	.15	.61	.10	-.02	-.36	-.20
	(1.33)	(1.86)	(1.84)	(12.27)	(6.42)	(1.34)	(6.90)	(3.98)

tentative.) The data do indicate that, with the exception of motor vehicle theft, the courts treated females more leniently than males. Finally, it seems that, on the average, older offenders were given longer sentences. However, this latter generalization must recognize some puzzling inconsistencies. We note, in particular, that some of the individual violent offenses have negative coefficients; and, most particularly, that older murderers appear to have received lighter sentences.

There is no question but that the court treated time incarcerated and time on post-prison probation as substitutable sanctions. All coefficients are negative; and, except for burglary and All Property offenses, they are all statistically significant. We have not had time to explore the theoretical determinants of the marginal rate of substitution between incarceration and probation, nor to fully analyze the empirical trade-offs suggested by Tables 4.11 and 4.12. We do note in passing, however, that the marginal rate of substitution varies substantially across offenses: for example, the ratios of in-prison time to post-prison probation time for homicide, robbery, and larceny are 1:1, 6:1, and 8:1, respectively.

In Chapter 2's theoretical development, we noted that the rational choice model did not provide a definite answer concerning the relation of sentence length to the crime rate, nor of sentence length to the incarceration rate. It is, therefore, of some interest to note that all eighteen coefficients of the CRM variable and all eighteen coefficients

of the incarceration variable are negative; that those of the CRM variable are all statistically significant, and that almost one-half of the incarceration coefficients are also statistically significant. It is beyond the scope of this research to identify the factors within the sentencing length model, developed in Chapter 2, that might produce such a strong inverse relation, or to explore the implications of the existence of this relation, but we believe the strength of these results and their stability across offense classes to be worth noting, to be potentially of great theoretical interest in explaining sentencing variation, and therefore to be worthy of further investigation.

2. The Demand for Law Enforcement Services

The empirical version of Equation (2.6) calls for a weighted crime rate. We have used the weights suggested by Sellin and Wolfgang (1964). We have chosen two indices for CRM, corresponding to our All Violent and All Property offenses. We rejected the use of a weighted aggregate that would emcompass all seven offenses because that aggregate was dominated by, and was virtually indistinguishable from, the property offense aggregate.

A very high degree of correlation exists among some of the regressors appearing in the empirical version of Equation (2.6), effectively frustrating any effort to derive meaningful coefficients for the full set of regressors appearing in the model. The correlation matrix at issue is as follows:

Table 4.13

REGRESSION RESULTS FOR THE DEMAND FOR LAW ENFORCEMENT: GEORGIA, 1978: OLS AND TSLS PROCEDURES

Equation	INCOME	MV	CRM_W Violent	CRM_W Property
PANEL A:	OLS Procedure			
(1)	.46 (1.98)	-.60 (1.32)	.12 (1.81)	-
(2)	.04 (.12)	-.27 (.57)	-	.28 (2.48)
(3)	-	-.23 (.82)	-	.29 (5.74)
(4)	-	.06 (.20)	.22 (4.70)	-
PANEL B:	TSLS Procedure			
(5)	.53 (1.41)	-.72 (1.02)	.10 (.74)	-
(6)	.30 (.66)	-.60 (.96)	-	.18 (1.09)
(7)	-	-.24 (.80)	-	.28 (4.92)
(8)	-	.16 (.49)	.27 (4.56)	-

	$CRM_{W.Prop}$ OLS	$CRM_{W.Prop}$ TSLS	INCOME OLS	INCOME TSLS	REV OLS	REV TSLS
$CRM_{W.Viol}$	.86	.94	.46	.54	.78	.92
$CRM_{W.Prop}$	--	--	.72	.76	.80	.84
INCOME	--	- -	--	--	.53	.53

Thus, in estimating the demand for law enforcement services, it is not possible to separate the effects associated with:

(i) violent offenses from those of property offenses;

(ii) property offenses from those associated with the community's ability to pay, as indexed by government revenue; and

(iii) violent offenses or property offenses from those associated with the community's ability and willingness to pay, as indexed by per capita income. Accordingly, we have estimated the demand for law enforcement using either violent offenses, as in Equations (1) and (5) of Table 4.13, or property offenses, as in Equations (2) and (6). Because we cannot use both CRM_W and REV in the same regression, we have dropped REV from the set of regressors. We believe, <u>a priori</u>, that the crime rate is a much more important influence on the demand for police services than is government revenue. Unfortunately, but unavoidably, the exclusion of REV from the regression very likely imparts an upward bias to the coefficient of CRM_W.

In the four formulations of the model presented in Table 4.13, the signs of the offense coefficient and of INCOME are always as expected, those of motor vehicle registrations are not. The weakest result for the coefficient of CRM_W is in Equation (6), and is probably due to the high degree of covariation that exists between INCOME and CRM_W in that equation.

Pairwise comparisons -- Equation (1) vs. (2); Equation (3) vs. (4); and their TSLS counterparts -- suggest that the coefficient of the property offense variable is greater than that of the violent offense variable; i.e., that the demand for protection against property crime is somewhat greater than the demand for protection against violent crime. If, indeed, the coefficients are significantly different,[3] it would only imply that, at present offense rate levels, a small percentage increase in property offenses would call for more police protection than an equally small percentage increase in violent crime. This interpretation of the coefficients assumes that the trade-off occurs at the margin, and within the neighborhood of existing rates for these two offense categories. The coefficients refer to marginal values. The <u>total</u> value of police protection against violent victimization <u>vis à vis</u> property victimization -- a concept analogous to the economist's notion of consumer surplus -- is not given by, nor may it be legitimately derived from, the regressions of Table 4.13.

3. Aggregate Arrests

Tables 4.14 and 4.15 present a set of regressions designed to explain the aggregate arrest rate. Initially, the empirical model was to use the All Violent offense rate as the statistical proxy for CRM in the

[3] We have not performed a formal statistical test for this.

first four regressions, and the All Property offense rate as the proxy in the next three equations.[4] Unfortunately, in the Georgia data set these offense aggregates display an unacceptably high degree of correlation with the other regressors in the equation. The more extreme correlation coefficients are presented in the following tabulation:

Offense	Other Regressor	Correlation Coefficient OLS	Correlation Coefficient TSLS
All Violent	COP	.61	.80
All Violent	SIZE	.80	.90
All Property	COP	.68	.90
All Property	INCOME	.72	.76

To resolve the multicollinearity problem, which is especially serious for the TSLS procedure, we have removed CRM from the set of regressors used to explain the aggregate arrest rate. The regression results for the model, so modified, appear in Tables 4.14 and 4.15.

[4] These aggregate offense rates were chosen because we expect that police productivity associated with an offense, X, is not only affected by X's offense rate, but also by the rates for those offenses that are somewhat "similar"; i.e., offenses that are likely to draw resources toward or away from X. For example, one would expect resource availability for the prevention and detection of robbery to be affected by the homicide rate. We chose to use two aggregates, one for violent offenses, the other for property offenses rather than one overall crime aggregate because we believe it possible that law enforcement responds differently to violent crime than it does to property crime. (Because the overall crime aggregate and the property aggregate are so similar, the latter also serves as a proxy for the overall rate.)

TABLE 4.14

DETERMINANTS OF THE AGGREGATE ARREST RATE: GEORGIA, 1978: OLS PROCEDURE

Dependent Variable	Independent Variable SIZE	P15-19	NW	INCOME	COP
Homicide	-.00 (.04)	-2.20 (1.34)	.06 (.20)	-.31 (.37)	-.91 (1.90)
Rape	-.06 (.99)	-2.79 (1.70)	.43 (1.43)	.34 (.41)	-.76 (1.58)
Assault	.05 (1.26)	-.31 (.25)	-.61 (2.70)	-.68 (1.07)	-.97 (2.64)
Robbery	-.14 (1.33)	-3.06 (1.02)	1.39 (2.57)	1.58 (1.04)	-2.02 (2.30)
Burglary	-.02 (.77)	-1.49 (2.07)	.18 (1.40)	-.66 (1.81)	-.40 (1.88)
Larceny	-.01 (.56)	-.45 (.61)	.21 (1.57)	-.43 (1.17)	-.33 (1.53)
Auto	-.09 (1.60)	-1.44 (.91)	.88 (3.10)	.46 (.58)	-1.16 (2.52)
All Violent	.01 (.45)	-1.04 (1.09)	-.30 (1.72)	-.47 (.99)	-.92 (3.30)
All Property	-.02 (.89)	-.81 (1.22)	.23 (1.89)	-.46 (1.39)	-.38 (1.94)

TABLE 4.15

DETERMINANTS OF THE AGGREGATE ARREST RATE: GEORGIA, 1978: TSLS PROCEDURE

Dependent Variable	Independent Variable SIZE	P15-19	NW	INCOME	COP
Homicide	-.00 (.04)	-2.18 (1.19)	.06 (.19)	-.28 (.25)	-.95 (.92)
Rape	-.06 (1.00)	-3.27 (1.80)	.32 (.94)	-.22 (.20)	-.09 (.09)
Assault	.06 (1.23)	.00 (.00)	-.54 (2.07)	-.31 (.35)	-1.41 (1.78)
Robbery	-.14 (1.27)	-3.82 (1.12)	1.22 (1.90)	.68 (.32)	-.94 (.49)
Burglary	-.02 (.77)	-1.68 (2.06)	.14 (.93)	-.87 (1.69)	-.14 (.29)
Larceny	-.01 (.57)	-.60 (.73)	.18 (1.15)	-.60 (1.16)	-.13 (.26)
Auto	-.09 (1.51)	-1.94 (1.06)	.77 (2.25)	-.13 (.11)	-.45 (.42)
All Violent	.02 (.44)	-.83 (.75)	-.25 (1.21)	-.23 (.33)	-1.21 (1.94)
All Property	-.02 (.88)	-.96 (1.28)	.19 (1.38)	-.64 (1.35)	-.16 (.37)

For comparison, the regressions with CRM included among the regressors are presented in Appendix B, Tables 4.14A and 4.15A. As the reader can verify, when CRM is removed from the regressions, the coefficients become more stable and more consistent across offense classes. In particular, the extreme and perverse COP coefficient that appears in the homicide equation when the TSLS procedure is used is eliminated. In addition, the very large, negative COP coefficients that occur in the other violent offense equations when the TSLS procedure is used become more moderate.

The results of the regression analysis may be summarized in the following brief remarks. Three of the regressors show little or no relation to the arrest rate. These are: INCOME, which appears to be totally unrelated to the arrest rate, and NW and SIZE, which offer minimal support for the contention that being black or living in a smaller community enhances one's chance of being arrested. On the other hand, the evidence suggests that a relation may exist between AR and P15-19. There is some indication that being a teenager reduced one's chance of being arrested, and that the reduction may have been substantial for violent offenses.

Finally, we observe a consistent inverse relation between law enforcement effort and the arrest rate. At first blush, the negative coefficient for COP appears to be counterintuitive. On closer consideration, however, one finds the empirical relation both plausible and instructive. If we let A and O represent the number of arrests and

number of offenses, respectively; then, from a linear representation of the theoretical arrest rate equation (Equation 2.3), one may obtain

$$\frac{\partial (A/0)}{\partial COP} = \beta,$$

where β is, of course, the true coefficient of the COP variable. After differentiating the right-hand term, we obtain

$$\left[(COP)(0/A)\right]^{-1} \left[\frac{\partial A}{A} - \frac{\partial 0}{0}\right]\left[\frac{\partial COP}{COP}\right]^{-1} = \beta \quad .$$

If higher offense rates induce the community to hire more police, and if more police produce more arrests, then β will be negative if, in addition, $\frac{\partial A}{A} < \frac{\partial 0}{0}$; i.e., if the percentage increase in offenses associated with, say, a one percent increase in COP is greater than the percentage increase in arrests associated with that same increase in COP.

From Table 4.13 we have seen that a one percent increase in COP has associated with it a three to ten percent increase in offenses. Thus, if a one percent increase in police services generated less than a three or ten percent increase in arrests, β would be negative. We have not had time to determine whether or not such a productivity condition existed, but it certainly would be a realistic one. Thus, the data of Tables 4.14 and 4.15 do not deny the existence of a positive marginal product for law enforcement effort (more police, more arrests). Rather, these data suggest that a positive productivity effect could have been offset by a relatively weak community response to an increase in the crime rate with respect to its demand for law enforcement services. If that demand were more elastic, the coefficient of COP would have been less negative.

4. The Incarceration Rate

The empirical explanation for the rate of incarceration uses the same set of regressors as does the equation for the aggregate arrest rate. Consequently, the same problem of multicollinearity among regressors exists; and, again, we have chosen to eliminate CRM from the set of independent variables. The results for the model, so modified, appear in Tables 4.16 and 4.17.[5] These data show that the size of community and its income level are not associated with an offender's risk of incarceration. The same is true for the age variable, except that its coefficient displays a much greater degree of variability. The results for the race variable are more mixed. Being black appears to increase one's chance of being incarcerated for robbery, may increase one's chance of incarceration for property offenses, and may reduce one's chance of incarceration for other violent offenses. Finally, we observe a strong inverse relation between law enforcement effort and AR_I, suggesting, again, that the community responds to an increase in the crime rate by purchasing more law

[5] The regressions with CRM included as an independent variable appear in Appendix B as Tables 4.16A and 4.17A. They differ from the regressions appearing in Tables 4.16 and 4.17 mostly in the TSLS procedure, and mostly for homicide. Without CRM, the other coefficients are more stable and consistent.

enforcement services, but that the increase is small relative to the increase in "good" arrests -- viz., arrests that lead to incarceration. More formally, the results imply that $\frac{\partial A_I}{A_I} < \frac{\partial O}{O}$ with respect to $\frac{\partial COP}{COP}$, where A_I and O are the number of arrests resulting in incarceration and the number of offenses, respectively.

TABLE 4.16

DETERMINANTS OF THE INCARCERATION RATE:
GEORGIA, 1978: OLS PROCEDURE

Dependent Variable	SIZE	P15-19	NW	INCOME	COP
			Independent Variable		
Homicide	.04 (.46)	-1.58 (.67)	-.21 (.50)	-1.11 (.94)	-.93 (1.35)
Rape	.03 (.28)	2.23 (.73)	-.36 (.65)	-.58 (.38)	.32 (.36)
Assault	.06 (.45)	8.07 (2.17)	-.39 (.57)	.43 (.23)	-2.83 (2.60)
Robbery	-.19 (1.09)	-2.16 (.43)	2.13 (2.37)	2.47 (.99)	-3.03 (2.08)
Burglary	.01 (.23)	-.51 (.30)	.21 (.71)	-1.34 (1.60)	-1.06 (2.17)
Larceny	.00 (.08)	.64 (.38)	.51 (1.68)	-.38 (.45)	-1.40 (2.86)
Auto	-.05 (.52)	-2.68 (.88)	.87 (1.59)	-.58 (.38)	-1.06 (1.19)
All Violent	.02 (.29)	2.67 (1.28)	-.11 (.30)	-.19 (.18)	-1.66 (2.71)
All Property	.01 (.16)	-.54 (.33)	.30 (1.03)	-1.13 (1.38)	-1.13 (2.37)

TABLE 4.17

DETERMINANTS OF THE INCARCERATION RATE:
GEORGIA, 1978: TSLS PROCEDURE

Dependent Variable	SIZE	P15-19	NW	INCOME	COP
			Independent Variable		
Homicide	.04 (.54)	-.36 (.15)	.06 (.13)	.32 (.21)	-2.65 (1.91)
Rape	.04 (.39)	4.55 (1.44)	.16 (.27)	2.13 (1.08)	-2.94 (1.64)
Assault	.07 (.56)	10.81 (2.80)	.22 (.31)	3.64 (1.51)	-6.68 (3.06)
Robbery	-.19 (1.04)	-2.58 (.46)	2.04 (1.96)	1.98 (.57)	-2.44 (.77)
Burglary	.01 (.24)	-.19 (.10)	.28 (.82)	-.98 (.83)	-1.50 (1.38)
Larceny	.01 (.08)	.77 (.40)	.54 (1.48)	-.22 (.18)	-1.59 (1.40)
Auto	-.05 (.46)	-1.08 (.34)	1.23 (2.06)	1.29 (.64)	-3.30 (1.78)
All Violent	.03 (.43)	4.57 (2.19)	.31 (.80)	2.02 (1.55)	-4.32 (3.66)
All Property	.01 (.19)	-.08 (.05)	.40 (1.19)	-.60 (.52)	-1.77 (1.67)

CHAPTER 5

EMPIRICAL RESULTS FOR NORTH CAROLINA

In this chapter we present the results of our effort to estimate Chapter 2's theoretical model, using data for the state of North Carolina. The format of this chapter generally parallels that of the preceding chapter. Nine offense categories are considered. Three variants of the principal equation are estimated. Both OLS and TSLS procedures are employed. The other equations of the model are also estimated. The results of this effort are presented in more summary fashion.

A. RESULTS FOR THE PRINCIPAL EQUATION: THE BASIC MODEL

The North Carolina data differ from that of Georgia in three respects: (i) It is not known whether the incarcerated offender received a split sentence. Hence, the variable, time to be served on post-prison probation (PBTN), does not appear in the regressions. (ii) The past criminal history of the incarcerated offender (used as an instrumental variable in the TSLS procedure) is only known in terms of the number of prior convictions. The nature of the offender's prior offense(s) is not known. (iii) The data refer primarily to the year 1979.

1. The Overall Pattern

At the most general level, we ask whether the three sanctions, AR_{NI}, AR_{I}, and SL have a deterrent effect on the seven Index offenses. We assume, as before, that the sample space consists of the joint distribution of the three sanctions and seven offenses, and that the 21 elements in the sample space are independent events. The data for the analysis consist of the 21 OLS coefficients appearing in Table 5.1 and the corresponding 21 TSLS coefficients appearing in Table 5.2. A one-sided null hypothesis is evaluated; viz., that sanctions either have no effect or have a positive effect on the offense rate. The alternative hypothesis is, of course, that the effect is negative. The sample data obtained from the two estimating procedures are presented in the following tabulation:

Procedure	Mean	Std. Dev.	t
OLS	-.18	.29	-2.87
TSLS	-.26	1.02	-1.18

$t_{.05}(20 \text{ d.f.}) = -1.72$ $t_{.01}(20 \text{ d.f.}) = -2.53$

The results reported in this tabulation are inconclusive. The set of coefficients derived from the OLS procedure are significant

TABLE 5.1

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE: ELASTICITIES AND ASYMTOTIC t-VALUES: NORTH CAROLINA, 1979, ORDINARY LEAST SQUARES

Eqn.	Depend. Var.	AR_{NI}	AR_I	SL	SIZE	P15-29	NW	EMPLOY	INC
		Independent Variable							
(1)	Homicide	-.03 (.36)	-.29 (1.73)	-.31 (1.19)	.03 (.75)	.04 (.11)	.30 (2.64)	-.68 (.35)	.12 (.21)
(2)	Rape	-.46 (2.41)	-.14 (1.67)	-.09 (.48)	.00 (.05)	1.67 (3.12)	.50 (3.49)	.95 (.37)	1.60 (2.33)
(3)	Assault	-.41 (1.45)	-.22 (1.29)	.25 (1.23)	.01 (.24)	-.01 (.02)	.31 (2.28)	.49 (.19)	-.08 (.13)
(4)	Robbery	-.20 (1.53)	-.43 (2.30)	.69 (2.36)	.18 (2.70)	2.42 (3.76)	.15 (.76)	-2.77 (.68)	.03 (.03)
(5)	Burglary	-.28 (1.08)	-.30 (2.76)	-.29 (1.44)	.04 (1.15)	.66 (1.67)	.16 (1.42)	.30 (.14)	.12 (.19)
(6)	Larceny	-.12 (.58)	-.42 (3.31)	-.02 (.11)	-.00 (.12)	.34 (.89)	.26 (2.73)	1.51 (.78)	1.69 (3.15)
(7)	Auto	-.69 (2.37)	-.18 (1.61)	.13 (1.45)	-.00 (.02)	.68 (1.37)	.14 (1.00)	2.09 (.80)	1.52 (1.98)
(8)	All Violent	-.32 (1.14)	-.38 (1.93)	-.04 (.15)	.02 (.52)	.24 (.61)	.31 (2.52)	-1.33 (.61)	.41 (.78)
(9)	All Property	-.14 (.51)	-.46 (3.58)	-.12 (.66)	.02 (.50)	.37 (.96)	.25 (2.61)	.56 (.30)	1.01 (1.88)

TABLE 5.2

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE: ELASTICITIES AND ASYMTOTIC t-VALUES: NORTH CAROLINA, 1979, TWO-STAGE LEAST SQUARES

Eqn.	Depend. Var.	Independent Variable: AR_{NI}	AR_I	SL	SIZE	P15-29	NW	EMPLOY	INC
(1)	Homicide	-.52 (1.32)	-1.34 (1.35)	-.37 (.20)	.02 (.22)	-.44 (.52)	.40 (1.24)	-3.70 (1.01)	.75 (.38)
(2)	Rape	-.26 (.56)	.33 (.78)	1.09 (1.36)	.03 (.50)	3.37 (2.15)	.05 (.12)	2.40 (.64)	1.50 (1.84)
(3)	Assault	-.87 (.79)	-.45 (.46)	.75 (.75)	-.01 (.20)	-.46 (.51)	.30 (.82)	-1.08 (.21)	-.24 (.33)
(4)	Robbery	-.01 (.01)	-1.65 (.43)	1.25 (.82)	.24 (1.46)	1.04 (.22)	-.00 (.01)	-13.78 (.42)	-3.25 (.36)
(5)	Burglary	-1.67 (1.91)	-.12 (.36)	.41 (.66)	.04 (1.02)	.20 (.34)	-.02 (.13)	-2.58 (.80)	-.88 (.90)
(6)	Larceny	1.32 (1.79)	-1.69 (2.71)	.61 (1.89)	.02 (.59)	.06 (.13)	.15 (1.24)	-5.24 (1.41)	-.72 (.57)
(7)	Auto	-2.22 (2.81)	-.23 (1.13)	.13 (.95)	.04 (.66)	-.08 (.13)	-.08 (.47)	-.43 (.15)	-.88 (.71)
(8)	All Violent	.84 (.21)	-1.17 (.60)	.42 (.24)	.01 (.11)	.67 (.28)	.65 (.59)	1.00 (.07)	1.07 (.48)
(9)	All Property	-2.58 (.52)	.14 (.08)	.23 (.54)	-.01 (.16)	-.45 (.35)	.23 (1.23)	1.74 (.31)	1.36 (.92)

at the 0.01 level, inclining one to reject the null hypothesis. Thus, they provide strong support for the deterrence hypothesis. On the other hand, the mean derived from the TSLS sample is negative, as one would expect if these sanctions acted to deter UCR offenders, but its value is statistically non-significant.

We note that these results are somewhat stronger than those obtained from the Georgia data set. The North Carolina means and <u>t</u>-statistics appearing in this tabulation are all greater in absolute value than their corresponding Georgia values.

2. Differences Among Sanctions

From an inspection of the pattern of coefficients appearing in Tables 5.1 and 5.2, one derives the strong impression that the three sanctions have very different effects on the offense rate. One notes, in particular, that most of the positive coefficients are associated with the sentence length variable. To evaluate the hypothesis that the sanctions have different effects, the coefficients were subjected to an analysis of variance. The results appear in Table 5.3. The results reported in this table are fairly conclusive. At the five percent significance level, the hypothesis that the three sanctions have similar effects must be rejected.

TABLE 5.3

ANALYSIS OF VARIANCE OF SANCTIONS COEFFICIENTS FOR SEVEN OFFENSES: OLS AND TSLS PROCEDURES

Procedure	Sum of Squares	d.f.	Mean Square	F Ratio
OLS				
Across Means	.572	2	.286	$F = \frac{.286}{.061} = 4.66$
Within	1.106	18	.061	
Total	1.678	20		
TSLS				
Across Means	7.06	2	3.53	$F = \frac{3.53}{0.77} = 4.57$
Within	13.90	18	0.77	
Total	20.96	20		

$F_{.95}(2, 18) = 3.55$ $F_{.99}(2, 18) = 6.01$

The following tabulation presents the means of the coefficients, together with their significance levels, based on the seven UCR offenses. The means of AR_{NI} and AR_I are negative, those of sentence length are positive. These data, taken in conjunction with the foregoing analysis of variance, support the view that, on the average, the risk of incarceration, and the risk of an arrest having a non-incarceration outcome are more likely to deter than the length of the prison sentence. The data also provide weak support for the belief that imprisonment is more of a deterrent than the other sanctions associated with arrest. Finally, we note that these results parallel those obtained from the Georgia data.

Means for Seven Offenses

Procedure	Sanction AR_{NI}	AR_I	SL
OLS	-.31**	-.28**	.05
TSLS	-.61	-.74*	.55

**, *: significant at the 1 and 5 percent level, respectively; one-tailed test, six degrees of freedom.

3. Sanction-Specific Analysis

The Deterrent Effect of the Risk of Incarceration

The primary evidence of Tables 5.1 and 5.2, taken in conjunction with the derivative evidence presented in the foregoing tabulation provides strong support for the contention that, overall, the risk of incarceration has a deterrent effect on UCR offenders. The unweighted means of the seven coefficients appearing in Tables 5.1 and 5.2 are statistically significant at the one and five percent level, depending upon the estimation procedure utilized. Moreover, all seven OLS coefficients, six of the seven TSLS coefficients, and three of the four aggregate offense coefficients are negative. Moreover, we shall argue below that the one positive aggregate offense coefficient misrepresents the underlying, true coefficient for this variable. Finally, the pattern displayed by

the individual coefficients supports the hypothesis that violent and property offenders are approximately equally responsive to the threat of punishment by incarceration.

We also note that these results parallel those reported for Georgia.

The Deterrent Effect of Other Arrest Outcomes

The evidence concerning the effect of arrests whose outcome does not result in imprisonment suggests that this variable also has a deterrent effect on UCR offenders. However, this conclusion is slightly less persuasive than that which relates to the risk of imprisonment. While all seven OLS coefficients are negative and their mean is statistically significant at the one percent level, one of the TSLS coefficients is positive and the TSLS mean is not statistically significant.

These results parallel those reported for Georgia, except in minor detail.

The Deterrent Effect of Length of Incarceration

The foregoing evidence provides no support for the contention that lengthening the term of imprisonment has a deterrent effect on UCR offenders. Three of the seven OLS coefficients and all but one of the TSLS coefficients are positive, and, therefore, are clearly inconsistent with the deterrence hypothesis. The TSLS coefficients for the offense aggregates are also positive. Because these results go beyond a simple negation of the deterrence hypothesis and almost affirm the existence of a positive CRM/SL relation, and because these data are so similar to those obtained for Georgia, they reinforce our suspicion that something is wrong either with the data or with the statistical design used in this analysis. In particular, we are made more intensely aware of the need to obtain a measure of sentence length that better approximates the unit cost associated with committing an offense, and also of the need to account for the various sentencing trade-offs alluded to in the preceding chapter.

4. Other Variables in the Principal Equation

SIZE

The data of Tables 5.1 and 5.2 are only mildly supportive of the view that larger communities tend to generate more crime. Two of the OLS coefficients and one TSLS coefficient have contrary signs. Moreover, the magnitudes of the eighteen coefficients tend to be relatively small. These results stand in contrast to those reported for Georgia, wherein the CRM/SIZE relation was found to be consistently positive and relatively large. The only important parallel that we discover is that in both the Georgia and North Carolina regressions robbery rates show the largest response to an increase in community size, suggesting the hypothesis that, indeed, there is some characteristic of larger communities, at least in this region of the country, that makes robbery more attractive.

P15-29 and NW

The data provide some support for the contention that non-whites and the population between the ages of fifteen and twenty-nine have greater criminal propensities. The OLS data are quite strong in this respect, the TSLS data less so, particularly with reference to the P15-29 variable.

These data parallel those for Georgia, except that, in the North Carolina sample, non-whites appear to be more predisposed to violent crime than to property crime. The Georgia data might incline one to the opposite view.

EMPLOY AND INCOME

The data do not support the view that better legitimate economic opportunities reduce crime. Six of the nine OLS coefficients and three of the nine TSLS coefficients have contrary, positive signs. Moreover, if anything, the results are worse where they should be better. That is, if the economic deprivation hypothesis were correct, one would expect potential offenders to resort to crimes that have an economic payoff, such as larceny and burglary. Yet these offenses seem just as likely to have perverse coefficients as offenses for which an economic payoff is more remote.

The income variable, which, in models such as ours, is customarily advanced as a proxy for the opportunities for illegitimate income, does not do much better than the legitimate opportunities variable. While eight of the nine OLS coefficients have the correct sign, five of the nine TSLS coefficients do not. Moreover, only one of the correct TSLS signs (All Property offenses) is associated with crimes from which one ordinarily expects an economic payoff.

These results are less supportive of their respective hypotheses than those obtained from the Georgia data. One should recall, however, that the latter were by no means conclusive results.

B. ALTERNATIVE SPECIFICATION OF THE PRINCIPAL EQUATION

The estimates presented above for the basic model for All Property offenses are not altogether satisfactory because of the existence of very high levels of multicollinearity among some regressors in the TSLS regression equation. In the next section of this report, the All Property offenses regression equation is reestimated with one or another of the offending regressors omitted.

The Georgia data were used to estimate three variants of the basic model. The first of these, in which an incapacitation variable was introduced, produced regression results which were so negative with respect to the existence of an incapacitative effect, that we have elected not to pursue the investigation of this effect any further. The other two variants of the basic model, one using the conditional probability of incarceration, the other the lagged crime rate, have been estimated from the North Carolina data and the results of this effort are reported below.

1. Variant One: The Basic Equation Corrected for Multicollinearity

In the basic model, the All Property offense coefficients show unusual instability between the OLS and TSLS procedures. In Table 5.4, in which columns (1) and (6) replicate the All Property offense data of Tables 5.1 and 5.2, it is seen that AR_I and P15-29 change sign and that the coefficient AR_{NI} undergoes a substantial increase in absolute value. These results are consistent with, and strongly suggest the existence of, a high degree of multicollinearity among the regressors. The following tabulation indicates that this is, in fact, the case.

Correlation Coefficient

Variable	OLS			TSLS		
	AR_{NI}	AR_I	P15-29	AR_{NI}	AR_I	P15-29
AR_{NI}	1.00	.41	-.48	1.00	.89	-.77
AR_I		1.00	-.38		1.00	-.50
P15-29			1.00			1.00

To reduce the ambiguities associated with, and engendered by, extremely high covariation among regressors, the All Property offenses regression was reestimated in several variants. Both OLS and TSLS results are provided in Table 5.4. The OLS data, which present no problem in this respect, may be used to assess the sensitivity of the various estimates to changes in procedure and specification.

When AR_{NI} is omitted from the regression, AR_I assumes a more likely, negative, possibly statistically significant coefficient in the TSLS equation, and remains virtually unchanged -- negative and statistically significant -- in the OLS equation. On the other hand, when AR_I is omitted from the regression, AR_{NI} retains its large, negative TSLS value but assumes a much larger _t_ ratio. We conclude from these equations that the aberrant, positive AR_I coefficient of Table 5.2 is a result of multicollinearity, that both arrest-based sanctions are, in fact, negative, and that AR_I's coefficient is, or comes close to being, statistically significant.

We indicated earlier that the employment variable is not systematically related to the offense rate. We use this occasion to reinforce the argument. We note the unusual variation in, as well as the perverse, positive signs for, this variable in Table 5.4, and we note, in addition, that the exclusion of INCOME from the regression converts the EMPLOY coefficient of column (1) from a positive 0.56 to a negative 1.05, and the coefficient of column (6) from a positive 1.74 to a negative 2.88. Given that the correlation coefficient between EMPLOY and INCOME is a mere -.11, this substantial change reflects extraordinary sensitivity to model

TABLE 5.4

VARIANT ONE OF THE BASIC MODEL: ALTERNATIVE ESTIMATES OF THE ALL PROPERTY OFFENSES EQUATION: NORTH CAROLINA, 1979

Independ. Variable	Regression Equation									
	OLS					TSLS				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
AR_{NI}	-.14	-	-.24	-.24	-.56	-2.58	-	-2.18	-1.02	-1.74
	(.50)		(.74)	(.99)	(1.84)	(.52)		(2.15)	(.48)	(3.67)
AR_{I}	-.46	-.46	-	-.50	-	.14	-.71	-	-.33	-
	(3.58)	(3.72)		(4.25)		(.08)	(2.07)		(.35)	
SL	-.12	-.13	-.06	-.11	-.04	.23	.08	.21	.14	.18
	(.66)	(.78)	(.27)	(.65)	(.16)	(.54)	(.25)	(.67)	(.42)	(.61)
SIZE	.02	.02	.01	.02	.01	-.01	.02	-.01	.01	-.00
	(.50)	(.54)	(.19)	(.52)	(.16)	(.16)	(.42)	(.15)	(.11)	(.10)
P15-29	.37	.44	.87	-	-	-.45	.15	-.37	-	-
	(.96)	(1.30)	(2.00)			(.35)	(.28)	(.49)		
NW	.25	.25	.24	.25	.24	.23	.30	.24	.27	.24
	(2.61)	(2.66)	(2.03)	(2.64)	(1.94)	(1.23)	(2.51)	(2.01)	(1.96)	(2.11)
EMPLOY	.56	.64	3.08	.04	2.34	1.74	-.77	1.32	.49	1.78
	(.30)	(.35)	(1.42)	(.02)	(1.03)	(.31)	(.28)	(.60)	(.12)	(.91)
INC	1.01	1.04	1.84	.91	1.79	1.36	.73	1.25	1.05	1.40
	(1.88)	(1.98)	(3.06)	(1.73)	(2.80)	(.92)	(.87)	(1.87)	(.90)	(2.39)

specification, and helps explain the instability of the coefficient in the basic model and in the two variants of the basic model whose results are presented below. More to the point, such sensitivity to model specification, as indicated in this table and elsewhere, coupled with frequent sign changes and wide variation in magnitude across offense categories, is inconsistent with the existence of an underlying, consistent, and strong relation between the offense rate and legitimate employment opportunity.

2. Variant Two: The Conditional Probability of Incarceration

In this version of the basic model AR and PI are substituted for AR_{NI} and AR_I. The coefficients derived from this version of the empirical model are reported in Tables 5.5 and 5.6. It is apparent from a comparison of the data presented in these tables with the data pertaining to the basic model (presented in Tables 5.1 and 5.2) that the reformulation of the measures of risk of arrest and incarceration produces almost no substantive change. The conclusions that were reached concerning the deterrent hypothesis are not materially affected by the basic model's respecification: the significance levels associated with arrest and imprisonment become slightly higher, on the average; the OLS sentence length coefficients remain almost identical to those of the basic model; and the TSLS sentence length coefficients change signs, in opposite direction, in the homicide and All Property offenses regressions. In addition, the conclusions that were

advanced concerning the other variables in the principal equation also remain essentially unchanged.

The only issue of consequence with respect to this variant of the basic model concerns the TSLS regression for All Property offenses (Table 5.6). The coefficients differ radically from their OLS counterparts in Table 5.5, and also differ radically from both the OLS and TSLS regressions of the basic model. The pattern of these coefficients -- the perverse sign of AR, the extraordinarily large magnitudes for AR, PI, INCOME, and P15-29, and the reversal of sign for EMPLOY and INCOME -- recalls the difficulties experienced in estimating the All Property offense equation in the basic model and suggests a similar origin.

The data confirm the expectation. The correlation coefficient between AR and PI is 0.85 and between PI and P15-29 is -0.76. To repair the difficulty, we follow the analytical procedure used for the other All Property offense equation. Column (6) of Table 5.7 reproduces the TSLS regression of Table 5.6, the other TSLS columns provide variants of that equation so as to assess the behavior of the deterrence variables under differing model specifications, and the OLS columns provide comparative results for regressors not afflicted by extreme levels of covariation. The results of Table 5.7 strongly support the view that the aberrant, positive AR coefficient derives from covariation with PI; and that, in fact, both AR and PI vary inversely with the All Property offense rate.

TABLE 5.5

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE, USING AGGREGATE ARRESTS AND THE CONDITIONAL PROBABILITY OF INCARCERATION: ELASTICITIES AND ASYMTOTIC t-VALUES: NORTH CAROLINA, 1979, ORDINARY LEAST SQUARES

Eqn.	Depend. Var.	Independent Variables							
		SL	AR	PI	SIZE	P15-29	NW	EMPLOY	INC
(1)	Homicide	-.31	-.35	-.21	.03	.05	.30	-.50	.21
		(1.15)	(1.45)	(1.37)	(.69)	(.14)	(2.58)	(.26)	(.37)
(2)	Rape	-.09	-.57	-.05	.00	1.71	.50	1.08	1.61
		(.43)	(2.55)	(.44)	(.06)	(3.18)	(3.26)	(.42)	(2.29)
(3)	Assault	.27	-.62	-.21	.01	.00	.30	.60	-.12
		(1.30)	(2.78)	(1.35)	(.33)	(.01)	(2.37)	(.24)	(.21)
(4)	Robbery	.61	-.45	-.16	.18	2.64	.14	-1.07	.33
		(1.95)	(2.07)	(.66)	(2.51)	(4.03)	(.67)	(.26)	(.25)
(5)	Burglary	-.33	-.51	-.34	.05	.66	.14	-.01	-.06
		(1.62)	(1.94)	(2.38)	(1.46)	(1.73)	(1.24)	(.00)	(.09)
(6)	Larceny	-.03	-.58	-.45	.01	.28	.22	.99	1.40
		(.18)	(2.30)	(3.06)	(.15)	(.73)	(2.23)	(.49)	(2.37)
(7)	Auto	.14	-.89	-.18	.00	.75	.13	1.88	1.44
		(1.55)	(2.88)	(1.41)	(.00)	(1.53)	(.96)	(.73)	(1.90)
(8)	All Violent	-.08	-.72	-.33	.02	.23	.29	-1.27	.35
		(.32)	(3.47)	(1.77)	(.63)	(.61)	(2.58)	(.59)	(.69)
(9)	All Property	-.13	-.60	-.50	.02	.31	.22	.06	.77
		(.77)	(2.18)	(3.57)	(.66)	(.83)	(2.36)	(.03)	(1.40)

TABLE 5.6

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE, USING AGGREGATE ARRESTS AND THE CONDITIONAL PROBABILITY OF INCARCERATION: ELASTICITIES AND ASYMTOTIC t-VALUES: NORTH CAROLINA, 1979, TWO-STAGE LEAST SQUARES

Eqn.	Depend. Var.	Independent Variables							
		SL	AR	PI	SIZE	P15-29	NW	EMPLOY	INC
(1)	Homicide	3.73	-4.06	-.78	-.21	-1.66	1.36	-2.38	5.65
		(.95)	(1.44)	(.59)	(.97)	(.94)	(1.46)	(.60)	(1.22)
(2)	Rape	.93	-.43	.24	.01	2.58	.18	.72	1.24
		(1.11)	(.77)	(.70)	(.24)	(2.19)	(.49)	(.22)	(1.41)
(3)	Assault	.64	-1.12	-.31	-.00	-.31	.28	-.64	-.27
		(.77)	(1.08)	(.41)	(.02)	(.38)	(.86)	(.11)	(.35)
(4)	Robbery	1.73	-.63	-2.64	.29	1.64	-.13	-6.35	-4.39
		(1.55)	(1.38)	(1.02)	(2.24)	(1.21)	(.36)	(.85)	(.91)
(5)	Burglary	.42	-1.86	.12	.04	.16	-.02	-2.69	-.89
		(.69)	(2.14)	(.28)	(.86)	(.27)	(.13)	(.85)	(.92)
(6)	Larceny	.50	-.86	-2.21	.07	-.51	-.07	-8.55	-2.55
		(1.67)	(1.91)	(2.57)	(1.42)	(.83)	(.37)	(1.73)	(1.29)
(7)	Auto	.13	-2.40	-.06	.04	-.03	-.08	-.39	-.83
		(1.07)	(2.88)	(.27)	(.66)	(.04)	(.46)	(.14)	(.66)
(8)	All Violent	.31	-.10	-1.12	.03	.93	.60	2.82	.93
		(.33)	(.06)	(.98)	(.53)	(.52)	(1.03)	(.27)	(.74)
(9)	All Property	-.76	13.40	-10.06	.30	4.67	.42	-27.11	-7.54
		(1.24)	(1.52)	(1.74)	(1.70)	(1.61)	(2.83)	(1.68)	(1.50)

TABLE 5.7

ALTERNATIVE ESTIMATES OF THE ALL PROPERTY OFFENSE EQUATION, USING VARIANT TWO: NORTH CAROLINA, 1979

Independent Variable	Regression Equation									
	OLS					TSLS				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SL	-.13	-.18	-.04	-.13	-.03	-.76	.06	.17	.09	.16
	(.77)	(.97)	(.20)	(.78)	(.13)	(1.24)	(.22)	(.58)	(.30)	(.55)
AR	-.60	-	-.45	-.73	-.74	13.40	-	-1.85	-.56	-1.58
	(2.18)		(1.33)	(3.19)	(2.52)	(1.52)		(2.14)	(.39)	(3.69)
PI	-.50	-.46	-	-.54	-	-10.06	-1.30	-	-1.03	-
	(3.57)	(3.02)		(4.04)		(1.74)	(2.32)		(.75)	
SIZE	.02	.02	.01	.02	.01	.30	.04	-.00	.03	-.00
	(.66)	(.64)	(.18)	(.70)	(.17)	(1.70)	(.91)	(.02)	(.53)	(.00)
P15-29	.31	.76	.71	-	-	4.67	.30	-.25	-	-
	(.83)	(2.26)	(1.62)			(1.61)	(.63)	(.36)		
NW	.22	.22	.24	.22	.24	.42	.27	.25	.27	.25
	(2.36)	(2.22)	(2.09)	(2.38)	(2.04)	(2.83)	(2.36)	(2.14)	(2.27)	(2.21)
EMPLOY	.06	1.10	2.70	-.42	1.96	-27.11	-2.95	.81	-2.43	1.22
	(.03)	(.56)	(1.26)	(.24)	(.90)	(1.68)	(.89)	(.35)	(.46)	(.62)
INCOME	.77	1.13	1.71	.66	1.61	-7.54	-.05	1.12	.11	1.25
	(1.40)	(1.99)	(2.87)	(1.25)	(2.62)	(1.50)	(.05)	(1.59)	(.07)	(2.09)

We might take advantage of this occasion to note the unusual instability of the employment coefficient compared to the other coefficients in the principal equation -- a characteristic of that variable which manifested itself, as well, in the basic model -- and to apply this evidence against the common view, and in favor of the general theoretical proposition that better employment opportunities do not necessarily elicit a reduction in criminal activity.

The thrust of the foregoing analysis is that the results for Variant Two generally parallel those of the basic model. They also parallel the basic model and Variant Two results obtained from the Georgia data. We conclude that the choice of variables to express the risk of being sanctioned by arrest and incarceration does not affect one's decision concerning the efficacy of arrest and incarceration as deterrence instruments.

3. Variant Three: The Distributed Lag Model

In this variant of the basic model, the lagged value of the dependent variable appears as a regressor in the principal equation. Tables 5.8 and 5.9 present the regression results for this variant. The most pertinent generalization that can be asserted from these data is that they diminish the strength of, but not the sense of, the conclusions which have been advanced concerning the deterrence hypothesis. While the differences between the results obtained from the basic model and those from its third variant are not dramatic, they are of peripheral interest because they provide another test of the robustness of the basic model. In the following tabulation, V.21A

TABLE 5.8

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE, ASSUMING SANCTIONS HAVE A DISTRIBUTED LAG: ELASTICITIES AND ASYMTOTIC t-VALUES: NORTH CAROLINA, 1979, ORDINARY LEAST SQUARES

Eqn.	Depend. Var.	Independent Variable								
		AR_{NI}	AR_{I}	SL	CRM_{-1}	SIZE	P15-29	NW	EMPLOY	INC
(1)	Homicide	.02 (.30)	-.21 (1.72)	-.25 (1.33)	.46 (4.61)	.02 (.90)	-.13 (.50)	.14 (1.67)	-1.60 (1.14)	-.06 (.15)
(2)	Rape	-.44 (4.09)	-.07 (1.49)	.10 (.84)	.72 (6.66)	-.04 (1.18)	.03 (.08)	.11 (1.13)	-1.90 (1.25)	.13 (.29)
(3)	Assault	-.02 (.11)	-.09 (.92)	.04 (.33)	.73 (6.91)	-.01 (.44)	-.21 (.91)	.09 (1.05)	.66 (.46)	.08 (.23)
(4)	Robbery	.05 (1.16)	-.00 (.03)	-.12 (1.09)	1.07 (14.04)	-.12 (3.96)	.06 (.23)	.12 (1.97)	2.66 (1.99)	1.00 (2.58)
(5)	Burglary	-.01 (.13)	-.12 (2.60)	-.17 (2.15)	.97 (10.84)	-.03 (2.07)	-.29 (1.65)	.07 (1.61)	.31 (.36)	.14 (.57)
(6)	Larceny	-.23 (3.66)	.00 (.06)	-.01 (.28)	1.02 (14.99)	-.05 (4.15)	-.17 (1.49)	.02 (.63)	1.03 (1.81)	.41 (2.28)
(7)	Auto	-.15 (1.42)	-.08 (2.15)	.06 (2.15)	.89 (13.35)	-.05 (2.58)	-.17 (.97)	.13 (2.84)	1.34 (1.57)	.73 (2.82)
(8)	All Violent	-.09 (.65)	-.08 (.78)	-.24 (1.78)	.75 (8.20)	-.01 (.75)	-.17 (.88)	.05 (.80)	-.35 (.32)	.29 (1.13)
(9)	All Property	-.14 (1.66)	-.07 (1.33)	-.06 (.97)	.99 (13.38)	-.04 (3.29)	-.23 (1.74)	.03 (.83)	.59 (.95)	.23 (1.28)

TABLE 5.9

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE, ASSUMING SANCTIONS HAVE A DISTRIBUTED LAG: ELASTICITIES AND ASYMTOTIC t-VALUES: NORTH CAROLINA, 1979, TWO-STAGE LEAST SQUARES

Eqn.	Depend. Var.	Independent Variable AR_{NI}	AR_I	SL	CRM_{-1}	SIZE	P15-29	NW	EMPLOY	INC
(1)	Homicide	-.46	-1.44	-.18	.22	.00	-.68	.39	-4.36	.75
		(1.54)	(2.12)	(.13)	(1.42)	(.07)	(1.21)	(1.65)	(1.65)	(.51)
(2)	Rape	-.11	.02	-.10	.72	-.01	.51	.02	-.45	.45
		(.32)	(.09)	(.37)	(4.16)	(.32)	(.70)	(.08)	(.18)	(.72)
(3)	Assault	-.94	-.67	1.02	.04	-.03	-.68	.33	-1.29	-.31
		(1.58)	(1.70)	(2.58)	(.13)	(1.10)	(2.12)	(1.84)	(.53)	(.92)
(4)	Robbery	.00	-.22	.13	.93	-.07	.25	.08	.33	.27
		(.03)	(.42)	(.41)	(5.14)	(.94)	(.57)	(.92)	(.07)	(.21)
(5)	Burglary	.22	-.02	-.19	1.10	-.05	-.17	.09	1.63	.55
		(.56)	(.14)	(.79)	(6.51)	(1.91)	(.70)	(1.24)	(.97)	(1.06)
(6)	Larceny	-.16	-.08	.02	.94	-.04	-.15	.03	.77	.40
		(.55)	(.26)	(.18)	(3.35)	(1.99)	(.77)	(.55)	(.65)	(1.82)
(7)	Auto	-.48	-.13	.07	.78	-.04	-.22	.11	.91	.41
		(1.10)	(1.78)	(1.63)	(5.69)	(1.65)	(1.15)	(1.62)	(.93)	(.85)
(8)	All Violent	-.81	.43	-.60	.79	-.01	-.51	-.18	-1.94	-.14
		(.68)	(.59)	(1.10)	(3.25)	(.33)	(.83)	(.53)	(.51)	(.19)
(9)	All Property	-.02	-.19	-.01	.89	-.03	-.19	.07	.20	.24
		(.07)	(1.22)	(.10)	(6.35)	(2.12)	(.81)	(1.22)	(.24)	(1.07)

we contrast the mean value of Variant Three's sanctions coefficients with those of the basic model, obtaining results that, in part, might have been predicted.

Procedure Model	Mean of Seven Coefficients		
	AR_{NI}	AR_I	SL
OLS Variant 3	-.11*	-.08**	-.05
Basic	-.31***	-.28***	.05
TSLS Variant 3	-.28*	-.36*	-.02
Basic	-.61	-.74**	.55

***, **, *: significant at the one, five and ten percent levels, one-tail, six degrees of freedom.

On the average, when CRM_{-1} is included as a regressor, the coefficients of AR_{NI} and AR_I are reduced to a third of their former absolute value if the OLS procedure is used, and to a half of their former value if TSLS is used. And, the inclusion of the lagged crime rate transforms the sentence length means into small negative elasticities.

If, momentarily, the All Violent offenses regression is set aside, the results for this variant of the basic model are seen to closely parallel those obtained from the Georgia data with respect to the

three sanctions variables. The AR_I coefficients, in particular, are remarkably similar. The others differ, sometimes substantially, from offense to offense, but, on the average, their orders of magnitude are similar.

The non-sanctions variables differ much more. SIZE, in particular, changes sign, implying, contrary to the other models and to the Georgia results, that, ceteris paribus, larger communities have lower crime rates. The changes in the other variables are less significant: for age, the relation to the offense rate, which was never particularly strong, simply disappears; for race and income it becomes stronger; and for EMPLOY it becomes weaker.

The TSLS estimate of the effect of the incarceration rate on the violent offense rate requires separate consideration for three reasons: (i) the positive effect which we have obtained for this regression is counterintuitive, (ii) the positive, aggregate coefficient derives from, and is a weighted mean of, individual coefficients whose contribution to the offense rate is predominantly negative,[1] and (iii) alternative, equally reasonable procedures and model specifications do yield negative effects for this variable. The source of the abnormality is, once again, covariation among regressors. Specifically, the correlation coefficient between AR_{NI} and AR_I is 0.75, that between AR_{NI} and CRM_{-1}

[1] Only rape has a positive coefficient, and it is quite small.

is -0.85. If we follow the general procedure employed above to deal with multicollinearity, we should estimate the equation without CRM_{-1}. When this is done, however, the regression reverts to the basic model, for which, as was shown above, it may be assumed that AR_I is negative. In the alternative specification, we omit AR_{NI} from the regression, in which case an AR_I coefficient equal to -.15 is obtained, with an absolute t value of 1.30. On the basis of these alternative specifications, and on the basis of the more general behavior of the AR_I coefficient which has been observed in the individual violent offense categories and in the other model variants, we conclude that, in the absence of extreme covariation, both AR_I and AR_{NI} would indicate the existence of a negative association with the All Violent offense rate.

C. THE MARGINAL IMPACT OF INCARCERATION

For each of the seven individual offenses, we have four directly comparable estimates of the deterrent effect of incarceration. These estimates are found in Tables 5.1, 5.2, 5.8, and 5.9. We follow the same procedure utilized in the preceding chapter, and develop a range of estimates of the marginal effect of incarceration on UCR offense rates. These estimates, which appear in Table 5.10, column (5), suggest the magnitude of the reduction in the number of offenses that would eventuate if one additional offender were to be incarcerated for a term equal to the mean time to be served for that particular offense. (These are, of course, point estimates of the

TABLE 5.10

REDUCTION IN OFFENSES ASSOCIATED WITH ONE ADDITIONAL INCARCERATION: NORTH CAROLINA, 1979

Offense	Number of Reported Offenses (1000's)	Number of Incar-cerations	Reporting Rate[a] (percent)	Range of Coeffi-cients[a]	Reduction in Number of Offenses[a]
	(1)	(2)	(3)	(4)	(5)
Homicide	1.1	394	95	.21-1.44	.06-.42
Rape	2.1	104	56	0-.14	0-.50
Assault	34	535	45	.09-.67	1.3-9.5
Robbery	7.8	680	60	0-1.65	0-3.2
Burglary	131	1440	55	.02-.30	.3-5.0
Larceny	242	1615	27	0-1.69	0-94.0
Auto	23	188	71	.08-.23	1.4-4.0
All Violent	45	1713	64	.08-1.17	.3-4.8
All Property	396	3243	49	.07-.71	1.7-17.7

[a]See respective footnote in Table 4.9.

true, underlying population coefficients. They should not be confused with statistical confidence intervals.)

The range of values appearing in column (5) is extremely wide for most offense classes, attesting to the critical importance of one's choice of model and estimating procedure, and arguing forcefully against an inclination to impute great precision to the deterrence estimates derived from this criminometric model -- or, for that matter, from criminometric models that were reviewed in Chapter 1. (In fairness to criminometric modeling, it should be pointed out that the use of the range to bracket a true regression coefficient emphasizes outlier values, and may overstate a model's sensitivity to specification error and to alternative estimating procedures.)

Despite their extreme variability, these data do provide important information. They imply, for example, that the marginal impact of incarceration is probably lower for violent offenses than it is for property offenses. They also suggest that the marginal impact of imprisonment for larceny may be very high.

The results and implications are similar to those obtained from the Georgia data.

The cost/benefit implications of incarceration are developed in the manner described in Chapter 4. The summary data are found in Table 5.11. Columns (2) and (3) provide alternative estimates of the social saving to be derived through incarcerating a UCR

offender. The data in each column are best treated as ordinal values, and comparisons between estimates should be made with this in mind. The Sellin-Wolfgang severity index provides the basic unit of value, and is combined in column (2) with the assumption of equal criminal justice costs per incarceration, regardless of the offense. Column (3) combines the Sellin-Wolfgang index with the assumption that criminal justice costs are proportional to the sentence length associated with individual UCR offenses.

These social saving estimates suggest that the relative payoff associated with increasing the incarceration rate for homicide, rape, and robbery, using either social saving concept, appears to be considerably less than that for some other UCR offenses.

Except for rape, which assumes greater importance in the Georgia sample, these results are similar to those obtained for Georgia. The North Carolina data also parallel those of Georgia in indicating that the social benefit from increasing the rate of incarceration of convicted larcenists is potentially very large.

We conclude this discussion by stressing the extremely speculative nature of these findings: The Sellin-Wolfgang index is controversial; the use of the range of coefficients focuses on extreme values; and, of course, the value of these estimates is substantially diminished because no statistically sound

TABLE 5.11

RELATIVE SOCIAL BENEFIT TO BE DERIVED THROUGH AN INCREASE IN THE INCARCERATION RATE: NORTH CAROLINA, 1979

Offense	Sellin-Wolfgang Index[a]	Social Saving	
		Gross	Net
	(1)	(2)	(3)
Homicide	100.0	6-42	.4-3.1
Rape	31.7	0-16	0-1.0
Assault	22.8	30-217	11-80
Robbery	12.0	0-38	0-4.0
Burglary	9.5	3-48	.8-14
Larceny	6.0	0-564	0-297
Auto	9.9	14-40	7-21
All Violent	22.1	7-106	.8-12
All Property	7.5	13-133	5-51

[a]See footnote, Table 4.10.

confidence interval has been assigned to them.

D. OTHER EQUATIONS IN THE BASIC MODEL

1. Sentence Length

The data presented in Table 5.12 are designed to explain variations in sentence length across jurisdictions. We report only the OLS results. It was seen in the Georgia study that the OLS and TSLS procedures yielded almost identical results. The same is true for North Carolina. Hence, the TSLS data need not be reported.

The North Carolina data support the Georgia data in affirming the commonly held opinion that the most important determinant of the length of prison sentence is the seriousness of the offense (or offenses) for which conviction was obtained. For every offense category, the ratio of the coefficient to its standard error exceeds 3.46, and in six of the nine offense categories the ratio is in excess of ten.

The North Carolina data cannot provide information concerning the seriousness of past offenses. The variable, priors, of necessity, is based on a simple count of the number of past convictions known to the criminal justice system. It is instructive, if not surprising, to observe that this crude, but commonly used measure provides a very poor explanation for sentence length. Except for assault, the coefficient of priors never

TABLE 5.12

DETERMINANTS OF LENGTH OF PRISON SENTENCE: NORTH CAROLINA, 1979: OLS PROCEDURE

Eqn.	Depend. Var.	sex	nw	age	priors	score	CRM	AR_I
(1)	Homicide	-.04 (2.75)	.02 (.45)	.16 (1.54)	.01 (.85)	.94 (7.36)	.09 (.59)	.20 (1.59)
(2)	Rape	-	.15 (1.74)	.23 (.90)	-.03 (1.02)	.53 (3.47)	-.10 (.34)	.09 (.28)
(3)	Assault	-.01 (1.42)	-.01 (.22)	-.13 (1.16)	.05 (2.64)	.78 (14.29)	-.00 (.01)	.02 (.12)
(4)	Robbery	-.01 (1.29)	.13 (2.55)	.53 (3.31)	.01 (.88)	1.32 (10.69)	.18 (1.04)	.37 (2.57)
(5)	Burglary	-.01 (1.70)	-.04 (1.10)	.48 (3.63)	.01 (.86)	1.18 (11.05)	-.19 (1.25)	-.16 (1.28)
(6)	Larceny	-.01 (.74)	.02 (.44)	.16 (1.25)	.00 (.12)	1.28 (12.23)	-.37 (2.16)	-.18 (1.31)
(7)	Auto	-.03 (2.12)	.22 (2.29)	.17 (.59)	-.07 (1.39)	1.12 (5.05)	.05 (.12)	-.21 (.64)
(8)	All Violent	-.02 (2.78)	.09 (2.78)	.30 (4.35)	.00 (.05)	1.17 (22.89)	.05 (.47)	.20 (2.35)
(9)	All Property	-.01 (2.00)	-.01 (.41)	.26 (2.88)	.00 (.26)	1.33 (18.92)	-.24 (2.08)	-.14 1.54

Independent Variables: sex, nw, age, priors, score, CRM, AR_I

approaches statistical significance, and in two instances it actually has a perverse sign. The most reasonable conclusion to be drawn from these data, particularly in view of the highly significant and more plausible results obtained from the Georgia sample, in which the priors index was based on the number and seriousness of past offenses, is that the measure used in the North Carolina sample is too crude, and that errors of measurement obscure the true relation between this variable and sentence length.

It is clear from these data, just as it was from the Georgia data, that the courts practic sex discrimination. All coefficients are negative, and four of them exceed their standard error by at least a factor of two. Except for larceny, the practical significance of the favored treatment accorded to females, in terms of time served, appears to be quite large, representing a twenty to thirty percent reduction in sentence, depending on the offense. Larceny has the smallest reduction -- twelve percent, which is equivalent to 2.7 months.

The courts appear to penalize older offenders. Except for assault, the age coefficients are positive, they have levels of significance at least as high as those of the sex variable, and their magnitudes suggest that age may have a larger absolute effect on sentence length than sex does. This finding is consistent with, and more conclusive than, that derived from the Georgia regressions.

Racial discrimination is more problematic. The coefficients of robbery and motor vehicle theft support the opinion that discrimination exists: both coefficients have t values in excess of two. But, on the other hand, burglary and assault have negative coefficients. Moreover, except for robbery and auto theft, the coefficients have rather small elasticities. Thus, these data and the Georgia data possess the consistency of presenting mixed results concerning the existence of racial discrimination. They diverge in suggesting that, in North Carolina, perhaps, non-whites (i.e. blacks) were treated, on the average, more harshly for violent offenses and less harshly for property offenses -- the opposite of the impression conveyed by the Georgia data.

We have shown that theory provides no definite expectation concerning the response of sentence length to crime rates or to incarceration rates; that the outcome depends upon the environment from which the sample was drawn. In the last chapter, on the basis of the Georgia sample, theoretical indeterminacy was displaced by a strong, consistent, inverse empirical relation between sentence length and offense and incarceration rates. The North Carolina data, on the other hand, fail to discover an equivalent regularity, and thereby reassert and underscore theory's essential indeterminacy. While sentence length appears to vary inversely with property offenses, thereby supporting Georgia's regressions, it bears no relation to the violent offense rate. And, while sentence length is positively -- not negatively! -- related to the incarceration rate for violent offenders, it stands unrelated to the incarceration rate for property offenders. These results, so very different from those obtained for Georgia, suggest the existence of strong interstate variation in sentencing practice; variation that derives, perhaps, from factors associated with the individual state's criminal justice system or its cultural milieu, but almost surely does not derive from state-specific socioeconomic or demographic influences.

2. The Aggregate Arrest Rate

Analysis of the aggregate arrest rate in North Carolina labors under precisely the same difficulty as analysis of the Georgia arrest rate, viz., the necessity to eliminate either the lagged offense rate or the law enforcement variable from the set of explanatory variables. This must be accomplished because of the extremely high degree of correlation that exists between the two variables, especially at the TSLS level. Accordingly, we present a more limited regression analysis of the arrest rate, omitting CRM_{-1} from the set of regressors, just as was done with the Georgia sample. The results are presented in Tables 5.13 and 5.14.[2] As the reader may verify, eliminating CRM_{-1} strengthens, somewhat,

[2]Parallel regressions, with CRM_{-1} included in the regressions, are presented in Appendix B, Tables 5.13A and 5.14A.

the negative relation of COP to the arrest rate, and has, at most, a minor effect on the other coefficients. CRM_{-1}, itself, is significantly negative in the OLS regressions, but less so in the TSLS regressions.

The results of the regression analysis may be summarized as follows: There is weak evidence to support the hypothesis that arrest rates are higher in smaller communities, that teenagers are less likely to be arrested, and that non-whites are "over-arrested" for some offenses and "under-arrested" for others. There appears to be no relation between arrests and income level. Finally, and most importantly, we observe a consistent, strong, inverse relation between law enforcement effort and the arrest rate.

These results closely parallel those obtained from the Georgia sample.

3. The Incarceration Rate

The empirical explanation for the rate of incarceration relies upon the same set of regressors as the aggregate arrest rate; and, subjected to the same indeterminacy occasioned by multicollinearity, is required to proceed without CRM_{-1} as an explanatory variable. The results of the regression analysis, using the reduced set of regressors, appears in Tables 5.15 and 5.16.[3] These data may be

[3]The results with CRM_{-1} included among the regressors appears in Tables 5.15A and 5.16A of Appendix B.

TABLE 5.13

DETERMINANTS OF THE AGGREGATE ARREST RATE: NORTH CAROLINA, 1979: ORDINARY LEAST SQUARES PROCEDURE

Eqn.	Depend. Var.	SIZE	P15-19	NW	INC	COP
		Independent Variables				
(1)	Homicide	-.01 (.45)	.08 (.16)	.09 (.88)	.48 (.90)	.16 (.47)
(2)	Rape	-.04 (.84)	-1.51 (2.51)	.46 (3.74)	-.49 (.72)	-.84 (1.99)
(3)	Assault	-.06 (1.41)	-.77 (1.36)	.02 (.13)	.46 (.72)	-.60 (1.52)
(4)	Robbery	.05 (.76)	.76 (.76)	-.40 (1.93)	-2.57 (2.28)	-.10 (.14)
(5)	Burglary	.00 (.06)	-.27 (.62)	-.06 (.67)	-.79 (1.57)	-.51 (1.62)
(6)	Larceny	-.01 (.29)	-.77 (1.75)	.16 (1.82)	.04 (.08)	-.34 (1.11)
(7)	Auto	.01 (.15)	-.24 (.43)	-.10 (.92)	-1.56 (2.53)	.22 (.56)
(8)	All Violent	-.05 (1.45)	-.75 (1.45)	.04 (.37)	.32 (.55)	-.72 (1.97)
(9)	All Property	-.00 (.19)	-.59 (1.64)	.07 (.92)	-.35 (.87)	-.36 (1.42)

TABLE 5.14

DETERMINANTS OF THE AGGREGATE ARREST RATE: NORTH CAROLINA, 1979: TWO-STAGE LEAST SQUARES PROCEDURE

Eqn.	Depend. Var.	SIZE	P15-19	NW	INC	COP
		Independent Variable				
(1)	Homicide	-.01 (.40)	.10 (.20)	.09 (.88)	.56 (.83)	.05 (.09)
(2)	Rape	-.02 (.44)	-1.18 (1.98)	.58 (4.35)	.61 (.70)	-2.19 (2.64)
(3)	Assault	-.05 (1.28)	-.71 (1.19)	.04 (.29)	.67 (.84)	-.87 (1.22)
(4)	Robbery	.09 (1.35)	1.42 (1.50)	-.17 (.82)	-.36 (.26)	-2.82 (2.24)
(5)	Burglary	.02 (.52)	-.02 (.05)	.03 (.27)	.06 (.10)	-1.55 (2.83)
(6)	Larceny	.00 (.03)	-.58 (1.33)	.23 (2.38)	.67 (1.07)	-1.12 (1.90)
(7)	Auto	.02 (.58)	.07 (.13)	.00 (.01)	-.54 (.74)	-1.05 (1.66)
(8)	All Violent	-.05 (1.29)	-.65 (1.23)	.07 (.64)	.64 (.93)	-1.11 (1.91)
(9)	All Property	.00 (.19)	-.42 (1.17)	.13 (1.65)	.24 (.47)	-1.09 (2.25)

TABLE 5.15

DETERMINANTS OF THE INCARCERATION RATE: NORTH CAROLINA, 1979:
ORDINARY LEAST SQUARES PROCEDURE

Eqn.	Depend. Var.	Independent Variable				
		SIZE	P15-19	NW	INC	COP
(1)	Homicide	-.04 (.65)	-1.04 (1.36)	.38 (2.40)	.13 (.15)	-.32 (.60)
(2)	Rape	-.13 (1.10)	-2.29 (1.39)	.87 (2.54)	.87 (.47)	-.61 (.53)
(3)	Assault	-.05 (.73)	-.89 (.97)	.29 (1.54)	.02 (.02)	-.42 (.64)
(4)	Robbery	.00 (.03)	-1.19 (.98)	.01 (.06)	-2.47 (1.81)	-.22 (.25)
(5)	Burglary	.02 (.26)	-.89 (.93)	.47 (2.36)	-.37 (.35)	-1.90 (2.83)
(6)	Larceny	-.01 (.16)	-.64 (.90)	.20 (1.36)	-.41 (.52)	-1.46 (2.94)
(7)	Auto	-.09 (1.04)	.09 (.07)	.51 (2.13)	1.47 (1.12)	-1.36 (1.66)
(8)	All Violent	-.08 (1.50)	-.92 (1.22)	.30 (1.91)	.63 (.75)	-.40 (.76)
(9)	All Property	.00 (.03)	-.66 (.96)	.33 (2.27)	-.37 (.47)	-1.63 (3.33)

TABLE 5.16

DETERMINANTS OF THE INCARCERATION RATE: NORTH CAROLINA, 1979:
TWO-STAGE LEAST SQUARES PROCEDURE

Eqn.	Depend. Var.	Independent Variable				
		SIZE	P15-19	NW	INC	COP
(1)	Homicide	-.04 (.68)	-1.09 (1.37)	.36 (2.08)	-.03 (.03)	-.13 (.13)
(2)	Rape	-.12 (.99)	-2.12 (1.23)	.93 (2.41)	1.45 (.58)	-1.32 (.55)
(3)	Assault	-.04 (.68)	-.85 (.89)	.31 (1.49)	.16 (.12)	-.58 (.51)
(4)	Robbery	.03 (.31)	-.74 (.61)	.17 (.63)	-.95 (.55)	-2.09 (1.28)
(5)	Burglary	.04 (.63)	-.41 (.43)	.63 (2.96)	1.24 (.90)	-3.88 (3.08)
(6)	Larceny	.00 (.03)	-.46 (.58)	.26 (1.50)	.19 (.17)	-2.21 (2.09)
(7)	Auto	-.07 (.87)	.35 (.30)	.61 (2.35)	2.37 (1.48)	-2.47 (1.76)
(8)	All Violent	-.08 (1.43)	-.91 (1.17)	.30 (1.81)	.67 (.66)	-.45 (.53)
(9)	All Property	.02 (.30)	-.39 (.51)	.42 (2.46)	.55 (.49)	-2.76 (2.63)

summarized as follows: Incarceration rates may be higher for violent offenses in smaller communities and are probably unrelated to community size for other offenses. Incarceration rates tend to be higher for non-whites, lower for younger persons, appear not to be related to income level (the results here are unusually variable), and are definitely inversely related to the level of law enforcement effort.

These results are consistent with, and, in their important elements, closely parallel the regression results that have been developed from the Georgia sample.

CHAPTER VI

SUMMARY AND CONCLUSIONS

A. THE REPORT IN OVERVIEW

1. Prior Research

The thesis that legal sanctions act as a deterrent to criminal activity has had a long and varied intellectual history. In the last decade, interest in the thesis has measurably quickened. Whether the heightened interest derives from the technically sophisticated, intellectually elegant garb that this thesis has recently assumed, or from the growing ranks of disaffected adherents to the rehabilitative ideal, which, as an alternative crime-control strategy, has experienced a widespread and serious erosion of support, is not material. The fact is, that one may observe and easily document a substantial increase in theoretical and empirical research activity directed at the deterrence hypothesis in recent years.

We have argued that the deterrence hypothesis derives from the particular and fundamental interpretation of human behavior that holds that individuals choose freely among competing, alternative actions, including decisions concerning legitimate or criminal actions, and that they are rational decision-makers whose ultimate objective is to maximize their own individual wellbeing. Under the somewhat stringent assumptions of the early formulation of the rational choice theory, initiated by Becker (1968) and advanced, principally, by Ehrlich (1973), one can readily and unambiguously deduce the proposition that, ceteris paribus, an increase in either the risk or the severity of sanctions reduces the crime rate. We have also shown that this basic formulation of the rational choice model has been modified in certain important respects, and that these modifications, while they have the virtue of imparting greater realism to the model, also prove the maxim that virtue imposes its own special costs, for realism is purchased, in this instance, with the forfeiture of an a priori certain outcome. In brief, the more general model does not produce the certain conclusion that sanctions deter.

As a result of this theoretical indeterminacy, the value of, and need for, empirical research becomes substantially greater. It is gratifying to note, therefore, that enormous empirical research effort has been directed at the deterrence hypothesis in recent years.

In the first chapter, we reviewed a subset of this recent research. Specifically, we surveyed all quantitative, methodologically sophisticated studies published in the English language since 1970 that relate to one or more of the seven UCR Index offenses, and that consider the effects of arrest or incarceration rates, or the length of prison sentence, on these offenses.

Our review consists, basically, of a simple counting of the number of models that support the deterrence hypothesis, together with a very rough measure of the degree to which, in some aggregative sense, their results might be viewed as statistically significant.

We recognize that the models whose results are summarized in Chapter 1 vary greatly in structure and specificity, and that the simple categorization that we employ suppresses information, understates diversity of findings, and may grossly misrepresent some of the findings. We recognize, also, that many of the analyses reported as, or interpreted to be, independent research, or that purport to be independent tests of a particular hypothesis, use the same general data set and many of the same variables, so that, to an imprecise but possibly significant degree, the results reported in the individual studies, or in individual regression equations within a particular study, do not qualify as independent data. For example, all studies, but one, that use length of prison sentence as a variable, use the same state-level cross-sectional data. And, for another example, about half of the tests of the hypothesis that the risk of incarceration deters UCR offenders derive from a single study; and, within that study, from relatively minor variations in a single, basic model, applied to one data set. Finally, we readily acknowledge the inadequacies inherent in a summary that focuses on signs of coefficients and relative magnitudes, and that ignores the real, often significant, but difficult or impossible to quantify, differences among studies that may be attributed to research design, data quality, etc. But, for all that, the review provides a meaningful and, we hope, reasonably faithful summary of an important subset of the deterrence literature.

In the surveyed literature, the deterrence hypothesis was evaluated on the basis of four measures of legal sanctions: the risk of arrest and incarceration, the length of prison sentence, and the conditional probability of incarceration, given that an arrest was made. Data for the evaluation derived from approximately a dozen separate studies for each sanction. To analyze the data in these studies, we adopted a compromise procedure between the one extreme that would treat each individual regression equation as if it were an equally valid, equally important observation, and the other extreme that would treat the composite result of all regression analyses contained within an individual study as if it were a single, valid observation.

We adopted a conservative test procedure with which to evaluate this literature, one which was less likely to cause rejection of the null hypothesis concerning the association of the crime rate to sanctions. Based on the results of this test procedure, we feel compelled to reject the null hypothesis of no association with respect to each of the four sanctions measures. The weakest test result pertained to the conditional probability of incarceration, and even here the test statistic required formal rejection of the null hypothesis at the 0.01 level. The conclusion that must be drawn from this analysis is that the literature which we have surveyed overwhelmingly supports the deterrence hypothesis.

Despite the formal, statistical strength of these results, the conclusion that they engender concerning the deterrence hypothesis requires important qualification. We note, first of all, that most of the data were drawn from the United States, and represent the populations of large geographical units: mostly states or the United

States taken as a single unit. The conclusion that sanctions are efficacious may not be valid with reference to populations represented by smaller geographical units. Second, all of the studies that have been reviewed can be faulted for one or another deficiency: improperly identified equations, potentially serious measurement error, omission of crucial variables (<u>including neglect of the incapacitation effect</u>), etc. It may be that, on balance, these deficiencies systematically bias study results in favor of the deterrence hypothesis. (The latter omission clearly does.)

2. <u>Research Objectives and Design</u>

With the foregoing reservations in mind, it is nonetheless true that the most reasonable, present working hypothesis that may be derived from this literature is that deterrence works -- at least with respect to UCR offenders. But the critics of the studies reviewed do substantiate the need to repair the deficiencies of past research and do point up the need for tests of the deterrence hypothesis that use other population aggregates. The present research effort addresses some of the issues found in this criticism. It achieves its objective by analysis of a data set that has two essential and desirable properties: (i) the data refer to population subsets that have not heretofore been analyzed, and (ii) the data are of such quality and detail that they deflect several of the criticisms of past research, without, at the same time, becoming vulnerable to serious, alternative criticism.

In Chapter 2, we have developed a theoretical model which explains offense rates as part of an interacting system of equations. The model was specifically designed to deal with the deterrence issue. The variables of direct interest appearing in the model are individual offense rates, the offense rate aggregate, and four sanctions instruments: the probability of arrest whose outcome is not incarceration; the probability of arrest resulting in incarceraration; the length of prison sentence; and the length of post-prison probation. (The arrest and incarceration sanctions were also given a conditional-probability-of-incarceration formulation.)

The theoretical model, in its structure and choice of variables, is a direct descendant of the criminometric models reviewed in Chapter 1. 1. The features of our model which distinguish it from its predecessors have more to do with the unique empirical possibilities offered by the data used in this Report than they do with theoretical innovation. At the base of our model -- and the models reviewed in Chapter 1. The features is the rational choice model, which is presumed to govern the individual's decision to commit or not to commit an offense, and also to govern the community's decision concerning the amount of crime-preventing, law enforcement effort that ought to be purchased.

The empirical model used in this Report is similar to the models reviewed in Chapter 1 in many respects. However, our model differs from its predecessors in that it includes two unconventional variables: one to measure the incapacitation effect, the other to appraise the deterrent effect of probation. Our model also differs from its predecessors by providing better, or at least different, but equally good, statistical proxies for two other variables: an employment

opportunities index that, we believe, more accurately reflects the experience of the population of potential offenders, and an <u>ex ante</u>, rather than <u>ex post</u>, measure of sentence length, i.e., a measure that looks forward from the inmate's time of conviction to his most likely release date, rather than backward, at time of release, to the number of years served.[1]

Our empirical model differs from its predecessors in that it treats sentence length as an endogenous variable, functionally related to the demographic, socioeconomic, and criminal history attributes of individual offenders. It differs also in that very few deterrence studies have utilized data disaggregated to the degree we have been able to achieve, and none have used the particular units of observation upon which our empirical research is based. And, finally, most empirical analysis has been confined to homicide and to aggregates of UCR offenses. We analyze all seven individual offenses.

The data used in this study are of two kinds: macro-data, whose unit of observation is the judicial district, and micro-data, whose unit of observation is the individual incarcerated offender. Depending upon the variable considered, these data either pertain to all judicial districts or to all offenders incarcerated for one or more UCR offenses within a given year. Two geographical regions are considered: the states of Georgia and North Carolina. The data for Georgia refer

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[1]Avio and Clark (1978) provide an excellent discussion of the advantages of <u>ex ante</u> measures, as compared to the almost universally used <u>ex post</u> measures.

to 1978, those for North Carolina to 1979.

Several versions of the theoretical model were estimated. We began with a basic model. This model was then modified so as to appraise the sensitivity of the deterrence measures to different model specifications. As particular innovations, we introduced an incapacitation variable into the principal equation, we assumed that sanctions operate with a distributed lag, we assigned a conditional probability format to the risk of incarceration, and we employed several different sets of regressors for the principal equation when extreme multicollinearity rendered measured deterrence effects untrustworthy.

Most specifications of the empirical model were estimated by ordinary least squares and two-stage least squares procedures. In all specifications of the model, linear functions were assumed, and the regressions were fitted to the natural numbers of the model's variables.

3. Empirical Results

The empirical results obtained from the Georgia and North Carolina data sets, presented and analyzed separately in Chapters 4 and 5, are summarized in this section of the chapter. Because the results obtained from analysis of the Georgia and North Carolina samples are similar, the following discussion has been structured by, and focuses upon, their common features. Accordingly, except where noted, the summary shall refer to the common behavior and experience of these two states.

The Deterrence Hypothesis

The data that have been examined strongly support the contention that legal sanctions deter criminal activity. However, this generalization must be qualified by recognizing that the existence of a deterrent effect receives stronger confirmation for some sanctions and some offenses than it does for other sanctions and offenses.

The strongest confirmation for the existence of a deterrent effect relates to the risk of incarceration. Whether we rely upon the basic model or one of its variants, and whether we use OLS or TSLS procedures, with few exceptions, we obtain negative coefficients, often with magnitudes exceeding twice their standard errors. Moreover, the exceptional, positive coefficients arise from the inability of multiple regression procedures to distinguish the separate effects of two regressors that covary to an extreme degree. In these instances, through secondary analysis, we have shown that the most reasonable inference to be drawn from the data is that the separate contribution of an increase in the risk of incarceration is to reduce the crime rate. Finally, we note that this conclusion is not affected by the relatively minor variation in model specification obtained from the omission of individual non-sanctions variables -- such as employment and income -- from the principal equation. While their omission sometimes produces significant changes in the other non-sanctions regression coefficients, the omission tends, in the usual case, not to affect the incarceration rate coefficient, (or the other sanctions coefficients); or, when there is a measurable effect, more often the tendency is to reinforce, rather than to weaken, the deterrence hypothesis.

Although we may safely reject the null hypothesis, the variation in the incarceration rate coefficient across models and estimating procedures is, nonetheless, very large, thereby considerably limiting the value and practical significance of the inferences to be drawn from our data. The principal results to be obtained from an analysis of the incarceration rate coefficients relate to the social gain to be obtained from a marginal increase in the incarceration rate. It varies substantially across offense categories; is not demonstrably higher for violent offenses; seems to be especially low for robbery; and may be very high for larceny.

The existence of an inverse relation between offense rates and arrests whose outcome is not incarceration has also been established, though with somewhat less certainty than for the incarceration rate relation. The hypothesis was sustained across models and procedures and was not measurably weakened by variations in model specification occasioned by alterations in the set of non-sanctions regressors used in the regression equation. However, on balance, the arrest coefficients are of smaller magnitude, are more dispersed, and are somewhat more likely to be positive than those of the incarceration coefficient. Because of the greater variability in its coefficient, no attempt was made to measure the marginal impact of arrest on offenses, using our social saving concept.

The evidence concerning the deterrent effect of post-prison probation is limited to the Georgia sample. The pattern of coefficients derived from this sample are such that the remarks that have been addressed to the arrest rate variable may be applied to the probation variable

in toto; i.e., we believe that the probation component of split-sentencing acts as a deterrent to UCR offenders.

The evidence concerning the sentence length variable, on the other hand, is not consistent with the deterrence (or incapacitation) hypothesis. Indeed, the results derived from the Georgia sample are, at best, neutral, and oftentimes are actually inconsistent with that hypothesis. In Chapter 4 we have explored the potential sources of the counterintuitive, aberrant behavior of the sentence length variable. Our conjecture is that the statistical measure of sentence length employed in this Report is probably seriously biased toward the generation of positive coefficients.

The Incapacitation Issue

Two related questions arise concerning the incapacitation effect. First, does incapacitation have a significant effect on the offense rate? We searched for a relation between the offense rate and the rate of release of convicted offenders, intending the latter to be a proxy for an incapacitation variable. We were unable to establish the existence of that relation. Thus, while we affirm, _a priori_ the existence of an incapacitation effect, we conclude that that effect cannot be large relative to the other factors influencing variation in offense rates across judicial districts.

The other, related question concerns the interpretation to be assigned to the coefficients of the four sanctions variables. Are they measures of deterrence, or of a mixture of deterrent and incapacitation effects? If the regression equation includes a valid incapacitation variable, the sanctions coefficients would provide stochastic estimates of the true deterrent effect of these sanctions. Thus, if the proxy we have used is a valid incapacitation variable, as we believe it is, then the sanctions coefficients which we have obtained are pure deterrence measures. This is so whether or not the incapacitation proxy appears in a particular regression equation, because the incapacitation effect was shown to be negligible.

On the other hand, if our proxy is a poor measure of the incapacitation effect, then the coefficient of the risk of incarceration and of the length of incarceration would overstate the deterrent effect. It would then be more accurate to say that they measure crime-control effects. Because arrests whose outcome is not incarceration can also involve some incapacitation -- through pretrial detention, diversion to half-way houses, community-based drug treatment centers, etc. -- its coefficient would also overstate deterrent effects, though less so, presumably, than the other two coefficients. Finally, because probation does not normally remove an offender from the community, the meaning of its coefficient would be unaffected: the coefficient would still measure a pure deterrent effect.

Determinants of Sentence Length

In our judgment, this Report makes an important contribution to the sentencing variation literature. We have examined the records

of approximately nine thousand offenders committed to prison for an Index offense. Our data set was unusually large and unusually detailed with respect to the variables associated with each observation. Most of the results obtained from this large sample of individual offenders were as expected. Within each individual UCR offense category, the overwhelmingly important variable was the combined effect of the number of, and the severity of, the offenses associated with the present incarceration. The offender's past criminal history and his age also have their expected effects on sentence length. So does the offender's sex. On the other hand, it appears that blacks receive lighter sentences for some violent offenses and harsher sentences for some property offenses than whites.

An interesting, and unexpected finding, obtained from the Georgia sample, but not from the North Carolina sample, is that sentence length varies inversely with the crime rate and the incarceration rate prevailing within the judicial district meting out the sentence. We have not attempted a formal explanation for this phenomenon, nor have we explored its implications. One reasonable conjecture, however, is that institutional arrangements in Georgia are such that, in a district experiencing higher offense rates, with their attendant higher incarceration rates, the district's prosecuting attorney is more likely to use sentence length as the principal component in a plea bargain; whereas, in North Carolina, the plea bargain takes a different form and, consequently, has different manifestations.

The Georgia sample is special in that it provides detailed evidence on the judiciary's use of split-sentencing. In the regression equation used to explain length of incarceration, we used the length of post-prison probation time as a regressor. Assuming that the regression is properly specified, and that the court behaves rationally, consciously adjusting sentence length to the severity of the offense, to the offender's prior criminal history, and, possibly, to selected demographic characteristics of the offender, one would expect the court to treat the two sentence length variables as substitutes; and, accordingly, one would expect the coefficient of the probation variable to be negative. This is precisely what we found.[2] The general pattern conclusively demonstrates the existence of a strong substitution effect, and permits rather precise estimates of the marginal rate of substitution of these two sanctions, by offense class.

Other Issues

On balance, the existence of better employment opportunities is negatively related to the offense rate, thereby lending credence to the argument of the earlier formulation of the rational choice model, and affirming the view that potential offenders can be diverted from illegitimate activity by the availability of legitimate

[2] The only exception was burglary, for which a significantly negative coefficient was not obtained. We have not attempted to explain this exception to the general pattern. It may have something to do with the nature of this particular offense, or with the class of offenders involved in this offense, or it may be the result of chance variation -- we do not know.

work opportunities. However, support for this hypothesis is substantially weakened because of the highly unstable behavior of the variable's coefficient when the model takes on different specifications.

A relation between per capita income and the offense rate cannot be unambiguously deduced from the theorist's rational choice model. Rather, its existence has been proposed out of the empiricist's need to include a measure of the economic payoff from criminal activity in his empirical model, and has been affirmed by the regularity with which the income variable has produced "good results" in regression analysis. This relation is, nonetheless, not sustained by our data. The coefficient of the income variable and its _t_-statistics are often large in magnitude, suggesting a significant effect on the offense rate. However, the coefficient is unusually sensitive to changes in model specification, undergoing large swings in magnitude and frequently changing sign.

The results obtained from the model's arrest, incarceration, and law enforcement equations require no review. The equations were estimated _pro forma_, for the sake of completeness. The issues associated with these equations, and the findings obtained therefrom, are peripheral to the interests and objectives of this Report.

B. EXTENSIONS OF THE ANALYSIS

In this section of the chapter, we provide a very brief extension of the analysis of the Georgia and North Carolina data. We do this in order to indicate how better estimates may be obtained for the sanctions variables, and also to develop an implication of the analysis that has important consequences for policy-making. These extensions require two separate lines of development. First, the basic model shall be reestimated, with the Georgia and North Carolina data sets treated as a pooled sample. And, second, the individual coefficients of the incarceration variable, obtained from the different models, shall be used to form a confidence interval estimate for the underlying population coefficient and for the social value to be derived from the incarceration of different classes of UCR offenders.

1. The Basic Model Derived from a Pooled Data Set

Before the basic model can be estimated from a pooled data set, two adjustments must be made in the data so as to render the separate North Carolina and Georgia samples fully comparable. First, post-prison probation data are not available for North Carolina. Hence, to merge the two data sets, the probation variable was omitted from the model. Second, somewhat different scaling factors were used in developing the measures of employment levels, of the seriousness of the charges associated with the present incarceration (score), and of the seriousness of past felony offenses (prior). For example, the North Carolina employment data, being more detailed, permitted the development of an employment index whose range is from zero to five; whereas, with the Georgia data, the range is from zero to three. Differences in the scaling factors, if allowed to exist, would carry the incorrect implication that real differences exist between the means of the state-level variables, and could significantly bias the regression results. To render the data comparable, and to safeguard

against this undesirable outcome, each state's data have been standardized to a zero mean and unitary standard deviation.

The basic model, modified as indicated, yields the set of coefficients appearing in Tables 6.1 and 6.2. The data in these tables clearly support the general conclusion that the three sanctions are effective crime-control instruments, and the particular hypothesis that these instruments act as a deterrent to UCR offenders. The seven offenses, three sanctions, and two estimating procedures associated with the data of these two tables provide 42 coefficients, of which all but five are negative. Assuming that these 42 observations are statistically independent, one may infer from this proportion, even without the formality of a statistical test for significance, that, on balance, these sanctions are negatively related to the offense rate.

More to the point, the larger sample, obtained by pooling the North Carolina and Georgia data, has the hoped-for and desirable result of producing more conclusive results. The findings obtained from these two tables lead to the same general conclusions that were obtained from the separate state samples. But the results are much tighter, especially those obtained from the OLS procedure. The only notable departure from the individual state results concerns the sentence length variable, for which the pooled data provide results which are much more consistent with the deterrence (and incapacitation) hypothesis. We shall not attempt a formal explanation for this variable's improved performance. We would note, however, that North Carolina's sentences are substantially longer than Georgia's, which would suggest that, probably,

TABLE 6.1

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE: ELASTICITIES AND ASYMTOTIC t VALUES: POOLED DATA, ORDINARY LEAST SQUARES

		Independent Variable							
Eqn.	Depend. Var.	AR_{NI}	AR_I	SL	SIZE	P15-29	NW	EMPLOY	INCOME
(1)	Homicide	-.16 (4.01)	-.22 (4.23)	-.02 (.14)	.08 (3.80)	.01 (.03)	.20 (2.40)	+( ) (.24)	-.36 (2.05)
(2)	Rape	-.22 (2.14)	-.06 (.90)	-.07 (.67)	.18 (5.58)	1.65 (3.30)	.09 (.69)	-( ) (1.88)	-.12 (.40)
(3)	Assault	-.43 (4.17)	-.07 (2.21)	-.05 (.42)	.05 (1.89)	.54 (1.42)	.15 (1.52)	-( ) (1.29)	.55 (3.08)
(4)	Robbery	-.12 (.86)	-.01 (.11)	-.02 (.10)	.47 (11.10)	1.89 (2.93)	.04 (.24)	-( ) (1.85)	-.29 (.84)
(5)	Burglary	-.31 (3.45)	-.25 (5.67)	-.05 (.47)	.07 (4.59)	.93 (3.87)	.13 (2.16)	-( ) (1.06)	.39 (3.08)
(6)	Larceny	-.12 (.97)	-.30 (5.47)	-.03 (.30)	.08 (4.52)	1.08 (4.03)	.19 (2.62)	-( ) (1.13)	.88 (6.73)
(7)	Auto	-.15 (1.54)	-.04 (.74)	-.03 (.57)	.18 (7.02)	1.06 (2.63)	-.32 (2.97)	-( ) (1.96)	-.00 (.02)
(8)	All Violent	-.41 (3.49)	-.18 (3.29)	-.05 (.31)	.12 (5.45)	.61 (1.73)	.15 (1.59)	-( ) (1.75)	.32 (1.69)
(9)	All Property	-.25 (2.09)	-.27 (5.30)	-.10 (.86)	.09 (5.43)	.94 (3.71)	.14 (2.06)	-( ) (1.20)	.49 (4.10)

TABLE 6.2

DETERMINANTS OF THE OFFENSE RATE, BY UCR OFFENSE: ELASTICITIES AND ASYMTOTIC t VALUES: POOLED DATA, TWO STAGE LEAST SQUARES

Eqn.	Depend. Var.	Independent Variable							
		AR_{NI}	AR_I	SL	SIZE	P15-19	NW	EMPLOY	INCOME
(1)	Homicide	.26 (.56)	-.21 (1.01)	.15 (.13)	.06 (2.16)	.19 (.44)	.33 (1.80)	-( ) (.37)	.06 (.17)
(2)	Rape	-3.53 (4.64)	-1.18 (4.17)	-1.49 (4.03)	-.29 (2.95)	-3.98 (3.14)	1.85 (5.18)	-( ) (.80)	5.18 (4.85)
(3)	Assault	-2.04 (2.48)	.19 (1.06)	-.46 (.91)	-.01 (.32)	-.50 (.69)	-.41 (1.33)	-( ) (2.17)	1.10 (2.92)
(4)	Robbery	.92 (.90)	-3.52 (3.03)	-3.65 (3.74)	-.02 (.17)	-6.49 (2.65)	3.12 (3.19)	-( ) (1.39)	2.32 (2.89)
(5)	Burglary	-1.48 (2.17)	-.34 (2.48)	.81 (1.60)	.02 (.82)	.11 (.26)	.18 (2.55)	-( ) (1.76)	.32 (1.42)
(6)	Larceny	-.15 (.12)	-.62 (2.68)	-.16 (.74)	.04 (1.91)	.78 (1.30)	.32 (2.94)	-( ) (.29)	1.15 (3.10)
(7)	Auto	-.36 (.39)	-1.01 (2.79)	-.31 (2.88)	.12 (2.89)	-.30 (.43)	.18 (1.19)	-( ) (.06)	-.48 (1.91)
(8)	All Violent	-2.31 (1.61)	-.02 (.06)	1.14 (.93)	.07 (1.93)	-.88 (.91)	-.34 (.85)	-( ) (1.70)	-.09 (.19)
(9)	All Property	-3.56 (2.80)	.04 (.16)	-.39 (1.81)	-.02 (.52)	-.92 (1.42)	.41 (4.31)	-( ) (2.60)	.78 (4.02)

NOTATION TO ACCOMPANY
TABLES 6.1 AND 6.2

Variable	Statistical Description
AR_{NI}	Arrests whose outcome is not incarceration relative to crimes known to the police
AR_I	Number of incarcerations relative to crimes known to the police
SL	Expected term of incarceration upon admission to prison
SIZE	An index whose ith observation is: $SIZE_i = \sum^{n}(c_{ij}p_{ij})/\sum^{n}p_{ij}$, wherein c, the size of the community, is weighted by p, the number of persons living in that community, there being n communities in all in a given district.
P15-29	Proportion of the population 15-29 years of age
NW	Proportion of non-whites in the population
EMPLOY	An offender-based employment index based on scores such as: 3: Employed full-time; 2: Employed part-time or has been unemployed for a short time; 1: Unemployed for long time or has never worked (but is capable of working)
INCOME	Per capita income of families and unrelated individuals

the new regressions pick up and reflect the existence of a strong interstate effect, and that this effect offsets the weak, or perverse, sentence length relation found within the Georgia sample.

A final comment concerning these data: the TSLS findings concerning the efficacy of sanctions, reported in Table 6.2, are similar to those obtained from the individual state samples in that the true sanctions relations are obscured by serious multicollinearity. For example, the correlation coefficients between AR_{NI} and AR_I in the Robbery and All Property offense regressions are 0.72 and 0.79, respectively. When one or the other of these variables is omitted from the regression, the remaining variable assumes a statistically highly significant, negative coefficient, just as happened in the individual state samples under similar circumstances. We conclude from such manipulation, and from the consistently negative OLS coefficients, that both coefficients should rightly be assigned negative values. Thus, our TSLS results, as reported in Table 6.2, understate the true effect of the risk of arrest and incarceration.

2. The Marginal Impact of Incarceration

In Chapters 4 and 5, we provided a range of estimates for the incarceration coefficient based on two models and two estimating procedures. A formal confidence interval estimate for the population coefficient was not developed from these data because the interval that would have been generated would have been too broad to have been of practical value for policy-making. However, the larger sample, obtained by pooling the coefficient data for Georgia and North Carolina, permits the development of interval estimates that do have practical implications.

The procedure used to pool the coefficient data is conventional: The sample, consisting of eight conceptually identical estimates of the probability of incarceration, is obtained from the equation for the basic model and its third variant,[3] from both estimating procedures, and from both states. We assume that each of these eight estimates is an equally important, equally valid, independent estimate of the true population coefficient, β. A pooled estimate of the standard error of the incarceration rate coefficient, s, is developed from the eight individual standard errors, using the conventional procedure for estimating a pooled variance. An 80 percent confidence interval is then obtained from the expression

$$\bar{b} + t_{.10}{*}s/\sqrt{8} \quad \leq \beta \leq \quad \bar{b} + t_{.90}{*}s/\sqrt{8}$$

where $t_{.10}$ and $t_{.90}$ are the 10th and 90th percentiles, respectively, of the $\underline{t}$ distribution for seven degrees of freedom, and $\bar{b}$ is, of course, the sample mean.

These interval estimates, in absolute value, appear in Column (4) of Table 6.3. Except for robbery (and assault, when measured to two significant digits), the interval excludes zero. These results are most gratifying, especially in view of the fact that, as a result of extreme multicollinearity, several of the TSLS incarceration rate coefficients used in forming $\bar{b}$ were positive, causing the negative

[3] The first variant deals only with the offense aggregates, the second with a different definition of the probabilities of arrest and incarceration.

impact of incarceration on the offense rate to be understated.

The values of Column (4), taken in conjunction with the average number of offenses and incarcerations in these two states, permits an 80 percent interval estimate of the marginal impact of incarceration on the number of offenses. (The procedure used is explained in Chapter 4.) The estimates appear in Column (5). One sees that the mean impact from incarcerating one additional offender, in terms of the number of offenses averted through deterrence or incapacitation, is substantially greater than the impact from incarcerating one additional violent offender.

Of course, the foregoing judgment makes no allowance for the seriousness of the offense, or for the costs associated with incarceration. To obtain a measure of the relative social saving associated with, and ascribable to, incarcerating an additional offender, we follow the procedure outlined in Chapter 4, and obtain the results which appear in Table 6.4. In Column (2) a "gross" social saving index is computed by simply weighting the values of Table 6.3, Column (5), by the Sellin-Wolfgang index. In Column (3), the index of Column (2) is deflated by the costs associated with incarceration, so as to correct, in some rough and ready fashion, for the fact that these costs vary substantially across offense classes. The resulting calculus provides a "net" social saving index. Using either concept of social saving, it is clear that we, as a society, benefit more from the incarceration of a burglar or larcenist than we do

TABLE 6.3

REDUCTION IN NUMBER OF OFFENSES ASSOCIATED WITH ONE ADDITIONAL INCARCERATION: POOLED DATA, 80 PERCENT CONFIDENCE INTERVALS

Offense	Number of[a] Reported Offenses (1000s)	Number of[a] Incar-cerations	Reporting Rate[b] (percent)	Confidence Interval Coefficient[c]	Confidence Interval Reduction in Offenses[d]
	(1)	(2)	(3)	(4)	(5)
Homicide	.92	359	95	.32-.73	.86-1.97
Rape	2.0	98	56	.13-.90	4.7-33
Assault	24	434	45	.003-.053	0.4-6.5
Robbery	8.1	682	60	0-3.96	0-78
Burglary	103	1448	55	.48-2.22	62-287
Larceny	184	1114	27	.16-.40	98-245
M.V. Theft	21	198	71	.06-.17	9.0-25

[a]Simple average of Georgia and North Carolina values.

[b]Based on criminal victimization data for the United States in 1975 (U.S. Federal Bureau of Investigation, 1977). The homicide value is a pure guess. The robbery (burglary) rate is a weighted mean of commercial establishment and individual (household) rates.

[c]The values are based on a sample of eight coefficients, derived from two models, two estimating procedures, and two states. The derivative, lower bound, positive robbery coefficient was assigned a zero value. All other values in this column are negative.

[d]Column (5) is derived from 100,000 * Col. (1) * Col. (4) divided by Col. (2) * Col. (3).

TABLE 6.4

RELATIVE SOCIAL BENEFIT TO BE DERIVED FROM AN INCREASE IN THE INCARCERATION RATE: POOLED DATA

Offense	Sellin-Wolfgang Index[a]	Social Saving Gross	Social Saving Net
	(1)	(2)	(3)
Homicide	1.000	.9-2.0	.08-.19
Rape	.317	1.5-10	.13-.88
Assault	.228	.1-1.5	.04-.64
Robbery	.120	0-9.4	0-1.4
Burglary	.095	5.9-27	2.1-9.6
Larceny	.060	5.9-15	3.7-9.4
M.V. Theft	.099	0.9-2.5	0.5-1.5

[a]Obtained from the Sellin and Wolfgang (1964) index, scaled to Homicide = 1.000.

from the incarceration of murderers, those convicted of assault, and probably also from those convicted of robbery and rape. The reader is advised to recall from Chapter 4 the assumptions underlying this argument and the boundaries within which conclusions drawn from these data are valid. In particular, one should recall that social saving is a relative concept. Individual benefits may only be compared one to another. One assumes, in effect, that the aggregate number of incarcerations is held constant. In particular, the average benefit associated with the seven offenses, taken as a whole, has not been evaluated; and, therefore, has no meaning. It could be that, viewed in some wider context, a positive benefit would accrue to an increase (or decrease!) in the overall rate of incarceration.

C. DIRECTIONS FOR FUTURE RESEARCH

We have shown that, except for sentence length, the Georgia and North Carolina samples produce similar results. Accordingly, we may presume that these sample data derive from the same underlying population. To enhance the power of the statistical analysis, it is desirable, therefore, to combine the individual state data into a single, pooled sample. We have done this for the basic model; and, as the reader has seen, we have obtained much more reliable and useful results. We believe, therefore, that further research with these macro-level data should be pursued with the pooled sample.

In retrospect, we realize that the statistical variable used to measure sentence length does not perform its intended function. It is essential, therefore, that an alternative measure be developed, one which would more accurately reflect the "average cost" of incarceration to a potential offender. A number of possibilities exist. The most promising option, one that would seem to meet the criteria set forth in Chapter 4 for the measure, would be to use the residuals of the sentence length regression equation; and, from these residuals, which are based on individual inmate data, to develop the requisite macro sentence length variable.

In retrospect, we also see the need to introduce a more discriminating set of instrumental variables into the TSLS procedure, so that the degree of covariation between the second stage arrest and incarceration probabilities can be reduced.

If an improved measure of sentence length can be developed, and if the multicollinearity problem in the second stage regression can be eliminated, the analysis of the marginal impact of incarceration on the offense rate should be refined and extended. We believe this analysis has considerable potential as an aid to crime-control policy-making. The estimates relating to incarceration's effect on the offense rate, as well as the attendant social savings estimates, should, of course, be based on the pooled data set. If it is not possible to eliminate multicollinearity in the TSLS procedure, the sample of regression coefficients should be expanded to include coefficients that bound the true, underlying population coefficient, so as to moderate the downward bias in impact and social saving estimates engendered by this phenomenon.

Assuming that the new model, estimated with the pooled data set, produces confidence interval impact and social saving values at least as narrow as those that we have obtained for the incarceration rate, it would be highly desirable to develop parallel interval estimates for the other three sanctions. (The estimates for the probation variable would only be developed for Georgia, of course.) If they do nothing else, these interval estimates indicate in a rather direct and forceful manner the state of the art of evaluating the crime-control effectiveness of these legal sanctions, and furnish a guideline against which to evaluate future research.

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APPENDIX A

DATA SOURCES, DEFINITIONS, AND ESTIMATION PROCEDURES

The following defines the data used in this Report, provides certain technical details concerning computational procedures, and indicates the source of these data. The presentation is ordered by variable and also by state, when the latter is required.

Population

(Used to derive offense, police, and other rates)

Georgia

U.S. Bureau of the Census. 1970 Census of Population, v.1, pt. 12 (Georgia), Table 9, "Population Land Area of Counties: 1970 and 1960," pp. 18-20. 1977 data derived by linear extrapolation.

(Population estimates are also available from the Georgia Office of Planning and Budget as published in Georgia, State Crime Commission. Statistical Analysis Center. Crime in Georgia. Atlanta, 1980. These figures differ somewhat from the estimates used in the Report. However, they were published after the major part of the Georgia computations were completed, and therefore, could not be incorporated into the Report.)

North Carolina

1978 and 1979 population figures were the estimates reported in North Carolina. Department of Justice. Police Information Network. Crime in North Carolina. (1979 Uniform Crime Report). Raleigh, n.d., pp. 74-127.

For jurisdictions where offenses were not reported, PIN excluded the population of the jurisdiction from the county totals. We readjusted these data in those instances in which offense data were adjusted for underreporting.

Total population figures were used in computations involving police employees, income, government revenue, size of place, age, and race.

Number of Offenses:

Georgia

1977 data were obtained by totalling figures supplied on computer printout sheets, and as handwritten data by the Georgia Crime Information Center. The data were presented in one computer run of all reported offenses dated March, 1980, plus two computer passes showing all unfounded offenses was of the same date. The computer runs were supplemented by handwritten sheets for Fulton County and DeKalb County showing offenses and unfounded offenses as of the same date.

1978 data were obtained from handwritten sheets sent to us by the Georgia Crime Information Center. They had compiled these figures, presumably from the computer files for that year and were not dated, but were sent to us in January of 1980. There were several minor inconsistencies within these handwritten sheets: in particular, mismatched county names and data, and numbers which did not reconcile with totals. These inconsistencies were resolved with the help of published data from the Georgia State Crime Commission Statistical Analysis Center. Crime in Georgia. Atlanta, May, 1980, Table 25, "County Crime Profiles," pp. 54-71. Other inconsistencies in both 1977 and 1978 data were revised by considering 1974 data given in the above publication, plus 1979 data given for a few counties by GCIC over the telephone.

North Carolina

North Carolina Department of Justice. Police Information Network. Crime in North Carolina. (1979 Uniform Crime Report), Raleigh, n.d., pp. 74-127.

This source provided the 1979 and revised 1978 figures. Where reporting was incomplete, estimates were based upon the two years given in the 1979 report above, plus the figures for 1977 contained in the 1977 Uniform Crime Report.

In two instances, the county sheriff's data were missing and could not be extrapolated from the above reports. We assigned offense data to these jurisdictions based upon the estimated state-wide offense rate for the population under the jurisdiction of county sheriffs. The missing counties were given their proportionate share of the offenses reported by all county sheriffs.

The above estimates were used in computing crime rate figures.

Number of Arrests

Georgia

Arrest data were obtained from computer printout and handwritten data supplied by the Georgia Crime Information Center. Arrests were reported in three computer passes dated March 1980. The computer data was supplemented by handwritten sheets for Fulton and DeKalb Counties. (Negligent manslaughter had to be removed from the homicide category.) We used the column labelled "Counts" for the arrest data since it presumably was a better proxy for persons arrested than the "Total Counts" column.

North Carolina

North Carolina. Department of Justice. Police Information Network. Crime in North Carolina. (1979 Uniform Crime Report), Raleigh, n.d., pp. 150-174.

We assumed that arrest data were reported if and only if offense data were reported. For example, a jurisdiction reporting offenses for just ten months of the year was assumed to have reported arrests for those same ten months.

Number of Incarcerations

The numerator of the AR_I ratio consists of all persons newly committed to prison for an Index offense. Calendar year 1978 was used for Georgia and 1979 for North Carolina. Returned escapees, parole and probation violators, etc. are excluded from the sample. The offense(s) resulting in commitment were defined in terms of the criminal statutes of Georgia and North Carolina. The writer was unable to obtain an authoritative match between statutory and UCR offenses; and, accordingly, exercised his own judgment in mapping the one into the other.

The data were provided by the respective state Department of Corrections on magnetic tape (hereinafter referred to as DOC Tapes).

Number of Police Employees

Georgia

U.S. Federal Bureau of Investigation. Crime in the United States, 1978 (Uniform Crime Reports for the U.S.), Washington, D.C., 1978; Table 60, "Number of Full-time Law Enforcement Employees, Cities 25,000 and over in Population, October 31, 1978;" Table 61, "Number of Full-time Law Enforcement Employees, Cities with Population under 25,000, October 31, 1978;" Table 62, "Number of Full-time Law Enforcement Employees, Universities and Colleges, October 31, 1978;" Table 63, "Number of Full-time Law Enforcement Employees, Suburban Counties, October 31, 1978;" Table 64, "Number of Full-time Law Enforcement Employees, Rural Counties, October 31, 1978."

Where cities fall into two counties, the proportion of the city's 1970 total population in each county (as determined from Georgia Department of Transportation. General Highway Maps) was used to apportion police employees.

The measure excludes state police, but includes compus police.

North Carolina

North Carolina. Department of Justice. Police Information Network. Crime in North Carolina. (1979 Uniform Crime Report). Raleigh, n.d., pp. 74-127.

Government Revenue

U.S. Bureau of the Census. 1977 Census of Governments, v.4, pt. 5, "Compendium of Government Finances," Table 54, "Selected local government finances by county area and by state, 1976-77," p. 347.

The series used was "General revenue, excluding interlocal."

Motor Vehicle Registrations

Georgia. Department of Revenue. Statistical Report, 1979. Table 13, "Selected Tax Statistics and Estimates by County, 1978," pp. 27-31.

Age (P15-29; P15-19)

U.S. Bureau of the Census. 1970 Census of Population, v.1, pt. 12 (Georgia), Table 35, "Age by Race and Sex, for Counties: 1970," p. 134-

U.S. Bureau of the Census. 1970 Census of Population, v.1, pt. 35 (North Carolina), Table 35, "Age by Race and Sex for Counties: 1970," p. 134-

Because we expect the age distribution of the population to be highly stable, we made no attempt to extrapolate these data beyond 1970.

Proportion Non-White

U.S. Bureau of the Census. 1970 Census of Population, v.1, pt. 12 (Georgia), Table 34, "Race by Sex, for Counties: 1970," p. 129.

U.S. Bureau of the Census. 1960 Census of Population, v.1, pt. 12 (Georgia), Table 27, "Age by Color and Sex, for Counties: 1960," p. 97.

U.S. Bureau of the Census. 1970 Census of Population, v.1, pt. 35 (North Carolina). Table 34, "Race by Sex for Counties: 1970," p. 129-

U.S. Bureau of the Census. 1960 Census of Population, v.1, pt. 35 (North Carolina), Table 27, "Age by Color and Sex for Counties: 1960," p. 98-

From the above sources, estimates for 1977 were computed as a linear extrapolation of the 1960 and 1970 percentages.

Personal Income

Georgia. Department of Revenue. Statistical Report, 1979. Table 12, "Personal Income Tax Data, 1977," pp. 24-26.

Survey of Current Business, v. 60, no. 4, April 1980, Table 2, "Total Personal Income and Per Capita Personal Income by County for Selected Years," provided data for 1979 for North Carolina.

Size of Place (SIZE)

Georgia

U.S. Bureau of the Census. 1970 Census of Population v.1, pt. 12, (Georgia), Table 6, "Population of Places: 1970 and 1960," for all incorporated and unincorporated places of 2500 or more.

Where cities lapped over into two counties, the Georgia Department of Transportation. General Highway Maps of Georgia, 1971 was used to allocate the 1970 population of individual cities to their respective counties.

This variable may be defined as a weighted mean size of community, S. The observation for judicial district j was estimated from

$$S_j = \frac{\sum_{i=1}^{n_j} P_{ij}^2}{\sum_{i=1}^{n_j} P_{ij}} + V * P_{Rj},$$

where n_j refers to the number of communities of at least 2500 persons found in judicial district j, and P_{ij} refers to the actual number of persons living in each of these communities. P_{ij} serves as both the observation's value and its weight -- hence the presence of the square of P_{ij} in the equation.

The last term in the equation is the residual. It accounts for persons living in communities of less than 2500 persons (including rural areas). The value of the residual observation is given by V. V represents the average size of community lived in by persons living in communities of less than 2500. We chose V = 800 as our best guess for this average value. P_{Rj} is the weight of the observation, and equals the total number of persons living in small communities and in rural areas.

A special problem was presented by Atlanta, which consists of the two counties, DeKalb and Fulton. These counties belong to different judicial districts. Using the aggregate population of the individual district for its size-of-place would have seriously understated the true size-of-place lived in by the district's residents. Hence in computing SIZE, we treated the residents of these two counties as belonging to a city of the size of the two counties combined. That is, we assumed, in effect, that the residents of these two counties lived in a community whose size equaled the actual size of Atlanta, with its associated residential communities.

North Carolina

North Carolina. Department of Justice. Police Information Network. Crime in North Carolina. (1979 Uniform Crime Report), Raleigh, n.d., pp. 74-127. Places having a population of 2000 or more were selected to represent non-rural places. All places were already assigned to counties in this publication.

Number of Ex-Offenders (for EXCON)

This variable may be defined as a weighted mean number of persons released from prison each half-year between the years 1975 and 1978, by judicial district. The index was obtained from the equation $\bar{E}_j = \sum_{i=1}^{8} w_i E_{ij}$, where E_{ij} represents the aggregate number of persons released from prison within the half-year, i, whose home district was j, and w_i is the longitudinal weight assigned to that time point. The set of weights chosen was w = [.01, .03, .06, .10, .15, .25, .25, .15], for t = -8, -7, ..., -1. The weights were impressionistically derived, and assume that the probability of recidivating is highest about one year after release. We experimented with a wide range of alternative weighting systems, including equal weights, and obtained such similar results, to a factor of proportionality, that the derivation of a set of weights based on a more careful examination of the recidivistic literature was deemed to be an unproductive enterprise.

Source: Georgia DOC Tape.

Sentence Length

The expected sentence length measure is obtained directly from the Georgia and North Carolina computer tapes. Sentence length represents the respective Department of Correction's best estimate of the time an inmate will have served when he is first released. The estimate is based largely upon past experience, and corrects for "good time" and for current parole policy. Several alternative measures of sentence length were available, but were rejected as unsatisfactory. One alternative was to equate sentence length with the time to be served until first possible parole date. This measure is deemed inferior because substantial divergencies exist between mandated consideration for parole and actual release on parole. The other measures were minimum sentence to be served and maximum sentence that could be served. These were rejected for obvious reasons.

Source: DOC Tapes.

Employment (EMPLOY)

North Carolina and Georgia use different measures of an offender's work history. The descriptive categories and the scoring system used to tabulate their data are given in Table A.1. The categories refer to the offender's work history at the time of the arrest associated with the present incarceration. The data are based on self-reports.

Source: DOC Tapes.

Seriousness of Offense (SCORE, score)

The number of offenses giving rise to the present incarceration were weighted by the Georgia Department of Corrections' ranking of 100 felony offenses. The individual offender's score was calculated from $\sum_{i=1}^{n} (101-R_i)$, in which R_i represents the rank of the ith offense, and n represents the number of recorded offenses -- a maximum of six for Georgia and two for North Carolina. Districts in which no one was committed to prison for a particular offense during the survey year were assigned the mean sentence length of the "non-empty" districts.

TABLE A.1

SCORING PROCEDURE FOR THE EMPLOYMENT VARIABLE

North Carolina Category	Value	Georgia Category	Value
Steady work record (WR) and was working regularly; or was student; or was houseperson	5	Employed full time or was student	3
Unstable WR and was working regularly; or stable WR and was working irregularly	4	Employed part-time; or unemployed for short time	2
Unstable WR and was working irregularly	3	Unemployed long time; or never worked but is capable of working	1
Stable WR and was unemployed	2	Incapable of work; or no report	Mean value
Unstable WR and was unemployed	1		
Physically disabled; or no report	Mean value		

In retrospect, we regret our decision to use the Department of Correction ranking system. Although our measures predict quite well, and are probably reasonable when used within offense categories, they sometimes provide unreasonable indices when offenders are compared across offense categories. For example, a person convicted of multiple larcenies can obtain a higher severity score than a person convicted of homicide. We suspect that the Sellin-Wolfgang severity index would have provided a better index of the legal and judicial valuation of the offenses under consideration.

Source: DOC Tapes.

Prior Criminal History (PRIOR, prior)

For Georgia, the index of severity of past criminal activity was obtained in the same way as the foregoing variable, and is subject to the same criticism. For North Carolina, the variable is defined as a simple count of the number of known past felony convictions.

Other Micro-Data

The data referring to an individual offender's age, sex, and race were obtained directly from DOC Tapes, and presented no conceptual or measurement problems.

APPENDIX B

MISCELLANEOUS DATA TABLES

B.1

TABLE 4.14A

DETERMINANTS OF THE AGGREGATE ARREST RATE: GEORGIA: 1978: OLS PROCEDURE

Eqn.	Dependent Variable	CRM	SIZE	P15-19	NW	INCOME	COP
				Independent Variable			
(1)	Homicide	-.40 (1.51)	.07 (.97)	-1.59 (.95)	.11 (.37)	.06 (.07)	-.60 (1.16)
(2)	Rape	-.18 (.65)	-.02 (.31)	-2.52 (1.48)	.45 (1.49)	.50 (.57)	-.62 (1.18)
(3)	Assault	-.60 (3.33)	.17 (3.27)	.61 (.53)	-.53 (2.64)	-.12 (.20)	-.50 (1.41)
(4)	Robbery	-.03 (.06)	-.13 (.94)	-3.01 (.96)	1.39 (2.51)	1.61 (1.00)	-2.00 (2.05)
(5)	Burglary	-.01 (.06)	-.08 (.53)	-1.47 (1.80)	1.82 (1.36)	-.64 (1.37)	-.39 (1.52)
(6)	Larceny	-.07 (.32)	-.01 (.20)	-.33 (.40)	.20 (1.50)	-.34 (.72)	-.29 (1.10)
(7)	Auto	-.07 (.16)	-.08 (1.08)	-1.31 (.73)	.88 (3.02)	.56 (.55)	-1.11 (2.00)
(8)	All Violent	-.44 (3.18)	.10 (2.49)	-.37 (.42)	-.24 (1.54)	-.06 (1.40)	-.57 (2.12)
(9)	All Property	-.03 (.17)	-.02 (.55)	-.75 (.99)	.22 (1.83)	-.42 (.98)	-.35 (1.52)

TABLE 4.15A

DETERMINANTS OF THE AGGREGATE ARREST RATE: GEORGIA, 1978: TSLS PROCEDURE

Eqn.	Dependent Variable	Independent Variable CRM	SIZE	P15-19	NW	INCOME	COP
(1)	Homicide	-2.43 (1.39)	.44 (1.36)	-.36 (.16)	-.04 (.12)	-.24 (.21)	3.59 (1.05)
(2)	Rape	2.68 (1.56)	-.55 (1.71)	-5.28 (2.40)	.44 (1.28)	-.27 (.24)	-5.10 (1.51)
(3)	Assault	2.27 (1.71)	-.36 (1.46)	-1.70 (1.00)	-.44 (1.69)	-.35 (.41)	-5.65 (2.18)
(4)	Robbery	1.50 (.45)	-.42 (.67)	-4.95 (1.16)	1.28 (1.94)	.66 (.30)	-3.75 (.57)
(5)	Burglary	.06 (.10)	-.03 (.38)	-1.74 (1.67)	.16 (.72)	-.90 (1.48)	-.24 (.21)
(6)	Larceny	.58 (.95)	-.08 (1.09)	-1.20 (1.16)	.32 (1.48)	-.90 (1.48)	-1.14 (.97)
(7)	Auto	.14 (.10)	-.10 (.66)	-2.08 (.89)	.81 (1.64)	-.20 (.14)	-.70 (.26)
(8)	All Violent	1.42 (1.35)	-.25 (1.24)	-1.90 (1.41)	-.19 (.90)	-.25 (.37)	-3.88 (1.87)
(9)	All Property	.39 (.69)	-.06 (.97)	-1.36 (1.43)	.29 (1.46)	-.84 (1.50)	-.84 (.78)

TABLE 4.16A

DETERMINANTS OF THE INCARCERATION
RATE: GEORGIA, 1978: OLS PROCEDURE

Dependent	Independent Variable					
Variable	CRM	SIZE	P15-19	NW	INCOME	COP
Homicide	-.35 (.91)	.10 (.95)	-1.05 (.43)	-.17 (.38)	-.79 (.64)	-.66 (.87)
Rape	-.01 (.01)	.03 (.22)	2.24 (.70)	-.36 (.64)	-.58 (.36)	.33 (.33)
Assault	-.97 (1.64)	.24 (1.43)	9.55 (2.55)	-.26 (.39)	1.33 (.68)	-2.07 (1.78)
Robbery	.04 (.04)	-.19 (.82)	-2.11 (.43)	2.12 (2.32)	2.43 (.92)	-3.06 (1.90)
Burglary	-.85 (1.90)	.11 (1.43)	1.02 (.57)	.15 (.50)	-.15 (.15)	-.48 (.86)
Larceny	-.95 (2.14)	.11 (1.47)	2.33 (1.31)	.43 (1.49)	.95 (.93)	-.76 (1.36)
Auto	-1.04 (1.24)	.06 (.42)	-.82 (.24)	.79 (1.44)	.88 (.46)	-.35 (.33)
All Violent	-.66 (2.01)	.14 (1.56)	3.68 (1.78)	-.03 (.08)	.42 (.40)	-1.14 (1.78)
All Property	-1.00 (2.33)	.12 (1.66)	1.24 (.72)	.22 (.80)	.26 (.27)	-.45 (.85)

TABLE 4.17A

DETERMINANTS OF THE INCARCERATION RATE:
GEORGIA, 1978: TSLS PROCEDURE

Dependent Variable	Independent Variable					
	CRM	SIZE	P15-19	NW	INCOME	COP
Homicide	-6.10 (2.80)	1.67 (2.86)	4.21 (1.51)	-.20 (.47)	.43 (.31)	8.75 (2.05)
Rape	-.73 (.24)	.18 (.30)	5.10 (1.29)	.13 (.21)	2.14 (1.07)	-1.57 (.26)
Assault	-1.59 (.42)	.36 (.51)	12.00 (2.49)	.16 (.21)	3.67 (1.50)	-3.70 (.50)
Robbery	2.28 (.42)	-.61 (.60)	-4.29 (.62)	2.14 (1.98)	1.93 (.55)	-6.71 (.63)
Burglary	-2.82 (2.13)	.31 (2.07)	2.72 (1.22)	-.42 (.89)	.47 (.36)	3.42 (1.35)
Larceny	-2.50 (1.76)	.27 (1.68)	3.35 (1.41)	-.08 (.16)	1.06 (.76)	2.76 (1.03)
Auto	-1.72 (.72)	.13 (.49)	.69 (.17)	.80 (.95)	2.17 (.91)	-.31 (.07)
All Violent	-2.22 (1.10)	.44 (1.16)	6.23 (2.42)	.21 (.54)	2.07 (1.59)	-.17 (.04)
All Property	-2.94 (2.29)	.32 (2.20)	2.95 (1.36)	-.32 (.72)	.91 (.71)	3.35 (1.37)

TABLE 5.13A

DETERMINANTS OF THE AGGREGATE ARREST RATE WITH THE LAGGED CRIME RATE AS A REGRESSOR: NORTH CAROLINA, 1979: OLS PROCEDURE

Eqn.	Depend. Var.	CRM	SIZE	P15-19	NW	INC	COP
		Independent Variable					
(1)	Homicide	-.17 (1.09)	-.01 (.17)	.19 (.39)	.12 (1.19)	.53 (1.00)	.21 (.63)
(2)	Rape	-.28 (1.49)	-.02 (.46)	-1.32 (2.20)	.53 (4.11)	-.40 (.60)	-.75 (1.80)
(3)	Assault	-.49 (3.11)	-.03 (.81)	-.44 (.90)	.12 (1.17)	.61 (1.10)	-.45 (1.30)
(4)	Robbery	-.64 (2.10)	.09 (1.31)	1.18 (1.24)	-.26 (1.25)	-2.38 (2.24)	.11 (.16)
(5)	Burglary	-.49 (2.69)	.01 (.20)	-.13 (.32)	-.00 (.49)	-.45 (.96)	.24 (.60)
(6)	Larceny	-.12 (.60)	-.01 (.26)	-.73 (1.63)	.17 (1.84)	.13 (.25)	-.15 (.35)
(7)	Auto	-.69 (3.26)	.01 (.33)	-.03 (.06)	-.07 (.77)	-1.08 (1.98)	1.27 (2.77)
(8)	All Violent	-.50 (3.69)	-.02 (.81)	-.41 (.97)	.15 (1.66)	.48 (1.00)	-.56 (1.87)
(9)	All Property	-.29 (1.89)	-.00 (.11)	-.50 (1.46)	.08 (1.14)	-.14 (.36)	.09 (.27)

TABLE 5.14A

DETERMINANTS OF THE AGGREGATE ARREST RATE WITH THE LAGGED CRIME RATE AS A REGRESSOR: NORTH CAROLINA, 1979: TSLS PROCEDURE

Eqn.	Depend. Var.	CRM	SIZE	P15-19	NW	INC	COP
		Independent Variable					
(1)	Homicide	-.38 (.81)	.00 (.06)	.26 (.48)	.14 (1.16)	.34 (.47)	.59 (.65)
(2)	Rape	-.46 (.91)	-.00 (.03)	-1.04 (1.69)	.62 (4.38)	.20 (.20)	-1.37 (1.12)
(3)	Assault	-.91 (1.58)	-.01 (.27)	-.31 (.50)	.17 (1.12)	.23 (.28)	.31 (.30)
(4)	Robbery	.03 (.04)	.09 (1.21)	1.40 (1.40)	-.18 (.76)	-.35 (.25)	-2.85 (1.95)
(5)	Burglary	-1.20 (2.84)	.01 (.29)	.02 (.06)	-.03 (.38)	-.15 (.28)	1.59 (1.32)
(6)	Larceny	.31 (.56)	.00 (.11)	-.58 (1.31)	.25 (2.40)	.76 (1.16)	-1.98 (1.20)
(7)	Auto	-.77 (1.28)	.02 (.49)	.12 (.23)	-.03 (.23)	-.58 (.82)	.86 (.53)
(8)	All Violent	-.53 (1.52)	-.02 (.50)	-.34 (.62)	.18 (1.36)	.65 (.98)	-.76 (1.24)
(9)	All Property	-.79 (1.76)	-.00 (.12)	-.43 (1.25)	.08 (.96)	-.03 (.06)	1.16 (.85)

TABLE 5.15A

DETERMINANTS OF THE INCARCERATION RATE WITH THE LAGGED CRIME RATE AS A REGRESSOR: NORTH CAROLINA, 1979: OLS PROCEDURE

Eqn.	Depend. Var.	Independent Variable					
		CRM	SIZE	P15-19	NW	INC	COP
(1)	Homicide	-.23 (.94)	-.22 (.39)	-.89 (1.13)	.43 (2.57)	.20 (.23)	-.25 (.45)
(2)	Rape	-.55 (1.04)	-.10 (.80)	-1.92 (1.14)	.99 (2.74)	1.04 (.56)	-.43 (.37)
(3)	Assault	-.78 (2.82)	-.01 (.10)	0.39 (.48)	.46 (2.58)	.25 (.27)	-.18 (.31)
(4)	Robbery	-.64 (1.69)	.04 (.45)	-.76 (.64)	.16 (.62)	-2.27 (1.72)	-.01 (.01)
(5)	Burglary	-1.01 (2.58)	.02 (.41)	-.59 (.68)	.51 (2.56)	.35 (.34)	-.35 (.42)
(6)	Larceny	-.62 (2.04)	-.00 (.07)	-.45 (.68)	.23 (1.63)	.03 (.03)	-.52 (.78)
(7)	Auto	-.24 (.44)	-.08 (1.01)	.16 (.13)	.52 (2.12)	1.64 (1.18)	-1.00 (.85)
(8)	All Violent	-.76 (3.96)	-.03 (.81)	-.41 (.68)	.47 (3.60)	.87 (1.29)	-.16 (.38)
(9)	All Property	-.80 (2.90)	.01 (.17)	-.42 (.69)	.36 (2.86)	.20 (.28)	-.40 (.66)

TABLE 5.16A

DETERMINANTS OF THE INCARCERATION RATE WITH THE LAGGED CRIME RATE AS A REGRESSOR: NORTH CAROLINA, 1979: TSLS PROCEDURE

Eqn.	Depend. Var.	Independent Variable CRM	SIZE	P15-19	NW	INC	COP
(1)	Homicide	.06 (.07)	-.04 (.61)	-1.11 (1.27)	.36 (1.73)	.00 (.00)	-.21 (.14)
(2)	Rape	.13 (.09)	-.12 (.92)	-2.16 (1.19)	.91 (2.18)	1.57 (.54)	-1.56 (.43)
(3)	Assault	-.11 (1.19)	.00 (.05)	-.36 (.35)	.47 (1.91)	-.38 (.28)	.87 (.52)
(4)	Robbery	-.52 (.70)	.05 (.54)	-.51 (.40)	.25 (.84)	-1.17 (.65)	-1.46 (.78)
(5)	Burglary	-1.26 (1.16)	.03 (.51)	-.36 (.38)	.57 (2.62)	1.02 (.74)	- .57 (1.81)
(6)	Larceny	- .64 (.64)	-.04 (.07)	-.46 (.57)	.22 (1.19)	.04 (.00)	- .44 (.15)
(7)	Auto	- .31 (.22)	-.07 (.86)	.37 (.31)	.59 (2.18)	2.35 (1.43)	-1.70 (.46)
(8)	All Violent	- .97 (1.95)	-.03 (.48)	-.35 (.44)	.49 (2.65)	.69 (.72)	.20 (.23)
(9)	All Property	-1.47 (1.47)	.00 (.05)	-.41 (.54)	.32 (1.81)	.05 (.04)	1.40 (.46)

END