Choosing Punishments: Crime Control Effects of Sentences

Report to the National Institute of Justice

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May, 1998

Acknowledgment

This is a report from a study of sentencing started more than 20 years ago in collaboration with the judges of the Essex County, New Jersey Court. Without the judges' interest, contributions, and concern for the improvement of sentencing, the study would not have been possible. The active contribution of the judges of the Court and the start of the study were due to Judge John A. Marzulli, then Assignment Judge for the Court. His wisdom and support throughout the study were valued. Judges Nicholas Scalara and Leo Yanoff particularly contributed also. The support of Robert D. Lipscher, then Director of the Administrative Office of the Courts of New Jersey, and of John McCarthy, Deputy Director for Criminal Practice, was essential to the study.

Many persons helped at the start of the study. Colleen Cosgrove assisted in planning the initial data collection done with the help of the judges. Bridget Stecher helped the project in various ways from its start. She and Bernadette Fiore contributed extensively to the data collection tasks. These graduate students of the School of Criminal Justice of Rutgers University collected data and performed other research tasks: Audrey Anderson; Sally Manning Cafasso; Charisse Coston; Tricia Critchfield; Harry Dammer; and Ken Kolb. Carol Kenney and Shirley Parker provided secretarial assistance. Ellen Plunkett gave clerical and computational help. Some data were provided by the Essex County Probation Department, and appreciation is expressed to Nicholas Fiore, Chief Probation Officer, and staff.

Portions of the initial data were collected through the help of others in the New Jersey criminal justice system. They included, from the outset, William Fauver, then Commissioner of the New Jersey Department of Corrections; Hank Pierre; Chris Simone; and Stan Repko; and Superintendents Donald Zelinski, Jack Rafferty; Phil Dwyer; Steve Domovich; and Sidney Hicks. Appreciation is expressed also to Vincent Corrado, Director of the Essex County Department of Public Safety, and Wardens Albert Collier and Donald Schmidt. The study benefited also from the help of Judge Christopher Dietz, then Chairman, New Jersey Parole Board, Fred Haley, and Vic D'Illio. Staff of the New Jersey Administrative Office of the Courts, including John McCarthy, Joseph Barracco, and Harvey Goldstein provide needed help in the early years of the study as well.

For the phase of the project described in this report, an Advisory Committee was especially helpful. The Committee was composed of John McCarthy, Deputy Director for Criminal

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Summary

Problem

Sanctioning policies typically are made without adequate knowledge of how punishments modify, control, or enhance the likelihood of future criminal behavior by the offenders sentenced. Sound comparisons of results of different punishments are unavailable, since experimental designs are rarely feasible.

Methods

The crime control effects of sanctions were studied by the next most rigorous means, using data well suited to the purpose. Effects on new offense behavior and criminal career patterns were assessed by "quasi-experimental," non-experimental, and other multivariate statistical methods. Statistical controls were used to take account of judicial selection of sentences, *a priori* risk of new arrests, and incapacitation due to confinement.

The study was based on 962 felony offenders in Essex County, New Jersey sentenced variously to confinement and non-custodial programs when considerable discretion was available to the 18 sentencing judges who collaborated in the study. The data included records of: judicial perceptions, predictions, and sentencing purposes; offender background; how sentences were actually carried out; and post-sentencing arrests and charges over the 20 years after sentencing.

Main Questions

Central questions concern the effects of different sanctions on later criminal careers. Also measured, and required for the best tests of the central questions, were the selection of different sanctions by judges, the validities of subjective and objective predictions of risks, and of time in the community at risk – that is, free of incapacitation effects of jail or prison.

Results

The sample of offenders had been convicted of crimes typically found in felony courts. By legal offense classifications, 37 percent were person offenders, 24 percent were convicted of drug offenses, 23 percent of property crimes, and 10 percent of weapons offenses. By a

Practice, New Jersey Administrative Office of the Courts; Stan Repko, Deputy Commissioner for Research and Planning, New Jersey Department of Corrections, and Wayne Fisher, Deputy Director, New Jersey Division of Criminal Justice. The Committee met early in the project to review the plans for the needed follow up data collection and for the required analyses. The group met later to review preliminary results and provide further advice, which was always helpful and appreciated. The follow up data collection and coding for this phase of the study were done under the general supervision of Stan Repko, a Member of the Advisory Committee, with the able direct supervision of the records retrieval and coding staff by Karen Buron. The computer searches and coding were carefully and well done by Lauri Applegate, Sandra Czenis, Susan Estwan, and Cynthia McQuaig, all of the New Jersey Department of Corrections.

The project was started with the support of Rutgers University and later funded in part by a grant from the National Institute of Justice, United States Department of Justice, as was the present study. The advice and help of staff of the National Institute of Justice, including Joel Garner, Pamela Lattimore, and Nancy LaVigne were appreciated.

This study was funded in part by the National Institute of Justice, United States Department of Justice, by grants 82-IJ-CX-0054 and 95-IJ-CX-0118. The views expressed are those of the author and do not necessarily reflect the views or endorsement of the Essex County Court, the judges who participated in the study, the New Jersey Administrative Office of the Courts, the National Institute of Justice, or of any other individuals or agencies.

classification based on seriousness judgments, one in six was convicted of an offense involving interpersonal confrontation or physical harm, although property crimes were most common. Fifteen percent were serious drug offenders whose crimes included sale of drugs other than marijuana. The average age was 29. Most were Black males. About half had prior jail terms; 16 percent had been in prison.

Results describe the sentences as imposed and executed, the sentencing purposes of the judges, and later arrests and charges. Next considered are the validity of judges' predictions and the measures of *a priori* risk and sentence selection needed for the analyses of effects. Then the effects of various sentencing choices are described.

Sentences

Most offenders were sentenced to incarceration with terms from one month to life, typically 6, 12, or 18 months in jail or 5, 7, or 10 years in prison. Sentences were suspended in whole or in part for many, typically with probation. The main choices, in terms of executed sentences, were a non-custodial sentence (42 percent), jail (29 percent), a youth facility (10 percent), and prison (19 percent). About 10 percent had "split" sentences of probation with jail. Others had special conditions, including fines and restitution.

Sentencing Purposes

Judges' purposes for sentences were focused mainly on crime control. Aims of rehabilitation and specific deterrence were prominent; other goals included incapacitation and general deterrence. The retributive aim was primary in 18 percent of cases.

Offenses After Sentencing

About a fourth of the offenders were never again arrested in the 20 years after sentencing; three-fourths were arrested at least once. More than half were re-arrested in the first five years. They were arrested 5.3 times on the average. Their arrest rate while not incapacitated was .28 arrests per year. For those re-arrested at least once, it was .36. The mean time to a new arrest was 3.5 years; the median was 2.2 years.

The most frequent outcome was no new arrest. Next most often there was an arrest on a nuisance or property crime charge. The first arrest after sentencing involved a person crime with personal confrontation or physical harm in 12 percent of the cases. Over the entire follow up period, these offenders were charged with 40 illegal homicides (murder or manslaughter), 455

robberies, 752 assaults, 928 burglaries, 18 rapes, and other crimes for a total of 9,346 allegations of new crimes.

Judges' Predictions

Judges' subjective predictions of any new crimes, of property crimes, and of person crimes were valid, but modestly so, accounting for about 6 percent of the variability in "any new arrest." The validity of predictions of some judges compared favorably with some empirically derived, formal prediction methods. Yet, for four of the 18 judges, none of the predictions was valid. Which judges would be better predictors was of course not known beforehand. The judges' predictions appeared to be influenced mainly by their assessments of the seriousness of the offense, their judgments of social stability, and the length of the arrest and conviction records.

Measures of Risk

Two risk measures were developed for use in controlling statistically for the *a priori* risk of new arrests. Each was based on information available at the time of sentencing. Risk Measure 1 was based on the whole sample. Risk Measure 2 was based on probationers only.

Risk Measure 1 included, as the best predictors, measures of age, the judges' ratings of the arrest record, race, heroin or barbiturate use in the two years prior to arrest, alcohol abuse, and type of crime (property or serious drug offense). In combination, these and other items accounted for 23 percent of the differences in new arrests. The same measure was correlated substantially with other outcomes such as the total number of arrests and charges.

Risk Measure 2 included most of those in the other scale plus items such as the number of prior probation sentences, sex, and prior incarceration for probation or parole violation. The scores on this measure accounted for 31 percent of the variation in the "any new arrest" criterion.

Other prediction methods were developed for other criteria — new arrests within 5 years, the total number of arrests, and the total number of person offenses.

Selection of Sentences by Judges

Measures of judicial selection of sentences were devised in order to control for it in the statistically designed studies of effects. These included the choice of confinement or not (the "in-out") decision; selection of the four main alternatives (non-custodial, jail, youth facility, or prison); of the three main custodial sanctions; and of "split" sentences.

Selection for any confinement (In-out).

In deciding whether to order confinement, judges appeared to be influenced mainly by their assessment of rehabilitation as an important aim of sentencing in the particular case, the recommendation of the probation officer for confinement (unless the P. O. and the judge used the same information in arriving at the decision), their own prediction of future crimes, and the seriousness of the offense. These and other items enabled correct classifications ("predictions") of confinement in 88 percent of the sample.

Selection of Type of Sanction

Selection of a non-custodial sentence or of jail, the youth facility, or prison appeared to be influenced mainly by the judges' predictions of any future crime, the perceived importance of rehabilitation for the case, the seriousness of the charge and prior criminal record, the number of counts of conviction, and the recommendation of the probation officer. The sentence type appeared to be influenced also by age and whether a property crime.

Selection for Split Sentences

In ordering split sentences, judges seemed to be influenced by their own predictions of future crimes, aggravating factors, and the relative importance of retributive and rehabilitative aims.

Effects of Sentences

The effects of different sentences were studied using statistical designs controlling for the measures of selection, *a priori* risk, time confined as a result of the sentence, and time exposed to risk in the community later (time minus incapacitation time for later confinements). Most analyses relied on the analysis of covariance; others on regression methods. Typically, a comparison was made between a "naive" interpretation of the observed outcomes without consideration of the effects of selection, risk, and incapacitation and "adjusted" values taking into account the potentially biasing factors.

Effects of the "In-out" Decision

Persons not confined had new arrests in 70 percent of cases, and those ~~not~~ confined were re-arrested 82 percent of the time. When adjustments were made for selection, *a priori* risk, and time in the community (not incapacitated) there was no statistically significant difference for the two groups. The results were the same when only the first five years after sentencing was

considered. Similarly, when selection, risk, and incapacitation were taken into account, confinement had no effect on the arrest rate or the number of arrests, charges, or charges for specific crime categories.

Effects of Type of Sentence

The type of sentence — non-custodial, jail, youth facility, or prison, had a statistically significant effect on new arrests during the first five years after sentencing. After adjusting for selection, *a priori* risk, and incapacitation, those sent to the youth facility had the highest adjusted percent of new arrests. The adjusted values for those given non-custodial, jail, or prison sentences were about the same. Results were similar with the 20 year follow up but the differences were not statistically significant.

When persons with non-custodial sanctions were excluded from the analysis, a statistically significant effect of placement in jail, youth facility, or prison was found with the 20 year follow up. The adjusted values for jail and prison were the same, but those sent to the youth facility had higher adjusted values for new arrests. The effect was not statistically significant when only the first five years after sentencing was considered.

The choice among the four alternatives had a small effect, due mainly to higher adjusted values for new arrest by those sent to the youth facility. This effect could not be explained by age, selection, *a priori* risk, or incapacitation. Placement in non-custodial programs, jail, or prison had no effect.

Effects of Maximum Sentence

No effect of the maximum sentence, beyond incapacitation, was found.

Effects of Time Served in Confinement

After adjustments for selection, *a priori* risk, and incapacitation, it was found that the time actually served in confinement had a statistically significant but small effect on new arrests, accounting for less than 2 percent of the variability in new arrests.

Effects of Split Sentences

The imposition of jail along with probation had no effect on new arrests.

Effects of Fines and Restitution

Neither fines nor restitution had a significant effect on new arrests during the first five years after sentencing. No difference was found, either, between those who complied with the fine or restitution order and those who did not.

Other Effects

Analyses of survival in the community after serving any incapacitative sentence indicated that effects of confinement in the youth facility or in prison may change over time and should be further investigated.

Implications

The diverse choices of sentencing purposes by judges in this study, and the selection of inconsistent purposes for the sentencing of individual cases, support the need for greater clarity and consistency in sentencing aims. The conflict between utilitarian and desert perspectives was apparent in this study, despite a general preference on the part of judges for utilitarian, crime control purposes. Clarity and consistency could be increased by acceptance of an internally consistent sentencing theory and its consistent application.

The subjective risk judgments by judges had a substantial effect on sentencing choices despite a modest validity. The use of more formal, empirically derived methods would enhance the rationality of sentencing when risk is determined by the sentencing theory accepted to be a relevant and justifiable consideration.

The main sentencing choices available to these judges had little effect on their crime control aims. Except for the effect of incapacitation, *whether* the offender was confined made no difference. *Where* the offender was confined made little difference – except perhaps for an unfavorable effect of placement in the youth facility. *The length of the maximum sentence* imposed made no difference. *The length of time actually confined* made a slight difference. When *jail* was imposed *along with probation*, it made no difference. *Fines or restitution* made no difference.

Aside from general deterrence (not studied) and incapacitation (in this study mainly providing a correction for the investigation of other crime control effects) little justification for differences in sentences was found from a crime control perspective. The different sanctions, varying in severity of punishment, may have served as a warning to others or as deserved punishment; but there was little or no evidence of rehabilitative or specific deterrence effects.

If the addition of jail time to a probation sentence is believed to have a specific deterrent effect, no evidence can be found in this study to justify the belief. Unless it is believed that jail time is required for desert, or the hope of an effective warning to others is maintained, abandoning the use of split sentences would be supported by this study.

Similarly, if considerations of general deterrence and deserved punishment are set aside, it must be concluded that confinement, or increased length of incarceration, served the crime control purpose of incapacitation but had little or no effect as a "treatment" with rehabilitative or specific deterrent effects.

Limitations

Conclusions from this study obviously cannot be generalized to other jurisdictions and other times. The study does not show, for example, that well designed interventions implemented with fidelity to a clear, coherent theory cannot be effective in meeting crime control objectives. No measures of the quality of rehabilitative programs or of the severity of sanctions were available.

Statistical designs such as used in this study set limits to generalizabilty because it cannot be assured that offenders compared after subjection to different sanctions were equivalent in all respects at the outset. Perhaps remaining but yet unknown selection factors, affecting either judicial decisions or the risk measures, affect the outcomes.

Other limitations are due to a crude measurement of outcomes. Simple measures of arrests and charges are not adequate to assess the full spectrum of costs and harms associated with either the sanctions imposed or the new crimes in the community.

Similarly, conclusions must be limited by the lack of information about the main independent variables of the study: adequate description of the programs of community treatments or jail and prison programs was not available. No measures of the quality of rehabilitative programs or of the severity of sanctions within the main types of sentences imposed were available.

The study did not investigate whether the sentences imposed were deserved or whether they were fair; and it did not investigate effects of general deterrence.

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Introduction

Attempted sentencing reforms aimed at crime control are rarely informed by rigorous analyses of the effectiveness of the policies and practices thereby changed. Each of the traditional aims of sentencing — retribution, general deterrence, incapacitation and treatment — is thrown into the pot, highly seasoned with rhetoric about getting tougher, giving criminals the punishment they deserve, keeping them off the street, and controlling crime. When stirred into a theoretically inconsistent mass, the resulting stew, concocted with something for everybody, is apt to please few. Expected consequences of legislative changes, sentencing guidelines policies, or limiting judicial discretion are announced and argued about; but rarely are these expectations based on evidence that can come only from careful examination of the results of sanctioning practices. Arguments for and against the use of various alternative punishments typically are made in the absence of information about the probable results of choices — whether made legislatively or judicially.

This lack is partly due to the fact that experiments designed to test central sentencing questions are not feasible. The term "experiments" implies that groups treated differently can be considered equivalent in all respects except for that differential treatment. Then observed differences in outcomes can be said (with a known probability of error) to be due to the treatment. The equivalence of the groups compared usually is sought by random assignments to the treatment conditions. Since that is rarely possible at sentencing, groups given different sentences cannot be considered equivalent at the outset, and the crime control effects of sentencing therefore cannot be compared fairly.

Because experimental designs for study of the impact of sentences on the subsequent criminal careers of convicted offenders are not feasible, the next most rigorous designs should be used. Rarely, however, are the data available to permit that, since data demonstrably relevant to selection biases due to factors associated with the decisions ordinarily are absent. As a result, little is known about the consequences for later criminal behavior of judicial choices concerning confinement and alternative non-custodial sentences. Punishment policies are therefore developed without sound information about how sanctioning modifies, controls, or enhances the likelihood of future criminal behavior by the convicted offenders sentenced. [1]

[1] Possible effects on others, i.e., general deterrence effects, are ignored in this study as beyond its scope. The study is limited to assessment of the utilitarian aims of crime control, and retributive sentencing perspectives also are set aside for purposes of this report.

The research reported here was based on a unique opportunity for application of rigorous quasi-experimental designs for the assessment of the effects of sentencing choices. More than 20 years ago, 18 judges of a felony court in an eastern U. S. county court completed, in collaboration with the author, a study of their decisions. [2] The purpose was to develop sentencing guideline models that might be developed and used voluntarily by the court. [3]

Just as an investment in the market may increase in value over time if circumstances are right, so too with data. The data collection for this study of judges' decisions had an unusual feature. The judges themselves completed, at the time of sentencing, a detailed record of their judgments, including their reasons for the sentences imposed. The validity of these judgments, and the consequences of the decisions taken, can only be learned after the passage of time and completion of the sentences. Moreover, the sentencing judges had, at that time, much broader discretion in sentencing than they now have, since the sentencing laws have been modified repeatedly in the jurisdiction of the study, as elsewhere, by passage of more determinate sentencing provisions and of various mandatory sentences. One result of the then wider discretionary power was a variability in sentences that is requisite to the study of differential sentencing effects. That is the subject of the proposed research.

The opportunity was therefore available to answer some central questions about the effects of sanctions through designs that correct for the non-equivalency of differently sentenced groups. The procedures also provided tests of the validity of various subjective judicial judgments. The statistical designs required measurement of the risk of future offending – that is, the *a priori* risk – and of the selection by judges from among the alternatives available.

In order to implement this plan, an extensive search of computerized criminal records from various sources was required in order to examine the later arrests, charges, convictions, and subsequent confinements of the persons sentenced. Next, a coding operation was needed in order to reduce these records to a form permitting their analysis.

The subsequent criminal careers of the persons sentenced in 1976 and 1977 were then examined, with analyses of the effects of sentences using quasi-experimental designs based on multivariate models of judicial sentencing behavior and on measures of other possible selection factors that could affect the validity of comparisons. This enabled comparisons of the effects of custodial and non-custodial sanctions, "split sentences," fines and other non-custodial sanctions, and the impact of length of confinement on the nature of subsequent offending. These quasi-experimental designs were supplemented by multivariate analyses of secondary, nonetheless important, questions about the validity of judges' judgments and their selection of alternative sanctions.

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[2] Gottfredson, D.M., and Stecher, B., *Sentencing Policy Models*, unpublished report to the Essex County Court, Newark, New Jersey: Rutgers University School of Criminal Justice, 1978.

[3] Theirs was a pioneer effort, for such guidelines were not yet in widespread use; the report *supra* note 2, explored various guidelines models.

The Questions for this Study

This research was designed to answer three central questions and also to investigate several secondary questions. Answering the latter questions (important in their own right) contributed to the strength of the research designed to answer the central questions.

Central Questions

The first general question is whether it makes any difference, for later offending, if the convicted person is sentenced to confinement. The judges had not only a choice to confine or not, but also a decision as to the place of confinement, if confined (jail, reformatory, or prison). Judges had broad discretion in these choices, although they were constrained for some offenders by limits on the length of sentence according to the legal class of conviction and by institutional policies. All could have had sentences to confinement, and all but about 14 percent could have been sentenced to more than a year in custody.

The second question is whether the length of sentence (imposed and also that actually served) makes any difference to measures of subsequent offending.

Third, the question may be asked whether sentences of probation combined with jail ("split" sentences), or with fines, restitution, or other alternative sanctions make any difference for later criminal behavior.

Secondary Questions

The secondary questions concern three prediction issues. These address the subjective risk predictions of the judges, prediction by more formal methods, and the selection of alternative sanctions. The first is a problem of validity. The second is required in order to control for risk in the desired comparisons of the effects of sentences. The third is needed in order to control for judicial selection.

Judges made predictions about recidivism (by any type of crime, property crime, or violent person crime) at the time of sentencing. The validity of these predictions was unknown. When prior studies have compared the validity of subjective and empirically derived objective measures of risk, the latter generally have outperformed the former. [4] Generalization of this result to judges may be questioned; and this never has been checked because judges typically do not record their predictions. The degree of validity of these predictions is the first secondary question.

[4] A number of reviews are available. See, for references to these and a summary at pages 36-38, S.D. Gottfredson, "Prediction: Methodological Issues," in *Prediction and Classification: Criminal Justice Decision Making* edited by D.M. Gottfredson and M. Tonry, Chicago: University of Chicago Press, 1987.

The best combination of available predictors is needed for the analyses of effects. This poses the second secondary question.

Judges selected various sentences, and their choices were analyzed by methods that discriminate among the alternatives. This was required in order to control for the bias of judicial selection in the comparisons of sanction effects. Thus, the "prediction" of the judges sentences is the third secondary question.

Methods

Sample

Data were collected for a sample of offenders sentenced by 18 judges between May, 1976 and June, 1977. All judges in the court with assignments to criminal cases participated. [5] The assignment judge reported that cases were assigned simply on the basis of judges' availability. After about a year, the judges had completed the final sample, which included 962 sentenced persons, distributed by judges as in Table 1. [6]

Table 1

Sample Size by Judges

Judge	Number
1	57
2	60
3	56
4	12
5	47
6	56
7	59
8	58
9	52
10	57
11	58
12	60
13	59
14	60
15	56
16	36
17	59
18	60
Total	962

Sample Representativeness

This procedure of course did not yield a random sample of offenders sentenced by the court, nor did it ensure that the subsamples for judges were representative of the whole. It is believed, however, that it did not result in any significant bias in relation to the court work load as a whole. An assessment of the representativeness of the sample was conducted by comparing characteristics of the persons included in it with those of a random sample for the calendar year 1975 from the same court (drawn by staff of the Criminal Justice Research Center, Albany, New York) and also with characteristics of the yearly court caseload. The present sample was similar to those groups in the distribution of offenses. Thus, although not randomly selected, the offender sample may be assumed to be representative of the court's sentencing activities during the year studied.

[5] One judge was assigned only welfare fraud cases and was excluded. Hence, such cases are not represented in the sample studied.

[6] Judges completed 982 forms at the time of sentencing. Of these, six were found later to be replicates. In 14 cases the offenders were returned to court during the period of sentencing data collection and were sentenced for a second time. In these cases, only the first sentencing was included.

On the other hand, there is no assurance that the judges had equivalent cases. Comparisons of judges' cases were made by analysis of variance of selected variables, and the results suggest caution in assuming judges had cases that could be considered equivalent in all respects. Significant differences were found in types of offense, age, prior arrest record, prior jail and prison sentences, and number of counts charged. The differences were small, and no significant differences were found on attributes including race, type of conviction (bench trial, jury trial, guilty plea), use of a weapon, drug use at the time of the offense, sex, or person offenses. [7]

Overview of Sample Characteristics

These offenders had been convicted of crimes typical of felony courts. The offenses were classified for this study in two ways: the legal offense classification of the New Jersey Criminal Code (2A); and a behavioral classification based on seriousness judgments (explained further later in this report). [8]

Legal Offense Groups

Based on the legally defined offenses, the most serious offenses of conviction are shown in Table 2. As Figure 1 shows, the most common type of offense was a person offense. (Robbery is included as a person offense. The code distinguished between armed robbery – 9 percent – and robbery –12 percent.) Illegal homicides accounted for 52 convictions. Nearly a fourth were convicted of property crimes. Eleven percent were convicted for burglary, (breaking and entering) and others for larceny, forgery, or fraud. Another fourth were to be sentenced for of drug offenses (11 percent for sales, and half the rest for possession with intent to sell). Ten percent were convicted of weapons offenses. Fewer than 3 percent were convicted of fraud, embezzlement, or receiving stolen property.

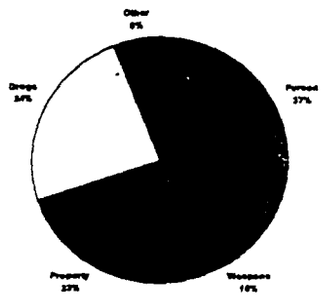

Figure 1
Most Serious Offense
of Conviction
(Legal Offense Groups

[7] Analyses reported here but not described in detail are available from the author.
[8] Gottfredson, S.D., and Taylor, R.B., "Community Context and Criminal Offenders," in Reiss, A., and Tonry, M., (Eds.) *Crime and Justice: An Annual Review of Research*, Chicago: University of Chicago Press, 1989; see also Gottfredson, S.S., and Taylor, R.B., "Person-Environment Interactions in the Prediction of Recidivism," in Sampson, R., and Byrne, J., (Eds.), *Environmental Criminology*, New York: Springer Verlag, 1976.

Table 2
Most Serious Offenses of Conviction (Legal Classifications)

Type of Offense	Number	Percent	Offense	Number	Percent
Person	355	37	Murder	37	3.85
			Manslaughter or vehicular homicide	15	1.56
			Atrocious assault and battery	60	6.24
			Assault with dangerous weapon or intent to kill, rob, or rape	28	2.91
			Rape	7	.73
			Assault on law enforcement officer	6	.62
			Armed robbery	85	8.84
			Robbery	117	12.16
Weapons	97	10	Possess concealed weapon or other firearms violation	97	10.08
Property	222	23	Larceny	30	3.12
			Breaking and entering	106	11.02
			Receiving stolen property	12	1.25
			Forgery or false pretenses	43	4.47
			Fraud or embezzlement	12	1.25
			Possess stolen auto	19	1.98
Drugs	235	24	Sale or distribution	107	11.12
			Possession with intent to sell	63	6.55
			Possession	65	6.55
Other	53	6	Other	53	5.51
Total	962	100		962	(100)

Table 3

Most Serious Offenses of Conviction

(Behavioral Classification)

Offense	Number	Percent
Nuisance Offenses such as prostitution, gambling, marijuana offenses, disorderly conduct	227	23.6
Frauds or other offenses involving deception	57	5.9
Property Offenses such as theft, property damage or loss, or other property crimes; includes robbery not involving physical assault	375	39.0
Serious Drug Offenses such as selling or manufacturing heroin, hallucinogens, or barbiturates or amphetamines	142	14.8
Personal Offenses including physical Assault or harm or other personal confrontations, including robbery	161	16.7
Total	962	100.0

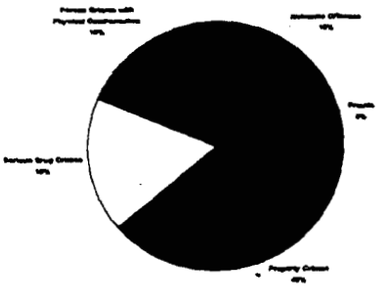

Figure 2
Most Serious Offense
of Conviction
(Behavioral Offense Groups)

Behavioral Offense Classifications

Using offense groupings for the classification based on seriousness categories, the offenses of conviction were distributed as shown in Table 3 and Figure 2.

One in six were convicted of offenses involving personal confrontation or physical harm. This includes robberies resulting in physical harm. Nearly that many (15 percent) were convicted of serious drug crimes including sales of drugs other than marijuana. Most commonly (39 percent) the offense was a property crime (including robbery when no physical assault was involved). About a fourth were classified as "nuisance" offenses such as prostitution, disorderly conduct, or marijuana possession.

Typically, these offenders were convicted on one count (three fourths), but 18 percent had two counts of conviction, and six percent three counts (one offender had 12). They had been charged with more than one count in 59 percent of the cases. Typically (the median) they had been charged with two counts.

The mean age at the time of the sentence was 28.6, the median was 26.2. The oldest offender was 73. Most of these sentenced offenders were Black (about two thirds). About a fourth were White, and five percent were Hispanics. Ninety percent were males. Most commonly, the offenses of conviction called for a maximum sentence of seven years (47 percent of all). Sentences for about a third of the sample could have ranged from ten years to life. All but about 14 percent could have been sentenced to terms of more than a year, that is, would have been

classed as felons in other jurisdictions. About half had prior jail terms; 16 percent had been in prison.

When recording their sentences, judges listed aggravating factors in 35 percent of the cases; they noted mitigating factors in 42 percent. Commonly noted aggravating factors were a serious or bizarre nature of the offense (96 cases); a large amount of drugs involved in the crime (27 cases); a crime committed while the person was on probation or parole (17 cases); a lengthy prior record (49 cases); a history of similar crimes or a series of crimes (27 cases). Most often noted mitigating factors were no prior record or a minor one (94 cases); a plea of guilty, sometimes noted as a result of a plea bargain, (listed as mitigating in 48 cases); and youth or immaturity of the offender (31 cases).

Data Sources

Judicial Questionnaire

The questionnaire completed by the judges was designed, with their collaboration, to provide information on their perceptions and judgments of the offense, offender, and the sentencing decision. [9] The judges provided, besides the general purposes they used to justify the sentence in each case: a description of the actual sentence imposed; assessments of the seriousness of the offense; an estimate of the propensity of the offender toward future crimes (of any type, for a person offense, and for a property offense); judgments of the lengths of the prior arrest and conviction records; a rating of the seriousness of the prior record, an assessment of the social stability of the offender; and a listing of the mitigating or aggravating factors affecting the choice of sentence. The judges used a scale, ranging in values from 1 to 9 for most of the items, to record their assessments.

Probation Staff

The probation staff completed more objective items extracted from case files. They included: charged and convicted offenses; the number of prior probations; probation revocations; jail terms; prison terms; the sentence recommendations of the prosecutor and of the probation department; and information on previous sentences being served by the offender at the time of the present sentencing. These data were collected after sentencing; they were not systematically provided to the judges but usually they were included in the presentence report.

[9] Forms used for data collection and other coding materials are available from the author.

Case Files

After a year, a further data collection for this sample was initiated to follow these cases in order to begin to determine how the sentences actually were carried out by the correctional system and to attempt an assessment of the impact of the sentence on subsequent criminal behavior. These data were collected by graduate student assistants from probation, jail, prison, and parole files, including detailed data about those offenders who were in community programs during the first few years after sentencing. When those data were collected, however, a large share of the offenders had little time in the community exposed to the risk of new offenses. Many were still confined, and funds were not available to support travel to all institutions for data collection. These data are therefore available for a subsample; but it is biased toward non-custodial sentences. In this study these data were used mainly for some analyses about the offenders who received a sentence of probation.

Follow up from Official Records

A first phase of the follow up data collection effort required the identification and collection of computerized criminal history reports; a second required the coding and entry of the data. The data collection began in October, 1995 and was finished in February, 1997. [10]

The first task required searches for records in five computerized criminal history systems and the New Jersey Department of Corrections, as follows:

- **The New Jersey Offender Based Transaction System Computerized Criminal History**. This system includes New Jersey arrest records, with arrest and conviction data from 1972 to the present.
- **The New Jersey Department of Corrections Offender Based Correctional Information System.** This includes New Jersey State correctional history data on admissions, departures, and status of state prisoners from 1976 to the present.
- **The United States Department of Justice Interstate Identification Index.** This system includes out of state arrest records of arrests and convictions from the date of entry into the system to the present.
- **The National Crime Information Center Wanted Persons File**. This includes current outstanding warrants from New Jersey and other state law enforcement agencies.
- **The New Jersey PROMIS/GAVEL Prosecutors Case Tracking System.** This system, used on a selective basis, includes pending or open New Jersey charges.

[10] A detailed description of these procedures is available from the author.

- **Record files of the New Jersey Department of Corrections.** These files, used on a selective basis, include data on entry, transfer, and exits from the New Jersey prisons.

The inquiry into these systems resulted in identification of 939 of the 982 original records from questionnaires completed by the judges. Of the remaining 43, we could not match 37 and 6 were duplicates. In 14 cases the offenders were returned to court during the period of sentencing data collection and were sentenced for a second time. In these cases, only the first sentencing was included. The 37 cases for whom no record could be found (4 percent of all) after this exhaustive search were coded as having no known further criminal record and retained in the sample. [11] The total sample was reduced, from the original 982 questionnaires completed by the judges to that number minus the 6 duplicates and the 14 returned soon to court, to the 962 cases in the full sample for the study.

The follow-up data were coded according to the procedures previously indicated. In order to code the arrest records, coding forms, associated instructions, and definitions for coding were devised, based on procedures developed and used in earlier studies. [12] These procedures attend to arrests known, charges filed, and dispositions noted as well as to issues of the nature and seriousness of offending. Experience with this coding scheme had demonstrated that the data for the dependent variables listed above can be obtained reliably from arrest records, by means of a coding operation by trained research assistants.

Measurement

These several sources of data provided an unusually complete record of arrests, charges, convictions, and subsequent sentencings and their implementation. [13] This allowed the accurate measurement of a variety of important outcome variables, as next explained.

Dependent Variables

Among the most critical variables for this study are those defining the outcome in terms of new offense behavior. Reviews of problems are available. [14] These include: the validity of

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[11] In these cases, as with every other case, "no arrests" means "no known arrests."

[12] Gottfredson, S.D., and Taylor, R.B., 1989; Gottfredson, S.S., and Taylor, R.B., 1976; Gottfredson, S.D., and Gottfredson, D.M., "Behavioral Prediction and the Problem of Incapacitation," Criminology, 32, 3, 1944, 441 - 474; Gottfredson, S.D., and Gottfredson, D.M., *Criminal Violence and Incapacitation: Wishes and Realities*,Final Report to the National Institute of Justice, Sacramento,: Justice Policy Research Corporation, 1991.

[13] Resources did not permit a search for death records, and the total number of deaths during the follow up period is unknown. Twelve persons were known to have died during the follow up period. Two had received non-custodial sentences, 4 jail, 4 the youth facility, and 2 prison. Before their deaths, none had been re-arrested. One person with a life sentence still was confined at the time of the follow up data collection.

[14] See, e.g., Blumstein, A., and Larson, R.C., "Problems in Modeling and Measuring Recidivism,"*Journal of Research in Crime and Delinquency*, 8, 1971, 124-132; Waldo, G. and Griswold, D., "Issues in the Measurement of Recidivism, "in National Research Council, *The Rehabilitation of Criminal Offenders*; Washington, DC.: National Academy of Sciences, 1979, 225-250; Gottfredson, D.M. and Gottfredson, M.R., "Data for Criminal Justice Evaluation: Some Resources and Pitfalls," in *Handbook of Criminal Justice Evaluation*, edited by M.W. Klein and K.S. Teilman, Beverly Hills: Sage, 1980; Maltz, M.,

available data as a measure of outcome; the inability of dichotomous success/failure criteria to capture the full range of post-release or post-sentencing adjustment (and statistical difficulties inherent in the use of a dichotomous criterion); the possibly confounding effect of "time at risk" when comparing experiences of offenders who have been in the community for varying lengths of time; and differing error rates depending upon the nature of the criterion chosen (e.g., arrest, conviction, or incarceration).Other concerns include frequent failures to observe a long enough follow-up period, which typically is too short to measure subsequent offending adequately; use of fixed follow up periods with a failure to examine failure rates over time; and the use of narrow definitions without recognizing the complexities of the concept of "recidivism." This study addresses some but not all of these problems.

Outcomes were measured for this study mainly by arrests and charges. These data were assumed to be more reliable than are convictions in the follow-up records to be used (although convictions, dispositions where shown, and dates for confinement were recorded). Arrest and charge records provide dates that are nearest in time to offense behaviors. The direction of errors expected (accepting as failure an arrest for an offense not committed versus excluding offense events as a result of attrition in the criminal justice processing from arrest to conviction) was assumed to be the better choice.

The measures used also emphasized classifications of the seriousness of new offense behaviors. A major development in the measurement of crime has been the effort to improve upon behavioral representations by assessing the seriousness of criminal acts. Measurement of the seriousness of crimes dates from Thurstone; [15] replications suggest that these judgments remain remarkably stable over time. [16] Others, using similar methods, have developed more comprehensive measures. [17]

The classification used was based on the multidimensional approach to scaling offense seriousness developed by S. Gottfredson. [18] Through principal components analyses of judgments of discrete criminal acts, these studies suggested that six dimensions underlie people's judgments of such acts. The first dimension, called "nuisance" offenses, includes such

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Recidivism, Orlando: Academic Press, 1984; Schmidt P and Wittee, A.D. *Predicting Recidivism Using Survival Models*, New York: Springer Verlag, 1988; Blumstein, A., Cohen, J., Roth, J.A., and Visher, C.A., (Eds)*Criminal Careers and "Career Criminals,"* Washington, D.C.: National Academy Press, 1968.

[15] Thurstone, L.L., "The Method of Paired Comparisons for Social Values,"*Journal of Abnormal and Social Psychology*, 21, 1927, 384-400.

[16] Coombs, C.H., "Thurstone's Measurement of Social Values Revisited, Forty Years Later,"*Journal of Personality and Social Psychology*, 6, 1967, 91-92; Krus, J. Sherman, J.L., and Krus, P.H., "Changing Values Over the Last Half Century: The Story of Thurstone's Crime Scales," Psychological Reports, 40, 1977, 207-211.

[17] Rossi, P.H., Waite, E., Bose, C.E., and Berk, R., "The Seriousness of Crime: Normative Structure and Individual Differences," *American Sociological Review*, 39, 1974, 224-237; Sellin, T., and Wolfgang, M.E., *The Measurement of Delinquency*, New York: Wiley, 1964.

[18] Gottfredson, S.D., *Measuring Offense Seriousness: A Dimensional Approach*, Baltimore: Center for Metropolitan Planning and Research, The Johns Hopkins University, 1981; Gottfredson, S.D., Young, K. and Laufer, W., "Interaction and Additivity in Offense Seriousness Scales," *Journal of Research in Crime and Delinquency*, 17, 1980, 26-41; Gottfredson, S.D., and Taylor, R. B., "Community Context and Criminal Offenders, in*Communities and Crime Prevention*, edited by T. Hope and M. Shaw, London: Her Majesty's Stationery Office, 1988.

offenses as prostitution, gambling, use and possession of marijuana, adultery, disorderly conduct, homosexual acts, exposures, and the like. This classification also includes parole and probation rules violations, and drunk driving. In general, people view crimes loaded on this dimension as relatively non-serious. The second dimension involves physical assault, personal harm, and interpersonal confrontation. The third represents theft, property damage or loss, and property crimes in general. The fourth dimension is called "crimes against the social order." In general, these are either crimes that are committed by an agent or agency in power (an employer, a real estate agent, a police officer, a manufacturer, a doctor, a public official), social crimes (e.g., racism,, polluting a water supply, marketing contaminated products, price fixing, false advertising), or both. Crimes loading on the fifth dimension involve serious drug offenses: selling or manufacturing heroin, hallucinogens, or barbiturates and amphetamines. Offenses loading on the sixth dimension largely involve fraud or deception. [19] The categories of conviction for which nearly all the persons in the sample to be studied were convicted, and those thought probably to be most important for the follow-up study, are personal confrontation offenses (homicide, rape, assault, robbery with physical harm or assault), property offenses not resulting in physical harm (burglary, robbery and attempts without physical harm or assault, larceny, auto theft), frauds (including forgery and bad checks), serious drug offenses, and nuisance offenses. [20]

One advantage of this dimensional approach to the scaling of offense seriousness that it allows a ready assessment of both the seriousness and the nature of criminal offenses. The materials used defined the procedures for coding criminal careers with this approach, adapted from that described by Gottfredson and Taylor. [21] Information was available only for coding the general classes or dimensions. It was not possible to score behaviors on seriousness within the classes, because that requires more detailed descriptions of the behavior involved in the offense, as typically found in police reports.

The dependent variables describing offender outcomes used for this report (according to the purposes of the various analyses) included the following:

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[19] These dimensions do not merely represent "ranges" along a single underlying dimension. Rather, they overlap one another substantially along the first order dimension of overall judged seriousness. As example, an offense classed as "property crime" may have a higher seriousness value than one classed as a "person crime," even though person offenses in general are scored as more serious.

[20] Robbery without assault or physical harm is considered to be an offense against persons in many offense typologies. The classification used here, however, was derived empirically from the seriousness assessments of very large samples of persons and has been demonstrated to be useful for diverse groups of decision makers (such as police officers and judges). If a robbery resulted in any physical harm, or was accompanied by an assault, then the incident was coded not as an offense against property but as against the person. Thus, the offenses classified as person offenses are those generally considered to be the most serious, even though robberies without physical harm, or burglaries without confrontation, are severe intrusions of persons. The reader should bear in mind the distinction between person offenses based on legal definitions (and usual practice, including all robberies as person offenses) and personal confrontation offenses in the behavioral classification used here.

[21] Gottfredson, S.D., and Taylor, R.B., "Person-environment Interactions in the Prediction of recidivism," in *The Social Ecology of Crime*, edited by J Byrne and R. Sampson, New York: Springer Verlag, 1986; Gottfredson, S.D., and Taylor, note 13, *supra*; Gottfredson, S.D. and Gottfredson, D.M., "Behavioral Prediction and the Problem of Incapacitation," *Criminology*, 32, 3, 1994, 441-474.

1. Seriousness Class, Most Serious Charge, first Arrest Episode
2. Number of Arrests to Desistance [22]
3. Number of Arrests for Nuisance Offenses (to Desistance);
4. Number of Arrests for Person Offenses (to Desistance);
5. Number of Arrests for Property Offenses (to Desistance);
6. Number of Arrests for Fraud Offenses (to Desistance);
7. Number of Charges to Desistance.
8. Number of Nuisance Charges to Desistance.
9. Number of Person Charges to Desistance.
10. Number of Property Charges to Desistance.
11. Number of Fraud Charges to Desistance.
12. Number of Serious Drug Charges to Desistance.
13. Any Subsequent Arrest (0=No, 1=Yes).
14. Time to First Arrest After Sentencing(months).
15. Time to First Arrest After Release From Confinement by the Present Sentence (months)
16. Seriousness Classification of First Charge Post-sentence.
17. Yearly Rate of Arrest (μ) [23].
18. Any Arrest During First Five Years After Sentence 0 = No, 1 = Yes).
19. Time Incarcerated Except for Present Offense (total time in jail or prisons after sentencing, days or months, exclusive of present sentence) This includes all time served after later arrests; and it includes time served in jails and in prisons.
20. Time Confined Before 5 Years After the Sentence Date. For some analyses, only the first five years after sentencing is examined. In this case the relevant period of incapacitation is the number of months confined in jail or prison before five years.
21. Time in Jail, Present sentence. This includes only the actual time served in jail as a result of the sentence.
22. Time in Prison, Present Sentence. This includes only the time served in prison as a result of the instant sentence until first release from that prison confinement.
23. Time Incapacitated by Present Sentence. This is the sum of jail and prison time served on the present sentence.

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[22] "Arrests to Desistance" was taken as the number of arrests in the follow-up period (approximately 20 years after sentencing). Since the probability of arrests decays rapidly over time, this seems reasonable, although it of course is not known that any offender actually "desisted."

[23] The numerator of μ is the number of arrests recorded during the follow-up period. The denominator is the length of time from sentencing or release from confinement on the present sentence to the end of the follow-up period, minus the cumulative lengths of time subsequently incarcerated (if any). This variable must be distinguished from λ in the sense used by Cohen (Cohen, J. "Research on Criminal Careers: Individual Frequency Rates and Offense Seriousness," in A. Blumstein, J. Cohen, J. Roth, and C. Visher, (Eds.) *Criminal Careers and "Career Criminals"* Washington, D.C: National Academy of Sciences, 1968) and others, who, in order to estimate crime rates, adjust μ (the rate of arrest) by an estimated likelihood of arrest given the commission of a crime.

Time Incapacitated

It is necessary, for all analyses of sentencing effects, to control for time at risk of offending in the community. The exposure to this risk varies among offenders according to the sentence imposed and as actually implemented, and also as a result of further confinement due to repeated offending during the follow-up period. Variables 19 through 23 served this purpose, depending on the relevant period of incapacitation for the problem studied. Dates in and out of confinement were subtracted to obtain the number of days confined, then days were converted to months. Similarly, dates between events were calculated in days and converted to months or years according to the analysis desired. The time exposed to the risk of re-arrest is the length of the follow up period minus the time incapacitated by the present sentence or later confinement.

Independent Variables

For each of the quasi-experimental designs intended to answer the "central questions" posed above, the independent variable is the type of sentence imposed by the court. This may be classified in several ways, however. A first analysis was based on the classification "custodial *vs.* non-custodial" sentence (the "in - out" sentencing decision). A second classified sentences as "jail, prison, reformatory, non-custodial." The three types of confinement (jail, prison, reformatory) were compared next. Other analyses were based on classification of the length of maximum sentence or of time served as relatively short, medium, or long. In additional analyses, length of time of maximum sentence and of time actually served were included as continuous variables. Whether probation combined with jail, fines, or restitution affected variation in the dependent variables was studied using similar methods.

For the various "secondary questions" listed previously, the independent variables were selected according to the nature of the questions, as follows:

For question 1, which calls for a test of the validity of subjective judicial perceptions, the three ratings made by judges of the probability of any crime, of a property crime, and of a person crime are the three independent variables.

For question 2, which requires study of judicial choices for measurement of selection effects, the independent variables are those available to the judge and found helpful in describing the sentencing decisions.

Analysis Plan

A variety of analytical methods were used to address the variety of questions posed. Statistical designs (or "quasi-experimental" designs) were intended to provide tests in which the expected bias due to lack of a true experiment was removed so far as possible, with the goal that

it may be ignored. [24] Others were methods commonly used in prediction studies. In order to clarify the nature of the analyses, they will be described in some detail for a few of the questions; other analyses were similar and will be presented in less detail..

Central Questions: Quasi-experimental and Non-experimental Designs

Does a sentence to confinement affect outcomes? This was the question for the first quasi-experimental design. The dependent variables, for separate analyses, were the dichotomous and continuous variables previously listed. The independent variable was confinement or not. Two different procedures were used to control for selection in order that comparisons of outcomes could be made.

In the first procedure, offenders were classified according to a function that seeks to model the judges' decisions. Regression, discriminant function, and logit analyses were completed to identify the judgmental variables most predictive of the incarceration decision (in - out). When a logistic regression, which is theoretically superior, [25] was calculated, no substantive difference of the result with that of the regression or discriminant functions analysis results were observed. (The regression analysis, with confinement scored 1 and non-confinement scored 0, is equivalent to the discriminant function within a transformation. [26]) For consistency and simplicity of presentation, the results with dichotomous dependent variables are given for the regression and discriminant analyses.

It often has been found that the legal class of the offense and some measure of the prior criminal record of the offender are among the best predictors of the incarceration decision. [27] To these and similar variables, the judges' ratings may be added in order to see whether discrimination may be improved thereby. The function used for the analyses of effects of sentences was the best resulting equation using both "legal" and "extra-legal" data elements. [28]

The second procedure classified offenders by *a priori* risk in order to control for that also. The best available measures of *a priori* risk of any new arrest (according to the analyses described later) were included in the analyses as covariates.

Thus, the basic design used was the analysis of covariance. The covariates typically were: (a) the best available measure or measures of judicial selection; (b) the best available

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[24] For a detailed discussion of the issues involved, see Berk, R. A., "Causal Inference as a Prediction Problem," in Gottfredson, D.M., and Tonry, M., (Eds.), *Prediction and Classification: Criminal Justice Decision Making, Volume 9 of Crime and Justice: A Review of Research*, Chicago: University of Chicago Press, 1987, 183-248.

[25] Press, S.J., and Wilson, S., "Choosing Between Logistic Regression and Discriminant Analysis," *Journal of the American Statistical Association*, 1978, 73, 364, 699-7050.

[26] See, e.g., Porebski, O.R., "On the Interrelated Nature of the Multivariate Statistics Used in Discriminatory Analysis," *British Journal of Mathematical and Statistical Psychology*, 1966, 19, 2, 1966, 197-214.

[27] Gottfredson, M.R., and Gottfredson, D.M., *Decisionmaking in Criminal Justice: Toward the Rational Exercise of Discretion* (2nd Ed.), New York: Plenum, 1988.

[28] The procedure typically used for the discriminant function and regression analyses was a stepwise one with .05 for inclusion and .10 for deletion of predictor variables.

variables such as the number of months served in confinement, it started with multiple regression. In each analysis, account was taken of potential confounding effects of judicial selection, *a priori* risk, and time at risk in the community.

The effects of the main sentencing alternatives on the length of time before arrest were examined also. A sentencing option may meet crime control goals if the commission of new crimes is delayed by the sanction. Survival analyses were done in order to explore whether the cumulative survival (or hazard) curves differ according to sentences.

Secondary Questions: Judges' Predictive Validity, Sentence Selection, and Risk

Each of the "secondary questions" implies a prediction study that could rely on well known and widely used analytical methods. An important distinction, however, must be made between two sets of the analyses, corresponding to two differing purposes. In the first case, the analysis of judges' predictions, the analysis is a validation study. That is, the purpose was to investigate the validity of measures thought, at the time of sentencing, to be predictive of later behavior. For these studies, the concern is the accuracy of predictions. Various measures for comparing validities are available. [29] For simplicity and uniformity in this report, the point biserial or Pearson product moment correlations will be reported (for dichotomous and continuous dependent variables respectively).

In the second case, the best measure or measures of *a priori risk* and of selection is wanted for the purpose of statistical control in the quasi-experimental methods described, *in the sample being studied*. For this purpose, validation is not needed; we wish to control for risk and selection as observed in this sample. Therefore, the available data were analyzed in order to obtain these equations for the sample being studied.

The efficiency of the risk measures will be reported simply by the correlations with outcomes. The efficiency of the classifications by the selection functions will be reported simply in terms of the percent correctly classified. [30]

Before turning to the results concerning these questions, brief descriptions of the sentences imposed and executed will be presented; and the purposes of the sentences as indicated by the judges will be described. Then some outcomes of the sentences, determined by the follow up study, will be reported.

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[29] For a review, see Gottfredson, S.D., and Gottfredson, D.M., "The Accuracy of Prediction Models," in Blumstein, A., Cohen, J., Roth, J.A., and Visher, C.A., (Eds.), *Research in Criminal Careers and "Career Criminals," Volume 2* Washington, D.C.: National Academy Press, 1986.

[30] When the discriminant function was used, *a priori* group proportions were set at equal for groups (a conservative assumption).

measure or measures of *a priori* risk; (c) time served on the present sentence and (d) time in the community (not incapacitated by jail or prison due to later arrests or convictions.)

One comparison was that of the outcomes of persons expected (on the basis of the discriminant analysis) to be confined but were not with the outcomes of those who were expected not to be confined but nevertheless were. This design has the advantages of a "matching" design, with persons matched according to the judgments of the judges found relevant to their sentencing, while controlling also for *a priori* risk and incapacitation. The results of this quasi-experiment will be easily interpretable, for example, to judges, legislators, or other policy makers who may be relatively unsophisticated about multivariate statistical methods.

In order to exploit the data fully, however, an alternative design, which provides similar information but can be based on all the cases, was used for this problem and for other analyses. This was simply the analysis of covariance with the best measures of judicial selection, *a priori* risk, time served on the sentence if appropriate, and time at risk in the community later as covariates. The adjusted means for the dependent variable in each analysis provide measures of that variable adjusted for selection and for *a priori* risk. (In the case of a dichotomous criterion such as "any arrest," scored 0 or 1, the mean is a proportion.) At the same time, the analysis shows the proportion of variance in the outcome attributable to (a) judicial selection, (b) *a priori* risk, (c) time free in the community or incapacitation, (d) unexplained factors.

The question whether it makes any difference if the offender is sent to jail, the reformatory, prison, or given a non-custodial sentence was studied in an analogous fashion. The analysis of covariance was used, with the several equations for the discriminate functions as covariates, along with the best measures of *a priori* risk and incapacitation.

In order to answer the second general question, whether the length of sentence imposed makes any difference, similar methods were used. Separate analyses were needed, however, for the sentences as imposed and as actually carried out. Similar controls for judicial selection, *a priori* risk, and time in the community were used. Groups of offenders also were classified as relatively short, average, and long termers after examining the distributions of time served, in order that an analysis similar to that described concerning the "in / out" decision could be completed. In addition, analyses of the partial correlations, without the arbitrary classifications of length, controlling for the potentially biasing factors: the best available equation for sentence length expectation; the best available *a priori* risk measure; and time free in the community.

The analysis of "split" sentences proceeded in the same way. Persons sentenced were classified into two groups: those with a sentence to probation only and those sentenced to probation combined with some time in jail.

Various multivariate methods were used, according the nature of the dependent variable for the analysis. For example, for dichotomous criteria such as "any new arrest or not," the analysis began with logistic regression and the regression analyses was reported. For continuous

Results

Incarcerative Sentences Imposed

Sentences imposed were quite variable: nearly all offenders (94 percent) were sentenced to some incarceration, but all or a portion of that confinement often was suspended. About 58 percent of these offenders actually were incarcerated. The distribution of maximum sentences up to two years is shown in Figure 3 and those for two years or more are shown in Figure 4.

If sentencing were an exact science, one might expect that the penalty imposed would be precisely selected to achieve the specific objectives determined to be appropriate. The exactly right sentence might be, for example, imprisonment for four years, three months, and 15 days. Or, it might be eight months in jail, with three months suspended to be served under probation supervision with drug testing once every four days and group counseling every day for the first six weeks. Of course, such precision is never expected; but it nevertheless is striking that an examination of distributions of sentences invariably shows the nature of approximate justice that is found in reality.

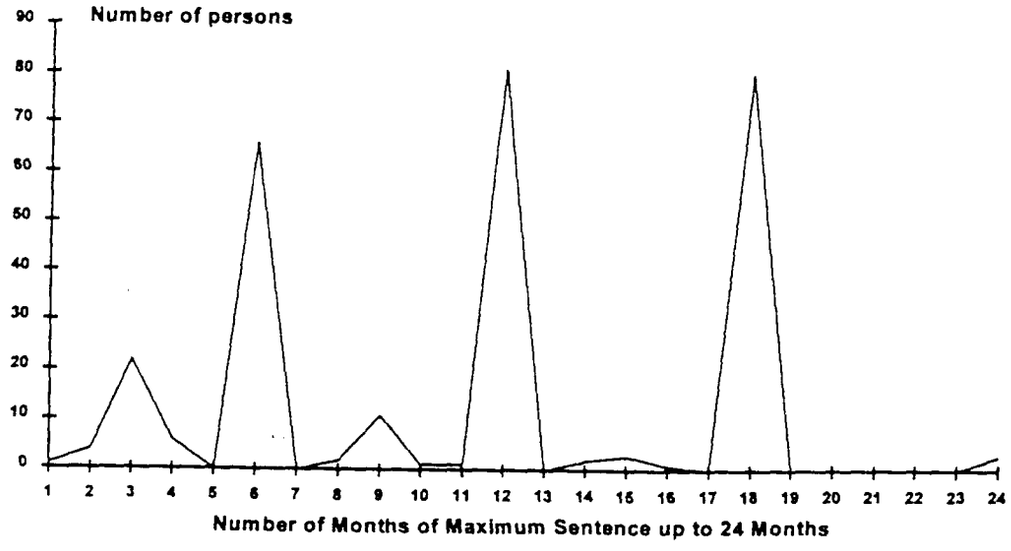

Figure 3:

Number of Months of Maximum Sentences of Two Years or Less

Note: These are sentences imposed, not sentences executed.

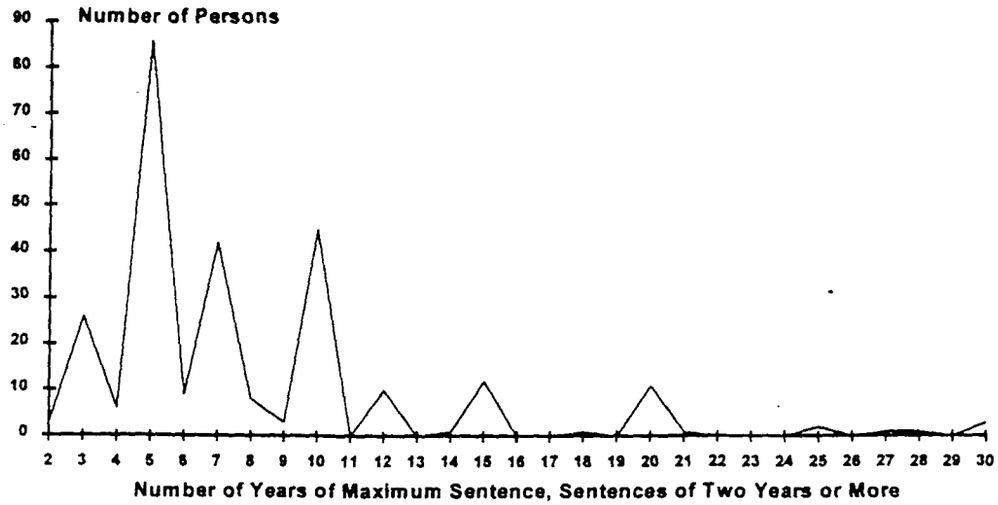

Figure 4:

Number of Years of Maximum Sentences of Two Years or More

Note: Not included are one sentence of 31 years, one of 40 years, one of 53 years, one of 57 years, and one of life. These are sentences imposed, not always executed.

Certain sentences seem to be preferred, for seemingly no particular reason. This is found in different places, at different times. In England in 1873, Sir Francis Galton studied the sentences of all males imprisoned that year. The frequencies of sentences in years are shown in Figure 5.

Galton wrote, about the sentences he observed:

> It would be expected that the various terms of imprisonment . . . should fall in a continuous series. Such, however, is not the case. . . . The extreme irregularity of the frequency of the different terms of imprisonment forces itself on the attention . . . [and] it is impossible to believe that a judicial system is fair which allots only 20 sentences to 6 years, allots as many as 240 to 5 years, as few as 60 to 4 years and as many as 360 to 3 years. [31]

Sentences of 3, 5, 7, and 10 years appeared to be preferred to the values in between. The result was an irregular distribution with a series of spikes. Galton noted a similar phenomenon in the case of sentences to months. Although there were about 300 sentences to 18 months, there were none to 17 and only 20 to 19 months. He also noted rhythmical series of 3, 6, 9, 12, 15, and 18 months and 3, 5, 7, and 10 years. Galton interpreted his results as due to "the undoubted fact

[31] Galton, F. as cited in Banks, E. (1964). "Reconviction of Young Offenders," *Current Legal Problems* 17:74, 74-76.

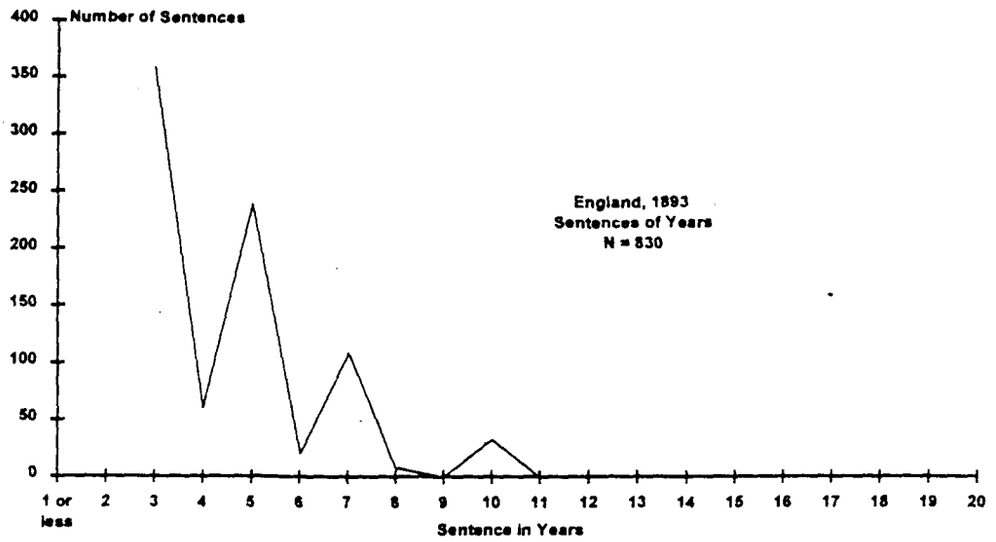

Figure 5

Number Chosen in Sentencing to Years in Prison, England, 1893

(data from Galton, as cited in Banks, E. (1964). "Reconviction of Young Offenders," *Current Legal Problems* 17:74, 74-76.

that almost all persons have a disposition to dwell on certain numbers, and an indisposition to use others." He added, "These trifles determine the choice of such widely different sentences as imprisonment for 3 or 5 years, 5 or 7, and of 7 or 10 for crimes whose penal deserts would otherwise be rated 4, 6 and 8 or 9 years respectively."

The <I>shape<D> of the distributions (aside from their "spiked" nature) in both figures also commonly is found. They are "skewed to the right," which means that there is a long tail to the right. Less severe sentences are given commonly; and, as sentences become more severe, they are more rare.

Sentences Executed

The sentence imposed often was not executed, because it was suspended, usually with placement on probation. Sentences as executed are depicted in Figure 6, showing the institutional sentences and Figure 7, non-custodial sentences. The main choices in sentences as actually carried out were, for practical purposes, limited to four: (1) a non-custodial sentence, typically with probation, with or without special conditions, which could include some time in jail, (2) jail, (3) the youth facility, and (4) prison. These include all sentences but about six percent (Figure 7).

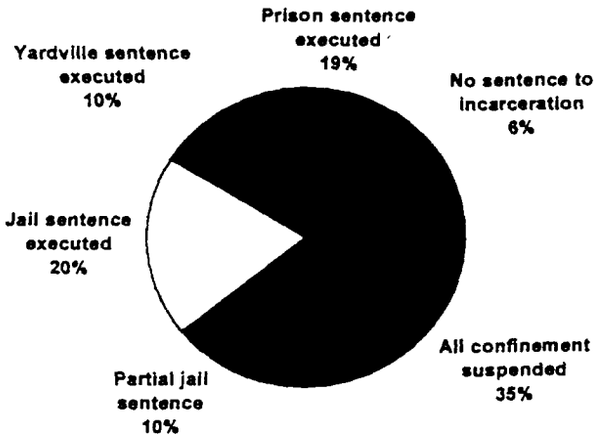

Figure 6

Executed Sentences to Confinement

Note: One person received a deferred sentence and one was a parole violator returned to prison who received no new sentence.

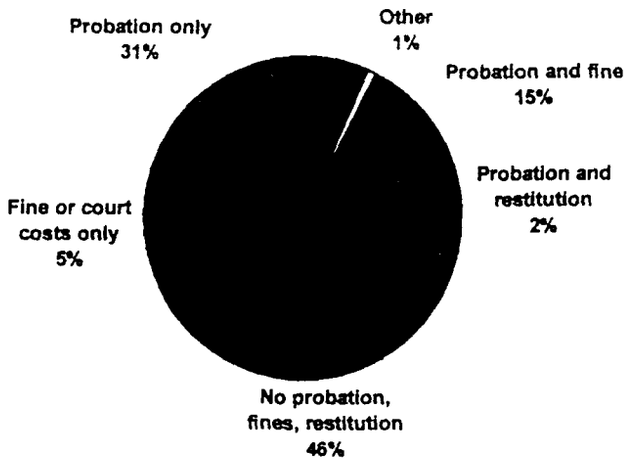

Figure 7

Non-custodial sentences

Note: "Other" includes 2 cases with restitution only and 2 others.

Table 4

Non-custodial and Custodial Sentences

Sentence	Number	Percent
Non-custodial	55	5.7
All suspended	349	36.3
Partial jail sentence suspended	93	9.7
Jail	189	19.6
Youth Institution	93	9.7
Prison	181	18.8
Deferred sentence	1	.1
No new sentence; parole violator returned to prison	1	.1
Total	962	100.0

Table 5

Non-custodial Sentences

Sentence	Number	Percent
No probation, fines, or restitution	441	45.8
Fine only (includes court costs)	51	5.3
Restitution only	2	.2
Probation only	295	30.7
Probation and fines	148	15.4
Probation and restitution	19	2.0
Probation and fines and restitution	19	2.0
Other non-custodial	1	.1
Missing data	1	.1
Total	962	100.0

Although only about six percent of offenders were sentenced to non-custodial sanctions only (such as fines or restitution) for more than a third the custodial sentence was suspended, typically with placement on probation (with or without special conditions). For ten percent, an imposed jail sentence was suspended in part, with probation following confinement in jail. General classifications of dispositions are shown in Tables 4 and 5.

The shortest maximum sentence to confinement was one month; the longest was life. Generally, when the offender was sentenced to the county facility it was for a definite term between one month and 18; if the offender was sent to the reformatory, the maximum term was usually between two and five years or for an indeterminate term up to 5 years. The maximum sentences to the state prison system generally ranged from 6 to 14 years.

Special Conditions

Various special conditions of sentences often were specified for offenders who received a probation sentence, including the following: participation in a drug treatment program (53); urine monitoring (30); alcohol treatment (15); mental health therapy (12) and dismissal from probation upon payment of any stipulated fine or restitution(15). Of all offenders sentenced, about 15 percent were placed on probation with special conditions other than time in jail.

Judges' Purposes

The judges' purposes for each sentence were likewise variable. The data collection form included the question "Which items listed below were included in your reasons for sentence and what weight did you attach to each item?" The judges then were provided with a list of the following general purposes: retribution; [32] incapacitation; rehabilitation; special deterrence; general deterrence; and other. They were asked to distribute 100 points among the purposes listed. Previously, the judges had collaborated in drawing up the list, as with the rest of the data collection instrument. The directions provided that "In distributing the 100 points, you may assign 100 for any one, provided the total is 100." The judges sometimes did select only one purpose in this manner. In most cases, however, the judges assigned varying numbers of points to several purposes.

As one way of examining these data, the judge's "primary" purpose was taken as that assigned the most points. The most popular was rehabilitation (37 percent). The most commonly identified "secondary" purpose (i.e., that most often cited second) was retribution, regardless of the primary purpose; and retribution was listed as primary in 18 percent of the cases. Special deterrence was selected as primary 10 percent of the time, and general deterrence 3 percent. Incapacitation was seen as primary in only 4 percent of the cases. (These counts do not include ties or assignment of 100 percent to the "other purpose" category, so they include only 72 percent of the cases.) Although incapacitation was cited as the primary purpose only four percent of the time, all judges except four cited it in some cases; one judge cited it as secondary only to rehabilitation, and one as secondary only to retribution. Thus, the four percent value taken as describing the primary purpose of sentences probably does not adequately represent the interest of these judges in an incapacitative intent.

The aim of changing the offender in order to reduce the probability of future crimes was strong. If rehabilitation and specific deterrence are combined as "treatment" orientations, then nearly half the sentences were said to have this goal as a primary purpose. Moreover, these judges were future oriented: the primary aim in about four out of five sentences was focused on crime control. The variability of purposes selected by the judges, and different emphases, are reflected in Figure 6, which charts the average scores for each purpose for each judge.

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[32] The term "desert" was not, in 1976 (the year of publication of *Doing Justice*, (von Hirsch, A. *Doing Justice: The Choice of Punishment*, New York: Hill and Wang, 1976) thought by the judges to be necessary to be included in their list.

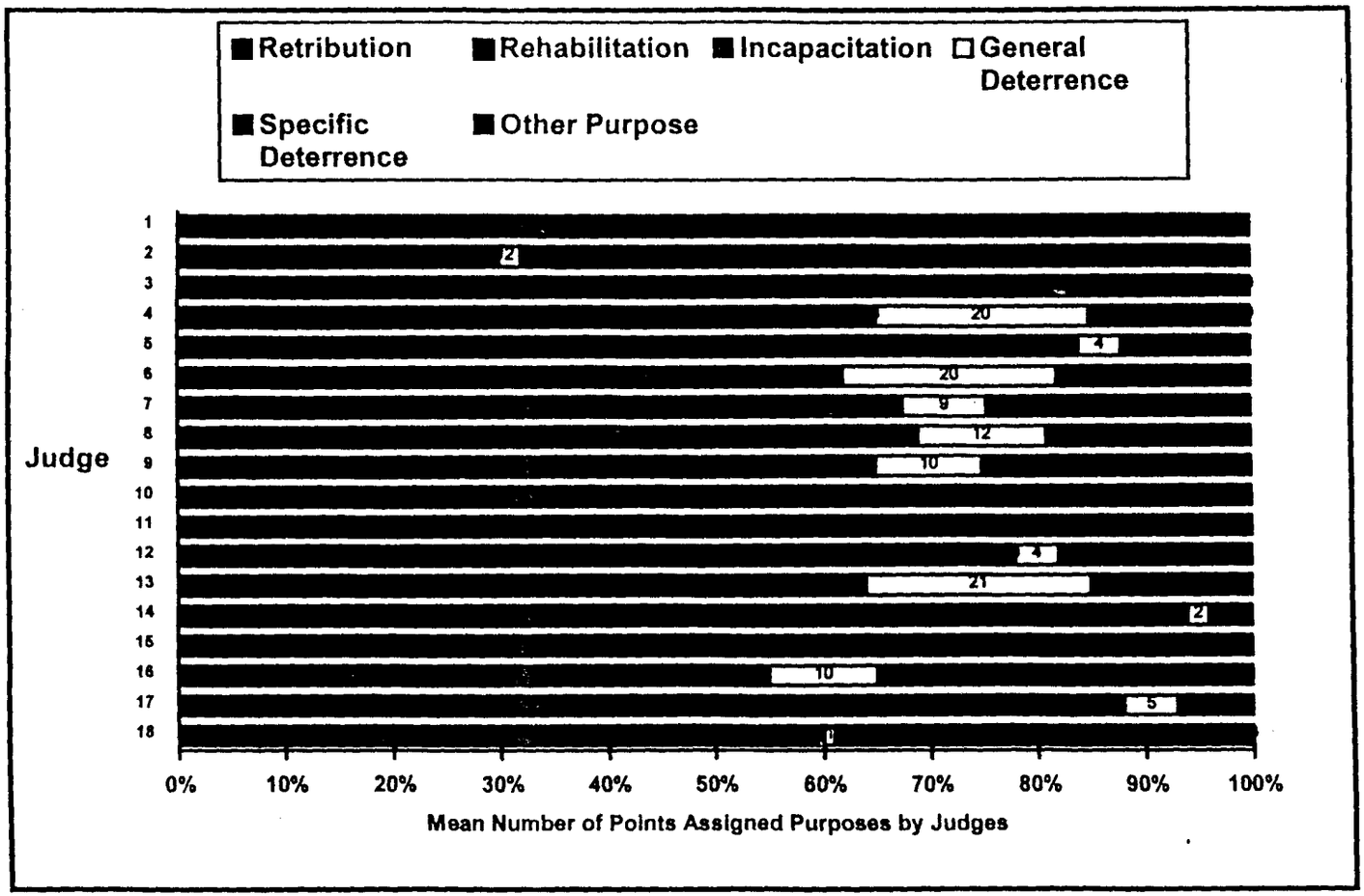

Figure 8: Average Number of Points Assigned by Each Judge to Each Purpose

Note: Points assigned by judge 7 were prorated to total 100 (ratings did not always sum to 100). Numbers of cases are as shown in Table 1, except for missing data: judge 1, 2 cases and Judge 2, 4 cases.

Because some differences in kinds of offenders assigned to the various judges were found, the question arises whether there is an independent effect of judge on the scores assigned to purposes. That is, one may question whether the differences in purpose statements are due entirely to differences in the kinds of offenders sentenced or whether some portion is due to differences in attitudes among the judges. This question was explored by an analysis of covariance in purpose ratings, for each purpose, according to the judge classification while controlling for type of offense (person, property, serious drug, and nuisance), age, prior jail or prison, number of counts charged, and the seriousness of the charge. In all cases except for the purpose "general deterrence," the covariates listed had a significant effect. Nevertheless, the value of F for the judge effect was significant at the one percent level of confidence in the case of each analysis of the purpose scores. Thus, although the differences in the ratings of purpose shown in Figure 7 may be said to be due in part to differences in the offenders sentenced by different judges, it also may be concluded that some of the differences are due to differences among the judges.

Offenses After Sentences

In the approximately 20 years after sentencing, most of these offenders continued to be involved with the criminal justice system. About a fourth were never arrested again, but most were, and they were arrested an average of 5.3 times. A little over half -- 55 percent -- were arrested before 5 years after the date of the sentence. The annualized arrest rate after serving any confinement on the present sentence (adjusted for later time at risk in the community--that is, not incapacitated by confinement)--(μ) -- was .277 for all offenders. For those with at least one arrest during the follow up period, it was .364.(The most arrest prone offender was arrested 74 times, but about 90 percent of those arrested were arrested a dozen times or fewer.) The mean time to a new arrest was 3.5 years; the median was 2.2 years.

Charges Classified by Legal Offense Groups

In terms of the legal offense classifications, they were charged with 40 illegal homicides (murders or manslaughters), 455 robberies, 752 assaults, 928 burglaries, 18 rapes, 507 weapons offenses, 682 receiving stolen property offenses, and 16 kidnappings, to list some charges out of the total of 9,346 allegations of new crimes. The median number of charges was 5; the mean was 9.7.

Charges Classified by Behavioral Offense Groups

The offenders in this sample were arrested a total of 5,117 times. Most often by far they were charged with property crimes or nuisance offenses: but they were charged also with a

substantial number of serious personal confrontation crimes and drug offenses. The charges at the first arrest after sentencing, classified into the kinds of offenses as used in this study, are shown in Table 6. The number and percents are those for the most serious charges among any made after the first arrest in the follow up period. The total number of offenses charged during the follow up period, grouped into the general classes, are shown in Table 7.

Table 6

Charges at First Arrest After Sentencing,

by Behavioral Offense Group

Type of Crime	Number of Persons	Percent
None	288	29.9
Nuisance	269	28.0
Fraud	35	3.6
Property	248	25.8
Serious Drug	7	.7
Personal Confrontation	115	12.0
Total Charges	962	100.0

Table 7

Number of Charges in Follow up

by Behavioral Offense Group

Type of Crime	Number of Charges	Percent
Nuisance	3,584	38.3
Fraud	376	4.0
Property	3,642	39.0
Serious Drug	93	1.0
Personal Confrontation	1,175	12.6
Other	476	5.1
Total Charges	9,346	100.0

Although the most frequent outcome was that the offender was not arrested again, the most common first new arrest after sentencing was for a nuisance charge (Table 6). Charges of property crimes were a close second. Serious drug offenses were rare. Considering only the first post-sentence arrest, 87 percent of the sentenced offenders either were not again arrested or were arrested and charged with nuisance offenses or some form of stealing. The next arrest / charge for the offenders sentenced was a person crime involving interpersonal confrontation or physical harm in 12 percent of the cases. Table 8 shows the offenses of conviction for the original sentence and the charges at first arrest, if any. [33]

[33] Transition probabilities are not examined in this report, although they are important and will be examined in further study. Generally, little specialization has been found in other studies. See Gottfredson, S. D. and Gottfredson, D. M. (1994).

Table 8

Charges of Conviction and at First Arrest After Sentence

Charge, First Arrest After Sentence	Charge at Conviction, Present Sentence										Total	
	Nuisance		Fraud		Property		Serious Drug		Person			
	N	Ro w%	N	Ro w%	N	Ro w%	N	Ro w%	N	Ro w%	N	Col %
None	87	30.2	23	8.0	79	27.4	38	13.2	61	21.2	288	29.9
Nuisance	70	26.0	14	5.2	88	32.7	61	22.7	36	13.4	269	28.0
Fraud	9	25.7	7	20.0	11	31.4	4	11.4	4	11.4	35	3.6
Property	37	14.9	7	2.8	153	61.7	19	7.7	32	12.9	248	25.8
Serious Drug	2	28.6	0	.0	3	42.9	2	28.6	0	.0	7	.7
Person	22	19.1	6	5.2	41	35.7	18	15.7	28	24.3	115	12.0
Total	227	23.6	57	5.9	375	39.0	142	14.8	161	16.7	962	100.0

Chi square = 114.55, 20 d.f., P <= .001; Cramer's statistic = .17; Contingency coefficient = .33

"Secondary" Questions: Judges' Predictions, Measuring Risk, and Judges' Sentencing Selections

The "secondary questions" must be addressed before turning to the questions of the effects of sentences because the results of those preliminary analyses are needed in order to perform the tests of sentence effects.

The Validity of Judges' Predictions

The judges made predictions of the likelihood of any crime, of a property crime, and of a person crime. The validity of each will be discussed for the judges as a whole and for the individual judges. Then an analysis aimed at determining the factors that may have influenced the judgments of risk will be reported.

In examining the validity of the three predictions, the correlation of the prediction scores with the most relevant criterion will be discussed, but in addition the correlations with other measures of offender outcomes will be noted. The main criterion examined is not crimes but any arrests; and this will be examined for the total follow up period and for the first five years after sentencing.

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*Behavioral Prediction and the Problem of Incapacitation," *Criminology* 32, 3 for discussion, including illustrations of measures of specialization and results with a long term follow up study of paroled offenders.

Predictions of Any Crimes

The judges' predictions of future crimes of any kind were valid, although quite modestly so (Figure 9). The validity coefficients shown in Table 9 (correlation coefficients [34], all statistically significant at the one percent level of confidence) show that for predictions of any arrests in the follow up period, the judges' estimates account for five percent of the variability observed. It is lower for the criterion "any arrest before 5 years," but the correlation of .182 may be misleading because of incapacitation as a result of the sentence. The partial (point biserial) correlation between the judges' predictions of any crime and any arrest before 5 years after the sentence, holding constant the time in jail or prison during that 5 years, was .256. This is the fairest test of the validity of the judges' predictions, with 6.6 percent of the variability in the outcome accounted for by the predictions.

Table 9

Validity of Judges' Predictions of Any Crimes (N = 960)

Outcome	Validity Coefficient
Any arrest	.224
Arrests (number)	.204
Charges (number)	.220
Nuisance offenses (number)	.154
Person offenses (number)	.104
Property offenses (number)	.214
Drug offenses (number)	.034
Fraud offenses (number)	.046
Other offenses (number)	.150
Any arrest before 5 years	.182

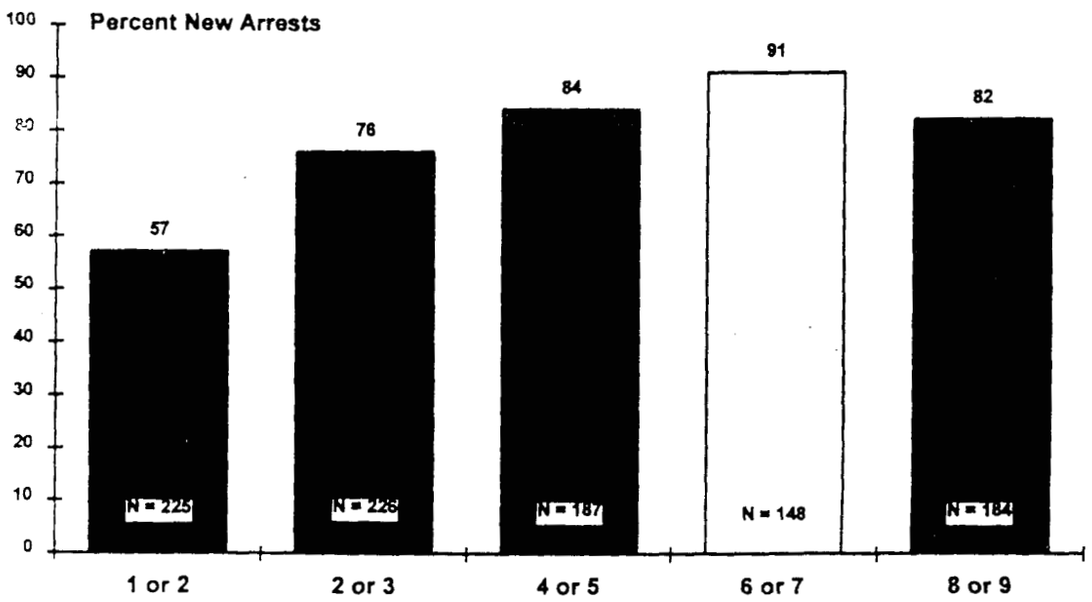

Figure 9

[34] For dichotomous outcomes (any arrest and any arrest before 5 years) the coefficients are point biserial correlations.

Predictions of Property Crimes

The validity of the judges' predictions of property crimes was investigated in the same way. The results are shown in Table 10. Correlation coefficients shown are significant at the five percent level of confidence. The point biserial correlation of scores with property offense charges is .24.

Table 10

Validity of Judges' Predictions of Property Crimes (N = 958)

Outcome	Validity Coefficient
Any arrest	.218
Arrests Number)	.196
Charges (number)	.218
Nuisance offenses Number	.125
Person offenses (Number)	.091
Property offenses (Number)	.236
Drug offenses (Number)	
Fraud offenses (Number)	.066
Other offenses (Number)	.132
Any arrest before 5 years	.172

Predictions of Person Crimes

The prediction of new crimes against persons is, as is well known, a more difficult problem. The judges' predictions were not without any validity, but, as shown in Table 11, it is modest indeed. As seen by the validity coefficient of .127 for the relation between the judges' predictions and the number of personal confrontation offenses for which these offenders were arrested during the follow up period, the prediction scores by the judges accounted for less than 2 percent of the variability in those arrests

Table 11

Validity of Judges' Predictions of Person Crimes (N = 958)

Outcome	Validity Coefficient
Any arrest	.143
Arrests Number)	.102
Charges (number)	.126
Nuisance offenses Number	.067
Person offenses (Number)	.127
Property offenses (Number)	.125
Drug offenses (Number)	
Fraud offenses (Number)	
Other offenses (Number)	.091
Any arrest before 5 years	.092

Predictions by Individual Judges

The validity of the predictions by individual judges is of interest for several reasons. One is that if any judge or judges are less able to predict well than are their colleagues, this would lower the validity assessments based on the whole group. Another is that if any judge can do better than the more formal methods of prediction using empirically derived measures, then we may be able to learn how that is done and thereby improve our predictive ability. The validities of the predictions of any crime, property crimes, and person crimes by the individual judges was next investigated. The results are summarized in Table 12.

There was no support for any validity of the predictions of four of the judges. On the other hand, the validity of prediction for the other judges compares somewhat favorably with the validity of some empirically derived, formal prediction methods. Of course, there was no way of knowing in advance which judges could predict well.

Table 12

Validity of Predictions by Individual Judges

Judge	Number of Cases	Validity of Predictions		
		Prediction of Any Crime	Prediction of Property Crime	Prediction of Person Crime
1	57	.333	.250	.288
2	60	.211	.320	
3	53		.301	
4	12	.640		
5	47	.433		
6	56	.346	.282	.290
7	59	.366		.260
8	58		.347	
9	52	.341	.321	.373
10	57	.279	.290	
11	58			
12	60			
13	59	.313	.147	.343
14	60	.323		
15	56			.338
16	36			
17	59	.242		
18	60			

Note: Correlations shown are significant at the five percent level of confidence for a one tailed test; blank cells indicate non-significant correlations (five percent level of confidence).

Correlates of Judges' Predictions

Although we cannot tell from the data available what information the judges used in arriving at their predictions, we can examine the correlates of the ratings. A way of exploring the judgments that may have influenced the judgments of risk is to regress the ratings on the other variables known to have been considered by the judge (or available) at the time of sentencing. When this was done for the predictions of any crime, the results shown in Table 13 were obtained.

Variables included in the analysis but not helpful in explaining the judges' predictions, in the context of the variables listed as included in the equation, were: the number of counts charged, the number of counts of conviction, serious drug offense, fraud offense, whether there were any mitigating factors, nuisance offense, the prosecutor's recommendation for a custodial

sentence, the seriousness of the offense classification, and the judges' ratings of the seriousness of the prior record.

Table 13

Regression of Predictions of Any Crime on Other Judicial Ratings, Type of Offense, and Age.

Variable	B	Standard Error of B	t*	β
Judges' ratings of the length of the arrest record	.29	.06	4.57	.18
Judges' ratings of the seriousness of the most serious offense charged	.31	.03	10.98	.26
Judges' ratings of the offender's social stability	-.72	.06	-10.93	-.26
Probation officer's recommendation for custodial sentence	.99	.13	7.54	.17
Property crime	.36	.14	2.60	.06
Person crime	-.66	.18	-3.74	-.08
Any aggravating factors	.52	.13	3.88	.09
Judges' ratings of length of the conviction record	.28	.07	4.13	.16
Age	-.02	.01	-3.73	-.08
Constant	3.28	.31	10.46	

* All significant at the one percent level of confidence.

R = .786, N = 931.

The analysis suggests that the judge, in making ratings of risk, may take into account particularly his or her judgments of offense seriousness, social stability, the lengths of the arrest and conviction record, and the judgment of the probation officer that a custodial sentence is called for. There is no way of knowing, of course, whether the probation officer and judge used similar information in arriving at their assessments or whether the judge was influenced by the recommendation. The Beta values at the right in the table are the standardized coefficients, which indicate the relative importance of the variables in the context of the others.

Measurement of Risk

Risk Measure 1

In order to develop the measure of *a priori* risk to be used in the analyses of sentencing effects, the criterion of any arrests during the follow up period was used as the dependent variable. This measure was regressed on predictor candidates from the pool of data known before sentencing. Because the measure with the largest association was desired only as a

control variable for the analyses, judges ratings, more objective items as coded from case files, and offenses all were explored. [35]

The resulting equation, called Risk Measure 1, is shown by Table 14. As seen by the beta coefficients at the right of the table, the best predictors of any new arrest (in the context of the other items listed) are age and the judges' ratings of the arrest record. These two variables in combination (by multiple regression) to predict any new arrests provided a multiple correlation of .42, accounting for about 18 percent of the variance. [36]

Table 14

Risk Measure 1: Regression of Any New Arrest on Selected Variables

Variable	B	Standard Error of B	t	β
Age at sentencing	-.01	.001	-9.78	-.30
Arrest record (judges' ratings)	.06	.01	7.90	.26
White	-.17	.04	-4.90	-.14
Any heroin or barbiturate use in the two years prior to arrest	.13	.04	3.44	.10
Number of prior probation sentences	-.03	.01	-3.90	.01
Alcohol use as problem drinker stated in record	.04	.01	2.80	.09
Seriousness of offense (judges' ratings)	-.01	.01	-2.35	-.07
Number of prior jail sentences	.02	.01	2.10	.02
Property Crime	.06	.03	2.43	.08
Current offense sale of drugs	.10	.04	2.20	.06
Constant	1.06	.06	19.13	

$R = .482$ $R^2 = .23$ t values are significant at the one percent level of confidence except for the judges' seriousness rating ($P < .02$); number of prior jail sentences ($P < .04$) and current offense sale of drugs ($P < .03$).

The measure accounts for nearly a quarter of the variability in the "any new arrest" criterion (23 percent).The correlation of .375 for the relation of the risk measure to new arrests before 5 years increased to .405 when controlling for incapacitation in jail or prison as a result of the sentence (i.e., the partial correlation coefficient was .405).

An example of the discrimination of offenders according to *a priori* risk is given by Figure 10. The percents with any new arrest are shown for five groups approximately equal in size (arbitrarily for convenience).

[35] Because it was thought that the error of excluding a predictive variable might reduce the degree of statistical control for risk in this sample, the five percent level of confidence was used; and because a large number of variables was examined, this increases the likelihood that a variable that may not be predictive in other samples will be included. Also, some items with substantial missing data were used, because it was found that the inclusion of these items, despite that flaw, improved prediction in this sample when the mean was substituted for the missing data.

[36] Had the judges used only their arrest ratings and age, combined appropriately, their predictions would have been substantially more accurate. The correlation of age with the criterion was -.30, that is, better than the judges' subjective predictions.

The scores actually are continuously distributed, and the actual (standardized) scores were used in the analyses that follow.

Figure 10 may be compared with Figure 9 for a rough comparison of the efficiency of prediction by this more formal method with that by the judges ratings.

The correlations of this risk measure with selected offender outcomes are shown in Table 15. The scores have substantial correlations with the total number of arrests and the number of charges.

Table 15

Correlations of Risk Measure 1 with Selected Offender Outcomes

Outcome	Correlation Coefficient
Any arrest	.482
Arrests Number)	.364
Charges (number)	.381
Nuisance offenses Number	.304
Person offenses (Number)	.174
Property offenses (Number)	.337
Drug offenses (Number)	.061
Fraud offenses (Number)	.122
Other offenses (Number)	.221
Any arrest before 5 years	.375

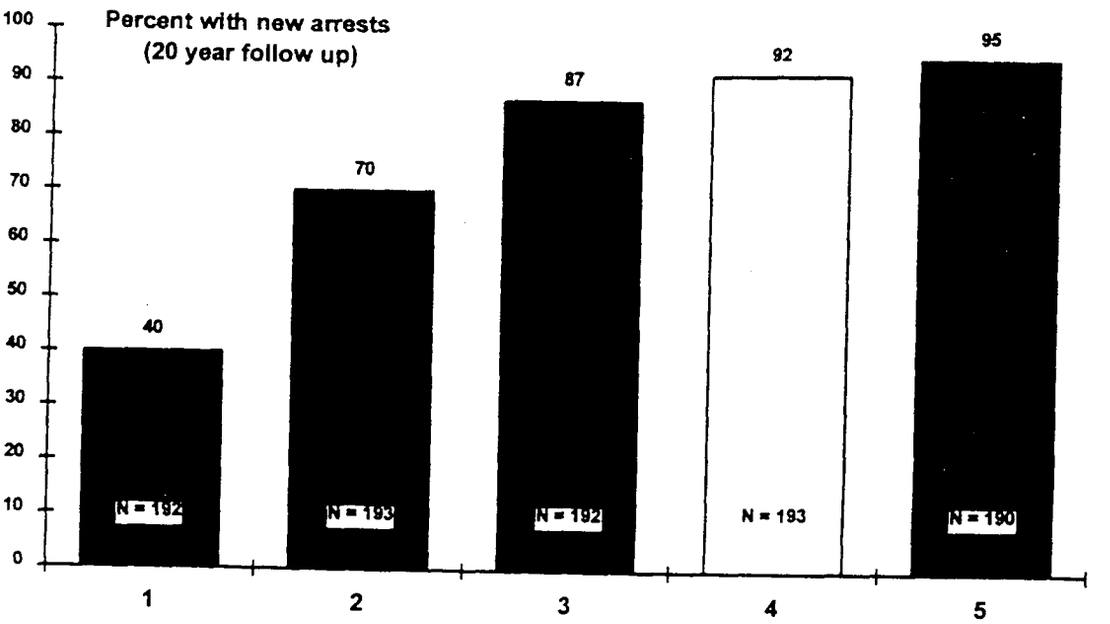

Figure 10

A priori Risk Groups and Percent with New Arrests in Follow up Period (Risk Measure 1)

Risk Measure 2

A different measure of risk was used for the analyses of probation effects, because of the availability of additional data on the characteristics of the offenders, coded from case files. The resulting equation and an illustration of the discrimination of offenders with and without new arrests by this measure is discussed in a later section of this report.

Selection of Sentences by Judges

The results of analyses in later sections of this report will describe the sentencing by judges in terms of sentences:

- to confinement or not (any confinement) (executed sentence)
- to a non-custodial disposition or confinement in jail, or the youth institution, or prison (executed sentence)
- if to confinement, whether to jail, the youth institution or prison (executed sentence)
- if to confinement, the maximum length of sentence (imposed sentence)
- if to probation, whether the sentence was "split," with some portion to be spent in jail.

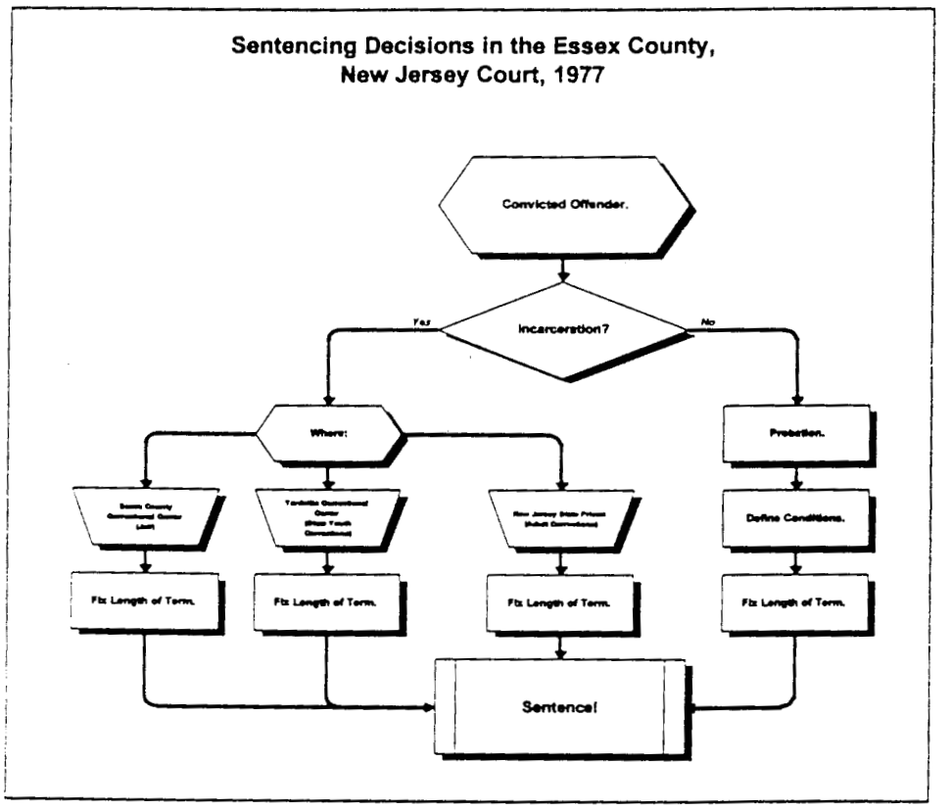

Figure 11

Simplified Flow Chart of Sentencing Decisions, Essex County Court, 1977

The sentencing decisions in the court at the time of the original data collection was considered to proceed somewhat like that shown in Figure 11. Of course, there is no way of knowing that the decision process proceeds in the sequence shown, but the chart does show the main alternatives available to the judges. For each of the major choice points, measures of selection of the decision are wanted in order to use these measures as controls in the analyses of sentencing effects.

The General Model for Analyses

The general model for many of the analyses to be presented is that used for the analyses of the effects of any confinement on any arrests after sentencing. The results of this analysis will be presented in some detail in order to explicate the method; additional analyses following the same model will be described more briefly. The general nature of these analyses may be summarized as follows:

- **Dependent Variable:** Any New Arrest (for example)
- **Classification Variable:** Any Confinement (jail, youth institution, or prison) vs Non-custodial sentence (for example)
- **Variables Controlled (Covariates)**
  - Selection for confinement (linear combination of independent variables explaining selection) (for example);
  - *A Priori* Risk (a linear combination of independent variables explaining the probability of new arrests on the basis of information known at the time of sentencing);
  - Time served as a result of the present sentence;
  - Time in the community (for example, time in follow up or in first 5 years minus time incapacitated by confinement in jail or prison).

The definition of the measure of selection by the judges will be described first. Then, with the selection measure controlled in the analysis, as well as the measures of *a priori* risk, time served, and time free in the community, the results of study of the effect of the decision (e.g., "in - out") can be presented.

If no effect is found, this must mean either that the decision to confine makes no difference in respect to that specific, limited criterion or that there are some unknown (unspecified) interactions between different classifications of offenders and different classifications of confinement that lead to the overall result of no difference in general. If an effect

is found, this must mean that the decision does make a difference, or there are additional (unknown) selection factors, additional (unknown) *a priori* risk factors, or unmeasured incapacitation effects (despite a rather complete accounting), or chance (unlikely).

Selection of Sentences to Confinement

Table 16

Standardized Discriminant Function Coefficients, Confinement or Not

Independent Variable	Coefficient
Age at sentencing (age in years)	-.139
Nuisance offense (offense class of conviction)	-.199
Seriousness of the offense (judges' ratings)	.322
Predictions of any crime (Judges' predictions)	.351
Social stability (judges' ratings)	-.164
Any mitigating factors listed by judge	-.182
Any aggravating factors listed by judge	.222
Prosecutor's recommendation for confinement	.154
Probation recommendation for confinement	.330
Importance of rehabilitation (judges' ratings)	-.445

The first analysis concerns only whether the offender was confined in any of the possible institutions – jail, youth institution, or prison – or received a non-custodial sentence. The procedures generally followed in order to define the selection effect were the same as those used here. Using the ratings made by the judges at sentencing (and age, which was known or could be roughly observed by the judge) and the offense classification, Fisher's discriminant function was calculated with the results shown in Table 16. The best discriminators of the selection of confinement were the judges' assessments of the importance of rehabilitation as a sentencing purpose, a recommendation against probation by the probation officer, and the judges' ratings of predictions of new crimes and of the seriousness of the offense. (Positive values reflect more likely confinement, negative values, less likely.)

Persons incarcerated had scores for rehabilitation as a purpose of the sentence that averaged 30, while those given non-custodial sentences had scores of 60 on the average. For those confined, the probation officer had recommended that disposition in 64 percent of cases; but for those not confined the recommendation for confinement was made 15 percent of the time. Judges predictions of any new crime were about 6 (on a scale of 10) for those incarcerated but were about 2 for those not confined. Similarly, the judges' average ratings of offense seriousness were 6.4 for the offenders sent to jail or prison but 3.8 for those not incarcerated.
The judge listed aggravating factors for just half the persons confined but 15 percent of the time for those not confined. Mitigating factors were found for 53 percent of the non-confined group and

32 percent of the confined offenders. Persons confined were on average nearly three years younger than those given non-custodial sentences.

The discriminant function correctly classified 88 percent of the sample, as shown in Table 17. The cases shown in the shaded part of the table are those incorrectly classified by the discriminant function. On the basis of the selection equation, 45 persons would be expected to be incarcerated but were not. Similarly, 69 persons would be expected to receive non-custodial sentences but nevertheless were confined, either in jail or prison. These errors in classification are of interest, because they are on the one hand a group of offenders similar on the basis of selection criteria to those confined but who were not, and on the other hand a group who were similar to those not confined but who nevertheless were incarcerated.

Table 17

Actual and "Predicted" Selections

to Incarceration or to Non-custodial Sentences

Actual Sentence	Predicted Sentence to Incarceration	Predicted Sentence to non-incarceration (non-custodial)	Number of cases
Not incarcerated	45	362	407
Incarcerated	486	69	555
Number of cases	531	431	962

Effects of Confinement on Any New Arrest

The results show that whether the offender is confined (not taking account of the place of confinement) makes no difference to the outcome of any new arrests during the follow up period after account has been taken of selection by the judge, the *a priori* risk, incapacitation by confinement in jail or prison as a result of the sentence, and exposure to the risk of new arrests in the community when not incapacitated as a result of new crimes. The analysis is summarized in Table 18. [37]

As shown in the table, the effects of selection, risk, and incapacitation are statistically significant, but there is no significant effect for whether the offender was confined. This result does not support the concept that there is any treatment effect on the new arrest outcome, either positive (as from specific deterrence or rehabilitation programs) or negative (as from criminogenisis).

[37] In these analyses, covariates were entered sequentially, before calculation of the values for the main effects.

Table 18

Analysis of Covariance in Any New Arrests, Summary Table,

for Whether or Not Confined

Source of Variation	Sum of Squares	DF	Mean Square	F	Probability of F < or =
Covariates	41.540	4	10.385	80.387	.001
Selection	6.594	1	6.594	51.040	.001
A Priori Risk	31.774	1	31.774	245.951	.001
In Community	1.807	1	1.807	13.989	.001
Time Served	1.365	1	1.365	10.568	.001
Main Effect	.021	1	.021	.165	.684
Confined or Not	.021	1	.021	.165	.684
Explained	41.561	5	8.312	64.343	.001
Residual	115.752	896	.129		
Total	157.314	901	.175		

N = 902 (60 cases had some missing data)

Comparison with a "Naive View"

The results of this analysis may be compared with a "naive view" of the new arrest outcome without the statistical controls. Table 19 shows the numbers confined and not confined and the numbers of persons with any new arrests during the follow up period.

The persons not confined had new arrests in 70 percent of the cases, but the persons confined had new arrests 82 percent of the time—a statistically significant difference. A naive conclusion, not taking account of risk, selection, or incapacitation, would be that non-custodial sentences are an effective treatment, decreasing the incidence of new arrests by the offenders concerned. Those sent to jail or prison were more often re-arrested. When the analysis reported in Table 18 has been done, however it may be seen that the percents with new arrests for the two groups, adjusted for the factors controlled, are 77 percent for the group not confined and 76 percent for the confined offenders--a non-significant difference. Figure 12 shows this correction.

Table 19

"Naive View" of Effects of Incarceration on Any New Arrests

Sentence:	Sentenced to Incarceration in jails or prisons				Total
Outcome Any New Arrest?	Not Confined Number	Percent New Arrests	Confined Number	Percent New Arrests	
No	124	30.5	100	18	224
Yes	283	69.5	455	82	738
Total	407		555		962

Chi Square = 20.3, 1 d.f., P<= .001; Phi = .145

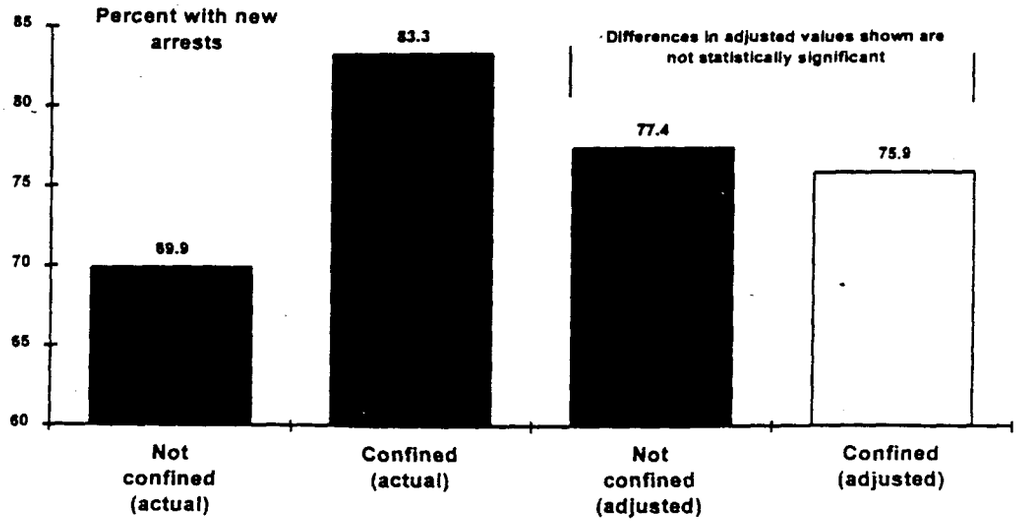

Figure 12

Actual and Adjusted Percent with Any New Arrests, by Sentences to Confinement or Non-custodial Sanctions

New Arrests, First Five Years

Because it may seem demanding to expect that sentencing effects may last over the twenty years that these offenders were followed after sentencing, a similar analysis was done using the criterion "any arrests during the first five years after sentencing." This an arbitrary period of time, but most offenders, even if confined, could be expected to have a substantial exposure to risk of new arrests in the community by that time. The analysis was the same except that only arrests before five years were counted, and only incapacitation during that period. The results are summarized in Table 20.

Table 20

Analysis of Covariance in Any New Arrests before 5 Years after Sentence, Summary Table, for Whether or Not Confined

Source of Variation	Sum of Squares	DF	Mean Square	F	Probability of F < or =
Covariates	43.298	4	10.825	54.053	.001
Selection	2.294	1	2.294	11.455	.001
A Priori Risk	31.162	1	31.162	155.608	.001
In Community	8.708	1	8.708	43.468	.001
Time Served	1.134	1	1.134	5.664	.018
Main Effect	.000	1	.000	.001	.981
Confined or Not	.000	1	.000	.001	.981
Explained	43.298	5	8.660	43.243	.001
Residual	179.430	896	.200		
Total	222.728	901	.247		

The results are the same when only the first 5 years after sentencing are considered. There is no effect of confinement on the criterion of any new arrests. A comparison of the naive interpretation and that made from this analysis is shown in Figure 13.

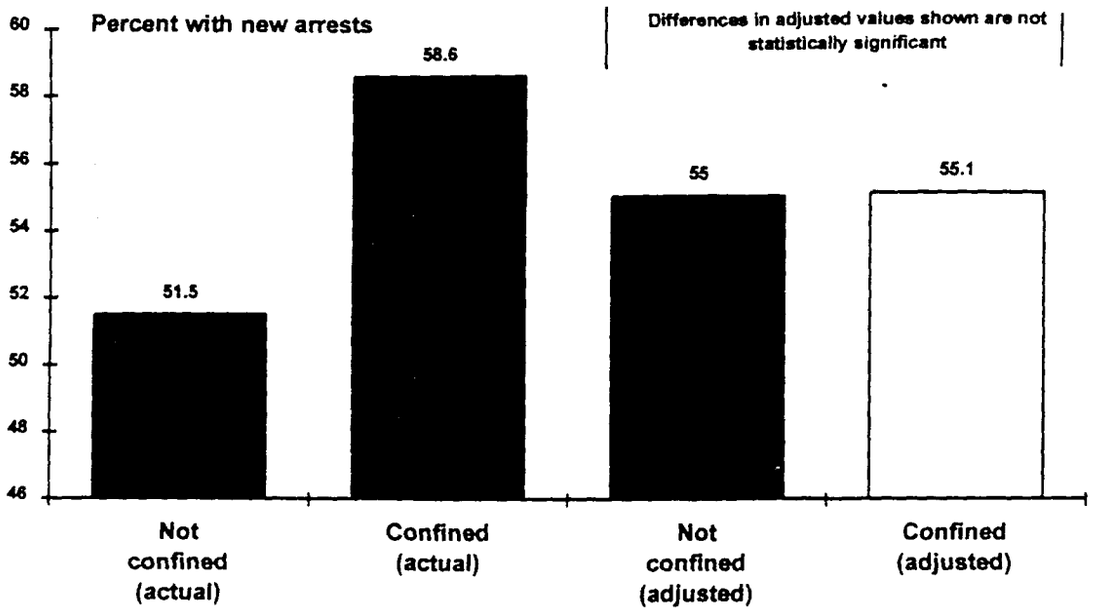

Figure 13

Actual and Adjusted Percent with Any New Arrests in the Five Years After Sentencing, by Sentences to Confinement or Non-custodial Sanctions

Alternative Method: Quasi-experimental Design

A different way of examining the effect of confinement may be helpful in explicating the logic of the statistical designs used. It is to compare the new arrest outcomes of the persons incorrectly classified by the selection equation. Recall that 45 persons would be expected to be confined (according to the formula) but were not, while 69 would be expected to receive non-custodial sentences but were actually incarcerated. Examining only the percents with new arrests after sentencing, however, would neglect any required control for *a priori* risk, any remaining selection effect, and the effects of incapacitation. When these controls are added by the analysis of covariance, the results summarized in Table 21 were obtained.

The table shows that there was no significant remaining effect of selection but that effects of *a priori* risk and incapacitation due to new crimes still were present. The main effect (whether or not confined) was not significant. The conclusion is the same as before: there was no effect of confinement on any new arrests.

A similar result was found when only the first 5 years after sentencing was considered, taking account of incapacitation due to the present or later offenses. The probability of the observed value of F under the null hypothesis was .70.

Table 21

Analysis of Covariance in Any Arrest, Summary Table, for Whether or Not Confined (Quasi-experimental Groups)

Source of Variation	Sum of Squares	DF	Mean Square	F	Probability of F < or =
Covariates	3.021	4	.755	6.037	.001
Selection	.343	1	.343	2.745	.101
A Priori Risk	1.804	1	1.804	14.421	.001
In Community	.698	1	.698	5.583	.020
Time Served	.175	1	.175	1.399	.240
Main Effect	.063	1	.063	.500	.481
Confined or Not	.063	1	.063	.500	.481
Explained	3.084	5	.617	4.930	.001
Residual	12.511	100	.125		
Total	15.594	105	.149		

N = 106 (8 cases had some missing data)

The effects of incarceration *vs.* non-custodial sentences were analyzed similarly for a number of other dependent variables. In each case, the covariates for selection, *a priori* risk, and total time confined in jail or prison were entered into the analysis first in order to control for their effects. In no case was a significant effect of confinement vs. non-confinement found after adjustment for the covariates. The results of these analyses, all based on 937 cases, are summarized in Table 22. In none of the analyses was the value of F for the sentencing effect statistically significant. Confinement had no effect on the total number of arrests in the follow up, or on the number of charges in specific crime categories.

These are quite gross comparisons, contrasting as they do only the fact of confinement or not, ignoring the type of confinement and non-custodial sanctions. Using similar procedures, the next analysis examined the four main choices available – that is, the choice among a non-custodial sentence, a sentence to jail, to the youth institution, and to prison. Then, the analysis was restricted to a comparison of the sentences to confinement, comparing the outcomes for offenders sent to jail, the youth facility, or prison. Each of these analyses followed the same logic of comparing the outcome criterion for the groups, controlling by analysis of covariance for selection, risk, time served on the present sentence, and time free in the community.

Table 22

Probability of Effect of Confinement vs. Non-confinement on Selected Outcomes

Outcome	Probability of Value of F
Arrest rate	.315
Arrests in follow up (total)	.134
Burglary charges (total)	.534
Charges, any (total)	.936
Frauds (total)	.297
Murder charges (total)	.130
Nuisance offenses (total)	.886
Person offenses (total)	.505
Property offenses (total)	.632
Rape charges (total)	.370
Receiving stolen property charges	.760
Robbery charges total)	.150
Serious drug offenses (total)	.796
Weapons charges (total)	.273

Effects of Types of Sanctions

The "Naive View"

The numbers with each of these *sentencing* outcomes, and some *offender* outcomes are shown in Table 23. The selected offender outcomes are the percents with new arrests during the entire follow up period and during the first five years after sentencing, and the average number of new arrests during the whole follow up period.

Table 23

Sentences and Subsequent New Arrests

Sentence	N	Percent with New Arrests in Follow up	Percent New Arrests in First 5 Years	Mean Number of Arrests in Follow up
Non-custodial	405	69.4	51.4	4.32
Jail;	282	83.0	63.1	6.62
Youth Institution	93	91.4	71.0	6.48
Prison	182	75.8	41.8	4.93
Total	962	76.7	54.9	5.32

As before, Table 23 gives a "naive view" of the effects of the different sentencing choices. There are substantial differences in the outcomes. They are statistically significant, but

that is not very informative because the comparisons are biased. In order to make fair comparisons, an (ethically prohibited) experiment with random assignment of sentences would be helpful, because it could be considered that the four groups of offenders were equivalent in respect to selection for the different sentences and the *a priori* risk of new offenses. It still would be necessary, however, to take account of the time the offenders were incapacitated – including confinement as a result of the sentence – in comparing the outcomes to determine the more effective placements in terms of crime control aims. Having no experiment, a statistical design is required in order to take account of these factors in the comparison

Judicial Selection of Types of Sanctions

As before, the first problem is to measure the selection by judges. A procedure similar to that already described was followed in order to define this measure by use of the discriminant function. In this case, there may be up to three discriminant function equations, because there are four groups. The same variables as before were used in the analysis – all were available to the judge at sentencing.

The results of the analysis are shown in Table 24. (It is based on 927 cases because 35 cases had at least one missing data element.) As reflected in Figure 14, the first function does most of the work of separating the groups, with scores on the average lowest for offenders sentenced to non-custodial sanctions, next higher for jailed offenders, next for those sent to the youth institution, and highest for those sent to prison. It mainly separates those with non-custodial sentences from those imprisoned.

Function 1 is based substantially on the judges' predictions of any future crime, perceptions of the importance of rehabilitation for the particular case, judgments about the seriousness of the charge and the length of the prior record, the number of counts of conviction, and the recommendation of the probation officer that the person be confined.

Function two helps mainly in separating offenders sent to jail or prison vs. those sent to the youth institution or given non-custodial sentences. The judges' judgments of the length of the conviction records, whether the offense was a property crime (and the judges' predictions of future property crimes), and age seem to influence the choice.

Function three, somewhat helpful in further distinguishing between offenders sentenced to jail or prison, is influenced by the number of counts of conviction, the length of the prior conviction record and its judged seriousness.

The three functions classified the offenders sentenced into the four groups as shown in Table 25. The functions together correctly classified 65 percent of offenders into the non-custodial, jail, reformatory, or prison groups.

Table 24

Discriminant Function Coefficients for Non-custodial, Jail, Youth Institution, and Prison Groups

Independent Variable	Function 1 Coefficient	Function 2 Coefficient	Function 3 Coefficient
Age at sentencing (age in years)	-.181	.299	.152
Seriousness of the charge dimension	.303	-.031	.231
Property offense (offense class of conviction)	.021	-.420	-.328
Counts of conviction	.308	.031	.705
Predictions of any crime (judges' predictions)	.494	.038	-.243
Predictions of property crime (judges' predictions)	.124	-.452	.383
Length of the conviction record (judges' ratings)	-.210	.509	-.548
Seriousness of the prior record (judges' ratings)	.290	.210	.495
Number of prior prison terms	.178	-.063	.257
Probation recommendation for confinement	.318	-.028	-.374
Importance of retribution (judges' -ratings)	.140	.201	-.258
Importance of rehabilitation (judges' ratings)	-.431	-.159	.049

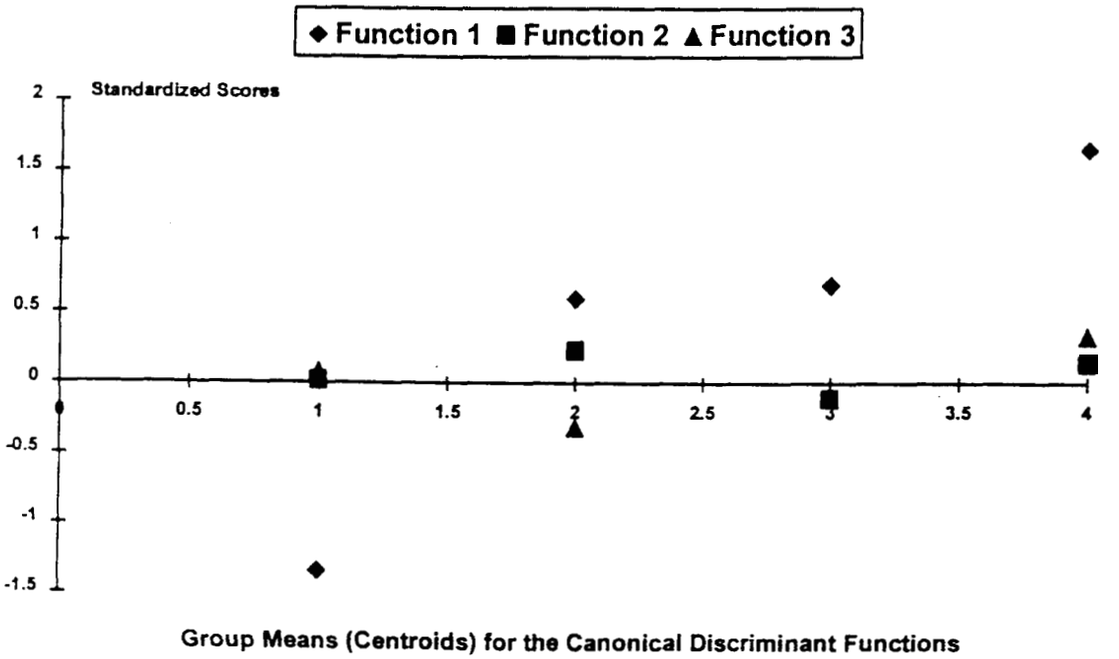

Figure 14
Group Means for the Discriminant Function Scores: Non-custodial, Jail, Youth Institution, and Prison Sentenced Groups

Table 25

Actual and "Predicted" Sentences to Non-custodial Sanctions, Jail, Youth Facility, or Prisons

Actual Sentence	Number of Persons	Predicted Sentence: Non-custodial		Jail		Youth Institution		Prison	
		N	%	N	%	N	%	N	%
Non-custodial	405	324	80.0	40	9.9	41	10.1	0	.0
Jail	282	45	16.0	132	46.8	56	19.9	49	17.4
Youth Institution	93	8	8.6	15	16.1	62	66.7	8	8.6
Prison	182	8	4.4	38	20.9	27	14.8	109	59.9

Effects of Sanction Types

Now we can examine the effects of sentences to these four types of sanctions while taking account of the measure of selection (the three discriminant functions), *a priori* risk (the regression equation for predicting new arrests), incapacitation due to the present sentence, and the time the offender was free in the community (the length of the follow up period minus time incapacitated in jails or prisons).

Any Arrest

The results of the analysis of covariance in the criterion "any arrests in follow up" are shown in Table 26. The selection functions, risk, time exposed to risk of new arrests in the community due to new offenses, and incapacitation by time served on the present sentence are significant; but the value of F for testing the effect of the type of sentence is not.. Despite the rather marked differences in percents with new arrests shown in the "naive view" in Table 23, the adjusted new arrest proportions among the four groups are not significantly different. The observed and adjusted values – the latter corrected by the results of the analysis of table 26, are shown in Figure 15.

The results of the analysis summarized in Table 26 show that an effect of the type of sentence this large or larger would be expected about 24 times in 100 repetitions of the study. The type of sentence has no effect on new arrests, when adjustments are made for the covariates. The adjusted values for new arrests are nearly the same for the non-custodial, jail, and prison groups. [38]

[38] Because the youth institution group has a higher value (bearing in mind that the differences are not statistically significant), and although age is taken into account in the *a priori* risk measure and the other covariates, the analysis was repeated with the addition of age as a covariate. This made no difference in the result.

Table 26

Analysis of Covariance in New Arrests, by Sentenced Group

(Non-custodial, Jail, Youth Facility, or Prison)

Source of Variation	Sum of Squares	DF	Mean Square	F	Probability of F < or =
Covariates	42.566	6	7.094	53.726	.001
Function 1	6.616	1	6.616	50.104	.001
Function 2	2.919	1	2.919	22.108	.001
Function 3	1.877	1	1.877	14.213	.001
A Priori Risk	28.018	1	28.018	212.178	.001
In Community	2.177	1	2.177	16.489	.001
Time Served	.959	1	.959	7.262	.007
Main Effect	.562	3	.187	1.419	.236
Type of Sentence	.562	3	.187	1.419	.236
Explained	43.128	9	4.792	36.290	.001
Residual	120.824	915	.132		
Total	163.952	924	.177		

N = 925 37 cases had some missing data

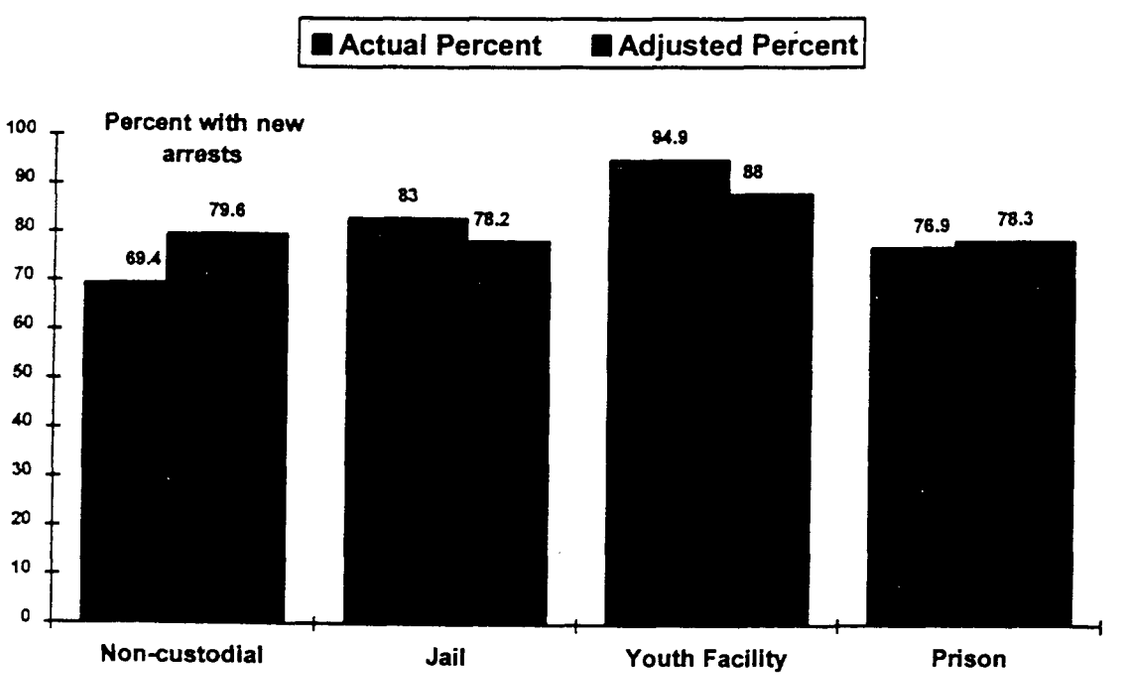

Figure 15

Actual and Adjusted Percents with New Arrests, According to Type of Sentence

Arrests Before 5 Years

The analysis was repeated, based only on the 802 offenders who served no time or up to five years in confinement on the sentence, except that the criterion used was whether there was any arrest before 5 years after the sentence date (controlling also for the number of months confined in jails or prisons during that 5 years). The offenders excluded from the analysis, of

course, were incapacitated and not at risk of new arrests in the community. The results were similar in that the selection measures, risk, incapacitation by time served are all significant. The time confined on the sentence (incapacitation) was in this case not statistically significant. The type of sentence, however, was significant at the five percent level of confidence. The summary table is Table 27, and the figure showing the actual and adjusted values is Figure 16.

Table 27

Analysis of Covariance in New Arrests Before 5 Years

by Sentenced Group (Non-custodial, Jail, Youth facility, or Prison)

Source of Variation	Sum of Squares	DF	Mean Square	F	Probability of F < or =
Covariates	36.073	5	7.215	36.540	.001
Function 1	8.309	1	8.309	42.082	.001
Function 2	3.591	1	3.591	18.188	.001
Function 3	1.300	1	1.300	6.586	.010
A Priori Risk	22.580	1	22.580	114.364	.001
Time Served	.293	1	.293	1.483	.224
Main Effect	1.748	3	.583	2.952	.032
Type of Sentence	1.748	3	.583	2.952	.032
Explained	37.821	8	4.728	23.945	.001
Residual	156.569	793	.197		
Total	194.390	801	.243		

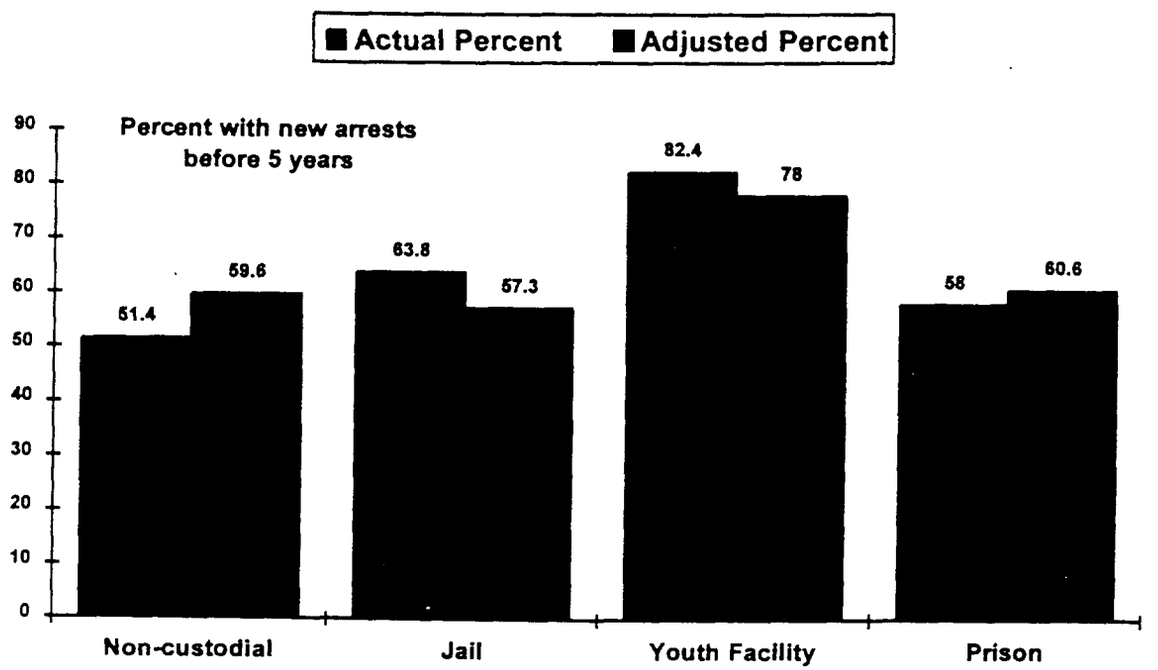

Figure 16

Actual and Adjusted Percents with New Arrests Before 5 Years, According to Type of Sentence

When only the first five years after sentencing is considered, excluding those offenders who were incapacitated for the whole time, confinement to the youth facility results in the highest

adjusted new arrest value. Non-custodial sanctions, jail, and prison have about the same effect. The adjusted values for the non-custodial sanctions and for prison are nearly the same.

Number of New Arrests

Results of a similar analysis of the effects of the type of sanction and the several variables to be controlled, using the offender outcome "total number of new arrests." are summarized in Table 28 and Figure 17.

Table 28

Analysis of Covariance in Number of New Arrests, by Sentenced Group

(Non-custodial, Jail, Youth Facility, or Prison)

Source of Variation	Sum of Squares	DF	Mean Square	F	Probability of F < or =
Covariates	12380.804	6	2063.467	49.907	.001
Function 1	1131.988	1	1131.988	27.378	.001
Function 2	1016.674	1	1016.674	24.589	.001
Function 3	610.601	1	610.601	14.768	.001
A priori Risk	4150.433	1	4150.433	100.382	.001
In Community	5401.207	1	5401.207	130.633	.001
Time Served	69.901	1	69.901	1.691	.194
Main Effect	159.860	3	53.287	1.289	.277
Type of Sentence	259.860	3	53.287	1.289	.277
Explained	12540.664	9	1393.407	33.701	.001
Residual	3781.859	915	41.346		
Total	50372.523	924	54.516		

Table 28 shows that the type of sentence does not affect the number of new arrests after taking into account the covariates considered in the analysis. The adjusted values, along with the actual percents, are shown in Figure 17, but the differences in adjusted percents are not statistically significant. As with the criterion "any new arrest," when the 20 year follow up period is considered, the type of sentence has no effect on the number of new arrests, when adjustments are made for the covariates.

Number of Charges for Kinds of Offending

When these analyses were repeated with more specific criteria, such as the numbers of person, property, rape, robbery, drug, or nuisance charges, the results were in each case similar. No effect of the type of sentence was found after controlling for the covariates.

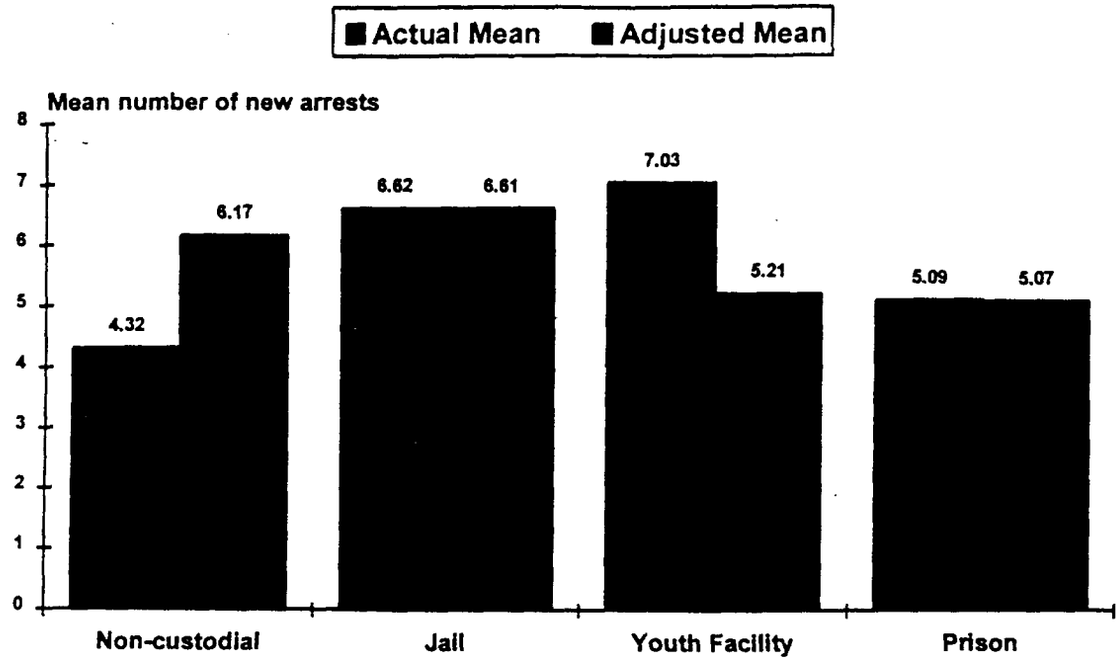

Figure 17

Actual and Adjusted Mean Number of New Arrests, According to Type of Sanction

Sentences to Types of Confinement

Judicial selection for confinement in jail, the youth institution or prison was analyzed also by the discriminant function. The same variables as included for study in the case of selection for confinement were included in the analysis. The resulting functions, which correctly classified 66 percent of the cases into the three groups, are shown in Table 29. [39]

Table 29
Standardized Discriminant Function
Coefficients, Jail, Youth Institution, or Prison

Independent Variable	Function 1 Coefficient	Function 2 Coefficient
Age at sentencing (Age in years)	-.101	-.387
Nuisance offense	-.336	.045
Seriousness of the offense (judges' ratings)	.341	.151
Property Crime	-.172	.537
Counts of Conviction	.312	-.151
Predictions of Person Crimes (judges' ratings)	.396	.175
Seriousness of prior offenses (judges' ratings)	.180	-.534
Any aggravating factors listed by judge	.265	.221
Importance of rehabilitation (judges' ratings)	-.166	.375
Importance of incapacitation (judges' ratings)	.350	-.053

[39] In all discriminant analyses for study of selection of penalties, the *priori* probabilities of group assignment were set as equal, giving a more conservative estimate of correct classifications than would be obtained by setting these at the observed values.

Effects of Type of Confinement on Any New Arrest

The analysis of the effects of placement in jail, the youth institution, or prison proceeded in the same way. The results of the analysis of the "any new arrests" criterion are given in the summary Table 30. The probability of an effect of confinement as large as that observed was found to be .05 -- that is, statistically significant at the five percent level of confidence The observed and adjusted values are shown in Figure 18.

Table 30
Analysis of Covariance in Any New Arrests, Summary Table,
for Placement in Jail, Youth Institution, or Prison

Source of Variation	Sum of Squares	DF	Mean Square	F	Probability of F < or =
Covariates	12.081	6	2.003	17.162	.001
Selection					
Confinement	.225	1	.225	1.927	.166
Function 1	.703	1	.703	6.022	.014
Function 2	.821	1	.821	7.036	.008
A Priori Risk	.8.324	1	8.324	71.328	.001
Time Served	.735	1	.735	6.297	.012
In Community	1.209	1	1.209	10.361	.001
Main Effect	.700	2	.350	3.001	.051
Confinement Type	.700	2	.350	3.001	.051
Explained	12.718	8	1.590	13.622	.001
Residual	58.003	497	.117		
Total	70.721	505	.140		

N = 506 51 cases had missing data

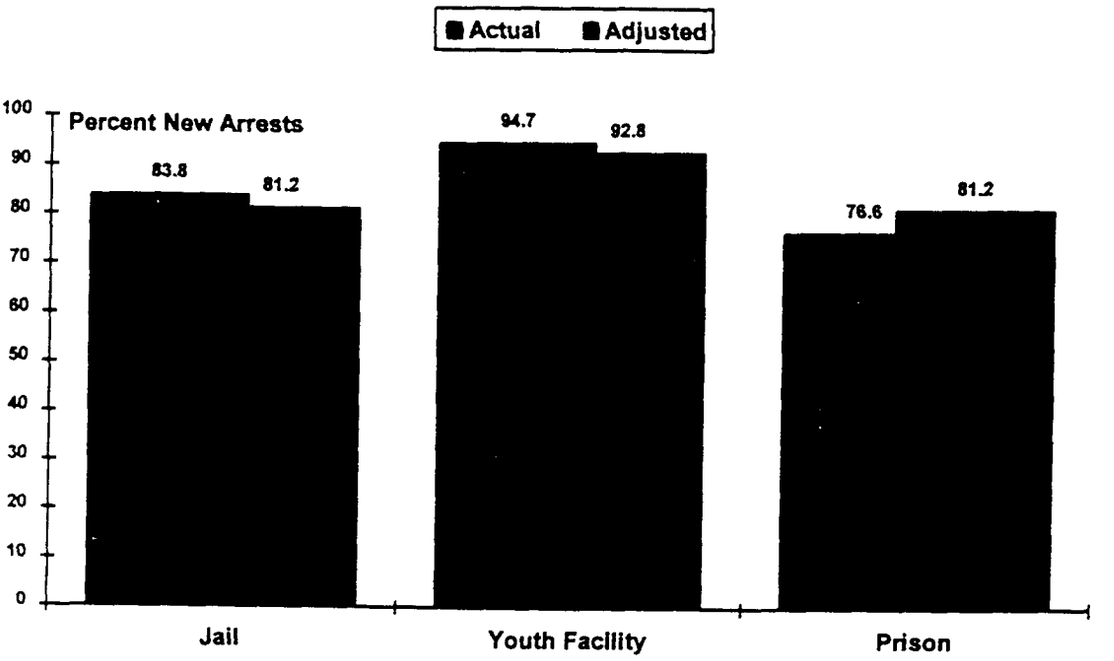

Figure 18

Percent with New Arrests in 20 Year Follow up, by Sentences to Jail, Youth Facility, or Prison

Results of the analysis of the new arrest criterion at five years were similar, except that the probability of the value of F for the main effect (type of confinement) was short of significance at the five percent level of confidence (P = .096). The analysis included only 384 persons sentenced to confinement who served less than five years in confinement as a result of the sentence that brought them into this study. The observed and adjusted values are shown in Figure 19.

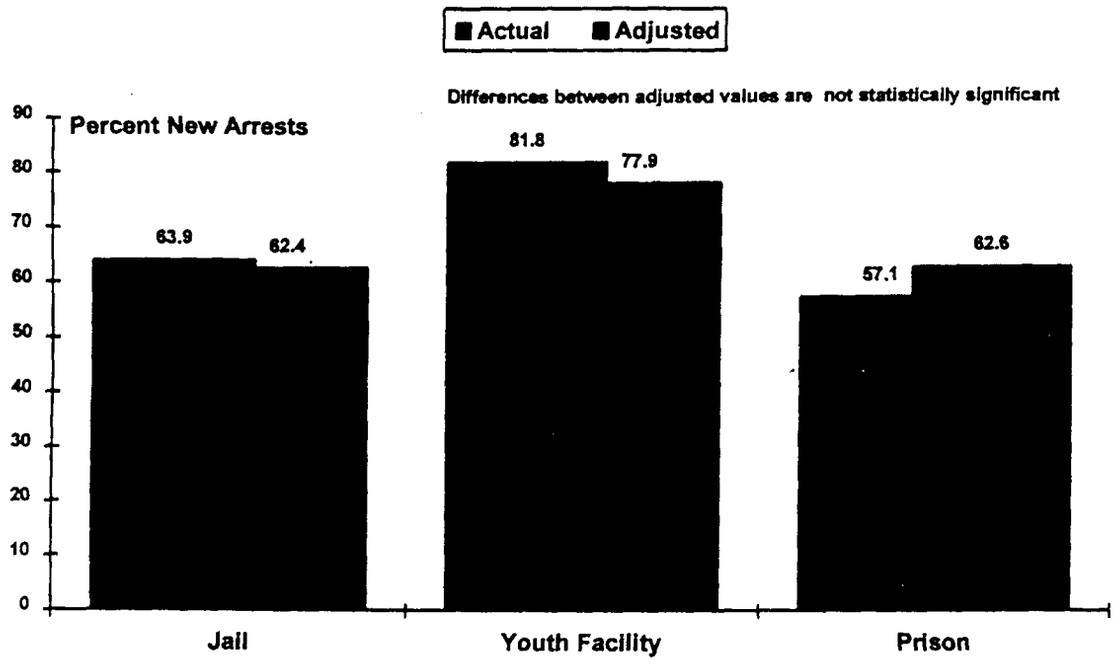

Figure 19.

Percent with New Arrests in 5 Year Follow up, by Sentences to Jail, Youth Facility, or Prison

The Effect of Maximum Sentences

Maximum Sentence Selection

After selecting the group of offenders sentenced to jail or prison, the maximum sentence was regressed on the items used in devising the risk measure. The six variables shown in Table 31 were most helpful in describing the selection of the maximum sentence.

The judge's view of the likelihood of a new person crime, the number of counts of conviction, a conviction of a person crime in the present instance, the rated seriousness of the offense, and the judge's perception of a need for incapacitation were significant predictors. Along with the judge's assessment of the seriousness of the conviction record, these items together accounted for 38 percent of the variability in maximum sentences.

Table 31
Regression of Maximum
Sentences on Selected Variables

Variable	B	Standard Error of B	t	β
Seriousness of offense (Judge's rating)	8.09	1.59	5.091	.21
Counts of conviction	14.32	2.19	6.550	.23
Person offense	43.75	7.15	6.115	.22
Incapacitative purpose (Judge's purpose)	.809	.136	5.962	.21
Prediction of person crimes (judge's rating)	6.84	1.157	5.911	.24
Conviction record	-5.98	1.662	-3.598	-.13
Constant	47.44	9.598	-4.942	

R = .617 Beta coefficients (standardized coefficients) are significant at the one percent level of confidence.

Maximum Sentence Effects

Offenders sentenced to jail or prison were classified into five groups, as shown in Table 32 along with the percents with new arrests observed during the follow up. The percents with new arrests vary quite markedly, with the shorter and longest maximum sentences having fewer new arrests, but the values shown do not take account of selection, risk, or incapacitation.

Table 32
Offenders Grouped According to Maximum
Sentences to Jail or prison

Maximum Sentence (Months)	Number of Persons	Percent	Percent with New Arrests
1 to 9	112	20.1	.81
10 to 18	169	30.3	.84
19 to 60	121	21.7	.88
61 to 120	106	19.0	.81
121 to 684	49	8.8	.65
Totals	557	100.0	

The effect of the maximum sentence length on new arrests may be better examined through procedures similar to those used before. The results of the analysis of covariance of new arrests, controlling for selection, risk, and time incapacitated are shown in Table 33. There is a significant effect of selection by the judge, the *a priori* risk of new arrests, and incapacitation; but there is no effect of the maximum sentence groups (beyond incapacitation).

Table 33

Analysis of Covariance in New Arrests, Maximum Sentence Groups

Source of Variation	Sum of Squares	DF	Mean Square	F	Probability of F < or =
Covariates	12.337	4	3.084	25.596	.001
Selection	.633	1	.633	5.251	.022
A Priori Risk	9.803	1	9.803	81.356	.001
Time Served	.509	1	.509	4.225	.040
In Community	1.392	1	1.392	11.553	.001
Main Effect	.313	4	.078	.650	.627
Maximum Sentence	.313	4	.078	.650	.627
Explained	12.650	8	1.581	13.123	.001
Residual	60.969	506	.120		
Total	73.619	514	143		

Some information about maximum sentences is lost when the offenders are grouped as in this analysis. Therefore, the partial correlations of maximum sentence, controlling for selection, risk, and incapacitation, were calculated. For 509 cases with jail or prison maximum sentences, the partial correlation coefficient was -.067 with the "any arrest" criterion (P<= .129).The correlation with the total number of arrests was -.142 (P < = .001). Its logarithmic transformation, however, (calculated because of the positively skewed distribution) was not significantly correlated with either criterion.

It was concluded that the maximum sentence imposed had no effect on new arrests after selection, risk, and incapacitation were taken into account.

The Effect of Time Served in Jail or Prison

In the jurisdiction studied, the actual time served in jail or prison was dependent in part on the sentence of the judge and in part, in the case of prison sentences, by decisions of a parole board. As a result, the selection of offenders for the sentence to confinement as actually executed must reflect in some part the decisions of both the judiciary and the paroling authority.

Among offenders who served some time in jail or prison as a result of the sentence, that time ranged from about six weeks to 252 months (21 years, or the total time of the follow up for the specific offender concerned). The mean time served was 44 months, the median, 21.6.

Selection Measure

The selection measure was defined by regressing time served on the variables that were available to the judge. The results, shown in Table 34, show that about 30 percent of the variability in time served is explained by the judge's rating of the seriousness of the offense, the judge's rating of the importance of incapacitation and of retribution as sentencing aims for the particular case, the number of counts charged, whether the offense or offenses was an offense

against persons including physical harm, and the judge's prediction of future person offenses if the offender were free in the community. All coefficients are positive: a higher score is predictive of a longer sentence.

Table 34

Regression of Time Served in Jail or Prison and Selected Variables

Variable	B	Standard Error of B	t	P
Judges' ratings of the seriousness of the most serious offense charged	5.19	1.09	4.751	.001
Judges' ratings of importance of incapacitation	.55	.09	6.324	.001
Counts charged (number)	3.68	.89	4.131	.001
Person Offense	16.51	4.75	3.476	.001
Judges' predictions of Person Offenses	2.37	.74	3.182	.002
Judges' ratings of the importance of retribution	.19	.06	3.077	.002
Constant	-25.38	6.75	-3.762	.001

R = .547 Beta coefficients (standardized coefficients) are significant at the one percent level of confidence.

Effect of Time Served on New Arrests

Time served was categorized into five groups as shown in Table 35, which shows also the observed outcomes for the criterion "any new arrests." The "naive conclusion" would be that the longest terms have an effect of fewer arrests, although those with sentences in the middle range result in the most.

Table 35

Confined Offenders Grouped According to Time Served in Jail or Prison

Months Served	Number of Persons	Percent	Percent with New Arrests
Up to 12 months	114	26.5	85.1
>12, up to 18 months	94	21.8	86.2
>18, up to 36 months	56	13.0	94.6
>36, up to 84 months	93	21.6	80.6
>84 months	74	17.2	74.3
All	431	100.0	

Chi square = 10.93, d.f. = 4, P = .03; Cramer's statistic = .15 9

The percents with new arrest shown in Table 35 of course do not take account of the potentially biasing factors of selection, risk, and incapacitation, and therefore again present a

biased view of the results of time served in terms of new arrests. In order to control for these concerns in the comparison, the analysis summarized in Table 36 was done.

Table 36

Analysis of Covariance of Any New Arrests, Time Served in Jail or Prison

Source of Variation	Sum of Squares	DF	Mean Square	F	Probability of F < or =
Covariates	9.023	7	1.289	11.251	.001
Selection					
Confinement	.173	1	.173	1.514	.219
Function 1, Type	.036	1	.036	.313	.576
Function 2, Type	.757	1	.757	6.603	.011
Function 3, Type	1.084	1	1.084	9.465	.002
Sentence length	.661	1	.661	5.768	.017
A Priori Risk	5.505	1	5.505	48.046	.001
In community	.807	1	.807	7.046	.008
Main Effect	.956	4	.239	2.085	.082
Time Served Group	.956	4	.239	2.085	.082
Explained	.979	11	.907	7.918	.001
Residual	47.090	411	.115		
Total	57.069	422	.135		

N = 423 (8 cases had some missing data)

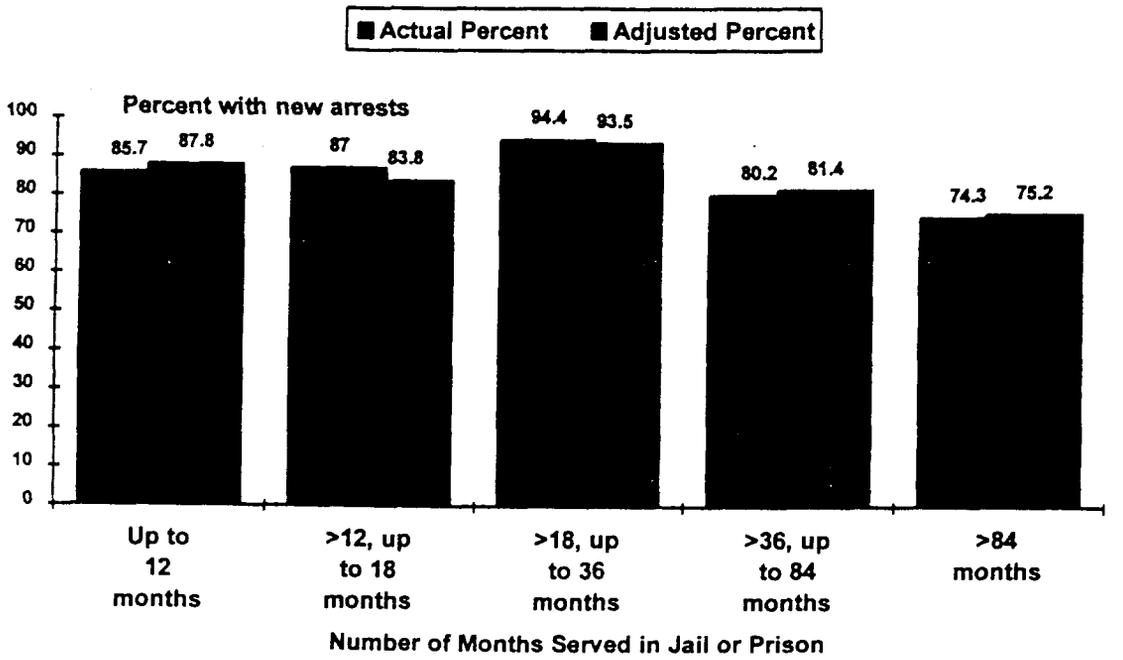

Figure 20

Actual and Adjusted Percents with New Arrests by Number of Months Served in Jail or Prison on the Present Sentence

The values observed (the "naive view") and those adjusted (the adjusted means, which are in this case percents) are shown in Figure 19, although the value of F for the effect of time served groups is not statistically significant (Table 36, P = .08).

Within the group confined, the correlation coefficient for months served in jail or prison and the measure "any arrests" and was -.107. (P = .015) for 520 cases. [40]The partial coefficient for months served and the outcome, holding constant the measures of selection, risk, and time free in the community, was -.131 (P=.003). Thus, there was a small but statistically significant effect of time served on new arrests. It was not found in the data of Table 35, but some information about time served was lost by the grouping of offenders into time served classifications for that analysis. The effect accounts for less than two percent of the variation in the new arrest outcome.[41]

Correlations calculated separately for the jail, youth institution, and prison groups are shown in Table 37. These suggest that the effect, if any, is confined to the prison group. The partial coefficient for the prison group alone, however, failed to reach significance at the five percent level of confidence (P = .08).

Table 37

Correlations of Months Served in Confinement with New Arrests, by Type of Institution

Group	N	Median	Mean	Pearson*			Partial**		
				N	r_{pbis}	P	N	r_p	P
Jail	282	12.00	11.63	282	.093	.118	267	.051	.403
Youth Institution	78	26.75	36.73	78	-.140	.221	69	-.115	.339
Prison	160	72.12	82.44	160	-.154	.051	150	-.144	.077
All Confined	520	18.00	37.18	520	-.107	.015	502	-.131	.003

* Point biserial correlation coefficients

** Controlling for selection (in-out = one function, type of sanction = three functions), *a priori* risk, and time in the community

Similar results with the analysis of covariance of the criterion "all arrests in follow up" were found. The analysis was the same as that done for Table 36, for the same subjects. The value of F for testing the significance of time served on the total number of arrests was, with 4 and 420 degrees of freedom, .62 (not significant, and would be expected by chance about 6 times in 10).The effects of selection, risk, and time in the community were significant. The correlation of month served and the number of new arrests in the follow up was not significant; neither was the partial correlation, controlling for selection, risk, and time in the community.

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[40] Because the time served distributions are skewed to the right, correlations with the logarithmic transformation of the variable were calculated, with no substantive differences to those reported.

[41] For these analyses, when jail time served was not confirmed by the follow up record checks (i.e., not shown in arrest records) and a jail sentence was imposed but not suspended in whole or part, then the portion of the jail sentence not suspended was assumed to have been served.

Non-custodial Sanctions and "Split" Sentences

Nearly half (48.4 percent) of the sample of offenders sentenced had terms of probation imposed, with or without some period of confinement as a condition (a "split sentence"). Of the 466 persons placed on probation, 152 (a third) had fines (or court costs) to pay. Restitution was required in only five percent of the cases (23 persons). Some jail time was imposed in 22.3 percent (104 persons).

Split Sentences

The "naive view" of the effect of split sentences is shown in Figure 21.

Those placed in jail along with probation had about five percent fewer new arrests, although the differences are not statistically significant (Chi square = .958, d.f. = 1, P = .328). Because no account is taken in this comparison of the potentially biasing factors, these data do not show whether there is any effect of split sentences. In order to control for them, attention was given first to the issue of selection of split sentences by the judges, next to that of the *a priori* risk of new arrests.

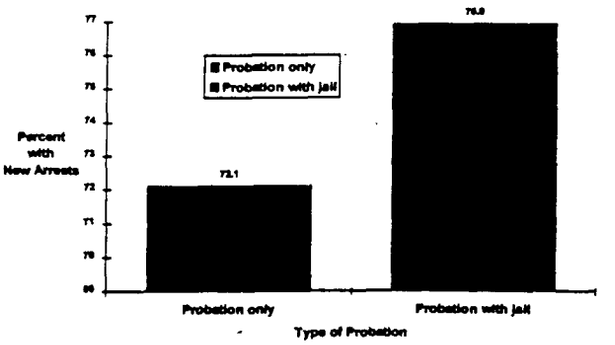


Figure 21
Split Sentences and New Arrests in Follow up

Selection for Split Sentences

The discriminant analysis for definition of the selection measure was based on the 466 persons included in the data for Figure 21, except that 20 were excluded because of at least one missing data element. The variables included were again those available to the judge at the time of sentencing. The results are summarized in Table 38.

The selections seem to be influenced by the judges' risk predictions for any crime, whether any aggravating factors were found, and the relative importance of retributive and rehabilitative aims for the particular case. The recommendation of the probation officer may have influenced the judgment, or the probation officer and the judge may have based their conclusions of similar aspects of the case.

This selection measure correctly classified 84 percent of the probation cases as split sentenced or not, as shown in Table 39. This measure will be used to control for selection in analysis of effects of split sentences.

Table 38
Standardized Discriminant Function
Coefficients, Split or Not

Independent Variable	Coefficient
Predictions of any crime(Judges' predictions)	.386
Any aggravating factors listed by judge	.364
Probation recommendation for confinement	.340
Importance of retribution (judges' ratings)	.272
Importance of rehabilitation(judges' ratings)	-.373

Table 39

Actual and "Predicted" Selections

for Split Sentences

Actual Sentence	Predicted Sentence to Probation without Jail	Predicted Sentence to Probation with Jail	Number of cases
Probation without Jail	308	52	360
Probation with jail	23	81	104
Total	331	133	464

A Priori Risk Measure 2: Probation Cases

The better the measure of risk (that is, the closer the association of risk scores with the offender outcomes), the more confidence can be held in the results of comparisons of effects of the different alternatives when risk is controlled. With this in mind, the larger data set of offender characteristics, generally available for this subset of offenders, was used for developing the risk measure because it was expected that the measure of risk could be improved.

The results are shown in Table 40, which summarizes the regression of the new arrests criterion on selected variables. Table 41 lists also the variables in the data set that did not improve prediction after inclusion of those listed in Table 40.

Many of the variables listed have been found in other studies to be related to a criterion similar to that used here; and many are indeed related to the incidence of new arrests. These variables, in the context of those included for the measure defined by Table 40, however, did not improve discrimination between offenders with and without new arrests. The point biserial correlations (for continuous independent variables) or phi coefficients (for attributes scored as dichotomous) with the criterion "any new arrest" are also shown.

Table 41 provides information about the relations of individual items to the new arrest criterion and includes other descriptive data. It shows, for example, that among the continuous

Table 40

Risk Measure 2: Regression of Any New Arrest on Selected Variables Available to Judges (Probation Sample Only)

Variable	B	Standard Error of B	t*	β
Social Stability (judges' ratings	-.108	.020	-.2.363	-.108
Age at Sentencing	-.013	.002	-6.939	-.286
Arrest Record Length (judges' ratings)	.059	.012	4.716	.219
White	-.196	.042	-4.638	-.186
Heroin / barbiturate use in the 2 years before arrest	.153	.046	3.333	.142
Number of prior probation sentences	-.025	.009	-2.734	-.111
Sex	-.172	.055	-3.10	-.125
Incarceration for probation or parole violation	-.0132	.006	-2.348	-.094
Current offense sale of drugs	.117	.048	2.409	.098
Current offense checks or burglary	.148	.068	2.167	.087
Constant	1.371	.101		

$R = .555$ $R^2 = .31$ t values are significant at the one percent level of confidence except for prior incarceration for probation or parole violation ($P < .02$; social stability($P < .02$) and current offense sale of drugs ($P < .02$).

variables age at first arrest and age at sentencing are by themselves good predictors of new arrests. Younger probationers more often have new arrests later. It shows also that these probationers were first arrested at age 22 but were 29 when sentenced. As another example, it shows that the judges' ratings of social stability are good predictors of new arrest and that this rating by the judges was a better predictor of new arrests than was their predictions of any new crimes. The judges' judgments about the length of the conviction record also was by itself a fairly good predictor. Note that the means shown for attribute data are proportions: for example, the judge listed aggravating factors in 22 percent of these cases; 9 percent were described in the case file as "problem drinkers," and 11 percent had used heroin or barbiturates as a juvenile. Among the attributes, the use of heroin or barbiturates during the 2 years just prior to the arrest for which the probationer was sentenced is a fair predictor, as is a notation in the case file that there has been problem use of opiates, derivatives, or synthetic substitutes for morphine. The items listed in Table 40, with the weights shown, are intended to provide the best linear combination of the items for the risk classification.

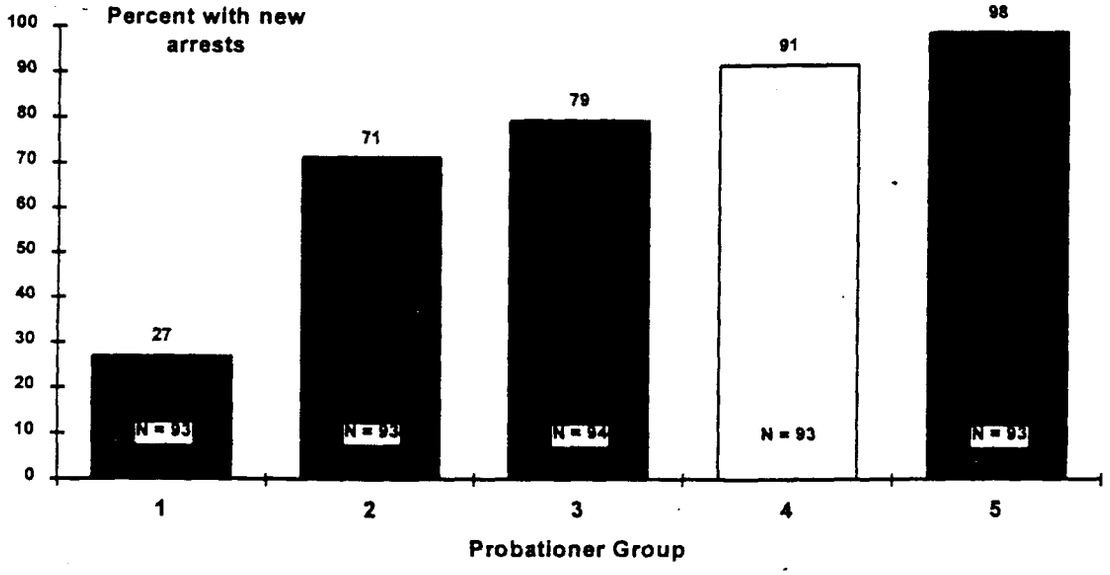
As an example of the discrimination of offenders later arrested and those not arrested, Figure 22 shows the percents with new arrests for five groups approximately equal in size (arbitrarily, for convenience). The relation of the scores to new arrest is stronger than that found

Table 41

Independent Variables Available for New Arrest Prediction

Independent Variable	Mean	S.D.	N	r	phi
Age at first arrest)	22.03	6.22	345	-.30	
Age at sentencing	29.17	9.49	459	-.30	
Aggravating factors listed by judge	.22	.41	466		ns
Alcohol use as problem drinker	.09	.39	388		ns
Alcohol use at offense	.10	.29	439		ns
Aliases	.35	.63	439	.08	
Any heroin or barbiturate use as juvenile	.11	.28	381		.16
Any heroin or barbiturate use in prior 2 years	.29	.41	382		.28
Arrest record length, judge's rating	1.63	1.65	465	.28	
Checks or burglary, current offense	.08	.26	439		ns
Codefendants	.56	.89	380	ns	
Conviction record length, judge's rating	1.18	1.45	465	.25	
Counts charged	1.90	1.30	466	ns	
Counts convicted	1.18	.75	466	ns	
Drug use at offense	.07	.26	386		ns
Employed at arrest	.46	.50	387		ns
Escape or absconding history	.04	.20	388		-.13
Family criminal history	.20	.39	438		.08
Fraud	.09	.28	466	ns	
History of drug offenses	.32	.42	370		.18
History of gambling offenses	.03	.16	370		ns
History of property offenses	.46	.44	370		.21
History of violent offenses	.34	.42	368		.08
Juvenile facility commitment(s)	.11	.28	378		.10
Longest arrest free period since first arrest	33.64	32.53	439		ns
Months in longest job since age 16	15.66	19.80	439	ns	
Person offense	.12	.33	466	ns	
Prediction of any crime, judge's rating	2.81	2.30	464	.23	
Prediction of person crime, judge's rating	1.25	1.71	464	.18	
Prediction of property crime, judge's rating	2.10	2.33	463	.21	
Prior arrests	2.38	2.64	439	.18	
Prior convictions	2.35	2.46	388	ns	
Prior incarceration for prob. or parole violation	1.67	3.15	439		.14
Prior jail sentences	.51	1.20	439	.16	
Prior prison incarcerations	.36	.85	355	.09	
Prior probation revocations	.86	2.13	387	-.10	
Prior probation sentences	1.41	1.95	388	-.08	
Prior probations	.80	1.18	466	.16	
Prior split sentences	.92	2.28	387	-.10	
Probation officer's recommendation for custodial sentence	.24	.43	466		.12
Problem use of opiates or derivatives	.21	.40	439		.23
Property offense	.29	.45	466		.16
Prosecutor's recommend for custodial sentence	.08	.27	466		.06
Sale of drugs, current offense	.18	.37	386		.09
Serious drug offense	.16	.37	466	ns	
Seriousness of charge dimension	3.64	1.73	466	ns	
Seriousness of offense, judge's rating	4.27	2.12	465	ns	
Seriousness of prior convictions, judge's rating	1.36	1.39	461	.20	
Sex (1 = male, 2 = female)	1.12	.32	458		-.11
Social stability, judge's rating	2.73	1.03	461	-.32	
Stolen property, current offense	.15	.33	385		.10
White	.23	.42	466		-.22

for the whole sample, which was based on more limited data; and both will be used for the analysis of split sentences.



Chi square = 147.82, 4 d.f., P <= .001, Cramer's statistic = .563

Figure 22

Probationer Groups Defined by Risk Measure 2 (Probationers Only)

The same set of independent variables was used to examine their utility for prediction of new arrests in the first five years after sentencing, of the total number of arrests in the follow up, and the total number of person offenses. The results are summarized in Table 42.

Table 42

Regression of Selected Follow up Criteria on Independent Variables

Dependent Variable	Predictor Variables	Standardized Regression Coefficient	Multiple Correlation Coefficient (β)
New Arrests Within 5 Years	Arrest record length (judge's rating)	.246	**.445**
	Age at sentencing	-.252	
	White	-.154	
	Heroin / barbiturate use in the 2 years before arrest	.134	
	Sex	-.122	
Number of Arrests	Number of prior jail sentences	.278	**.477**
	Age at sentencing	-.166	
	Social stability (judge' rating)	-.113	

(Table continued on next page)

	Number of counts charged	.106	
	White	-.093	
	Serious drug offense	-.103	
	Juvenile facility commitment(s)	.140	
	Heroin / barbiturate use in the 2 years before arrest	.122	
	Prior prison incarcerations	-.161	
	Prior jail incarcerations	.163	
	Prior probation revocations	.104	
Number of Person Offenses with Confrontation or Physical Harm	Age at sentencing	-.215	.289
	Heroin / barbiturate use in the 2 years before arrest	-.182	
	Arrest record length (judge's rating)	.147	
	Sex	-.098	

Effects of Split Sentences

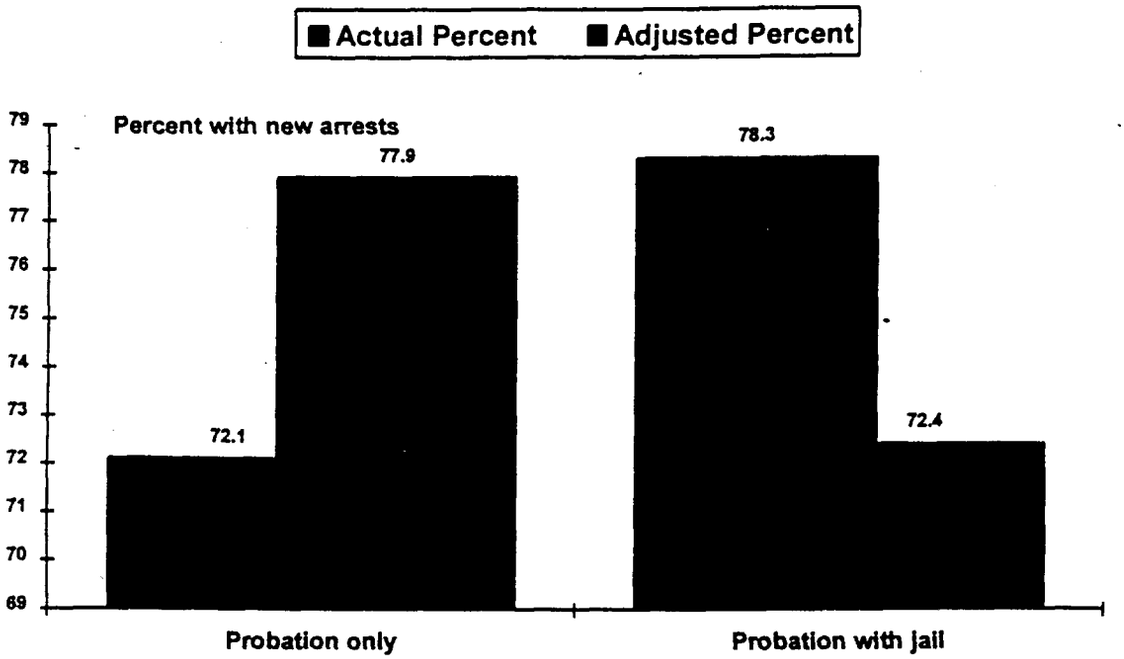
The analysis of covariance, similar to the analyses already described, was used to examine whether split sentences had any effect on any new arrests during the entire follow up period. The results are shown in Table 43.

Table 43

Analysis of Covariance of Any New Arrests, Split Sentence

Source of Variation	Sum of Squares	DF	Mean Square	F	Probability of F < or =
Covariates	26.470	5	5.294	39.604	.001
Selection (Split)	.767	1	.767	5.738	.017
A Priori Risk					
Risk Measure 1	22.201	1	22.201	166.083	.001
Risk Measure 2	2.212	1	2.212	16.548	.001
Time Served	.045	1	.045	.334	.564
In Community	1.245	1	1.245	9.315	.002
Main Effect	.114	1	.114	.856	.356
Split Sentence	.114	1	.114	.856	.356
Explained	26.584	6	4.431	33.146	.001
Residual	54.940	411	.134		
Total	81.524	417			

N = 418 (48 cases had missing data)



Actual and Adjusted Percents with New Arrests in Follow up, for Probationers With and Without Split Sentences

Figure 23

The Table shows the significant effects of selection, risk, and incapacitation (time in the community). The value of F for the time served is not significant. The main interest is in the value of F for split sentences (that is, for whether or not jail was imposed as well as probation). It is not statistically significant. Figure 23, which may be compared with the "naive" interpretation of Figure 21, shows the observed and adjusted values of the "any new arrest" variable. There is no effect of split sentences on the "any new arrest" criterion.

The same analysis was done using the criterion of any new arrest during the first five years after sentencing (adjusting time in the community to the five years as a maximum). The results were similar, with statistically significant effects for the measures of risk and time in jail. The value of F for the selection measure was not significant (P = .119) and neither was the split sentence effect (P = .809).

Fines and Restitution

Those persons who were placed on probation with fines were less often found in the follow up record search to have had new arrests later, compared with those on straight probation or probation with restitution. These differences are shown in Figure 24. The "naive interpretation" would be that fining results in fewer new arrests.

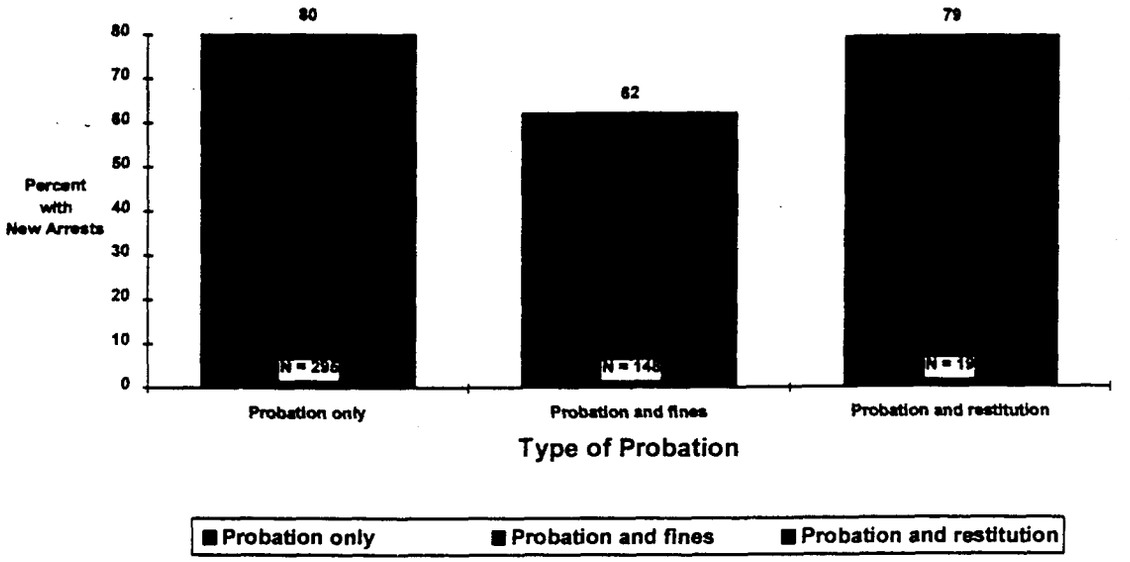


Figure 24

Probation With and Without Fines and Restitution, with Percent New Arrests

Note: 4 persons with probation, fines, and restitution none of whom had arrests in follow up) excluded. Also excluded are persons who had fines or restitution imposed without probation: 51 persons had fines only; of these, 43 percent had new arrests. Two persons had restitution only (both re-arrested).

When only the first five years after sentencing is considered (without taking account of selection, risk, or incapacitation) results were similar. Among probationers fined, 41 percent had new arrests before five years, although 61 percent not fined had new arrests.

It was similar with restitution: 35 percent of those required to make restitution had new arrests, but 56 percent of those without the requirement had new arrests.

Effect of Fines or Restitution

The effect of fines and restitution, however "disappeared" when risk and incapacitation effects were considered in the analysis. Table 44 shows the result of regressing the variable "any new arrests before five years" on risk, incapacitation, selection for the confinement decision (taken as a proxy for selection for these sentences), jail, fines, and restitution. The measures of risk, the time incapacitated and that in the community during the first five years, and the function for the "in-out" decision were entered into the analysis first, in order to control for their effects. Only *a priori* risk measure 2 and the time incapacitated during the first five years were included in the resulting equation. Dummy variables for any fine and for any restitution were then added. The result showed that whether the offender was fined or restitution was required had no statistically

significant effect on new arrests before five years, after risk and incapacitation were taken into account. The analysis is summarized in Table 42.

Table 44

Regression of Any New Arrest Before Five Years on Risk, Incapacitation, Fines and Restitution

Variable	B	Standard Error of B	t*	β	P
A Priori Risk: Risk Measure 2	.204	.022	9.225	.411	.001
Incapacitation	-.009	.004	-1.972	-.084	.049
Restitution	-.064	.045	-1.427	-.060	.154
Fines	-.072	.047	-1.536	-.068	.125
Constant	.584	.026	22.213		.001

R = .445 R^2 = .198 N = 453

Among all 204 persons required to pay a fine or make restitution, regardless of other aspects of the sentence (that is, probation, jail, or prison), 60 percent were known to have complied. Thirteen percent were incarcerated before completing payment, five percent absconded, payment was suspended for three percent. Among those who complied, 58 percent had new arrests before five years; among those who did not, the percent with new arrests was 61 percent. The difference was not significant (Chi square = .184, d.f. = 1, P = .67).

Survival Without Arrest, Over Time

It is plausible that a differential effect of sentencing choices – such as non-custodial sanctions, jail, youth corrections, or prison – is in different rates of survival in the community without arrest. Again, the interest is in a measure that takes account of selection, risk, and incapacitation. It is plausible also that the effects of different sanctions on survival change over time differently for different groups – that is, that there is an interaction between the sanction selected and time affecting the outcome. This would be the case, for example, if confinement had a specific deterrent effect that lessened over time.

A cumulative survival function can be calculated by Cox regression that shows the proportion of cases surviving at any particular point in time, based on the predictive variables (usually called covariates for these analyses) in the model. Cases with no arrests (called censored cases) are included. A hazard function can be derived from the survival function, showing hazard rates for any different groups.

The survival and hazard curves wanted are those for new arrests from the time of release from confinement on the present sentence (when the sentence called for incarceration) or from the date of sentencing, if not confined. For this purpose, the time served on the present sentence was subtracted from the time to arrest, and the functions shown for confined and non-

confined offenders in Figures 25 and 26 are the result. Whether the offender was confined or not makes little or no difference to the survival without arrest (or the hazard of arrest) over time in the community when the function is calculated on the basis of *a priori* risk, total time incapacitated, and incapacitation by the present sentence. The survival function shows the proportion of cases not arrested at a given time after any confinement from the present

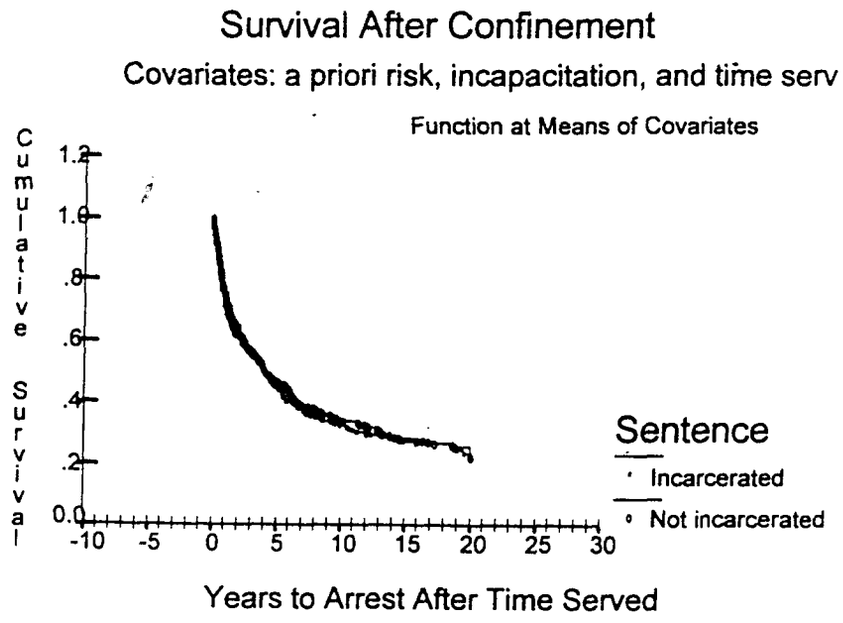


Figure 25
Survival Without Arrest After Serving Required Confinement, Confined and Not Confined,

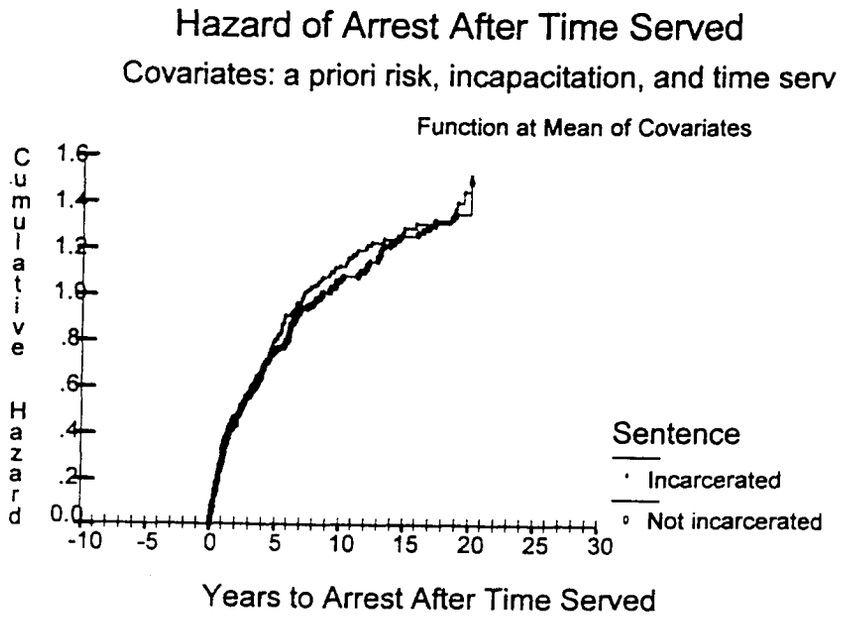


Figure 26
Hazard of Arrest After Serving Required Confinement, Confined and Not Confined,

sentence. The hazard function indicates the rates, at given times, of arrests, given no arrest until that time. That is, it is an arrest rate per unit of time.

The cumulative hazard rates show arrest rates for the sample at any point in time after release or sentencing to non-custodial sanctions. For example, Figure 26 shows that at 15 years in the community without incarceration, the cumulative rate is about twice the rate at 3 years. The rates are based on the time free in the community and on the *a priori* risk measure. Consistently with the analyses already reported, whether the offender was confined by the sentence received makes no difference in new arrests beyond incapacitation (by the sentence or as a result of later arrests).

A similar analysis of the cumulative survival and hazard functions was done for the groups given non-custodial, jail, youth facility, and prison sentences. A plot of the charts showed that the lines crossed. This indicates that the functions may be non-proportional -- and an assumption of the model is that they are. That is, the Cox regression, also called a proportional hazard model, may not be appropriate because the hazards (risk of new arrests) do not increase proportionally for the two groups. The risk of new arrests for the groups sentenced differently may not be related by a constant multiplier; their ratio depends upon time. This would be the case if there is an interaction between time and the sanction selection, as mentioned above.

A test of that hypothesis is given by changing the model to incorporate separate baseline hazard functions for each group. Because the main question of interest is in whether survival without arrest is different for the groups, a model is required that incorporates non-proportional hazards over time and provides an estimate of the effect of the different sentences. When this model was fitted to the data for time to arrest for the four groups, with the interaction of time and the sanction included as candidates for the equation, it was found that the proportional hazard model is not appropriate, because the interaction of time by sanctions was significant. [42]

The coefficients for the time by sanction interactions considered separately for each sanction showed that they were significant only for confinement in the youth facility and in prison. They indicated, by their signs, that, when contrasted with the other sanction groups, the hazard rate increases more rapidly over time for those who had been confined to the youth institution, less so for those imprisoned.

Charts analogous to those of Figures 25 and 26 cannot be plotted to depict these differences, but it may be concluded that they indicate an effect of the different types of sentences. Survival in the community without arrest after release from confinement in the youth facility decreases more rapidly than does survival without arrest for persons who have been imprisoned.

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[42] This analysis was based on 477 randomly selected cases (a random half of all with no missing data) because the large amount of memory required when using the whole sample exceeded available memory in the computer used. The Wald statistic, with 3 degrees of freedom, was 14.61, P = .002.

Because no differences were found for outcomes for persons given non-custodial sanctions or jail, and the time-dependent effect was limited to the youth facility and imprisoned groups, it was hypothesized that when groups were combined as (1) non-custodial or jail and (2) youth facility or prison the proportional hazard model would be appropriate. The survival and hazard functions were calculated for the persons who had either (county) non-custodial or jail sentences contrasted with those who received (state) youth facility or prison sentences. Figure 27 shows the result for the hazard function. The time dependent analysis showed no significant effect for the interaction of time and type of sentence, so the proportional hazard model may be considered appropriate. The hazard rates are higher for the youth facility and prison confined offenders because they were poorer risks to begin with and were more incapacitated, not because the placements were differentially effective. [43]

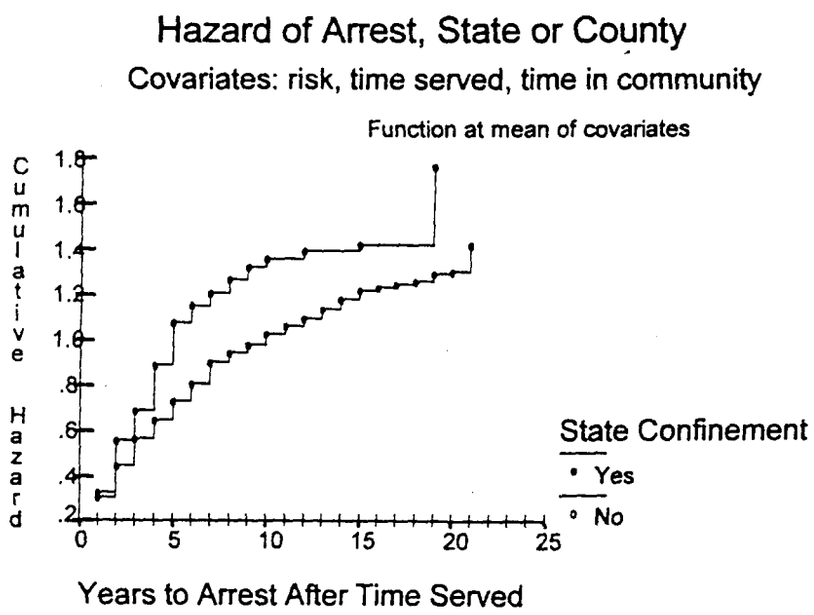

Figure 27

Hazard of Arrest After Serving Required Confinement, State (youth facility or prison) or County (non-custodial or jail) Sentences,

[43] The analysis of covariance in "Any New Arrest" showed, for 899 cases, significant effects (<= .001) for Functions 2 and 3 of the selection functions for the four main groups, risk measure 1, time served on the present sentence, and time in the community without incapacitation. The effect for "to state or county" was not significant (P= .41).

Conclusions

Results

The results of this study may be summarized according to the "main questions" and "secondary questions" posed at the outset. First, those questions about judicial selection of different sanctions, the validity of judges' predictions of risk, the measurement of risk and measures of incapacitation may be considered. Then, the questions of effects of the different sanctions used will be discussed.

Judicial Purposes

The judges participating in this study were focused mainly on crime control, emphasizing rehabilitation and specific deterrence as purposes of the sentence. Retribution was a stated main purpose in 18 percent of cases, and incapacitation and general deterrence were sometimes said to be most important; but the treatment aim -- including rehabilitation or specific deterrence -- was most prominent. Purposes often were mixed for a given case, and judges differed in their selection among purposes.

Sentences

Most offenders in the sample were sentenced to incarceration, but that often was suspended, with placement on probation. The main choices, in terms of sentences as executed, were a non-custodial sentence (42 percent), jail (29 percent), a youth facility (10 percent) and prison (19 percent). About 10 percent had "split" sentences of probation with jail. Some had special conditions other than or in addition to jail, including fines and restitution.

Judges' Predictions

Judges' subjective predictions of any new crimes, of property crimes, and of person crimes were valid, but modestly so. The validity of predictions of some judges compared favorably with some empirically derived, formal prediction methods. Yet, for four of the 18 judges, none of the predictions was valid. Which judges would be better predictors was of course not known beforehand. The judges' predictions appeared to be influenced mainly by their assessments of the seriousness of the offense, their judgments of social stability, and the length of the arrest and conviction records. Had the judges systematically used only the age of the offender as a predictor, their predictions would have been more accurate. If they systematically used their subjective predictions and age together, they would have done yet better in prediction.

limited utility

Formal Prediction Methods

Two measures of *a priori* risk were developed for use in the analyses of sentencing effects. The first, based on the whole sample, accounted in this sample for about a fourth of the variance in the "any new arrest" criterion. Risk Measure 1 included, as the best predictors,

measures of age, the judges' ratings of the arrest record, race, heroin or barbiturate use in the two years prior to arrest, alcohol abuse, and type of crime (property or serious drug offense). The same measure was valid as a predictor of other outcomes such as the total number of arrests and charges.

The second, called Risk Measure 2, was based only on the probationer sample. It included most of those in the other scale plus items such as the number of prior probation sentences, sex, and prior incarceration for probation or parole violation. The scores on this measure accounted for 31 percent of the variation in the "any new arrest" criterion.

Selection of Sentences

Measures of judicial selection of sentences were devised in order to control for it in the statistically designed studies of effects. These included the choice of confinement or not (the "in-out") decision; selection of the four main alternatives (non-custodial, jail, youth facility, or prison); of the three main custodial sanctions; and of "split" sentences.

Selection for any confinement (In-out).

In deciding whether to order confinement, judges appeared to be influenced mainly by their assessment of rehabilitation as an important aim of sentencing in the particular case, the recommendation of the probation officer for confinement (unless the P. O. and the judge used the same information in arriving at the decision), their own prediction of future crimes, and the seriousness of the offense. These and other items enabled correct classifications ("predictions") of confinement in 88 percent of the sample.

Selection of Type of Sanction

Selection of a non-custodial sentence or of jail, the youth facility, or prison appeared to be influenced mainly by the judges' predictions of any future crime, the perceived importance of rehabilitation for the case, the seriousness of the charge and prior criminal record, the number of counts of conviction, and the recommendation of the probation officer. The sentence type appeared to be influenced also by age and whether a property crime.

Selection for Split Sentences

In ordering split sentences, judges seemed to be influenced by their own predictions of future crimes, aggravating factors, and the relative importance of retributive and rehabilitative aims.

Offenses After Sentencing

About a fourth of the offenders were never again arrested in the 20 years after sentencing; three-fourths were arrested at least once. More than half were re-arrested in the first five years. They were arrested 5.3 times on the average. Their arrest rate while not incapacitated was .28 arrests per year. For those with at least one arrest, it was .36 per year. The mean time to a new arrest was 3.5 years; the median was 2.2 years.

The most frequent outcome was no new arrest. Next most often there was an arrest on a nuisance or property crime charge. The first arrest after sentencing involved a person crime with personal confrontation or physical harm in 12 percent of the cases. Over the entire follow up period, these offenders were charged with 40 illegal homicides (murder or manslaughter), 455 robberies, 752 assaults, 928 burglaries, 18 rapes, and other crimes for a total of 9,346 allegations of new crimes.

Effects of Sentences

The effects of different sentences were studied using statistical designs controlling for the measures of selection, *a priori* risk, time confined as a result of the sentence, and time exposed to risk in the community later (time minus incapacitation time for later confinements). Most analyses relied on the analysis of covariance; others on regression methods. Typically, a comparison was made between a "naive" interpretation of the observed outcomes without consideration of the effects of selection, risk, and incapacitation and "adjusted" values taking into account the potentially biasing factors.

Effects of the "In-out" Decision

Persons not confined had new arrests in 70 percent of cases, and those not confined were re-arrested 82 percent of the time. When adjustments were made for selection, *a priori* risk, and time in the community (not incapacitated) there was no statistically significant difference for the two groups. The results were the same when only the first five years after sentencing was considered. Similarly, when selection, risk, and incapacitation were taken into account, confinement had no effect on the arrest rate or the number of arrests, charges, or charges for specific crime categories.

Effects of Type of Sentence

The type of sentence — non-custodial, jail, youth facility, or prison, had a statistically significant effect on new arrests during the first five years after sentencing. After adjusting for selection, *a priori* risk, and incapacitation, those sent to the youth facility had the highest adjusted percent of new arrests. The adjusted values for those given non-custodial, jail, or prison sentences were about the same. Results were similar with the 20 year follow up but the differences were not statistically significant.

When persons with non-custodial sanctions were excluded from the analysis, a statistically significant effect of placement in jail, youth facility, or prison was found with the 20 year follow up. The adjusted values for jail and prison were the same, but those sent to the youth facility had higher adjusted values for new arrests. The effect was not statistically significant when only the first five years after sentencing was considered.

The choice among the four alternatives had a small effect, due mainly to higher adjusted values for new arrest by those sent to the youth facility. This effect could not be explained by age, selection, *a priori* risk, or incapacitation. Placement in non-custodial programs, jail, or prison had no effect.

Effects of Maximum Sentence

No effect of the maximum sentence, beyond incapacitation, was found.

Effects of Time Served in Confinement

After adjustments for selection, *a priori* risk, and incapacitation, it was found that the time actually served in confinement had a statistically significant but small effect on new arrests, accounting for less than two percent of the variability in new arrests.

Effects of Split Sentences, Fines and Restitution

The imposition of jail along with probation had no effect on new arrests. Neither fines nor restitution had a significant effect on new arrests during the first five years after sentencing. No difference was found, either, between those who complied with the fine or restitution order and those who did not.

control purpose of incapacitation but had little of no effect as a "treatment" with rehabilitative or specific deterrent effects.

It may be concluded also that judges can collaborate in research about the consequences of their sentences and that there is much to be learned by following up the offenders sentenced in order to determine whether sentencing aims are fulfilled. And, it may be argued that statistical designs can be informative in the absence of opportunities for experiments.

Limitations

Statistical designs such as used in this study set limits to generalizabilty because it cannot be assured that offenders compared after subjection to different sanctions were equivalent in all respects at the outset. Perhaps remaining but yet unknown selection factors, affecting either judicial decisions or the risk measures, affect the outcomes.

Other limitations are due to a crude measurement of outcomes. Simple measures of arrests and charges are not adequate to assess the full spectrum of costs and harms associated with either the sanctions imposed or the new crimes in the community.

The proportions of variance explained by the analyses presented in this report are modest, indeed. Figure 28 is based on the analysis of the "Any New Arrest" criterion for the first five years, in which a small but statistically significant effect was found for the type of sentence (Table 27). The variance explained by the sentence is less than one percent. Although about a fifth of the variability in new arrests was explained, this was due to *a priori risk* and selection by the judges. The most striking feature of the chart is the large area represented by the 80 percent of the variation that is unexplained.

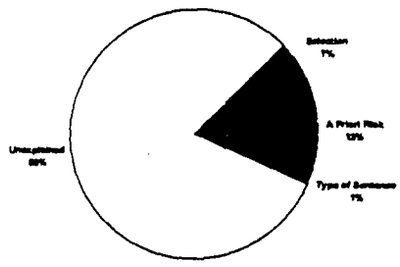

Figure 28
Explained and Unexplained Variance
in "Any New Arrest" Before 5 Years

Conclusions must be limited by the lack of information about the main independent variables of the study: adequate description of the programs of community treatments or jail and prison programs was not available.

If different kinds of sanctions are differentially effective for different kinds of offenders, that would not be learned from the analyses reported here.

Conclusions from this study obviously cannot be generalized to other jurisdictions and other times. The study does not show, for example, that well designed interventions implemented

Other Effects

Analyses of survival in the community after serving any incapacitative sentence indicated that effects of confinement in the youth facility or in prison may change over time and should be further investigated.

Implications

The diverse choices of sentencing purposes by judges in this study, and the selection of inconsistent purposes for the sentencing of individual cases, support the need for greater clarity and consistency in sentencing aims. The conflict between utilitarian and desert perspectives was apparent in this study, despite a general preference on the part of judges for utilitarian, crime control purposes. Clarity and consistency could be increased by acceptance of an internally consistent sentencing theory, consistently applied.

The subjective risk judgments by judges had a substantial effect on sentencing choices despite a modest validity. The use of more formal, empirically derived methods would enhance the rationality of sentencing when risk is determined by the sentencing theory accepted to be a relevant and justifiable consideration.

The main sentencing choices available to these judges had little effect on their crime control aims. Except for the effect of incapacitation, whether the offender was confined made no difference. Where the offender was confined made little difference -- except perhaps for an unfavorable effect of placement in the youth facility. The length of the maximum sentence imposed made no difference. The length of time actually confined made only a slight difference. When jail was imposed along with probation, it made no difference. Fines or restitution made no difference.

Aside from general deterrence (not studied) and incapacitation (in this study mainly providing a correction for the investigation of other crime control effects) little justification for the sentences from a crime control perspective was found. The various sanctions, differing in severity of punishment, may have served as a warning to others or as deserved punishment; but there was little or no evidence of rehabilitative or specific deterrence effects.

If the addition of jail time to a probation sentence is believed to have a specific deterrent effect, no evidence can be found in this study to justify the belief. Unless it is believed that jail time is required for desert, or the hope of an effective warning to others is maintained, abandoning the use of split sentences would be supported by the results of this study.

Similarly, if considerations of general deterrence and deserved punishment are set aside, it must be concluded that confinement, or increased length of incarceration, served the crime

with fidelity to a clear, coherent theory cannot be effective in meeting crime control objectives. No measures of the quality of rehabilitative programs or of the severity of sanctions were available. The study did not investigate whether the sentences imposed were deserved or whether they were fair.