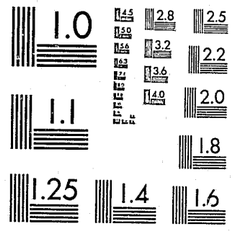
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SENTENCING BY MATHEMATICS: AN EVALUATION OF THE EARLY ATTEMPTS TO DEVELOP AND IMPLEMENT SENTENCING GUIDELINES

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REWRITTEN

Abstract

This is the report of an evaluation of empirically based, voluntary sentencing guidelines adopted by judges in four urban courts. The study focuses primarily on the Denver District Court and the Philadelphia Court of Common Pleas, with secondary emphasis on the Cook County Circuit Court (Chicago) and the Essex County Court (Newark).

The report raises serious questions concerning the empirical approach to the formulation of sentencing guidelines. It is concluded that the idea of empirically based sentencing guidelines is fundamentally flawed, and that one can have little confidence that the guidelines examined in this study actually represent the implicit policies or past practices of the judges in the relevant jurisdictions. In the authors' view, it is not feasible to create guidelines reflecting national, public policy by means of statistical analysis of prior sentencing decisions. A new approach to the problem of structuring judicial sentencing discretion is called for.

The authors find that the guidelines had no measurable impact on judicial sentencing behavior. Compliance rates were lower than expected, and sentence disparity was not reduced. The guidelines did not affect plea bargaining practices. On the contrary, the failure to account for the existing plea bargaining practices appears to be part of the reason for the guidelines' inefficacy.

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Chapter 1

Introduction

This is a study of empirically based, judicially adopted, voluntary sentencing guidelines in four urban courts. Its three major components are an examination of the empirical basis of the guidelines, an assessment of their impact on sentences, and an inquiry into the relationship between plea bargaining and sentencing guidelines. The four courts that served as sites for this study are the Denver District Court, in Denver, Colorado; the Philadelphia Court of Common Pleas, in Philadelphia, Pennsylvania; the Cook County Circuit Court, in Chicago, Illinois; and the Essex County Court, in Newark, New Jersey. Our research is based on reanalyses of the case record data used to construct the Denver, Chicago, and Newark guidelines; analyses of case record data collected by us as part of this project in Denver and Philadelphia; and interviews that we conducted in Denver, Chicago, and Philadelphia. Thus, our examination of the guidelines' empirical basis is limited primarily to Denver, Chicago, and Newark, whereas our assessment of the impact of sentencing guidelines on sentences and the exercise of discretion by prosecution and defense is limited exclusively to Denver, Chicago, and Philadelphia, with primary emphasis on Denver and Philadelphia.

In this chapter we shall briefly discuss the recent developments in sentencing reform that provide the background for the present inquiry. This is followed by a discussion of the research issues, and a description of the research design and methods employed in this study.

Background: Recent Trends in Sentencing

Our law and policy of sentencing are changing. Whether we are progressing, regressing, or swinging from side to side like a pendulum has been and should continue to be questioned. Nevertheless, our immediate course has been set in a general way, and we face strategic and tactical problems.

Identification of the causes of historical change such as that occurring in sentencing today is a risky business best left to historians, who will have the benefit of greater hindsight than we. A brief account of relevant events and trends, however, places current developments in perspective and suggests what the near future is likely to hold.

The most striking trend in public discussion of sentencing law and policy during the last ten or fifteen years is the growing disillusionment with rehabilitation, both as a practice and as an ideal. It is common belief that neither prisons nor many nonincarcerative treatment programs rehabilitate offenders. This belief derives in part from the conclusions of social researchers who have been unable to find any negative

relationship between participation in treatment programs and recidivism.[1] Although one of the most prominent of these researchers recently reported evidence that some programs do reduce recidivism, it remains to be seen whether his cautionary advice will be heeded.[2] Momentum and expense militate against renewed emphasis on treatment programs. Moreover, philosophical objections have been made to the notion that offenders should be deprived of their liberty for the purpose of rehabilitating them.[3] Both the practical and the philosophical difficulties are illustrated poignantly by accounts of parole release hearings characterized by sham, hypocrisy, and unfair procedure.[4]

Renunciation of the rehabilitative ideal and growing public fear of crime led naturally to a re-emphasis of deterrence, incapacitation, and retribution. Once the primary justification for indeterminate sentencing laws had been removed, concern focused on the undue consequences of judges' and parole boards' broad discretionary powers.

One of the supposed effects of indeterminate sentencing laws is a lower probability of punishment than would occur under a more determinate structure. The popular wisdom holds that deterrence requires "certainty of punishment," i.e., a high probability of punishment. In economists' terms, it is assumed that criminals are risk-preferring rather than risk-neutral or risk-aversive.[5] Despite the paucity of empirical evidence in support of this assumption, determinate sentencing legislation is promoted partly on the theory that it will increase the probability of punishment and thereby enhance deterrence.

The discretionary powers that inhere in indeterminate sentencing are believed to have engendered unacceptable disparities in sentences. Although usage varies, sentence disparity generally refers to unjustified differences in sentences.[6] Popular belief in the existence of pervasive disparity is reinforced by scholarly writers who assert that sentence disparity is well documented by research.[7] The empirical support is surprisingly thin, but there is every reason to suppose that inconsistent decisions result from unstructured discretion delegated to decision makers whose backgrounds and attitudes about an

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1. See, e.g., D. Lipton, R. Martinson, and J. Wilks, The Effectiveness of Correctional Treatment (1975).
2. R. Martinson, New Findings, New Views: A Note of Caution Regarding Sentencing Reform, 7 Hofstra L. Rev. 243 (1979).
3. F. Allen, The Borderland of Criminal Justice 25-41 (1964); L. Fuller, The Morality of Law 165 (1964).
4. Stanford, A Model Clockwork-Orange Prison, N.Y. Times Magazine, September 12, 1972, at 9; L. Sutton and L. Holmberg, Doing Time (1979) (documentary film).
5. See R. Posner, Economic Analysis of Law 170 (2d ed. 1977).
6. For a detailed discussion of the definition and measurement of sentence disparity, see pp. 109-137.
7. See, e.g., J. Coffee, The Repressed Issues of Sentencing: Accountability, Predictability, and Equality in the Era of the Sentencing Commission, 66 Geo. L. J. 975, 979 (1978).

emotion-laden subject are as diverse as the cases they must decide. Presumed disparity is condemned both because it violates the principle of equality[8] and because it is thought to cause unrest among prisoners.[9] The standardless discretion that permits disparity arouses lawyers' traditional concerns with fundamental fairness and rationality.[10] Unfettered discretion is antithetical to the rule of law.[11] The total absence of principles or objective standards to guide discretion renders sentence decisions largely unreviewable. The potential for injustice is manifest.

The demands for reform of sentencing have divergent sources but a common theme. All seek in some way to limit the discretion of public officials responsible for administering criminal sanctions under indeterminate sentencing laws. Judges and parole boards have been at the center of attention, although there is increasing recognition of the roles of other officials.[12]

The debate about sentencing has not been idle. Lawmakers have responded swiftly if not precipitously. The number of states that have revised their sentencing laws has grown annually since 1975, when the Maine legislature passed a new criminal code that eliminated parole release discretion.[13] The legislative enactments have varied considerably in substance and terminology,[14] but all reflect trends in the current debate.

The legislatures have not been alone in responding to demands for reform. Judges and parole boards have experimented with new principles, standards, and procedures. Judges have participated in sentencing institutes and sentencing councils.[15] Parole boards, led by the U.S. Parole Commission, have formulated guidelines for the release of prisoners.[16] Judges have experimented with guidelines for the

8. See id.
9. See New York State Special Commission on Attica, Attica: The Official Report of the New York State Commission on Attica (1972).
10. See M. Frankel, Lawlessness in Sentencing, 41 U. Cinn. L. Rev. 1 (1972).
11. See K. Davis, Discretionary Justice (1977).
12. See, e.g., A. Alschuler, Sentencing Reform and Prosecutorial Power: A Critique of Recent Proposals for "Fixed" and "Presumptive" Sentencing, 126 U. Pa. L. Rev. 550 (1978); M. Rubinstein and T. White, Alaska's Ban on Plea Bargaining, 13 L. and Soc'y Rev. 367 (1979).
13. Me. Rev. Stat. tit. 17A, §1252 (1979 Supp.).
14. See S. Lagoy, F. Hussey, and J. Kramer, A Comparative Assessment of Determinate Sentencing in Four Pioneer States, 1978 Crime and Delinquency 385.
15. See L. Orland and H. Tyler, Justice in Sentencing (1974); S. Diamond and H. Zeisel, Sentencing Councils: A Study of Sentence Disparity and Its Reduction, 43 U. Chi. L. Rev. 109 (1975).
16. See D. Gottfredson, L. Wilkins, and P. Hoffman, Guidelines for Parole and Sentencing (1978).

imposition of sentences.[17] Indeed, some of these efforts by judges and parole boards to control their own discretion preceded the first legislative attempts to do so.

Changes now occur so frequently that it is almost impossible to keep abreast of them. Popular, professional, and scholarly publications contain innumerable references to mandatory, determinate, presumptive, flat-time, and definite sentencing, as well as to sentencing commissions, sentencing guidelines, and parole guidelines. Inconsistent use of these terms causes confusion.

In a discussion of current changes in sentencing law and policy, it is useful to begin by asking two questions. First, whose discretion is being constrained? Most recent efforts have been aimed at the discretionary powers of the judge or the parole board--occasionally both at once. Attempts to constrain the discretionary powers of police, prosecutors, prison administrators, and executive clemency boards have been less common. The second question is, What is the nature of the constraint on discretionary action? Professor Kenneth C. Davis distinguishes between confining, structuring, and checking discretion.[18] To confine discretion is to circumscribe it.[19] A legislative requirement that a given class of crimes be punished by imprisonment for not less than five years nor more than 10 years constitutes confinement of the judges' sentencing discretion. Eliminating discretion can be thought of as a special case of confinement where the boundaries are constricted to the point where there is no room left for discretionary actions.

To structure discretion is to control the manner in which it is exercised within the established boundaries.[20] For example, judges' sentencing discretion may be structured by requiring that they state reasons for imposing particular sentences, and by further requiring that the reasons belong to a specified class of acceptable reasons. Finally, checking discretion means subjecting it to review.[21] Appellate review of sentences and the use of sentence review panels are two examples of checking discretion.

The recent spate of changes in sentencing laws and policies therefore can be classified along three dimensions: the office of the decision maker whose discretion was constrained (or enlarged), whether the change was self-imposed or externally imposed, and the nature of the constraint. This classification scheme helps to minimize confusion in discussions about the variety of changes indiscriminately referred to as "determinate sentencing."

17. See L. Wilkins, J. Kress, D. Gottfredson, J. Calpin, and A. Gelman, Sentencing Guidelines: Structuring Judicial Discretion (1978).
18. K. Davis, supra note 11.
19. See id. at 52-96.
20. See id. at 97-141.
21. See id. at 142-161.

The discretion exercised by parole boards in releasing prisoners was an early target of reform efforts. In 1973 the U.S. Board of Parole, later retitled the U.S. Parole Commission, in response to public criticism, adopted a set of numerical guidelines for prisoner release decisions.[22] The Board's purpose in adopting the guidelines was to structure its own discretion so as to enhance substantive and procedural fairness.[23] In 1975 the Maine legislature eliminated parole release discretion entirely; in fact, it effectively eliminated the parole board.[24] Parole reform has continued along both paths: a few states have followed the example of the U. S. Parole Board by using guidelines to structure release discretion,[25] while other states have followed Maine's example by eliminating parole release discretion.[26]

In the wake of these parole reforms, there have been a large number of proposals to curtail judicial sentencing discretion. The legislatures of Indiana,[27] Illinois,[28] and Colorado[29] have enacted statutes that confine judicial sentencing discretion to narrower ranges of incarcerative sentences. California,[30] Arizona,[31] and New Mexico,[32] have created statutory presumptions in favor of a particular sentence for each statutory crime classification. These presumptions can be overcome by a finding in the record of specific aggravating or mitigating circumstances. For the most part, the range of sentences available to the judge where aggravating or mitigating circumstances are found is narrower than under the prior laws.[33] Legislation of this kind, which confines and structures judicial sentencing discretion simultaneously, has been variously called presumptive, determinate, flat, and definite. Finally, Pennsylvania[34] and Minnesota[35] have enacted legislation creating administrative sentencing commissions charged with the duty of creating sentencing guidelines which, when promulgated, will have the force of presumptions requiring statements of reasons in the record to justify departures. Besides confining and structuring discretion, several of these enactments have sought to check judicial sentencing discretion by providing for more extensive appellate review of sentences than existed under prior law.

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22. D. Gottfredson et al., supra note 16.
23. Id.
24. Me. Rev. Stat. tit. 17A, §§1251-54.
25. See D. Gottfredson et al., supra note 16, at 37.
26. See, e.g., Cal. Penal Code §1168 (1980 Supp.); Ind. Code Ann. §35-50-2-4 to 3-4 (1979 Supp.); Ill. Ann. Stat. ch. 38, §1005-10-1 to 10-3 (1980-81 Supp.); Ariz. Rev. Stat. Ann. §13-701 (1978); Colo. Rev. Stat. §16-11-302 (1979 Supp.).
27. Ind. Code Ann. §§35-50-2-4 to 3-4 (1979 Supp.).
28. Ill. Ann. Stat. ch. 38, §1005-8-1 (1980-81 Supp.).
29. Colo. Rev. Stat §§18-1-105 (1979 Supp.).
30. Cal. Penal Code §1170 (1980 Supp.).
31. Ariz. Rev. Stat. Ann. §13-701, 702 (1978).
32. N.M. Stat. Ann. §31-18-15, 15.1 (1979 Supp.).
33. The principal exception to this is California, where the new law had the effect of expanding the sentencing judge's discretion.
34. 18 Pa. Cons. Stat. Ann. §§1381-86 (1979-80 Supp.).
35. 1978 Minn. Laws ch. 723.

Courts in several states have begun attempts to structure judicial sentencing discretion in the absence of statutory requirements. Trial courts in Denver, Newark, Chicago, and Phoenix led the way by participating in a federally funded program conducted by the Criminal Justice Research Center (CJRC) to develop empirically based sentencing guidelines. The New Jersey Administrative Office of the Courts followed suit by developing statewide sentencing guidelines, which pre-empted the Newark guidelines.[36] The Philadelphia Court of Common Pleas also developed a set of empirically based sentencing guidelines.[37] The state courts of Michigan and Massachusetts have begun to develop similar guidelines; Maryland and Florida are involved in a federally-funded project to develop multicounty regional sentencing guidelines based on the original CJRC model. Other state court systems, including those of Rhode Island, Nebraska, South Carolina, Wisconsin, and Texas, are in various stages of exploring the possibilities of sentencing guidelines.

The popularity of sentencing guidelines suggests that they have become the leading alternative to legislatively fixed or presumptive sentences. In fact, it is clear that in some instances sentencing guideline reform has been undertaken in an effort to avoid more restrictive sentencing legislation. Although there are now several variations on and departures from the original CJRC model, all of the models now being developed share a common heritage. Let us now examine the history of empirically based guidelines for sentencing.

Sentencing Guidelines

Sentencing guidelines, as they exist today, are an outgrowth of criminological research. Early in the twentieth century, criminologists began to apply statistical techniques to make actuarial predictions of dangerousness.[38] It was thought that such predictions could improve the ability of paroling authorities to make appropriate release decisions. These techniques were formally applied to actual decision making for the first time in the early 1970s when the U.S. Board of Parole adopted numerical guidelines based in part on actuarial predictions of recidivism.[39]

The federal parole guidelines, formulated by criminologists, were based on the results of three distinct pieces of research. The first took as its point of departure the assumption, which was supported by some empirical evidence, that decision makers consider relatively few items of information in reaching decisions.[40] After each parole

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36. The New Jersey sentencing guidelines differ in some important respects from the other guidelines that have been developed to date. Perhaps the most important difference is that, instead of providing recommended sentences for each combination of offense and offender scores, the New Jersey guidelines simply present the percent incarcerated and median sentence length for each combination of specific offense and offender characteristics. Thus, the New Jersey guidelines are more strictly descriptive (as opposed to normative) than are the guidelines evaluated in this report.
37. The Philadelphia guidelines were developed by the court's administrative staff with consultation by some of the CJRC researchers.
38. See D. Gottfredson et al., supra note 16, at 43-47.
39. Id. at 13-37.
40. See D. Gottfredson et al., supra note 16, at 95-103. See also Chapter 2, note 67 and accompanying text.

release hearing, parole board members rated the case just heard according to (1) the seriousness of the instant offense, (2) parole prognosis (the probability of recidivism), (3) institutional discipline, and (4) participation in institutional programs. The researchers found that the parole board members' ratings of the seriousness of the instant offense were correlated highest of the four scales with the outcome of the initial decision whether to release. The second best predictor of the outcome was the board members' rating of parole prognosis. These two variables combined were found to account for approximately half of the variation in the initial decision whether to release. The two other variables, institutional discipline and participation in institutional programs, accounted for little variation in addition to that already accounted for by the seriousness of the offense and parole prognosis. The researchers concluded that there were two and only two important factors in the initial decision whether to release a prisoner: the seriousness of the offense and the parole prognosis. In subsequent decisions, where the parole board had previously denied a prisoner parole, the institutional discipline rating was found to be more highly correlated with the outcome of the decision, prompting the researchers to conclude that at later hearings institutional behavior was an important determinant of parole release.[41]

The second in this series of three studies was a study of the correlates of parole success and failure.[42] The researchers examined the records of federal prisoners who had been paroled to determine which had been recidivists. Information was collected about each parolee's social, psychological, criminal, and institutional history. Of the items found to correlate with recidivism, the researchers rejected those that posed "ethical" problems (e.g., prior arrests not leading to conviction), those that occurred with insufficient frequency to make them useful predictors (e.g., escape history), and those that were redundant with other items (e.g., length of longest employment, which overlaps with whether the offender was employed within two years preceding incarceration). Nine variables, called salient factors, were used to construct a scale for predicting recidivism. These variables included the number of prior convictions, the number of prior incarcerations, the offender's age at first commitment, whether the instant offense was auto theft, whether the offender's parole had been revoked, the offender's drug history, the offender's educational attainment, whether the offender had been employed for at least six months during his or her last two years in the community, and whether the offender planned to live with a spouse or children upon release.

The third study required parole hearing examiners and parole board members to rank offenses according to seriousness.[43] The researchers found a high degree of consensus among the hearing examiners and among the board members, as well as a high degree of test-retest reliability.

41. See D. Gottfredson et al., supra note 16, at 15-16, 183-198.
42. See id. at 41-67.
43. See id. at 69-80.

On the basis of the research just described, the researchers created a mathematical model of parole release decision making. The model consisted of two scales, one measuring seriousness of the offense and the other measuring parole prognosis. The first was based on the median time served for various groups of offenses; these groups were defined on the basis of the seriousness ratings developed in the third phase of the research. The parole prognosis, or salient factor, score was similar to that developed in the parole prediction study described above. The median time served was calculated from a sample of prior parole release decisions for each combination of offense seriousness and salient factor scores. In the subsequent construction of decision guidelines, the median scores were "bracketed" to provide ranges within which discretion remained unstructured. The researchers described the resulting model as a representation of previously implicit paroling policy that could be made explicit if the Parole Board were to implement it as guidelines for prisoner release decisions.

The U.S. Board of Parole began to use the model on a trial basis in October 1972 when the federal paroling function was regionalized.[44] Hearing examiners were instructed to provide written reasons whenever their recommendations departed from the guidelines model. Provisions were made for periodic review and revision of the guidelines. In November 1973 the model was incorporated in regulations applicable to the entire federal prison system.[45] Despite several legal attacks they remain in force today.[46]

At about the time the U.S. parole guidelines were implemented, criticism of judicial sentencing discretion was intensifying. One prominent federal judge called for codification of sentencing criteria in the form of a "detailed calculus of sentencing factors, including [the] use of arithmetical weightings."[47] Before long, some of the researchers who had developed the U.S. parole guidelines proposed to develop similar guidelines for judicial sentencing. This proposal was accepted by the Law Enforcement Assistance Administration, which funded the Criminal Justice Research Center (CJRC) of Albany, New York, to conduct research into the feasibility of judicial sentencing guidelines.[48]

The feasibility study began in July 1974. After approaching courts in several jurisdictions, the researchers selected four courts to participate in the research. The District Court of Denver, Colorado, and the district courts of Vermont were active participants, while the Essex County Court of Newark, New Jersey, and the Polk County District Court of Des Moines, Iowa, were designated "observers." Like the U.S. parole guidelines, the sentencing guidelines developed by the CJRC researchers were to be based on empirical research. The researchers collected data about criminal cases for which Denver and Vermont judges had already

44. See id. at 18-19.
45. 18 C.F.R. §2.
46. See D. Gottfredson et al., supra note 16, at 37.
47. M. Frankel, supra note 10, at 47.
48. L. Wilkins et al., supra note 17.

imposed sentences. Using these data the researchers built mathematical models of the sentencing decision. The important factors in sentencing decisions were identified by the strength of statistical correlations with the outcomes of sentencing decisions. Like the U. S. Parole Board, the judges were asked to rate the seriousness of crimes. Finally, the researchers created models that incorporated the judges' ratings of the seriousness of crimes and the results of the studies of past sentencing decisions.

Like the parole guidelines, these models used two scales to predict decisions. The first was a measure of offense seriousness, and included the judges' subjective ratings of seriousness as well as more objective measures relating to the use of weapons, the infliction of personal injury, and the sale of drugs during the commission of the crime. A second scale measured the background of the offender. It comprised various measures of the offender's criminal history (e.g., number of prior convictions, incarcerations, and probation or parole revocations) and social stability (e.g., employment status and drug or alcohol dependence). Unlike the parole guidelines project, however, the sentencing guidelines project did not attempt to predict recidivism.

The CJRC researchers found that their models accounted for 14 to 53 percent of the variation in sentencing decisions. When the mean sentence for each combination of offense and offender scores was bracketed to provide for a range of sentences within which discretion was to remain unstructured, the resulting models were reported to accurately predict 80 to 85 percent of sentences, in the sense that 80 to 85 percent of all sentences in the guidelines construction sample fell within the guideline ranges.[49] On the basis of these results and acceptance of the models by an advisory committee of judges from the research sites, the researchers concluded that the development of judicial sentencing guidelines similar to the U.S. parole guidelines was feasible and desirable.[50]

The worksheet eventually adopted in Denver to assist judges in the calculation of the guideline sentence for each offender is presented in Figure 1-1. The recommended sentence is calculated on the basis of three values--offense class, offense score, and offender score--which derive exclusively from the ten particulars enumerated on the worksheet. The offense class indicates which of several two-dimensional grids is appropriate for the case (There is a separate grid for each felony and each misdemeanor classification; there are also separate grids for drug offenses. Figure 1-2 represents the grid used for class 3 felonies in Denver.) Using the appropriate grid, the judge locates the cell designated by the intersection of the Offender Score (horizontal axis) and the Offense Score (vertical axis). The information contained in the cell recommends both the type ("OUT" signifies probation or another nonincarcerative term) and the length of sentence. Regarding length, the first range of years in the cell represents the boundaries within which the minimum term is said to "properly" fall for the kinds of case represented by the three values that define the cell. In similar fashion, the second range designates the lower and upper limits for the maximum term. The worksheets and all corresponding guidelines grids for Philadelphia and Denver are presented in Appendix E.

Following the feasibility study, the Law Enforcement Assistance Administration awarded another grant to the Criminal Justice Research Center in July 1976 to implement sentencing guidelines in four jurisdictions. Vermont withdrew from the project, but the Essex County Court changed its status from observer to active participant. The Newark and Denver courts were joined by the Cook County Circuit Court, of Chicago and the Maricopa County Superior Court, of Phoenix, Arizona, as implementation sites. The processes of data collection and model construction were repeated in the Chicago and Phoenix courts. As with the Denver and Newark courts, alternative models were presented to the judges, who selected one for implementation after modifying it to suit their preferences.

Sentencing guidelines were implemented in Denver in November 1976, Chicago in June 1977, Newark in January 1978, and Phoenix in April 1978.

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49. But see pp. 82-109.
50. See L. Wilkins et al., supra note 17, at 27-28.

Figure 1-1

Sentencing Guidelines Worksheet

OFFENDER _____ DOCKET NUMBER _____

JUDGE _____ DATE _____

Offense(s) Convicted Of: _____

_____

_____

OFFENSE CLASS (MOST SERIOUS OFFENSE)

OFFENSE SCORE

A. Intra-Class Rank _____

B. Seriousness Modifier _____

0 = No injury 0 = No weapon 0 = No sale of drugs
1 = Injury 1 = Weapon 1 = Sale of drugs
2 = Death

C. Victim Modifier (Crime Against Person) _____

0 = Unknown victim
1 = Known victim

OFFENSE CLASS

OFFENDER SCORE

A. Current Legal Status _____

0 = Not on probation/parole, escape
1 = On probation/parole, escape

B. Prior Juvenile Convictions _____

0 = No convictions
1 = 1-3 convictions
2 = 4 or more convictions

C. Prior Adult Misdemeanor Convictions _____

0 = No convictions
1 = 1-3 convictions
2 = 4 or more convictions

D. Prior Adult Felony Convictions _____

0 = No convictions
1 = 1 conviction
2 = 2 or more convictions

E. Prior Adult Probation/Parole Revocations _____

0 = None
2 = One or more revocations

F. Prior Adult Incarcerations (Over 30 days) _____

0 = None
1 = 1 incarceration
2 = 2 or more incarcerations

OFFENDER SCORE

GUIDELINE SENTENCE _____

ACTUAL SENTENCE _____

REASONS (If actual sentence does not fall within guideline range):

11 9-2

FELONY .3

Figure 1-2
Sentencing Guidelines Decision Grid

Offense Score	Offender Score: 0-1	2	3	4	5-8	9-10	11-13
4-5	5-7 yrs. minimum 8-10 yrs. maximum	7-9 yrs. minimum 12-15 yrs. maximum	10-12 yrs. minimum 15-20 yrs. maximum	12-15 yrs. minimum 15-20 yrs. maximum	12-15 yrs. minimum 15-20 yrs. maximum	17-22 yrs. minimum 35-40 yrs. maximum	17-22 yrs. minimum 35-40 yrs. maximum
3	OUT	7-9 yrs. minimum 12-15 yrs. maximum	7-9 yrs. minimum 12-15 yrs. maximum	7-9 yrs. minimum 12-15 yrs. maximum	8-10 yrs. minimum 15-20 yrs. maximum	17-22 yrs. minimum 35-40 yrs. maximum	17-22 yrs. minimum 35-40 yrs. maximum
2	OUT	5-7 yrs. minimum 12-15 yrs. maximum	5-7 yrs. minimum 12-15 yrs. maximum	5-7 yrs. minimum 12-15 yrs. maximum	8-10 yrs. minimum 12-15 yrs. maximum	17-22 yrs. minimum 35-40 yrs. maximum	17-22 yrs. minimum 35-40 yrs. maximum
1	OUT	OUT	OUT	5-7 yrs. minimum 12-15 yrs. maximum	5-7 yrs. minimum 12-15 yrs. maximum	8-10 yrs. minimum 12-15 yrs. maximum	8-10 yrs. minimum 15-20 yrs. maximum

In each court, judges agreed, (or were requested by the presiding judge of the criminal division, to consult the guidelines and to provide written reasons whenever their sentences departed from those recommended by the guidelines. Compliance, however, was voluntary; there were no sanctions for disregarding the guidelines altogether or failing to provide written reasons for individual departures from the guidelines. Provision was made for continuing data collection and periodic review and modification of the guidelines.

The idea of numerical guidelines for sentencing began to attract national attention. Drafters of the proposed revision of the federal criminal code included a provision that would mandate promulgation of sentencing guidelines by an administrative agency to be called the U. S. Sentencing Commission.[51] While the bill languished in Congress, the legislatures of Minnesota and Pennsylvania enacted similar laws.[52] The Philadelphia Court of Common Pleas began its own effort to develop sentencing guidelines in consultation with from the researchers who developed the original Denver guidelines. The New Jersey Administrative Office of the Courts undertook a similar effort, as did the Michigan Office of the State Court Administrator. Other states currently are considering the adoption of statewide sentencing guidelines.[53] Most of the projects that have been conducted thus far have been based largely on the empirical paradigm created by the researchers at the Criminal Justice Research Center.

The sentencing guidelines now being used, developed, or considered around the country differ in several ways. The first basis for distinction is the nature of the promulgating authority. In some jurisdictions, guidelines are being developed by administrative agencies pursuant to legislative mandate, while in other jurisdictions guidelines are being (or have been) developed in the absence of express legislative authority by state judiciaries' administrative offices under the general rulemaking or supervisory authority of the courts of last resort. Those now in use in Philadelphia were adopted voluntarily by the local judges. The authority of the agency that promulgates sentencing guidelines has obvious implications for the legal consequences of noncompliance. Those guidelines that were not created pursuant to legislative mandate or under judicial rulemaking authority lack the force and effect of law, so noncompliance does not trigger any legal consequences such as reversal on appeal.

There are many variations in the exact form and contents of sentencing guidelines in different jurisdictions. Although all use grids to present recommended sentences for combinations of case characteristics, there is nothing essential about the use of grids, and it is probably only a matter of time before someone develops gridless guidelines. There is already significant variation in the number and

51. See E. Kennedy, Symposium on Sentencing, Part I: Introduction, 7 Hofstra L. Rev. 1 (1978).
52. See p. 5.
53. See p. 6.

organization of guideline grids. Some jurisdictions (e.g., Minnesota) use a single grid for all crimes, while others (e.g., Denver, Chicago) have a grid for each statutory class of felonies and misdemeanors, and still others (e.g., Newark, Philadelphia) employ different grids for crimes against persons, crimes against property, and victimless crimes. Indeed, the New Jersey guidelines include a grid for each offense.

The factors used to compute guideline sentences are similar from one jurisdiction to another, but their exact number and scoring differ. Most guideline models include sets of variables pertaining to offense seriousness (e.g., the use of a weapon, infliction of injury) and the offender's criminal history (e.g., number of prior convictions, incarcerations, probation or parole revocations). Some, however, include variables pertaining to the offender's social stability, such as employment status and educational achievement, while others do not.[54]

Finally, it must be emphasized that sentencing guidelines need not be based on statistical analyses of past sentencing practices, although there is as yet little variation in this regard. There is no reason why sentencing guidelines could not be wholly the product of conscious choices made by responsible agencies without reference to empirical evidence about past practices. Simulation exercises provide yet another method by which to formulate sentencing guidelines. Using this method, judges impose sentences in hypothetical cases that systematically vary in their particulars. Statistical analysis of the results of these exercises would provide a basis for sentencing guidelines analogous to mathematical models based on past sentencing decisions, but the simulation experiment, unlike the nonexperimental study of prior decisions, would minimize problems of control and measurement.[55] Of course, judges' sentences in hypothetical cases may be different from the sentences they mete out to flesh-and-blood defendants. To date, only Rhode Island has chosen the simulation method to create sentencing guidelines.

The sentencing guidelines approach to structuring judicial discretion has won a sizable following in the United States. It is viewed by many as the leading alternative to legislatively specified presumptive sentences. Some who believe that the complexities of sentencing require more flexibility than is usually afforded by legislative attempts to confine judicial sentencing discretion have argued strongly and sometimes successfully on behalf of sentencing guidelines. Until now, however, the debate has been conducted in the absence of empirical evidence about the impact of sentencing guidelines and without benefit of a close examination of the methods by which sentencing guidelines have been developed and implemented.

54. The Philadelphia guidelines, for example, include employment status and educational achievement as determinative factors, whereas the Denver and Chicago guidelines do not. Cf. Minnesota Sentencing Guidelines Commission, Report to the Legislature 26 (January 1, 1980) ("Sentencing should be neutral with respect to race, gender, social or economic status of convicted felons.").
55. See Chapter 2, note 109 and accompanying text.

Research Issues

In October 1978 the National Center for State Courts undertook this study of sentencing guidelines. The purpose of the research, as defined by the National Center and the National Institute of Justice, which funded the project, is to evaluate the development, implementation, and impact of empirically based sentencing guidelines implemented by local judiciaries. It is hoped that the results of this research will make a contribution to the continuing debate about whether and how to structure judicial sentencing discretion, and that future reform efforts will benefit from our work.

The issues under investigation may be grouped into four general categories. The first three correspond to chapters 2 through 4 of this report. The fourth is treated in a report by the Institute of Economic and Policy Studies found in Appendix A.

We begin in Chapter 2 by examining the empirical basis of sentencing guidelines. The use of modeling techniques in the formulation of public policy raises serious technical and substantive questions. What are the capabilities and limitations of statistical modeling? Is modeling an appropriate technique for understanding the sentencing process? Can we infer implicit policies from such models? Is statistical modeling an appropriate technique for deciding and implementing sentencing policy? How should the model be specified? What is the appropriate unit of analysis? How should we decide which variables to include and exclude? What problems are associated with data collection and measurement? What problems are encountered in estimating parameters? In what form should the models be presented? In an attempt to answer these questions, we review the relevant social science literature, reanalyze some of the data that were used to construct sentencing guidelines, and analyze data collected specially for this evaluation.

The heart of the present research is our study of the impact of voluntary sentencing guidelines on the exercise of judicial sentencing discretion. The primary purpose of sentencing guidelines is to structure judicial discretion in order to reduce disparities without removing ostensibly necessary flexibility. In Chapter 3 we examine the extent of judicial compliance with sentencing guidelines. We then define and measure sentence disparity in several ways to determine whether guidelines have succeeded in reducing it. Finally we investigate whether sentencing guidelines have affected the overall severity of sentences.

In Chapter 4 we consider the role of sentencing guidelines within the larger system of discretionary decision making, which includes the prosecution and the defense. Criminal justice frequently is described as a process or system. Like other participants in processes or systems, the participants in criminal justice are interdependent. The actions of each are influenced by those of the others. The criminal justice system is imbued with discretion at every stage--from investigation through arrest, charging, and conviction to sentencing and corrections. It is reasonable to think that a change in the exercise of discretion by one decision maker will be accompanied by changes in the discretionary actions of others. One hypothesis is that when judicial sentencing discretion

is constrained so that sentences become more predictable or predetermined, control (discretion) over sentences will shift to the attorneys via charge bargaining. We test this hypothesis and explore the ramifications of plea bargaining for the structuring of judicial sentencing discretion.

Research Design and Methods

Voluntary sentencing guidelines have been implemented in five local jurisdictions. Ironically, while the popularity of sentencing guidelines has grown, determinate sentencing legislation has made obsolete the guidelines in Phoenix[56] and Denver,[57] and required modification of those in Chicago.[58] Statewide sentencing guidelines supplanted the Newark guidelines. The Philadelphia guidelines, which were adopted in March 1979, will be obsolete when the Pennsylvania Sentencing Commission promulgates statewide sentencing guidelines.[59] The impermanency of local guidelines augurs statewide rather than local efforts to constrain judges' sentencing discretion. Nevertheless, we believe that voluntary sentencing guidelines implemented in local jurisdictions may be regarded profitably as case studies in structuring judicial sentencing discretion, the results of which must be interpreted in light of the similarities and differences between them and statewide or federal sentencing guidelines.

In this section we shall discuss some of the issues of design and methods that attend this research. We begin by explaining our choice of research sites, which, it turned out, involved very little choice at all. Next, the basic design of the study is discussed, along with its limitations. Following that, we describe the case record data that we collected for the impact study, and briefly summarize the statistical techniques to which they were subjected. The next subject is our use of interviews. We conclude with a note about our tendencies in drawing conclusions and implications based on our observations.

Site selection. Of the five local jurisdictions in which voluntary sentencing guidelines have been implemented, two had ceased using them by the time we began our interviews and case record data collection. The Newark guidelines had been in effect only ten months before they were supplanted by the New Jersey statewide guidelines. Our preliminary

56. See note 31 and accompanying text.
57. See note 29 and accompanying text.
58. See note 28 and accompanying text; see also pp. 178-179.
59. See note 34 supra; Chapter 4, note 76.

interviews indicated that the Newark guidelines had enjoyed only limited use during their short lifespan. Moreover, it was apparent to us that Newark judges' and lawyers' recollection of the local guidelines and their impact had been clouded by the introduction of a different but not entirely dissimilar reform. Furthermore, it appeared that we would not have access to the case files from which to collect case record data. For these reasons, our study of the Newark guidelines was limited to a reanalysis of the data on which they were constructed; these data were made available to us in computer-readable form by the guidelines researchers.

The story of the Phoenix sentencing guidelines is not unlike that of the Newark guidelines. They were implemented in April 1978, when it was already known that the Arizona legislature was likely to pass determinate sentencing legislation. Consequently, implementation was half-hearted. When the new legislation took effect on October 1, 1978, the guidelines were obsolete. Although our preliminary interviews revealed some sentiment for revising the guidelines, it was clear that the Phoenix court's experience with guidelines was too limited to warrant choosing it as a research site. In part by default, our attention was focused on the three remaining cities in which sentencing guidelines had been developed.

As one of the sites of the original feasibility study, the Denver District Court had used sentencing guidelines longer than had any of the other four jurisdictions. Guidelines were used by the Denver judges for more than two and one-half years, from November 1976 until July 1979, when Colorado's new sentencing laws took effect.[60] During that time the guidelines remained substantially unaltered.

The Denver District Court is the trial court of general jurisdiction for the City and County of Denver. The District Court has jurisdiction over all cases in which a felony has been charged. The County Court, a trial court of limited jurisdiction, has jurisdiction over all cases in which only misdemeanors are charged. In addition, the County Court judges serve as magistrates in felony cases; they issue warrants, conduct initial appearances, hear bail motions, and hold preliminary hearings. If the charges in a complaint are bound over at the preliminary hearing (upon a finding of probable cause), an information is filed in the District Court, where the defendant subsequently is arraigned.[61] This is followed by a "plea and setting" hearing at which the defendant enters a plea; if the plea is "not guilty," a trial date is set.

Six judges of the Denver District Court are assigned to the criminal division at a time, where they generally remain for two years until they are rotated off in staggered fashion. Approximately 2,000 felony cases are filed annually in the District Court, of which roughly 600 result in convictions and another 600 result in deferred prosecution or deferred judgment. The work of the criminal division is managed by its presiding judge, who reports to the chief judge of the District.

60. See Chapter 4, note 21.

61. A few cases are initiated by grand jury indictment or direct filing of an information in the District Court without the filing of a complaint in the County Court, but these are rare.

The Philadelphia Court of Common Pleas is the trial court of general jurisdiction for the City and County of Philadelphia. With approximately 40 judges assigned to hear criminal cases, the court has original jurisdiction over all felonies. The Municipal Court has original jurisdiction over all misdemeanors. The Court of Common Pleas also has appellate jurisdiction over misdemeanor cases, in which defendants are entitled to trials de novo.

Judges of the Municipal Court serve as magistrates in felony cases. Complaints are filed by the police in the Municipal Court for preliminary hearings. Upon finding probable cause, the Municipal Court judge binds over the charges and the District Attorney files an information in the Court of Common Pleas, where the defendant is arraigned, enters a plea, and is tried (if necessary) and sentenced. There are roughly 5,000 convictions annually in the Court of Common Pleas.

The Philadelphia Court of Common Pleas officially implemented sentencing guidelines on March 5, 1979, after a two-and-one-half-year development effort by a committee of judges assisted by researchers from the court's administrative office. The recency of the guidelines' implementation presents both advantages and disadvantages for our research. We were, of course, unable to observe any long-term effects in the six months between the implementation of guidelines and the end of our data collection period on August 31, 1979. It remains to be seen whether there were delayed effects. On the other hand, we were able to directly observe the implementation process, a luxury we could not have enjoyed in a court that had implemented guidelines before October 1978, when our research began.

Both the Denver District Court and the Philadelphia Court of Common Pleas generously allowed us access to the official court records, from which we collected data on cases disposed of before and after the implementation of guidelines. Both courts also gave us permission to interview judges about the development, implementation, and impact of sentencing guidelines. Therefore, the Denver and Philadelphia courts became the primary focus of our study.

When we began our study, the Cook County Circuit Court appeared to offer important advantages as a research site. The Chicago guidelines had been implemented in June 1977. By February 1978, Illinois's new determinate sentencing law had rendered obsolete the original guidelines, but they were revised to accommodate the new law. The revised guidelines remain in effect at this writing. The revision of the guidelines and the effects of the new law would have complicated any attempt to measure the guidelines' impact, but they also would have provided the opportunity to study the guidelines' adaptability to a statutory sentencing structure that mandates flat sentences and allows relatively narrow ranges of judicial sentencing discretion. For that reason, we were disappointed to learn that, although we were given permission to interview judges, we could not have access to the case files from which to collect impact data.

It turned out, however, to be fortunate that we did not undertake a case-file data-collection effort in Chicago, for our interviews there convinced us that the Chicago guidelines never were fully implemented in the way they were meant to be by their originators. Nevertheless, our interviews in Chicago bore fruit. From them, we gained insight into the nature of the implementation process, as well as into the relationship between judicial sentencing discretion and the plea bargaining process.

Like the Denver and Philadelphia guidelines, the Chicago guidelines apply only to cases that are filed as felonies. The Cook County Circuit Court, which serves the city of Chicago and its suburbs within Cook County, includes a criminal division that handles felony cases, and several municipal departments that serve as magistrate courts in felony cases and have original jurisdiction over misdemeanors. Cases originating within the city of Chicago, if bound over at preliminary hearing, are filed in one of four branches of the criminal division, which is served by 41 judges.

The Chicago guidelines were adopted originally in only two of the four Chicago branches of the criminal division. After the new determinate sentencing law went into effect on February 1, 1978, the revised guidelines were made applicable to the other two Chicago branches of the criminal division. Thus, the guidelines apply only to crimes committed within the city of Chicago.

The research design and its limitations. This research is a case study of four urban courts. As the foregoing account of site selection demonstrates, our research sites were not chosen randomly from the population that comprises all courts in the United States. They were the only courts that were capable of serving our research needs. Therefore, we can make no statistical inferences about courts in general or even about particular kinds of courts. Nevertheless, there is much to learn from a case study of four courts.

For the purposes of our research, each court may be regarded as a case that has several significant characteristics. These characteristics include an incarceration rate (the number of incarcerative sentences divided by the total number of sentences), an average sentence length, an average period of time between filing of the information and sentencing, and so forth. Our investigation seeks to measure the impact of a change (or intervention) that occurred in the Denver and Philadelphia courts. In order to do so, we compare the characteristics of each court before the implementation of sentencing guidelines with its characteristics after guidelines were implemented. For some purposes we have measured the characteristics of each court at several points in time before and after the adoption of guidelines. Strictly speaking, this study employs a nonexperimental, single-group, pretest-posttest design.[62] Where measures are taken at several points in time before and after the implementation of guidelines, the design resembles that of an interrupted time series, but none of the sophisticated statistical techniques often associated with that design are employed here.[63]

Single group, pretest-posttest designs have important limitations which should be kept in mind when interpreting the research results. Cook and Campbell identify five threats to the validity of this type of research design: history, statistical regression, maturation, testing, and instrumentation.[64] Of these, history poses the greatest difficulty for the present research. The implementation of sentencing guidelines is not the only event that could have affected sentences in our research sites. For example, it is easy to imagine sentences being influenced by prison overcrowding, new policies in the prosecutor's office, legislative consideration of new sentencing laws, or growing public consternation about crime. Statistical regression in this context refers to the possibility that, because of random (stochastic) variation, the pretest scores might be substantially different from the mean, and the posttest scores, assuming no treatment effect, therefore would likely be closer to the mean. The danger is that differences attributable to random variation might be interpreted erroneously as evidence of a treatment effect. Observed differences between measures taken before and after an intervention might also be the result of maturation rather than a treatment effect. A pre-existing trend toward, say, more severe sentences might wholly account for an observed increase in the severity of sentences from the pretest to the posttest.

Testing effects are effects that are caused not by the treatment per se but by exposure to outcome measures. Insofar as this evaluation is concerned, testing effects are unlikely because of the archival nature of the research. It is quite possible, on the other hand, that the guidelines implementation project produced testing effects. As we shall see, however, there is very little evidence to suggest any effects, whether or not due to testing or treatment.

Archival research is particularly susceptible to problems of instrumentation. Archival researchers are at the mercy of past recordkeeping procedures. If, for example, a change occurred in the definitions of crimes between the pretest and the posttest, the researcher might be misled into erroneous conclusions. Although we encountered no apparent changes in recordkeeping procedures during the relevant period of time, there is a danger that such changes escaped our attention.

Multiple observations in time, often referred to as interrupted time series, allow the researcher to detect and adjust for pre-existing trends. It is thereby possible to solve problems posed by maturation and statistical regression. Interrupted time series also enable the researcher to distinguish among different kinds of effects. Over time we

62. See T. Cook and D. Campbell, Quasi-Experimentation: Design and Analysis Issues for Field Settings 99-103 (1979).

63. See, e.g., G. Glass, V. Willson, and J. Gottman, Design and Analysis of Time-Series Experiments (1975).

64. T. Cook and D. Campbell, supra note 62, at 99-103.

It turned out, however, to be fortunate that we did not undertake a case-file data-collection effort in Chicago, for our interviews there convinced us that the Chicago guidelines never were fully implemented in the way they were meant to be by their originators. Nevertheless, our interviews in Chicago bore fruit. From them, we gained insight into the nature of the implementation process, as well as into the relationship between judicial sentencing discretion and the plea bargaining process.

Like the Denver and Philadelphia guidelines, the Chicago guidelines apply only to cases that are filed as felonies. The Cook County Circuit Court, which serves the city of Chicago and its suburbs within Cook County, includes a criminal division that handles felony cases, and several municipal departments that serve as magistrate courts in felony cases and have original jurisdiction over misdemeanors. Cases originating within the city of Chicago, if bound over at preliminary hearing, are filed in one of four branches of the criminal division, which is served by 41 judges.

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may observe a change in the level (or intercept) of a measure, or we may observe a change in the trend (or slope) of a measure. Effects may be continuous (unchanging over time) or they may be discontinuous (changing in magnitude or direction). Effects also may be instantaneous or delayed.

The capabilities and limitations of the research design are considered in greater detail throughout this report as we introduce analyses and interpret our findings. The reader is forewarned, however, not to expect, in the words of Cook and Campbell, "hard-headed causal inferences" from the simple before-after and interrupted time series designs used here to evaluate the impact of sentencing guidelines.[65] Our findings are suggestive and in some instances strongly so, but they are necessarily limited.

Case record data. Chapter 2 includes descriptions and reanalyses of the case record data that were collected and used in the construction of the Denver, Chicago, and Newark sentencing guidelines by researchers at the Criminal Justice Research Center. These data, in machine-readable form, together with the codebooks, project reports, and all other available documentation of the procedures by which those guidelines were developed, were provided to us by the CJRC researchers and the National Institute of Justice.

The data on which our impact measures are based were collected anew by the National Center staff from case files in Denver and Philadelphia. These data include large amounts of information about the offense, the offender's background and criminal history, and the processing of the case. The data items are described more fully in Chapters 3 and 4, and are listed in Appendix D.

The Denver impact data include all convictions for which charges were filed in the Denver District Court between May 1, 1975, and October 31, 1978, and for which sentences were imposed by April 30, 1978. These data therefore constitute a population rather than a sample in the ordinary sense of the term. Of the 3,605 convictions in our data set, sentences were imposed before the implementation of guidelines in 1,208 cases; after guidelines, in 2,397 cases.

The Philadelphia case record data consist of a 10-percent random sample of Common Pleas criminal cases in which sentences were imposed between January 1, 1976, and March 4, 1979, and a 45-percent random sample of cases in which sentences were imposed between March 5, 1979 (the date guidelines were implemented), and August 31, 1979. The postguidelines sampling ratio is larger than the preguidelines sampling ratio in order to allow for a sufficient number of cases in which to study compliance.

Interviews. A large part of the information used in this study was obtained through personal interviews with judges, prosecutors, defense attorneys, and, to a lesser extent, court administrators, probation officials, and prisoners. Most of the interviews with judges, prosecutors, and defense attorneys were semi-structured, consisting of both closed- and open-ended questions.

65. Id. at 103.

Some, however, were only very loosely structured. In Chicago, where the respondents' experience with sentencing guidelines differed radically from that of their Denver and Philadelphia counterparts, loosely structured interviews were necessary. The small number of court administrators, probation officials, and prisoners we interviewed precluded the development of highly structured interview guides for those respondents.

No attempt was made to select interview respondents randomly. Respondents were selected on an availability basis, but the sampling was purposive to the extent that we deliberately attempted to include persons with divergent backgrounds and points of view. Table 1-1 displays the interview sample sizes.

Table 1-1
Interview Sample Sizes

Position	Denver	Philadelphia	Chicago	Newark	Total
Judges	10	22	14	2	48
Prosecutors	15	25	7	0	47
Public Defenders	11	26	7	0	44
Private Defense Attorneys	10	3	1	0	14
Prisoners	0	8	0	0	8
Probation Officials	2	2	0	0	4
Court Administrators	2	1	1	0	4
Total	50	87	30	2	169

The interviews with judges, prosecutors, and defense attorneys were supplemented by self-administered questionnaires, which were given to the respondents upon completion of the interview. These consisted entirely of multiple-choice questions. Appendix C contains more detailed information about the interview and questionnaire contents, formats, and results.

In reconstructing the research procedures by which the guidelines were developed in Chicago, Denver, and Newark, we relied upon the primary sources prepared by the Criminal Justice Research Center--the data themselves, codebooks, and project reports. We consulted with members of the CJRC research staff to obtain the clarification we considered necessary. We did not "debrief" the CJRC researchers, but relied primarily upon the documentation made available to us.

Analytical techniques. Several kinds of statistical analyses are employed in this study. Univariate, bivariate, and multivariate techniques are used. Some bivariate and (a few) multivariate analyses are done by crossclassification, while others involve regression. For some purposes structural equation (path) models are used. These techniques are described where their results are reported--in Chapters 2 through 4. Our general goal, of course, was to match the technique to the task at hand.

Not all of our findings are the product of statistical analysis. Some information is not easily quantified or even anticipated. Much of what we learned came from the perceptive comments of the persons we interviewed. Therefore, some of our methods were not scientific but

journalistic.[66] The reader will find Chapter 4 peppered with comments from persons we interviewed. Unless otherwise noted, such quotations are merely illustrative; they are not necessarily representative of our respondents' answers.

Inferences, interpretations, implications, and arguments. This is a study of a complex phenomenon by an interdisciplinary team whose members were trained in law, criminal justice, and social science. As a result of our varied backgrounds, each member of the team brought a different approach to the research. This report is a blend of those different approaches. We believe that our diversity of outlooks is one of its strengths.

In writing this report, we have adopted a strategy that borrows from the distinct traditions of social science and law. We have been cautious in making statistical inferences and interpretations from our quantitative analyses. On the other hand, once the inferences and interpretations have been made, we have been relatively unrestrained in pointing out the implications of our findings. Indeed, on a few occasions we proceed to make arguments about the matters of law and public policy that are implicated by our findings. Ours is a broad view of evaluation. In this way, we have tried to draw upon the best of two different traditions. We hope that social scientists will appreciate our policy arguments for what they are, and that our law-trained readers will observe the traditional safeguards of science.

66. The journalistic method has been used to great advantage by Professor Alschuler. See A. Alschuler, The Prosecutor's Role in Plea Bargaining, 36 U. Chi. L. Rev. 50 (1968); A. Alschuler, The Defense Attorney's Role in Plea Bargaining, 84 Yale L. J. 1179 (1975); A. Alschuler, The Trial Judge's Role in Plea Bargaining, Part I, 76 Columbia L. Rev. 1059 (1976).

Chapter 2

Modeling the Sentencing Process: An Examination of Empirically Based Sentencing Guidelines

As we saw in Chapter 1, empirically derived sentencing guidelines are a newly popular method of constraining judicial sentencing discretion.[1] This method is generally viewed as the leading alternative to legislatively specified presumptive sentences. Nonetheless it should be recognized that sentencing guidelines need not be based on empirical research into past sentencing practices. Instead, sentencing guidelines could be entirely the product of deliberate policy choices made by a public agency without any attempt to incorporate past practices.[2]

This chapter is a critique of the empirical approach to sentencing guidelines pioneered by researchers at the Criminal Justice Research Center (CJRC). Our discussion is concerned with the idea and the practice of modeling the sentencing process and the subsequent adoption of the model as a statement of public policy.

The observations offered in this chapter are based on an examination of reports of the research on which the Denver, Chicago, and Newark

1. See pp. 6-11.
2. Cf. L. Sutton, Predicting Sentences in Federal Courts: The Feasibility of a National Sentencing Policy 29-30 (Utilization of Criminal Justice Statistics Project, Analytic Report 19, U.S. Department of Justice, 1978); A. Partridge and M. Leavitt, "The Feasibility of a National Sentencing Policy": A Critique, 43(1) Fed. Probation 69 (March 1979) (a forceful if not sharp attack on the use of the methods examined in this chapter). See generally M. Frankel, Criminal Sentencing: Law Without Order 118-123 (1973); P. O'Donnell, M. Churgin, and D. Curtis, Toward a Just and Effective Sentencing System: Agenda for Legislative Reform (1977); M. Zalman, A Commission Model of Sentencing, 53 Notre Dame Law. 266 (1977); L. Wilkins, J. Kress, D. Gottfredson, J. Calpin, and A. Gelman, Sentencing Guidelines: Structuring Judicial Discretion (1978); D. Gottfredson, L. Wilkins, and P. Hoffman, Guidelines for Parole and Sentencing (1978); J. Coffee, The Repressed Issues of Sentencing: Accountability, Predictability, and Equality in the Era of the Sentencing Commission, 66 Geo. L. J. 975 (1978); R. Singer, In Favor of "Presumptive Sentences" Set by a Sentencing Commission, 1978 Crime and Delinquency 401; H. Tyler, Sentencing Guidelines: Control of Discretion in Federal Sentencing, 7 Hofstra L. Rev. 11 (1978); K. Flaxman, The Hidden Dangers of Sentencing Guidelines, 7 Hofstra L. Rev. 259 (1979); M. Tonry, The Sentencing Commission in Sentencing Reform, 7 Hofstra L. Rev. 315 (1979).

sentencing guidelines were based, and on reanalyses of the data that were used in the original Denver sentencing guidelines feasibility research and the subsequent Chicago and Newark sentencing guidelines implementation research. Although occasional references are made to the Philadelphia guidelines, we undertook no reanalysis of the data on which they were constructed. The Philadelphia guidelines were created by means of the same general methods as the Denver, Chicago, and Newark guidelines by different researchers under different auspices in a different location. Therefore, although criticisms of the general approach and methods of the CJRC research apply with equal force to the Philadelphia guidelines research, criticisms of specific research practices are limited to the original Denver, Chicago, and Newark projects.

Furthermore, we did not subject the procedures by which sentencing guidelines were developed in Chicago and Newark to the same degree of scrutiny to which we put the Denver process. Similarly, we did not attempt the full reanalysis of the original data used in constructing the Chicago and Newark guidelines that we did for Denver. After reviewing the Denver experience, we became convinced that the difficulties of the CJRC approach to guidelines development are so fundamental that further review of the details of the research implementation would not be worthwhile. We have taken pains in the discussions that follow to limit certain criticisms to the specific jurisdiction or jurisdictions in which they were disclosed by our investigations. The reader is warned not to generalize such criticisms to the other sites in the study. Our larger conceptual objections, summarized in the conclusion to this chapter, apply to the guidelines developed in all three sites.

The Use of Modeling in the Social Sciences

The sentencing guidelines of Denver, Chicago, Newark, Phoenix, and Philadelphia, as well as those of the states of Minnesota and New Jersey, are mathematical models of judicial sentencing behavior.[3] We shall refer repeatedly to models and modeling. To those who are untrained in the ways of scientists, however, modeling may seem a strange and arcane subject.

If we are to study the complex behavior of individuals and groups, we will quickly find ourselves overwhelmed by a myriad of facts unless we are prepared to filter out some and focus on others. Behavior has too many facets to allow us to pay attention to all of them at once. A model is a tool that is used by scientists to define the facts in which they are interested and to relate those facts to one another. It is an abstraction of reality.[4] Models are explicit, formal statements of systematic relationships.[5]

We can distinguish two kinds of models in the social sciences, although the exact dividing line between them is not always clear. Deductive modeling begins with axiomatic or theoretical statements from

which are derived specific predictions about behavior. Inductive models, on the other hand, rely less on axioms and theories, but derive

3. The original Denver, Newark, and Phoenix sentencing guidelines are no longer in use. The Denver and Phoenix guidelines were made obsolete by new sentencing legislation in Colorado and Arizona. The Newark guidelines were supplanted by statewide guidelines, which differ in several important respects from the guidelines reviewed in this chapter. Sentencing guidelines are now being developed in several jurisdictions, including the states of Michigan, Massachusetts, and Pennsylvania and multicounty regions within Maryland and Florida. Other states, including New York, Nebraska, Texas, and Wisconsin are in various stages of exploring the possibility.

4. E. Hanushek and J. Jackson, Statistical Methods for Social Scientists 1 (1977) [hereinafter cited as Hanushek and Jackson]. See generally C. Lave and J. March, An Introduction to Models in the Social Sciences (1975).

5. See Hanushek and Jackson at 1-2.

predictions from previous empirical observations and the researcher's informed guesses. Most models used in the social sciences, including those used to create sentencing guidelines, are of the inductive type.[6]

Models, therefore, are formal statements of systematic relationships that entail predictions about behavior. In order to determine whether the model is an accurate description of reality, the scientist undertakes empirical studies to see how well the model fits the data produced by observation. If the data do not fit the model, the scientist revises or discards the model and tries again. If the data fit the model reasonably well, the scientist has more confidence that the model accurately portrays the phenomena being studied, and he or she devises other tests to determine how generalizable the model is to other circumstances.

Models are akin to scientific theories. They are tested and evolve in a manner similar to that of theories.[7] To the extent that models and theories differ, it is that models usually are more specific and concrete, while theories are more general and abstract. But the distinction is not sharp and the terms often are used interchangeably.

Mathematical models are simply models that consist of statements in mathematical form. These are wonderfully convenient devices. Their mathematical statements can be precise and highly testable. Such models are easy to manipulate with the help of a computer. Moreover, they frequently lend themselves easily to practical applications. One prominent example is the use of linear models in decision making.[8]

The Use of Modeling in Law and Criminal Justice Reform

The use of models offers some of the same benefits to law-related research and reform that it offers to other areas of study. In the context of law-related research, models are explicit statements of systematic relationships in legal institutions. They expose assumptions, stimulate thought, invite debate, facilitate testing, and permit application. Some of the best examples of modeling legal institutions include the work of social researchers who have applied statistical models to the disposition of criminal cases,[9] and the application of

6. See id.

7. See generally T. Kuhn, The Structure of Scientific Revolutions (2d ed. 1970); K. Popper, The Logic of Scientific Discovery (1968).

8. See R. Dawes and B. Corrigan, Linear Models in Decision Making, 81 Psych. Bull. 95 (1974).

9. E.g., J. Hagan, Parameters of Criminal Prosecution: An Application of Path Analysis to a Problem of Criminal Justice, 65 J. Crim. L. and Criminology 536 (1974); P. Burke and A. Turk, Factors Affecting Postarrest Dispositions: A Model for Analysis, 22 Soc. Prob. 313 (1975); W. Rhodes, A Study of Sentencing in the Hennepin County and Ramsey County District Courts, 6 J. Legal Studies 333 (1977); L. Tiffany, Y. Avichai, and G. Peters, A Statistical Analysis of Sentencing in Federal Courts: Defendants Convicted After Trial, 1967-68, 4 J. Legal Studies 369 (1975).

principles of operations research and systems analysis to the analysis of legal structure and process.[10] The use of models based on microeconomic theory also has proved valuable in the context of legal institutions.[11]

Modeling techniques have been applied to a variety of specific areas within the legal system at a variety of organizational or systemic levels. For example, researchers have modeled the processes of pre-trial release,[12] prosecutorial charging,[13] judicial sentencing,[14] parole release,[15] and appellate decision making.[16] Models have been constructed to describe the behavior of individual decision makers as well as groups of decision makers such as those comprised by a court or all of the courts within a particular jurisdiction.

There are several ways in which the use of modeling can assist those who seek to reform legal institutions. The first, and most obvious, is that modeling can enhance our understanding of the way in which legal institutions currently operate. Our ability to assess the need for change depends upon our understanding of the status quo. In the absence of knowledge about the current state of affairs, we may fail to reform when reform is needed, attempt to reform when reform is unnecessary, or implement a change that is ill suited to the real problems. Part of the

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10. See S. Nagel and M. Neef, Legal Policy Analysis (1977) (deductive modeling used to analyze a variety of criminal justice problems in an effort to identify policies that will optimize the achievement of systemic goals); S. Nagel, Modeling the Criminal Justice System (1977).

11. See, e.g., G. Stigler, The Optimum Enforcement of Laws, 78 J. Pol. Econ. 526 (1970); I. Ehrlich, The Deterrent Effect of Criminal Law Enforcement, 1 J. Legal Studies 259 (1972). See generally R. Posner, Economic Analysis of Law (2d ed. 1977). But see A. Polinsky, Economic Analysis as a Potentially Defective Product: A Buyer's Guide to Posner's "Economic Analysis of Law", 87 Harv. L. Rev. 1655 (1974); A. Leff, Economic Analysis of Law: Some Realism About Nominalism, 60 Va. L. Rev. 451 (1974).

12. E.g., W. Landes, The Bail System: An Economic Approach, 2 J. Legal Studies 79 (1973); W. Landes, Legality and Reality: Some Evidence on Criminal Procedure, 3 J. Legal Studies 287 (1974); S. Clarke, J. Freeman, and G. Koch, Bail Risk: A Multivariate Analysis, 5 J. Legal Studies 341 (1976).

13. See B. Forst and K. Brosi, A Theoretical and Empirical Analysis of the Prosecutor, 6 J. Legal Studies 177 (1977).

14. E.g., J. Hagan, supra note 9; L. Tiffany et al., supra note 9; W. Rhodes, supra note 9; L. Wilkins et al., supra note 2; J. Gibson, Race as a Determinant of Criminal Sentences: A Methodological Critique and a Case Study, 12 L. and Soc'y Rev. 455 (1978).

15. E.g., D. Gottfredson et al., supra note 2.

16. E.g., C. Haar, J. Sawyer, and S. Cummings, Computer Power and Legal Reasoning: A Case Study of Judicial Decision Prediction in Zoning Amendment Cases, 3 A.B.A. Res. J. 651 (1977); G. Schubert, The Judicial Mind (1965); G. Schubert, The Judicial Mind Revisited (1974).

rationale behind the research upon which the sentencing guidelines were built was that we should understand judges' current practices before we attempt to change them.[17] It is difficult to argue with this proposition. Systematic study of sentencing practices obviously can help to inform future decisions.

The second way in which modeling can assist reform is by enabling reformers to predict the likely impact of proposed change. Once a suitable model of a process or system has been created, it is sometimes possible to simulate the effects of a change in one or more components of the system by varying the components of the model.[18] By substituting new values into the model, we can see how the model's predictions change. For example, a model of the sentencing process might permit us to predict the effects of a change in the law--such as mandatory imprisonment for convictions of weapons offenses--on judicial sentencing practices or prison populations. A reform group or legislature might take these predictions into account when it debates the merits of such a change. Presumably, the ability to make rational decisions is enhanced by accurate predictions of impact.

As we shall see, models can play even larger roles in the change process than those of facilitation of understanding and prediction of impact. If we are interested in making a formal statement of policy without making a deliberate change in the current practices, except perhaps to make them more uniform, we can use a model of current behavior as a normative, behavior-shaping device. In other words, we can substitute the model for deliberate policy choices, once we have chosen to emulate the past. In that sense, the model serves as a mechanism for deciding (or avoiding a decision about) what policy should be adopted. It was this novel use of modeling that was employed by the CJRC researchers who created the first guidelines for judicial sentencing.[19]

The CJRC researchers, and those who have followed suit, reasoned that it is wasteful to disregard the accumulated knowledge of experienced judges.[20] They proposed and executed the construction of inductive models from which they said implicit sentencing policies could be discovered.[21] We can exemplify this reasoning in its simplest terms as follows: if we observe that offenders who had one prior felony conviction received prison terms that were, on the average, one year longer than those given to otherwise similarly situated offenders who had no prior felony convictions, we can infer that judges have a policy of adding one year to an offender's prison term if he or she has a prior felony conviction.

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17. See L. Wilkins et al., supra note 2.

18. See A. Rutherford et al., Prison Population and Policy Choices (U. S. Department of Justice, 1977) (models used to project the impact of various sentencing policies on prison populations).

19. See L. Wilkins et al., supra note 2. But see p. 47 herein.

20. L. Wilkins et al., supra note 2, at xiii.

21. Id. at 10.

The difficulty with this reasoning is that it may assume too much. It may be, although it seems unlikely, that individual judges have no consistent policies, implicit or otherwise. It is likely, however, that different judges have different policies based on different reasoning or values. The key words are "on the average." It is possible that the average tendency is something with which every single judge would disagree, and which is without philosophical justification.

To pick an extreme example, suppose that some judges, perhaps a small minority, rely heavily on factors that pertain to offenders' ability to reform, while other judges, perhaps a majority, pay no attention to these factors because they believe that offenders should not be incarcerated for the purpose of rehabilitation. The former might choose shorter prison terms for those offenders they deemed more easily reformed, on the theory that they do not need as much time to reform, and that their exposure to the detrimental aspects of prison life should be minimized. On the average, offenders whose characteristics indicate a high degree of capacity for rehabilitation may receive slightly shorter prison terms than do those whose prospects of reform appear bleak. This slight difference in length of incarceration, however, may be correctionally insignificant. That is, the increment may make no difference in terms of the offender's progress toward rehabilitation. A judge who considers rehabilitation to be an appropriate factor in determining the length of imprisonment would have preferred a greater difference in the sentences, while another judge would have preferred no difference, but both might agree that the middle position is indefensible.

This is not to say that inductive modeling of the sentencing process cannot arrive at sensible policy positions. Our example is admittedly extreme and probably unusual. Nevertheless, it does illustrate the point that a model of the sentencing process may reveal tendencies that do not make theoretical sense, quite apart from the possibility that we will discover practices that are condemned by law, such as racially discriminatory practices. It is a long step from the descriptive statements that constitute a model to the normative statements that constitute policy.[22] We submit that it is a step that should be taken only after considerable deliberation, and that in no event should it merely be assumed that the descriptive statements of a model ought to become policy. Such a use of modeling techniques goes well beyond the purposes for which they originally were created and usually are employed.

The researchers who developed the first sentencing guidelines were not unaware of the need for deliberate policy choices rather than the uncritical adoption of a model on empirical grounds.[23] They permitted the judges in the courts for which they created guidelines to adjust the models to suit their policy preferences. Our interviews with some of those judges revealed that some changes were made at their request. A comparison of regression models reported by the CJRC researchers and the guidelines that eventually were implemented reveals several differences,

22. See L. Sutton, supra note 2, at 29-30.

23. See L. Wilkins et al., supra note 2, at 10. The problem seems not to be a failure to recognize the need for deliberate policy choices, but a failure to establish adequate processes by which they could be made.

some of which appear to have resulted from judicial modifications.[24] Nevertheless, the judges' direct impact on the development of the guidelines appears to have been limited. In an enterprise of this kind, the researchers inevitably make many decisions that have significant policy implications.[25] The range and importance of those decisions will become clear in the discussions of research methods that follow in this chapter. Of course, the judges were free to reject any guidelines with which they disagreed, but it must be questioned whether they were able to review effectively the many small decisions that were made by the researchers. Indeed, one of the researchers has since questioned publicly whether researchers should thus be put in a position to make policy.[26] Good researchers do not necessarily make good policy, and policymaking is not usually so far removed from the public view.

The researchers' domination of policymaking is not the product of any ill-motivation. The problem inheres in policymakers' limited ability and willingness to understand and evaluate mathematical models. When policymakers fail to comprehend and control technology, technologists become policymakers in the breach.

A law professor once observed that "some people do not relate to numbers." Although understanding some mathematical models probably requires less sophistication than many people imagine, it does require some degree of knowledge, patience, and perseverance. Policymakers, especially those trained in disciplines such as law to express thoughts verbally rather than quantitatively, frequently are disinclined to evaluate mathematical models in detail.

During our interviews in Chicago, Denver, and Newark, we observed a curious ambivalence about the statistical methods by which the guidelines were created. The reactions were in some ways extreme. On the one hand, judges expressed distrust of anything they were unable to understand, and many indicated that they did not understand the details of the process by which the guidelines were created. On the other hand, many unquestioningly accepted the guidelines as true representations of past practices. To those judges, "what the computer said" must be true. Not infrequently, we heard both positions espoused by the same judge. One thing is clear, however: by and large, judges did not consider themselves competent to evaluate the methods that were used, and therefore were required to take on faith much that at least arguably should have been reviewed and ultimately decided by them.

It must be observed, however, that lack of familiarity with and understanding of the guidelines models was less a problem in Philadelphia

24. For example, despite their apparently greater empirical importance than some of the variables included in the Denver guidelines, the number of offenses at conviction, the offender's employment history, the offender's educational level, and the offender's history of abusing drugs or alcohol were omitted from the final Denver model. See L. Wilkins et al., supra note 2, at 13.

25. See L. Wilkins et al., supra note 2, at 8, 22, 24.

26. S. Zimmerman, Problems of Design in Sentencing Guideline Instruments (paper presented to the Academy of Criminal Justice Sciences, March 1979).

than elsewhere. This may be attributed to the fact that the guidelines were developed by a committee of judges appointed by the President Judge and assisted by an unusually able court research staff. In a sense, the Philadelphia guidelines were more indigenous than those in Denver, Chicago, and Newark. The members of the committee appear to have felt more confident in their understanding of the modeling techniques. Whether this greater confidence was warranted cannot be ascertained, but it did not seem to be shared by judges who were not members of the committee.

It seems to us that this demonstrates a major difficulty in using modeling techniques as a mechanism for deciding public policy. Although perhaps some of it can be eliminated by clearer and more careful explanation, it seems unlikely that judges, who are unusually well educated as a group, will achieve sufficient understanding without special training and experience. This may be taken as an argument in favor of special commissions, perhaps like those in Minnesota[27] and Pennsylvania,[28] whose members can acquire the expertise necessary to intelligently evaluate proposed models.[29] In any event, we cannot overemphasize the importance of knowing and intelligent choices made by officials who have a public trust. Modeling, when used as a mechanism of deciding policy, is a technology that, not unlike other technologies, poses certain dangers if not understood and controlled by the public.

The last way in which modeling can assist reform efforts in legal institutions is as a vehicle for implementing change. In this case, sentencing guidelines took the form of a mathematical model. In a concrete sense, the model was handed to judges who were then asked to generate and consider recommended sentences before imposing actual sentences. The model consisted of a series of grids and a computation sheet, also referred to as a worksheet. Judges were requested to calculate an offender score based on a number of factors relating to the offender's background; calculate an "offense score" based upon the circumstances of the offense; select the appropriate grid according to the statutory classification or the generic type of the offense, (e.g., violent, property, drugs); locate the cell of the grid at the intersection of the offense and offender scores; and record the recommended sentence displayed in the cell.[30] These relatively simple manipulations of a model caused

27. See 1978 Minn. Laws ch. 723.

28. See 18 Pa. Cons. Stat. Ann. §1381-86 (1979-80 Supp.).

29. This would require substantial amounts of time, extraordinarily good communication between policymaking officials and researchers, and a strong commitment to achieve a thorough understanding of the methods used and the possible interpretations of results. Occasional meetings will not suffice for this purpose. Moreover, it is crucial that the researchers be able and willing to impart their knowledge freely and with great clarity and precision.

30. This function was performed by the Probation Department in Denver.

difficulty for some judges. High error rates were observed in Philadelphia.[31]

The objections to the use of models as vehicles for implementing change do not seem to be as strong as the arguments against their use as a mechanism of deciding policy. Sentencing is an inherently quantitative activity. The development of meaningful standards for sentencing necessarily entails quantitative statements, which can usefully be assembled in a model. This line of reasoning suggests the creation of mathematical sentencing guidelines by a public, deliberative body, informed by but not wedded to (by conscious intent or ignorance) empirical models of past sentencing practices.

We believe that modeling techniques have a place in sentencing reform. As in other areas of study they are valuable aids to understanding current practices and predicting future impacts. We hesitate, however, to conclude on a priori grounds that mathematical models are an appropriate mechanism for deciding questions of public policy. Once policy has been decided, they may be useful vehicles for effecting policy decisions, provided appropriate safeguards keep their use relatively error free.

In this section we have concentrated on the idea of modeling and its applicability to legal reform, and we shall now consider more specific issues pertaining to modeling as a technique for understanding and changing the sentencing process. Technical questions cannot be avoided. Their implications for public policy are inescapable and sometimes profound. The quantitatively unsophisticated reader may regard the balance of this chapter as a survey of the issues and problems of empirically based sentencing guidelines, an introduction to modeling in the context of sentencing reform, and a review of some current knowledge about sentencing practices. If he or she finishes the chapter having skimmed the more technical discussions but with a greater appreciation of the complexities and uncertainties of

31. Estimates of the percentage of cases in which an error was made in the calculation of a guideline sentence vary from 10 to 25 in Philadelphia. See p. 93. In Denver, where one member of the probation staff was responsible for computing all guideline sentences, errors were infrequent. But see p. 173. Another possible solution to the problem of errors in the computation of guideline sentences is computerization. A program could be written so that a clerk could enter the relevant facts of a particular case and the computer would respond with the guideline sentence. One advantage of computerization is that the computations, though not necessarily the factual information on which they are based, would be accurate. Another is that more complex models could be accommodated. The principal disadvantage, aside from the inevitable abhorrence of "sentencing by computer" that many judges would feel, is that computerization of sentencing guidelines would make them even more of a black box than they are already. That is, the underlying policy choices would become even less susceptible to judicial control and public scrutiny.

empirically based sentencing guidelines, our purpose will have been served. As to those readers who are acquainted with but not expert in quantitative methods, and who may at some point consider undertaking research and reform similar to that being evaluated, we hope that the remainder of this chapter will serve as a cautionary warning and a guide to some of the issues that will or should be encountered. Finally, the ensuing sections are intended to share with those readers who are expert in quantitative methods some of the analytical problems encountered in sentencing guidelines research in the hope of enlisting their expertise in an effort to find solutions.

Specification of a Model

The most fundamental aspect of modeling is the specification of a model. Model specification refers to the delineation of variables and the relationships among them.[32] There is no single correct way to specify a model. Specification depends upon the nature of the subject matter, the purpose of the model, the results of previous empirical studies, and the researcher's informed guesses.[33] Errors of specification manifest themselves as imperfect fits between models and data. Specification errors, however, are the rule rather than the exception in the social sciences, where theories usually are not sufficiently developed to permit complete specification.[34] As we shall see, some specification errors are more costly than others.

The unit of analysis. The unit of analysis is the object of study, the things to be measured and modeled. Reference here is not to the topic of research but to the entities about which the research questions are asked. The typical unit of analysis is the individual. It may, however, be a group, an organization, a nation, or even the entire planet. Units of analysis also can be social artifacts: buildings, books, crimes, judicial decisions. As the unit of analysis becomes larger, the number of units in the relevant population decreases dramatically. It is possible to measure behavior at one level and aggregate the measurements at another level. For example, individuals can be surveyed within different organizations, with the responses subsequently aggregated by organization. In that case, the organization would become the unit of analysis.

Like other research, sentencing research can be conducted at various levels of aggregation. The criminal charge, the criminal case, the offender, the judge, the court, or the jurisdiction could serve as the unit of analysis. The CJRC researchers chose the case as their unit of analysis.[35] Their data pertain to various aspects of the case: the circumstances of the offense, the background of the offender, and the sentence. The research questions were framed in terms of what happens to

32. See Hanushek and Jackson, at 80.
33. Id.
34. Id.
35. L. Wilkins et al., supra note 2, at 10.

cases. The judge would have been another logical choice for a unit of analysis, in which case the research questions would have been framed in terms of the behavior of the judge.

The choice of a unit of analysis is pregnant with consequences, whether it is made by researchers or imposed by judges. A model may exist at one level of aggregation while the real research questions exist at another level. We have alluded to a problem caused by the unit of analysis used by the researchers.[36] After building a model that described what happened in individual cases, the characteristics (functional forms and parameters) of the model were ascribed to the court as a whole.[37] But as we saw earlier, it is quite possible that the model does not adequately represent the decisions of any individual judge, and that the resulting mathematical statements are indefensible on philosophical or legal grounds. In other words, the average tendencies of all judges combined may be meaningless or undesirable, despite the fact that some or all of the judges individually may follow sensible, consistent patterns.

There is another way to view the problem. Many statistical analyses, including those used to create the guidelines, assume that observations are made independently of each other. Two events are statistically independent of each other if the occurrence of one does not affect the probability of the other.[38] Violations of the assumption of independent observations may lead to erroneous conclusions. If some observations are clustered with others, perhaps because they emanate from the same source, they should not be entered indiscriminately into a data set with observations deriving from other sources.

It is reasonable to think that decisions made by one judge bear more similarity to each other than to the decisions of another judge. If we mix together the decisions of both judges and treat them as if they have no clustering tendency, we run the risk of violating the assumption of independence. Between-cluster and within-cluster variation is not distinguished. Unknown distortions may occur. At the least, the between-judge variations will be relegated to the error term in the

36. See pp. 25-26.
37. In this sense, the data were aggregated and the court became the unit of analysis. Since the researchers sought to make no statistical inferences from a sample of courts to courts in general, the research amounted to case studies of the individual courts. The problem is that analyses performed at one level of analysis may yield conclusions that do not apply at another level. This is known as the ecological fallacy. See H. Alker, A Typology of Ecological Fallacies, in H. Alker, Quantitative Ecological Analysis in the Social Sciences (1969); W. Robinson, Ecological Correlations and the Behavior of Individuals, 15 Amer. Soc. Rev. 351 (1950) H. Blalock, Causal Inferences in Nonexperimental Research 97-114 (1964). But see Hanushek and Jackson, at 84-86.
38. See H. Blalock, Social Statistics 124-129 (Rev. 2d ed. 1979); P. Hoel, Introduction to Mathematical Statistics 15 (4th Ed. 1971).

model.[39] The result will be a model that has relatively little explanatory or predictive ability as measured by the squared multiple correlation coefficient (R^2).[40] We would have little confidence in such a model. At the worst, the model will be a misleading or incorrect representation of reality.

The problem can be solved in part by including in the model information about which judge decided each case. This can be done by using so-called dummy variables.[41] This was not done by the CJRC researchers, who did not collect information about judge identity.[42]

The complete solution, however, amounts to treating the judge as the unit of analysis. In order to do so, a model would be estimated for each judge. The parameters of the judge-specific models would be compared. If the parameters were sufficiently similar from one judge to another, or if the differences resided primarily in the intercept constants (indicating that judges vary in their overall severity but consider the same factors with roughly equal weight),[43] then the data could be aggregated sensibly. If, on the other hand, the parameters differ in ways that indicate that different judges consider different factors, it

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39. The error term is the residual variation in the dependent variable (i.e., sentence) which cannot be accounted for by the variables in the model. See Hanushek and Jackson, at 25, 27, 110; J. Johnston, Econometric Methods 10-11 (2d ed. 1972).

40. The multiple correlation coefficient (R) is the correlation between the actual values of the dependent variable and the predicted values yielded by a multiple regression equation. The squared multiple correlation coefficient (R^2) is defined as the proportion of variance in the dependent variable that is explained by the regression model. Therefore, by definition, the larger the error term the lower the squared multiple correlation coefficient.

41. The use of dummy variables in regression analysis is discussed below. See p. 71. For now we note only that the term is not a slander of judges but a statistical term.

42. See L. Wilkins et al., supra note 2, at Appendix B, pp. 39-44.

43. Regression models take the following form:

$$Y = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \ldots \beta_K X_K + U$$

where Y is the dependent variable (sentence); X_1, X_2, . . . X_K are the independent or predictor variables (e.g., offense seriousness, adult criminal record); β_0 is the intercept constant; β_1, β_2, . . . β_K are the regression coefficients which indicate the relationship between each independent variable and the dependent variable when all other variables are held constant by mathematical adjustment; and U is the error (residual) term, which includes all influences on Y other than those contained in the rest of the model (e.g., excluded variables and other specification errors, measurement errors, and stochastic (random) behavior). See Hanushek and Jackson, at 25-27. The regression coefficients (β) are measures of the unique effects of the individual independent variables on the dependent variable.

would make little sense to combine all decisions in an aggregate model of decision making at the level of the court. In that case, it would be more appropriate to present the judges with the judge-specific models so that they can resolve their differences by individual reflection and group deliberation. Unless they resolve their differences, a single set of empirically based guidelines would be a fiction.

Using data collected for our study of the impact of the Denver sentencing guidelines,[44] we conducted an analysis to test the proposition that judge-specific models might differ from the aggregate model. The procedure was as follows. We selected the seven judges who had imposed the greatest number of sentences within our data collection period.[45] Their cases were aggregated (n = 1,822) in one regression analysis in which 13 predictor variables were used. The dependent variable for all analyses was the decision whether to incarcerate, coded dichotomously (0 = nonincarceration; 1 = incarceration).[46]

After regressing sentence on the predictor variables for the aggregate sample, we performed the same regression separately on the cases decided by each of the seven judges. The results are summarized in Table 2-1. They should be read as follows. In order to compare the importance of a particular variable across judges, read the unstandardized regression coefficients (the first number in each cell of the table) across the page.[47] In order to compare the relative

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44. For a description of the data, see pp. 18. Briefly, they include all convictions in the Denver District Court for which informations were filed between May 1975 and October 1978 and sentences were imposed by April 1979. Thus, both pre- and postguidelines cases are included. With respect to the homogeneity of the pre- and postguidelines samples, see pp. 129-133.

45. This was done to assure a minimum of 150 cases per judge. The fewer cases per variable, the less reliable the results. See F. Kerlinger and E. Pedhazur, Multiple Regression in Behavioral Research 446-448 (1973). Of course, since the seven judges were selected purposively rather than randomly, we cannot assume that they are representative of all judges who served on the criminal bench during that period of time. In fact, there is reason to suspect that they might differ from their less prolific colleagues in some important ways (e.g., tenure on the bench, propensity to induce guilty pleas by participation in plea bargaining or differential sentencing on the basis of method of conviction). Nevertheless, these judges did dispose of a large proportion of the District Court's caseload and, in any event, our purpose is simply to demonstrate a potential difficulty and to present a method by which to test for it.

46. For a discussion of dichotomous dependent variables in regression analysis, see pp. 77-78.

47. It is inappropriate to compare standardized regression coefficients from one sample (judge) to another because they are sample specific. Standardized regression coefficients are determined partly by the sample variances of the predictor variables. See Hanushek and Jackson, at 78.

importance of different variables to a particular judge, read the standardized regression coefficients (the second number in each cell of the table) down the page.[48] The third number in each cell is the significance (probability) level. A high significance level indicates that the estimate is not reliably greater than zero.[49] The unstandardized regression coefficients are graphed in Figure 2-1 in order to facilitate visual inspection of their spread.

The individual regressions show substantial differences between judges for some variables. For example, employment status appears to be far more important to Judge G ($b = -0.186$; $p \leq .001$) than it is to Judge C ($b = -0.054$; $p = 0.412$). This coefficient can be interpreted as the difference in terms of the probability of incarceration that employment status makes when all other variables in the regression equation are held constant. In other words, for offenders sentenced by Judge G, those who were unemployed had a probability of receiving a prison sentence that was 18.6 percent greater than did those who were employed at least part time. For Judge C, employment status made a much smaller difference (5.4 percent), which may be regarded as the product of chance variation.

The importance of the type of victim[50] also appears to vary from judge to judge, although the coefficients must be regarded as unstable because of their high probability levels. The offender's race appears to have been a factor only for Judge G. On the other hand, whether the victim was harmed or put in jeopardy of harm seems to be uniformly unimportant to the decision whether to incarcerate.[51] (Injury to the victim, by the way, is a variable in the Denver sentencing guidelines.)

The results discussed above and summarized in Table 2-1 suggest that different judges consider different factors with different weights when they sentence. For that reason, it may be statistically and logically inappropriate to pool all judges' cases together if we wish to discover implicit sentencing policy. The point is that there is no single policy to be discovered. A pooled regression may turn up nothing more than meaningless averages of a variety of policies. Employment status, for example, appears to be important to one judge but irrelevant to another. While either position may be justified, the middle position is not necessarily acceptable.

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48. Where, as here, the predictor variables are measured in different units, it is difficult to compare two or more unstandardized coefficients in the same equation in order to assess their relative importance. Either one can make the necessary mental adjustments or one can use the standardized coefficients, which express relationships in unit-free terms (i.e., in terms of standard deviations).

49. Because of the large number of parameters being estimated, we recommend that no parameter having a probability level greater than .010 be considered reliably greater than zero.

50. Type of victim is a dichotomous variable which is coded as a dummy (0 = business or no victim; 1 = private person).

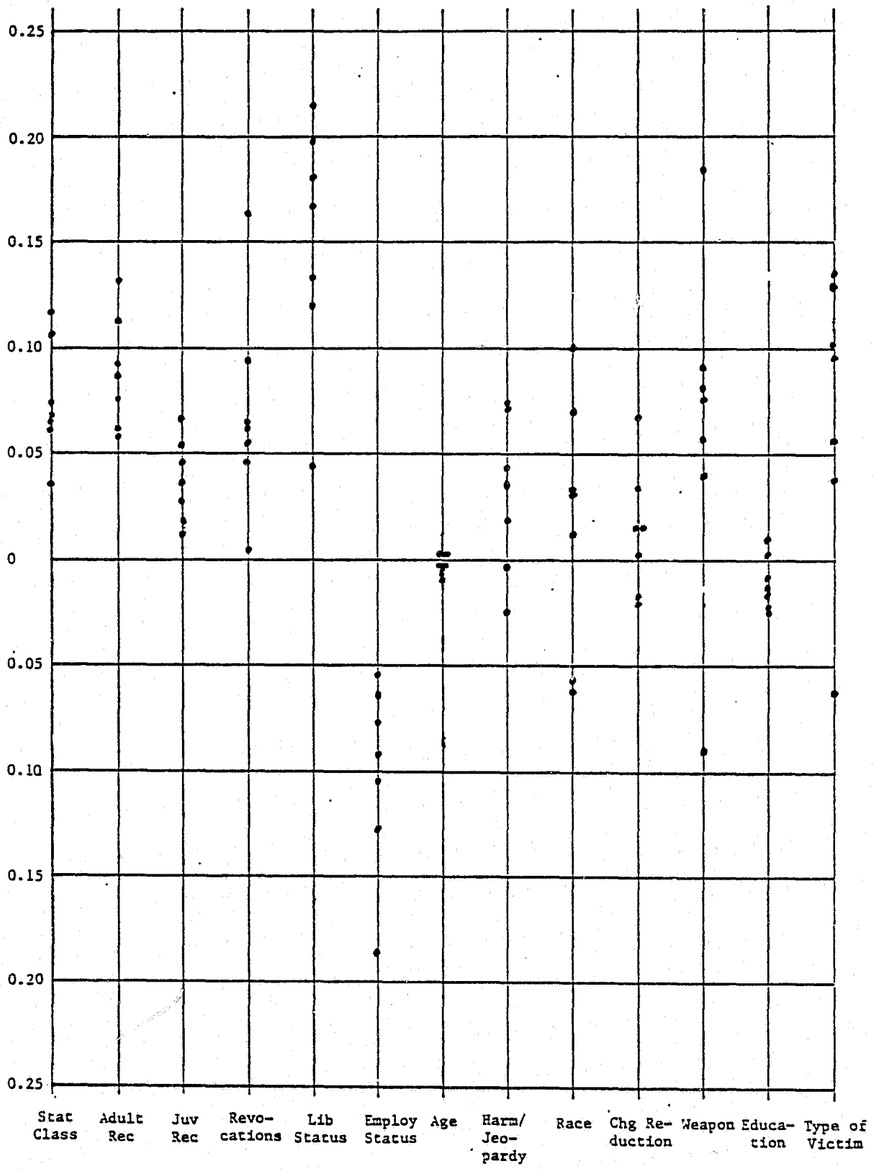
51. But see pp. 72-76.

Table 2-1
Demonstration of Separate Regression Models for Individual Judges
Denver

Predictor Variables	Judges A	B	C	D	E	F	G	AGGREGATE
STATUTORY CLASS	0.108*	0.068	0.064	0.036	0.073	0.118	0.065	0.072
	0.334	0.243	0.191	0.110	0.249	0.406	0.218	0.236
	0.001	0.038	0.080	0.274	0.003	0.001	0.043	0.001
ADULT RECORD	0.092	0.087	0.131	0.112	0.064	0.058	0.076	0.087
	0.215	0.218	0.295	0.274	0.159	0.140	0.193	0.211
	0.003	0.001	0.001	0.001	0.007	0.045	0.004	0.001
JUVENILE RECORD	0.011	0.054	0.028	0.046	0.068	0.036	0.019	0.037
	0.029	0.148	0.070	0.123	0.188	0.100	0.054	0.099
	0.631	0.014	0.350	0.038	0.001	0.117	0.356	0.001
REVOCATIONS	0.055	0.065	0.163	0.095	0.046	0.005	0.064	0.058
	0.092	0.104	0.225	0.155	0.073	0.008	0.118	0.096
	0.145	0.089	0.004	0.016	0.181	0.903	0.059	0.001
LIBERTY STATUS	0.199	0.045	0.133	0.120	0.168	0.180	0.214	0.154
	0.193	0.045	0.122	0.119	0.174	0.191	0.221	0.155
	0.001	0.456	0.072	0.030	0.001	0.002	0.001	0.001
EMPLOYMENT STATUS	-0.063	-0.078	-0.054	-0.129	-0.104	-0.092	-0.186	-0.115
	-0.064	-0.080	-0.055	-0.130	-0.111	-0.096	-0.197	-0.119
	0.250	0.189	0.412	0.026	0.028	0.095	0.001	0.001
AGE	-0.008	-0.002	-0.009	-0.002	0.001	0.001	-0.004	-0.003
	-0.133	-0.028	-0.134	-0.037	0.026	0.012	-0.082	-0.055
	0.039	0.656	0.084	0.534	0.658	0.853	0.199	0.020
HARM/JEOPARDY	-0.001	0.042	0.074	0.037	-0.024	0.071	0.019	0.028
	-0.002	0.066	0.120	0.068	-0.044	0.119	0.035	0.049
	0.982	0.366	0.192	0.404	0.535	0.098	0.621	0.076
RACE	0.070	0.033	0.107	0.031	-0.061	-0.057	0.130	0.026
	0.069	0.031	0.102	0.029	-0.062	-0.060	0.135	0.026
	0.202	0.585	0.117	0.597	0.214	0.301	0.010	0.212
CHARGE REDUCTION	0.034	-0.017	-0.020	0.002	0.016	0.069	0.016	0.015
	0.093	-0.052	-0.052	0.004	0.047	0.211	0.044	0.043
	0.312	0.640	0.624	0.965	0.553	0.066	0.676	0.245
WEAPON	0.076	0.040	-0.090	0.184	0.081	0.091	0.058	0.066
	0.069	0.038	-0.079	0.169	0.079	0.080	0.052	0.061
	0.347	0.594	0.403	0.039	0.244	0.238	0.453	0.027
EDUCATION	-0.012	-0.021	-0.022	-0.009	0.003	-0.013	0.010	-0.008
	-0.048	-0.098	-0.095	-0.036	0.012	-0.063	0.044	-0.035
	0.392	0.095	0.175	0.527	0.822	0.283	0.422	0.099
TYPE OF VICTIM	0.135	0.097	0.057	0.104	0.130	0.039	-0.061	0.095
	0.097	0.070	0.045	0.069	0.094	0.024	-0.036	0.066
	0.094	0.242	0.537	0.234	0.062	0.684	0.512	0.002
R^2	0.288	0.394	0.366	0.373	0.259	0.245	0.357	0.283
NUMBER OF CASES	279	233	171	251	347	272	269	1822
F	8.22	10.96	6.98	10.82	8.94	6.44	10.89	54.82
PROBABILITY	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001

*The first number listed opposite each predictor variable is the unstandardized regression coefficient. The second number listed is the standardized regression coefficient. The third number listed is the significance level.

Figure 2-1
Plot of Unstandardized Regression Coefficients
Separate Judge Regressions
Denver



Inasmuch as we suspect that cases decided by different judges represent different statistical populations (i.e., judges' individual regression equations are different), before aggregating them we should test the hypothesis that they constitute a heterogeneous population. This can be done by a test for heterogeneity of regression, also known as the Chow test.[52] The reader should note that we do not seek to compare individual regression coefficients, but rather sets (vectors) of regression coefficients. That is, we want to compare all of the regression coefficients for each judge simultaneously with those of the other judges. Therefore, the usual t-tests for individual coefficients are inappropriate.[53]

The easiest way to conduct the test for heterogeneity of regression is to include the predictor variables in a single regression equation with intercept and slope dummy variables for each judge except one.[54] Thus, the total number of terms on the right-hand side of the equation (predictor variables plus intercept constants) is given by $M(K + 1)$, where M is the number of judges and K is the number of original predictor variables. The residual (error) sum of squares from this equation can then be compared to the residual sum of squares from the equation having no judge-dummy variables, after adjusting for the degrees of freedom. If the difference is significant, we can conclude that the groups of cases decided by different judges represent different populations for purposes of the regression, and therefore should not be pooled.

If there is a significant difference between the equation with the judge-dummies (unrestricted coefficients) and the equation without them (restricted), we may wish to determine whether the difference is attributable to different intercepts (differences in overall sentence severity among judges), or different slopes (differences in the weighting of factors), or both. Our reasoning suggests that a difference in slopes would be more problematic than a difference in intercepts. In other words, if judges weight various factors equally but differ in their overall propensity to incarcerate, taking an average propensity to incarcerate is defensible. On the other hand, if judges weight different factors differently, as evidenced by significantly different regression slopes, taking average weights for each factor may produce meaningless results.

In order to determine whether a difference between the restricted and unrestricted models is attributable to differences in slopes or intercepts, a third equation is needed. This equation should include the original predictor variables and intercept dummy variables for each judge

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52. See Hanushek and Jackson, at 124-129; D. Huang, Regression and Econometric Methods 103-113; N. Timm, Multivariate Analysis with Applications in Education and Psychology 331-340 (1975); G. Chow, Tests of Equality Between Sets of Coefficients in Two Linear Regressions, 28 Econometrica 591 (1960); J. Johnston, Econometric Methods 192-207 (2d Ed. 1972).

53. Hanushek and Jackson, at 125.

54. Id. at 128; N. Timm, supra note 52, at 331-333.

except one, but no slope dummy variables. The difference between the residual sum of squares from this equation and that from the restricted equation can be compared with the residual sum of squares from the unrestricted equation, once the adjustment is made for degrees of freedom. This comparison is a test of the difference in intercepts. The difference between the residual sum of squares from the third equation and that in the unrestricted equation can be compared with the residual sum of squares in the unrestricted equation, again adjusting for degrees of freedom, in order to test the difference in slopes.

The test of heterogeneity of regression was performed on the regressions reported above. The results are summarized in Table 2-2.

Table 2-2
Test of Heterogeneity of Regression
Judge-specific Regressions
Denver

Source	Degrees of Freedom	Sum of Squares	Mean Square	F	F (.25)
Coincidence	84	19.437762	0.231402	1.394	1.11
Intercepts	6	5.513845	0.918974	5.534	1.31
Parallelism	78	13.923917	0.178512	1.075	1.10
Residual (Separate)	1,724	286.279410	0.166055		
Residual (Pooled)	1,808	305.717172			

Number of cases = 1,822
Number of groups = 7
Number of predictors = 13

Before interpreting the results, we must consider the nature of the test. In the usual situation, we use significance tests because we are looking for differences and therefore need to insure against erroneous conclusions that differences exist when they actually do not (i.e., Type I errors). In this case, however, we seek to pool the data into a single regression for the sake of efficiency and parsimony. We therefore are looking for the absence of differences. In order to guard against the potentially costly error of erroneously concluding that there are no differences when in fact there are differences (i.e., a Type II error), we must use a higher probability, such as $p < .25$, as our confidence level. This serves to increase the power of the test by reducing the likelihood of Type II errors.[55]

From Table 2-2 we can see that the difference between the restricted and unrestricted models is significant at $p < .25$; in fact, it would be significant at $p < .10$. Thus, it appears that the groups of cases decided by different judges represent distinct populations. The

55. See Hanushek and Jackson, at 129 (recommending a confidence level of $p < .25$ in this kind of situation for similar but not identical reasons).

difference in the intercepts is significant at $p < .001$, whereas the difference in slopes does not quite reach significance at $p < .25$. Most of the differences seem to be attributable to the fact that judges differ in their overall severity rather than the weights they give to the thirteen variables. Nevertheless, this must be considered a borderline case. Pooling is not clearly appropriate, but neither is it clearly inappropriate.

A similar analysis was performed using our Philadelphia sample.[56] The six judges who disposed of the most cases in our sample were selected for judge-specific regressions.[57] The results of the test of heterogeneity of regression are reported in Table 2-3 below. (See Appendix B, Table B-1, for detailed results of the individual, judge-specific regressions.)

Table 2-3
Test of Heterogeneity of Regression
Judge-specific Regressions
Philadelphia

Source	Degrees of Freedom	Sum of Squares	Mean Square	F	Probability
Coincidence	40	13.3536	0.3338	2.2306	0.001
Intercepts	5	6.6733	1.3347	8.9192	0.001
Parallelism	35	6.6802	0.1909	1.2757	0.136
Residual (Separate regressions)	627	93.9758	0.1496		
Residual (Pooled regressions)	668	107.3294			

Number of cases = 676
Number of groups = 6
Number of predictors = 7

56. For a description of the Philadelphia case data, see p. 18. Briefly, they comprise a 10-percent random sample of convictions for which sentences were imposed between January 1, 1976, and March 4, 1979, and a 45-percent random sample of convictions for which sentences were imposed between March 5, 1979, and August 31, 1979. Note that this analysis includes both pre- and postguidelines cases. With respect to the homogeneity of regression between pre- and postguidelines cases, see pp. 133-137.

57. Again, the six judges were selected purposively rather than randomly, so no statistical inferences can be made about the general population. See note 45 supra.

It is clear from Table 2-3 that the cases disposed of by individual judges constitute distinct populations; they are heterogeneous with respect to the regression. The test of coincidence shows that the individual regression lines are significantly different from each other ($p < .001$). Moreover, the test of parallelism reveals that there are significant differences in the slopes of the regression lines ($p = .136$), indicating that judges consider different factors with different weights in arriving at a decision whether to incarcerate. Therefore, it would seem to be inappropriate to aggregate the cases decided by these six judges into a single regression model on which to base sentencing guidelines.[58]

These results strongly suggest a need to test for heterogeneity of regression before constructing aggregate regression models. Where it is found that the cases decided by different judges do not constitute a homogeneous population, some other approach must be used. One solution is to construct individual, judge-specific models for presentation to those who are ultimately responsible for making sentencing policy. The availability of judge-specific models should help to identify the variety of sentencing policies apparently followed by different judges, as well as to stimulate debate and perhaps eventually consensus. The disadvantage of this approach is that it requires more data, and is therefore more costly, than a single regression model. The use of simulated cases in which judges are asked to impose hypothetical sentences is another alternative.[59]

The inclusion and exclusion of variables. The selection of variables to make up a model is by no means a case of more is better. When creating models, scientists strive for parsimony as well as accuracy.[60] The goal is generally to explain as much variation in the dependent variable (in this case, sentences) as possible using as few predictors as possible. Although the logic that values parsimony may not apply with equal force to models created for the purpose of deciding policy,[61] there are practical limitations on the number of variables that can be included in the model. Too many variables reduce the comprehensibility and usefulness of a model. Moreover, it is usually the case that the marginal increments in the explanatory power of a model, as measured by R^2, begin to diminish at some point as variables are added to the

58. This finding does not necessarily imply that the sample of cases upon which the guidelines were constructed was heterogeneous with respect to the regression reported here. The reader should note that our sample encompasses a different period of time than did the guidelines construction sample, which was drawn from cases disposed of in 1975.

59. See p. 51.

60. See F. Kerlinger and E. Pedhazur, supra note 45, at 285.

61. Policy decisions frequently are supposed to be the product of many, often competing, considerations. Where this is the case, it makes no sense to create a model that oversimplifies the decision process.

model.[62] Eventually the addition of another variable adds only a trivial amount to the variance explained.

The addition of variables also reduces confidence in the estimates of model parameters by increasing their standard error.[63] Coefficients estimated on the basis of relatively few cases per variable in the model are unstable (i.e., subject to wide fluctuations on the basis of chance).[64] To put it another way, overspecified models tend to capitalize on random variation. This is seen when the model is applied to another sample of cases with the result that the explanatory power (R^2) of the model is lower than it was using the original data set. This phenomenon is called "shrinkage."[65]

There is another argument, which was stressed by the guidelines' originators,[66] in favor of limiting the number of variables in the model. Some research suggests that human decision makers consider relatively few items of information.[67] Although most people think they

62. This can be seen readily by examining the results of stepwise multiple regression analysis; the increment in variance explained (R^2) usually diminishes rapidly as the number of variables in the model grows large. See generally J. Neter and W. Wasserman, Applied Linear Statistical Models 382-386 (1974); J. Cohen and P. Cohen, Applied Multiple Regression/Correlation Analysis for the Social Sciences 102-104 (1975); F. Kerlinger and E. Pedhazur, supra note 45, at 290-295; R. Darlington, Multiple Regression in Psychological Research and Practice, 69 Psych. Bull. 161, 178 (1968). When the R^2 statistic is adjusted for shrinkage, it can be seen to decrease as variables are entered in stepwise fashion. See generally J. Cohen and P. Cohen, supra, at 106-108; J. Nunnally, Psychometric Theory 163-165 (1967).

63. J. Cohen and P. Cohen, supra note 62, at 111.

64. Hanushek and Jackson, at 60-65.

65. See J. Cohen and P. Cohen, supra note 62, at 106-107; F. Kerlinger and E. Pedhazur, supra note 45, at 282-284.

66. See D. Gottfredson et al., supra note 2, at 95-103; L. Wilkins et al., supra note 2, at 8.

67. See D. Gottfredson et al., supra note 2, at 95-103; J. Carroll and J. Payne, The Psychology of the Parole Decision Process: A Joint Application of Attribution Theory and Information Processing Psychology, in J. Carroll and J. Payne, Cognition and Social Behavior (1976). See also L. Wilkins, Evaluation of Penal Measures 138 (1969). The CJRC researchers found that after a small number of variables were included in the regression models, the inclusion of additional variables did not significantly augment the models' explanatory power (R^2). The CJRC researchers concluded that judges consider only a few items of information when they make sentencing decisions. L. Wilkins et al., supra note 2, at xvii, 12, 26. Although it is true that the incremental explanatory power (R^2) of linear regression models of judicial sentencing behavior decreases rapidly after the first few variables are entered, it does not follow that sentences are determined by only a few items of information. That conclusion would be supported (though not conclusively proven) by a finding that a few variables account for nearly all of the variance in sentencing decisions, but the CJRC models, as well as our own and those of other researchers, generally account for less than half of the variance in sentences. See pp. 51-52. The low explanatory power of the regression models suggests instead that the failure of additional variables to produce significant increments in R^2 should be attributed not to judges' use of extremely limited information, but to the inadequacy of linear regression models as explanations of judicial sentencing behavior. See pp. 52a, 56-61.

employ large quantities of information in complex ways, some scientists have theorized that we actually pare the information down to a manageable quantity before deciding.[68] On the basis of this theory, supported by several studies, the CJRC researchers concluded that it was necessary and appropriate to include relatively few variables in the guidelines model.

The operational aspects of sentencing guidelines provide another reason to limit the number of variables in a guideline model. A model containing too many variables is cumbersome to use. The risk of miscalculating a guideline sentence becomes greater as the model becomes more complex.[69]

Limitations on the number of variables included in a model and on the resources available for data collection require choices about what information is to be gathered and used. Variables generally are selected on the basis of theory, previous empirical research, and the researcher's hunches about what is important. In sentencing research, part of the relevant theory is legal. For example, a reading of the criminal code suggests that the type of crime, the use of a weapon, and prior criminal record are relevant variables. Previous empirical research confirms this.[70] Local practices also may point to relevant variables. For example, if, as in the Philadelphia Court of Common Pleas, cases are assigned to different groups of judges depending upon the apparent seriousness of the case or the defendant's willingness to waive trial rights, we may want to use the group of judges that the case is assigned to or the method of conviction as a variable in a model of the sentencing process.[71]

The criteria for selecting variables enumerated above involve a priori choices; that is, choices that are made on the basis of knowledge acquired before the contemplated analysis rather than as a result of it. Choices can also be made post hoc on the basis of observed relationships in the data. This is usually done when theory is not sufficiently developed to permit a priori selection of variables. The approach used by the CJRC researchers was that of post hoc selection.[72] The current state of knowledge about sentencing makes a priori specification difficult and risky.

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68. In a sense, legal education entails learning a process by which information can be pared down in a logical, orderly fashion. Thus it might be expected that law-trained persons consider fewer items of information than does the general population. This speculation goes well beyond the research to date. Moreover, it is easy to imagine the opposite result: that law-trained decision makers, operating in a legal environment, tend to consider more items of information than does the general population. The point is that research findings about one kind of decision or decision maker may not be generalizable to other decisions or decision makers.
69. See note 31 and accompanying text supra.
70. See generally the works cited in note 9 supra.
71. See pp. 189-196.
72. L. Wilkins et al., supra note 2, at 10-11.

In all three sites--Chicago, Denver, and Newark--the CJRC researchers began by examining simple bivariate relationships using crosstabulations and Pearson correlations.[73] Predictor variables that were correlated with sentences to a specified degree[74] were considered further; variables that did not meet the criteria were dropped from consideration, except for some that were retained for unspecified theoretical reasons.

The danger of using bivariate relationships as a threshold criterion for inclusion of variables in a model is that important variables may be excluded because their true relationships to the dependent variable (sentence) are disguised by other variables. It may be, for example, that two variables appear to have no or little relationship to each other when in fact they would show a strong relationship if another variable were controlled. In that case, the control variable is called a "suppressor."[75] That is, it has suppressed the true relationship between the two variables of interest by concealing its strength either partly or entirely. Two variables may even appear to have a relationship that is opposite to their true relationship because of the effects of a third variable. In this case, the third variable is sometimes called a "distorter."[76] The distorter variable may cause a researcher to exclude a potentially useful predictor variable because it seems to be nonsensically related to the dependent variable. In either case, the result may be that useful or important variables are excluded from a model because of the influence of other variables.

Exclusion of potentially useful predictor variables can be avoided by examining their relationships to the dependent variable after potential suppressor and distorter variables have been controlled. This can be done most easily by examining partial correlations.[77] Of course, this requires more work on the part of the researcher. It can become tedious if the number of variables is large. Nonetheless, the price of not examining partial correlations is the serious risk of overlooking important relationships and consequently misspecifying the model.

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73. J. Calpin et al., The Analytical Basis for the Formulation of Sentencing Policy (January 1978 Draft, U.S. Department of Justice). Bivariate relationships are relationships between two variables when no other variables are controlled. See generally J. Mueller, K. Scheussler, and H. Costner, Statistical Reasoning in Sociology 183-185, 222-229 (1977); M. Rosenberg, The Logic of Survey Analysis (1968).
74. Using the Pearson correlation coefficient, the following cutoffs were established: for Chicago, $r \geq .14$ and $p < .005$; for Newark, $r \geq .20$ and $p \leq .001$. J. Calpin et al., supra note 73.
75. See generally M. Rosenberg, supra note 73, at 84-94; J. Cohen and P. Cohen, supra note 62, at 87-91; R. Darlington, supra note 62, at 163-165.
76. See M. Rosenberg, supra note 73, at 94-98.
77. Partial correlations are correlations in which one or more variables are held constant by statistical adjustment. See J. Mueller et al., supra note 73, at 275-301; H. Blalock, supra note 38, at 455-462; J. Cohen and P. Cohen, supra note 62, at 73-84; F. Kerlinger and E. Pedhazur, supra note 45, at 81-99.

The next step in the creation of the Denver guidelines was to submit the variables that survived the initial screening to an iterative model-generating procedure known as stepwise multiple regression.[78] It was this method that was used to identify the variables to be included in the sentencing guidelines. Stepwise regression is a procedure by which a computer program determines which variables are included in the model and the order in which they are included, according to predetermined statistical criteria.[79] The procedure is as follows. The program first selects the variable that alone accounts for the greatest amount of variance (variation) in the dependent variable, as measured by the squared zero-order (bivariate) correlation coefficient (r^2). After the first variable has been selected, the program next selects the variable that will add the largest amount of explained variation (predictive accuracy) to the model.[80] This procedure is repeated until no variables can be found that satisfy the other statistical criteria for inclusion.

The regression model can be used to generate expected or "predicted" values of the dependent variable (e.g., sentence) for each case in the sample. To a greater or lesser degree, these predictions differ from the actual values. In other words, the model is an imperfect representation of reality. The correlation between the predicted and actual values is called the multiple correlation coefficient (R)[81]. When squared, it (R^2) is a measure of the degree to which the model fits the data. This is called goodness of fit by statisticians.[82] A high R^2 statistic indicates a close fit between the model and the data. Conversely, a low R^2 statistic indicates a poor fit. R^2 statistics can be interpreted as the proportion of variance (variation) in the dependent variable that is accounted for by the model. This can be symbolized as follows:

$$R^2 = \frac{\text{Explained Variance}}{\text{Total Variance}}$$

Like any other proportion, it ranges from 0 to 1, and can be expressed as a percentage. Thus, an R^2 of .50 means that the model accounts for 50 percent of the variance in the dependent variable; the remaining 50 percent is relegated to the error term.[83]

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78. L. Wilkins et al., supra note 2, at 10-11.
79. See generally the works cited in note 62 supra.
80. This is accomplished by selecting from the remaining variables the one with the highest squared semi-partial correlation coefficient. See N. Nie, C. Hull, J. Jenkins, K. Steinbrenner, and D. Brent, Statistical Package for the Social Sciences 345-347 (2d Ed. 1975) (the program used by the CJRC researchers). This is equivalent to residualizing the remaining variables on the variable already selected for the model and choosing the residualized variable that has the highest correlation with the dependent variable. F. Kerlinger and E. Pedhazur, supra note 45, at 95.
81. See note 40 supra.
82. R^2 also is referred to as the coefficient of determination. F. Kerlinger and E. Pedhazur, supra note 45, at 15.
83. See note 43 and accompanying text supra.

Stepwise multiple regression is best suited to the situation in which we lack a comprehensive theory about the behavior being studied and wish only to make the best (i.e., most accurate) predictions on the basis of empirical observation.[84] It is true that there is no comprehensive theory of sentencing that would allow us to choose variables on purely theoretical grounds. In developing empirically based sentencing guidelines, however, we do not seek merely to predict judicial sentencing behavior by whatever means are available to us. Suppose that, by coincidence or for some reason we fail to understand, there is a high correlation between sentences and offenders' height. In that case, we could predict sentences accurately if we knew offenders' height. Yet surely we would not want to create normative guidelines for sentencing based on height. The variables that best predict sentences are not necessarily the same ones that judges use or ought to use.

This highlights the distinction in science between prediction and explanation.[85] The goal of most social science research is to explain or understand social phenomena. A good explanation enables us to make predictions, which can be tested against further observations. These predictions may have useful applications, but they are, in a sense, a byproduct of scientists' efforts to understand and explain.

There are times, however, when our primary interest is prediction rather than explanation. For example, college admissions officers need to make predictions about which applicants will successfully complete the program of study, but they are not particularly interested in why some students fail while others pass. Therefore, if an admissions test is observed to predict success and failure accurately, the admissions officer will benefit from its use despite the fact that it does not identify the characteristics that cause success or failure. In other words, the test predicts performance but it provides no explanation.

Predictive devices have a place in legal institutions and the administration of criminal justice. Decision makers frequently need to base their decisions on predictions of behavior. Pretrial release decisions are one example. A magistrate may need to make judgments about whether individual defendants are likely to appear in court if they are released on bail. It is not necessary that the magistrate understand why some defendants appear and some do not. It is only necessary that the magistrate make accurate predictions based on whatever characteristics serve as efficient predictors. The stepwise multiple regression approach is best suited to this situation because we care only about prediction, not explanation. So long as the model predicts efficiently and does not include any variables that are legally suspect, such as the offender's race, we will be satisfied.[86]

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84. See J. Cohen and P. Cohen, supra note 62, at 104.
85. See H. Blalock, supra note 37, at 10, 38-44; F. Kerlinger and E. Pedhazur, supra note 45, at 281-331. See generally D. Heise, Causal Analysis 3-35 (1975).
86. For a discussion of legally suspect classifications and their use in sentencing, see pp. 109-112.

Parole release decisions are another example of decision makers' need to make predictions. One of the factors supposedly taken into account by parole boards when they decide whether to release a prisoner is the likelihood of recidivism.[87] This obviously entails a prediction about the offender's behavior. In making such predictions, parole boards may be assisted by actuarial devices. These actuarial devices may take the form of models much like the sentencing guidelines models. It is important to recognize, however, that in one case we are talking about a model that assists decision makers by giving them a prediction about the behavior (recidivism) of the subject of their decision (the prisoner), while in the other case we are talking about a model that predicts the behavior (sentencing) of the decision maker in order to control or guide decisions.[88]

There is a growing body of literature on the use of actuarial predictions in law.[89] The role of actuarial predictions in adjudicatory decisions remains debatable. It seems to us that equal protection doctrine, under certain circumstances, may require that the predictors bear some plausible, if not demonstrable, causal relationship to the predicted events.[90] Whether or not this is the case, there is far greater reason to demand explanatory accuracy, and not just predictive accuracy, when the model pertains to the behavior of the decision maker and when it is intended to control or guide discretionary decisions. It is not enough that the model predicts sentences. If the model is to become a statement of policy, it must embody a thorough understanding of the judge's behavior. We must know what variables actually cause judges to sentence as they do. Moreover, the current practices must be acceptable according to legal and policy standards. Stepwise multiple regression analysis of data collected from criminal case files cannot deliver all that.

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87. D. Gottfredson et al., supra note 2, at 8.

88. This important distinction has eluded at least one prominent legal scholar. See J. Coffee, supra note 2, at note 150 and accompanying text.

89. E.g., A. Dershowitz, The Law of Dangerousness: Some Fictions About Predictions, 23 J. Legal Educ. 24 (1970); A. von Hirsch, Prediction of Criminal Conduct and Preventive Confinement of Convicted Persons, 21 Buffalo L. Rev. 717 (1972); A. Dershowitz, Preventive Confinement: A Suggested Framework for Constitutional Analysis, 51 Texas L. Rev. 1277 (1973); S. Shah, Dangerousness: A Paradigm for Exploring Some Issues in Law and Psychology, 1978 Am. Psych. 224 (1978); B. Underwood, Law and the Crystal Ball: Predicting Behavior with Statistical Inference and Individualized Judgment, 88 Yale L. J. 1408 (1979).

90. The argument here is that, under equal protection doctrine, the prediction device may not use classifications (predictors) that bear no rational, causal relationship to the predicted events that give the state a legitimate interest in making the classification, especially where fundamental rights (e.g., liberty) are at stake. This clearly goes beyond the current development of the law. See generally B. Underwood, supra note 89, at 1444-47; G. Gunther, Foreword: In Search of Evolving Doctrine on a Changing Court: A Model for a Newer Equal Protection, 86 Harv. L. Rev. 1 (1972).

A brief review of the history of sentencing guidelines reveals the leap of faith that was made between prediction and explanation. The original parole guidelines research, predecessor of the current sentencing guidelines, began with an attempt to predict recidivism.[91] According to the researchers,

> It became apparent early in the project that, as other research endeavors had shown, the mere presentation of an experience table (prediction device) was not seen by the parole board members as a dramatic aid.[92]
>
> [The U.S. Board of Parole] was told by project staff that it might itself rank-order offense types by seriousness and then, for each risk classification (the determination of which could be aided by a statistical predictive device), decide upon an explicit policy. However, the board expressed doubt about engaging upon this task until it had developed more familiarity with this type of device. Therefore, the project staff was requested to provide sets of policy guidelines . . . based upon the project's coded material reflecting board policy during the preceding two years.[93]

Their account illustrates the concerns expressed earlier in this chapter.[94] Faced with the task of making policy in quantitative form, the U.S. Board of Parole allowed primary responsibility for this task to devolve on the researchers, reserving to itself only the power to review and modify the resulting model.[95] That is when the researchers' role changed from actuary to policymaker. The CJRC process of developing sentencing guidelines, which are intended to control the exercise of discretion rather than to assist judges in the making of actuarial predictions about recidivism,[96] placed researchers in a similar policymaking position.

The nature of the problem appears to have been obscured to some extent by the researchers' assertion that sentencing guidelines are not prescriptive but merely descriptive.[97] According to this assertion, the guidelines are descriptive information about average tendencies which are to be considered only as information, not as recommendations or exhortations. Judges were to consult the guidelines and then make up their own minds, knowing that they are free to depart from the guidelines so long as they articulate a reason for having done so.

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91. D. Gottfredson, supra note 2, at 14-18.

92. Id. at 15.

93. Id. at 16-17.

94. See pp. 26-28.

95. D. Gottfredson et al., supra note 2, at 17.

96. See P. Hoffman and L. DeGostin, An Argument for Self-Imposed Explicit Judicial Sentencing Standards, 3 J. Crim. Just. 195 (1975).

97. See L. Wilkins et al., supra note 2, at 31 ("[T]he guidelines . . . are essentially descriptive, not prescriptive."). But cf. D. Gottfredson et al., supra note 2, at 141 ("It is . . . clearly incorrect to talk of guidelines as descriptive, since the very term indicates prescription."). Our interviews suggest that some judges' objections to empirically derived sentencing guidelines were quieted by the assertion that the guidelines were descriptive rather than prescriptive.

If sentencing guidelines were merely descriptive or predictive, their lack of explanatory value is arguably less important. So long as the prediction for an individual case is not given weight in the judge's decision calculus, no harm results from the use of predictors that do not accurately represent the way judges make sentencing decisions.

There is little doubt that judges felt free to depart from the guidelines. The evidence suggests that the argument may have been pushed too far.[98] Some judges concluded that, if the guidelines were merely descriptive and not prescriptive, it was unnecessary to consult them before the imposition of sentence. This was frequently the case in Chicago, where judges usually computed the guideline sentences only after imposing sentences, if at all. A remark typical of Chicago judges was, "I like to see how close I came."[99]

Nevertheless, it would be incorrect to think that empirically based sentencing guidelines are merely descriptive. Sentencing guidelines, as the name implies, are intended to guide judges' sentencing discretion. It is hard to imagine why anyone would create sentencing guidelines if their purpose were not to affect judges' decisions. To think otherwise would be to regard them as windowdressing, or worse. As the behavior of Chicago judges poignantly illustrates, it is even more difficult to understand why judges should consult the sentencing guidelines before sentencing rather than afterwards if the guidelines are not intended to influence their decisions. There is no way around the fact that sentencing guidelines are an inherently normative device. That judges are permitted to depart from the guidelines without fear of reversal or sanction implies more about the guidelines' likely inefficacy than about their essential character as a behavior-shaping mechanism.

The failure to distinguish adequately between prediction and explanation is a serious problem with the development of sentencing guidelines, but technical criticisms also counsel against the use of stepwise multiple regression to specify guideline models.[100] In sentencing research, as in most social research, predictor variables usually are correlated with each other to some extent. Persons convicted of armed robbery, for example, may tend to have more serious prior records, lower educational achievement, and higher unemployment rates than do persons convicted of theft. When two predictor variables are

98. Judges in Denver and Philadelphia departed from the guidelines more frequently than their originators expected or intended. See pp. 82-109.

99. See pp. 179-180.

100. See generally Hanushek and Jackson, at 95-96; J. Cohen and P. Cohen, supra note 62, at 102-104.

correlated with each other, they are said to be collinear. When three or more predictor variables are intercorrelated, they are termed multicollinear. Frequently the terms collinearity and multicollinearity are used interchangeably.[101]

Suppose that, in a sample of cases, all robbers had one prior felony conviction while all thieves had no prior felony convictions. Suppose further that all robbers were sentenced to four years' imprisonment while all thieves received sentences of two years' incarceration. This is a case of perfect collinearity: the correlation between type of crime and number of prior felony convictions is 1.0. If we wish to build a model to predict length of incarceration (the dependent variable), we will have no trouble accounting for all of the variance in the dependent variable. If the case for which we want to predict sentence is a robbery, we can predict with 100-percent accuracy that the sentence is four years. On the other hand, if the case is a theft, we can predict with perfect accuracy that the sentence is two years.

Notice, however, that we also can predict sentences with perfect accuracy if we use the number of prior felony convictions as the predictor. All we need to do is predict four years' imprisonment for those offenders having one prior felony conviction and two years' imprisonment for those having no prior felony convictions. Information about the number of prior felony convictions is, for the purposes of prediction, redundant with information about the type of crime. In this situation it is impossible to infer mathematically whether judges give robbers longer sentences than thieves because robbery is a more serious crime, because the robbers have more serious prior records, or both.

If we were to submit the data from our hypothetical sample to stepwise multiple regression, once one predictor variable had been selected the other would add nothing to the variance in the dependent variable accounted for by the model (R^2). Consequently, the second variable would not be selected.[102] This is where the use of stepwise multiple regression can lead to misinterpretation. The temptation would be to conclude erroneously

101. For discussions of the problem of multicollinearity, see Hanushek and Jackson, at 86-96, 131-133; J. Neter and W. Wasserman, supra note 62, at 250-254, 339-347; J. Johnston, supra note 52, at 159-168; H. Theil, Principles of Econometrics 147-154; H. Blalock, Correlated Independent Variables: The Problem of Multicollinearity, 42 Amer. J. Soc. 233 (1963); D. Farrar and R. Glauber, Multicollinearity in Regression Analysis: The Problem Revisited, 49 Rev. Econ. and Stat. 92 (1967); R. Gordon, Issues in Multiple Regression, 73 Amer. J. Soc. 592 (1968); Y. Haitovsky, Multicollinearity in Regression Analysis: Comment, 59 Rev. Econ. and Stat. 486 (1969); R. Rockwell, Assessment of Multicollinearity: The Haitovsky Test of the Determinant, 1975 Soc. Methods and Research 308 (1975).

102. In this case there is perfect collinearity, so the two independent variables are linearly dependent. The computation of least squares estimators (regression coefficients) requires inversion of the data matrix, which is impossible if two or more variables are linearly dependent. Therefore, the regression equation cannot include both variables in our hypothetical example. See J. Johnston, supra note 52, at 160.

that the variable selected by the program is very important to judges, while the second variable is completely unimportant or irrelevant. Yet we know that there is no basis for this inference. All we know is that, in our sample, the two variables are completely redundant.

Our hypothetical example is extreme, but the problem exists to a lesser degree when, as is often the case, the predictor variables are imperfectly intercorrelated. Where a variable has been excluded from a model because of its redundancy with a variable already included in the model, the parameter estimate for the included variable will be biased.[103] That is, the importance of the included variable (ß) will be systematically misrepresented.[104]

In Denver the guidelines researchers used the order in which variables were selected by a stepwise multiple regression program as the measure of their relative importance. The order of entry, however, is determined partly by the intercorrelations among the predictor variables. Therefore, it is not wholly indicative of the importance of variables. Moreover, the primary criterion for inclusion of variables in stepwise regression is the increment in R^2, but the R^2 statistic is sample specific. That is, its magnitude depends partly on the variances of the predictor variables in a particular sample.[105] Therefore, the order of entry (or R^2 increment, which depends upon the order of entry[106]) is an inappropriate measure of the importance of variables.[107]

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103. Hanushek and Jackson, at 96.

104. If, on the other hand, collinear variables are forced into the model, the parameter estimates will be unbiased but they also will tend to be unreliable. In other words, the importance of the variables will not be systematically over- or understated, but the estimates will fluctuate depending on the characteristics of a particular sample, and therefore cannot readily be generalized to the larger population of criminal cases. See Hanushek and Jackson, at 86-90; J. Johnston, supra note 52, at 160-162. This problem, however, is not peculiar to stepwise regression; it occurs whenever collinear variables are used in any multiple regression.

105. See Hanushek and Jackson, at 59, 95.

106. See Hanushek and Jackson, at 95.

107. It should be noted here that the models eventually implemented in the four jurisdictions did not employ parameter estimates as weights for the guidelines variables. Instead, integer weights were used. Most of the offense- and offender-related factors in the guidelines were assigned weights of zero or one. Some factors were assigned weights of two or three, but, without exception, integer values were used. To justify this, the CJRC researchers relied on findings that date back to the early parole success prediction studies. See L. Wilkins et al., supra note 2, at 14-15 and note 38; D. Gottfredson et al., supra note 2 at 42. These studies showed that the predictions yielded by unit weights were no less accurate than those produced by the estimated parameters (fractional coefficients). Although these findings suggest that the use of estimated parameters in sentencing guidelines would not enhance their predictive efficacy, they also imply that these kinds of models provide only crude predictions, and even cruder explanations, of the predicted events.

Multicollinearity is essentially a problem of insufficient information.[108] Our hypothetical sample of robberies and thefts did not permit us to determine the relative importance of the type of crime and the number of prior felony convictions because there is no information about how judges sentence robbers with no prior felony convictions or thieves with one prior felony conviction. No doubt judges have subjective information about the importance of those two variables relative to each other. One solution would be to ask judges what factors they consider when they sentence offenders. Although this approach entails a different set of problems related mostly to judges' ability and willingness to accurately describe their behavior, it has much to recommend it. For one thing, variables can be systematically manipulated so as to avoid the problems of multicollinearity. The control offered by that kind of experimental research is much greater than that offered by post hoc analyses of case data.[109]

As we have seen, the selection of variables by stepwise multiple regression is a risky business at best. Let us briefly consider the models that were created by the CJRC researchers. As far as we are able to tell from their reports, only in Denver was stepwise regression used to select variables.[110] It appears that the variables included in the Chicago and Newark models were selected on the basis of their simple bivariate relationships with the dependent variable.[111] As we noted above, selection of variables on the basis of bivariate relationships creates considerable risk of overlooking more complex relationships such as suppression or distortion. The researchers reported that they confirmed their selection of variables with multiple regression and discriminant function analyses, but the results of these analyses were not reported for Chicago and therefore cannot be reviewed.[112]

An examination of the regression results reported by the guidelines researchers reveals that their models had surprisingly little predictive accuracy, quite apart from the issue of their validity as explanations of judges' sentencing behavior. Where the decision whether to incarcerate, coded dichotomously as incarceration/nonincarceration, was taken as the dependent variable, R^2 was typically about .300.[113] In other words, the models typically explained only 30 percent of the variation in sentences. The Newark model of the decision whether to incarcerate yielded an R^2 of .144, which indicates that only 14.4 percent of the variance was accounted for by the model. Where length of incarceration was taken as the dependent variable, the predictive

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108. Hanushek and Jackson, 92-93. See also H. Theil, supra note 101, at 154.

109. See generally D. Campbell and J. Stanley, Experimental and Quasi-experimental Designs for Research (1963).

110. See L. Wilkins et al., supra note 2, at 10.

111. See J. Calpin et al., supra note 73.

112. See J. Calpin et al., supra note 73. See also note 187 infra.

113. The CJRC researchers reported an R^2 of .530 for one Denver regression model. Our attempt to replicate that regression model using listwise deletion of cases with missing observations produced a sample that was too small to sustain the analysis. See pp. 66-67 infra. If the sample were as small as we found it to be, the 53-percent variance explained statistic reported by the guidelines researchers would have exaggerated the predictive accuracy of the model by capitalizing on random variation.

accuracy of the models, as measured by R^2, tended to be even lower than those for the incarceration/nonincarceration models.[114]

These results are striking. Not only do the guidelines models fail to explain what causes judges to sentence as they do, and not only are the results of questionable generalizability, but the models do not even predict accurately the sentences in the samples on which they are based. The models do not fit the data very well.

It would be well to consider at this point the CJRC researchers' claim that the guidelines models accurately predicted 80 to 85 percent of the sentences in the samples of cases. At first blush this statistic is far more impressive than the variance-explained statistics from the regression analyses. But, assuming that it is accurate, the 80-percent figure does not necessarily imply any greater predictive accuracy than does the lower variance-explained statistic. This is true for two reasons. First, with respect to the decision whether to incarcerate, the 80-percent statistic capitalizes on the dichotomous nature of the decision. Every case results in either an incarcerative sentence or a nonincarcerative sentence. One can predict at least half of the sentences correctly just by predicting in every case whatever is the more frequent sentence in the aggregate. If, as is roughly true in Denver, for example, 60 percent of convictions result in incarceration, then one would accurately predict 60 percent of the decisions whether to incarcerate simply by predicting incarceration in every case. The 80-percent accuracy of the guidelines would represent only a 33-percent improvement over a rational guessing strategy ((80 - 60)/60). This 33-percent figure is comparable to the variance-explained statistics from the regression models.

With respect to the decision about length of incarceration, the 80-percent statistic exaggerates the predictive accuracy of the models in a different way. For each grid cell that calls for an incarcerative sentence, the guidelines identify a range of sentence lengths. Each range was established by calculating the mean sentence length for the particular combination of offense and offender scores, and then expanding it by equal intervals above and below the mean in order to encompass the desired number of cases. By increasing the ranges, one can achieve greater "accuracy" of prediction. In fact, one can achieve 100-percent accuracy if one is willing to increase the ranges sufficiently. But accuracy is achieved only at the expense of precision. As the guideline ranges increase, the guidelines provide less guidance to the judge. The wider the ranges, the greater the variation within them. The variance-explained statistic, in effect, measures both the variation between ranges and the variation within ranges. This explains why a guidelines model that accurately predicts 80 percent of the sentences could account for only 30 percent (or less) of the variance in sentence lengths. An examination of the Denver guidelines, for example, reveals relatively wide ranges of sentence lengths, many of which consequently overlap with each other.

Our point is not to fault the guidelines researchers for their inability to specify better-fitting models. Statistical models of judicial sentencing behavior rarely explain more than 50 percent of the variance in sentences.[115] Our own regression models, which are presented in this chapter and in Chapters 3 and 4, do not exceed 45-percent variance explained. The point is that these kinds of statistical models provide only crude explanations of judicial sentencing behavior.

There are three plausible explanations for the low predictive accuracy of the guidelines regression models. The first is that the model was inadequately specified, and that a properly specified model would have fitted the data better.[116] We have already identified some parts of the original research that we would have conducted differently; additional criticisms appear later in this chapter. We note, however, that the use of more appropriate statistical techniques would not necessarily have increased dramatically the percent of variance explained.

The second explanation is that limitations of the data preclude greater predictive accuracy. These data, like most data used in sentencing research, included only information that was found in the case files. Judges, of course, have access to information beyond what is recorded in the case files. They hear testimony and argument, observe the defendant in the courtroom, and so forth. Some of this information probably influences their decisions. Additionally, errors in the in coding of case file information may have contributed to the problem.

Finally, there is the possibility that sentences are simply unpredictable. If this were truly the case, it would mean that sentence disparity is the rule rather than the exception. It is difficult to believe, however, that sentences are as disparate as this interpretation of the low multiple correlation coefficients would suggest. It is not uncommon for experienced attorneys to report that they know "the going rates" for various offenses and offenders.[117] In other words, they believe that they can predict sentences with a high degree of accuracy, especially if they know which judge will impose the sentence. Moreover, our own experience in coding information from case files leads us to believe that sentences are not so unpredictable. We hesitate to ascribe the poor fits of the guidelines models entirely to the underlying unpredictability of sentences. It seems more likely to us to inhere instead in the data problems and the limitations of social science modeling.

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114. See L. Wilkins et al., supra note 2; J. Calpin et al., supra note 73.

115. But cf. L. Tiffany et al., supra note 9, at note 14 (R^2 = 0.70 for additive model; R^2 = 0.76 for interaction model).

116. We have already discussed the problems of selecting variables for the model. The problem of functional forms is considered below. See pp. 59-61.

117. See p. 164.

One possible source of statistical error is the rare event. Events or attributes that occur infrequently present special problems for modeling. Suppose that only one percent of convicted offenders assaulted the arresting officer, while 99 percent surrendered peaceably. The variable that measures whether the offender assaulted the arresting officer is said to be highly skewed. That is, the number of "no" responses far exceeded the number of "yes" responses. Highly skewed variables have little variance to share with the dependent variable (sentence), and therefore tend to show lower correlations than do normally distributed variables.[118] These low correlations do not necessarily mean that the variable is unimportant. They reflect the fact that the event (resisting arrest) occurs so infrequently that it does not account for differing results in the vast majority of cases. When it occurs, however, it may be very important.

Highly skewed variables are likely to be excluded from a model either because their low bivariate correlations cause them to be eliminated during initial screening, or because they are unlikely to be selected by stepwise regression, or because if they are forced into a regression equation they will contribute little to the prediction of the dependent variable. An examination of the data from which the guidelines were constructed discloses a substantial number of skewed variables. For example, 24 variables in the Denver sample, 15 in Newark, and 9 in Chicago had 75 percent or more observations within a single category.[119]

The exclusion of highly skewed variables is not a trivial matter. Speculation on the way judges make sentencing decisions suggests that the omission of such variables is a premature jettisoning of potentially valuable information. The speculation goes as follows. The very infrequency of certain negative events makes them highly salient to the sentencing judge. Sometimes their rarity is associated with heinousness. When a judge notes the presence of one or more of these attributes or behaviors, he or she may be more inclined to order incarceration where a probationary sentence would otherwise have been imposed. The more of these events or attributes the judge notices, the more likely may be a sentence of incarceration. Thus, rare events may be very important in individual cases. Moreover, variables that are statistically unhelpful when considered one at a time collectively may enhance predictions to a significant degree.

We demonstrate this notion by constructing a simple additive index composed of highly skewed variables which would not otherwise have been

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118. In the case of a dichotomous variable such as the one discussed here, a statistically normal distribution would be 50 percent "yes" and 50 percent "no."

119. These figures do not include variables with substantial numbers of missing observations. The problem of missing observations is discussed below. See pp. 66-69.

retained in a regression equation.[120] We call this scale the Index of Rare Events. For each rare event that appeared in the offender's record, one point was assigned on the Index of Rare Events. As we can see in Table 2-4, the index is highly predictive of the probability of incarceration. The percentage incarcerated ranged almost monotonically (i.e., it almost always increased as the values of the Index increased) from 13.6 to 77.8.

Table 2-4
"Index of Rare Events"
as a Predictor of the Judge's In/Out Decision

Score on Index of Rare Events	Percent Incarcerated
0	13.6
1	27.3
2	45.8
3	40.0
4	58.3
5	—
6	—
7	77.8

This is only one simple approach to preserving the information contained in highly skewed variables. We make no claim for its validity. There are more sophisticated methods of solving this problem. Nevertheless, we think that the Index of Rare Events demonstrates the potential value of using rare events to predict sentences. It seems unnecessary and unwise to discard information about important events because of their infrequency.

120. This index was calculated for the Denver guidelines construction sample, and included the following variables: offender's use of aliases, number of targets involved in present offense, offender's relationship to the criminal justice system at time of offense, adjustment in offender's pretrial liberty status, probation revocation history, longest incarceration sentenced to in any previous conviction, parole revocation history, number of times offender was incarcerated after conviction for a similar offense, offender's escape history, offender's wedlock status, offender's family's willingness to provide assistance in current situation, offender's spouse's liberty status, whether offender is meeting support obligations, offender's illegitimate children, offender's intelligence evaluation, offender's attendance in school, offender's behavior at school, employment evaluation at present offense/most recent job, employment evaluation at second most recent offense/second most recent job, number of official contacts with mental health system by offender, offender's sexual orientation, probation officer's presentence investigation assessment, and the offender's behavior at arrest/present offense. These variables were selected without regard for their appropriateness as sentencing criteria.

The exclusion of legally irrelevant variables from models of the sentencing process is another possible source of statistical error. By legally irrelevant variables we mean variables that are legally inappropriate sentencing considerations. For example, the Equal Protection Clause of the Fourteenth Amendment prohibits racially discriminatory sentencing practices. Therefore, the offender's race, like other constitutionally suspect classifications, is legally irrelevant to the sentence.[121] Other variables, such as height, eye color, or shoe size, are legally irrelevant because they bear no reasonable relationship to the purposes of criminal sanctions.[122] The offender's pretrial release status (i.e., whether the offender was released on bail or personal recognizance pending adjudication) is of questionable legal relevance to sentencing. An offender's pretrial release status depends partly upon the discretionary decisions of criminal justice officials (e.g., whether to release on personal recognizance, how high to set bail) and the financial resources of the defendant. To that extent, pretrial release status is an inappropriate sentencing consideration. On the other hand, the offender's pretrial release status also may reflect her or his conduct pending adjudication. Criminal misconduct and failure to appear in court frequently result in revocation of bail. For the purpose of modeling, the situation is complicated further by the fact that some of the determinants of pretrial release (e.g., prior record, seriousness of the instant offense, employment status) are also factors that influence sentencing. Therefore, part of the correlation between pretrial release and sentence may be the result of the pretrial release variable proxying other variables not included in the model.[123]

Of course, the inclusion of legally irrelevant variables in sentencing guidelines is unacceptable. This does not mean, however, that legally irrelevant variables should be excluded from the initial attempts to build models that explain judges' sentencing behavior. If legally irrelevant variables show significant relationships with sentence after other variables have been controlled, there are two possible explanations. One is that judges are influenced by improper considerations when they sentence. If this were true, it would be an important finding. Presumably some remedial action would be in order. The second possible explanation is that the model has been inadequately specified. It may be that the legally irrelevant variables are serving as proxies for legitimate variables that are correlated with both sentence and the irrelevant variables, but which have been excluded from the model. The obvious solution to this problem is to incorporate these variables into the model. Sometimes, however, the necessary data are unavailable.

None of the preliminary models reported by the CJRC researchers included any legally irrelevant variables. Eight variables were selected for the Chicago model, reportedly by examination of the bivariate

121. See pp. 109-112.
122. See pp. 109-112.
123. See p. 186.

correlations, and confirmed by stepwise multiple regression.[124] In an effort to replicate these findings, we took the same data that were used to construct the guidelines and performed a stepwise regression using a large set of variables including the eight selected by the CJRC researchers. We found, however, that the first eight variables selected by the stepwise procedure included only three of those chosen by the CJRC researchers. Moreover, the offender's race and bail status were among the first eight variables selected. The results of this analysis are presented in Table 2-5.

As we have argued, both the order in which variables are selected by a stepwise multiple regression procedure and the increment in variance explained (which depends partly on the order of selection) are poor measures of their importance. Comparison of the regression coefficients, once all of the variables have been entered, is a better way to judge the relative importance of variables.[125] As we can see from Table 2-5, pretrial release status has a larger standardized regression coefficient than any other variable. The standardized regression coefficient for race is slightly larger than that for the statutory class of the offense at conviction, which was selected by the CJRC researchers for use in the guidelines model. These results do not prove that Chicago judges consider the offender's race or bail status when they sentence. As noted above, further analysis might well show that these variables serve as proxies for other legitimate considerations. Nevertheless, candor compels us to report these results. If we are truly to understand why judges sentence as they do, these problems must be faced squarely. A priori exclusion of legally irrelevant variables can only serve to obstruct the understanding required for construction of a suitable model.

Interactions. The failure to specify appropriate interaction terms is another source of statistical error. An interaction is a conditional relationship in which the effect of one predictor variable on the dependent variable depends on one or more of the other predictor variables. Suppose, for example, that older offenders (e.g., 20 years or older) are sentenced to imprisonment at the same rate, say 65 percent, as younger offenders. Suppose further that persons convicted of auto theft are incarcerated at the same rate, say 65 percent again, as persons convicted of other crimes. We would conclude on the basis of these bivariate (or zero-order) relationships that neither the offender's age nor the type of crime (measured as auto theft or other) is related to the decision whether to incarcerate. It may be, however, that older auto thieves and younger non-auto thieves are imprisoned at the rate of 90 percent, while younger auto thieves and older non-auto thieves are incarcerated at the rate of 40 percent. This is illustrated in Table 2-6.

124. J. Calpin et al., supra note 73.
125. See pp. 33-34.

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124. J. Calpin et al., supra note 73.
125. See pp. 33-34.

Table 2-5
Comparison of Our Stepwise Regression of the
Chicago Guidelines Construction Data
With Those Reported by the CJRC Researchers

Variables Selected by CJRC Researchers and Reported
Confirmed by Stepwise Multiple Regression*

Number of prior adult felony convictions
Number of prior adult convictions (felony and misdemeanor)
Number of prior adult incarcerations longer than 30 days
Number of prior adult felony convictions for crimes not against persons
Liberty status at time of offense
Statutory class of offense at conviction
Number of counts convicted of
Number of prior adult felony convictions for crimes against persons

Results of Reanalysis

Variable**	Unstandardized Regression Coefficient	Standardized Regression Coefficient
Bail Status	-.273	-.279
Weapon Usage	.174	.195
Number of prior adult convictions	.042	.222
Liberty status at time of offense	.161	.177
Number of counts charged	.045	.140
Seriousness rating (by judge)	.039	.111
Race	.124	.087
Statutory class of offense at conviction	-.027	-.077

n = 272
R^2 = .338
Adjusted R^2 = .318

* Listed in descending order of bivariate correlations.
**Listed in the order selected by the stepwise regression program.

Table 2-6
Hypothetical Interaction
Percent Incarcerated by Age and Type of Crime

	Age		
Type of Crime	Young	Old	Young and Old Combined
Auto	40*	90	65
Other	90	40	65
All crimes	65	65	65

*Percentages are incarceration rates, not row or column percentages.

The relationship illustrated in Table 2-6 is called an interaction. That is, the two independent variables (age and type of crime) interactively affect the dependent variable (sentence). The effect of each independent variable on the dependent variable depends upon the other independent variable. If we had restricted ourselves to an examination of the bivariate relationships, we would have concluded erroneously that age and type of crime are unrelated to the decision whether to incarcerate.

Interactions are not unheard of in sentencing research.[126] Moreover, there are good reasons to think that some factors may interact with each other when judges decide what sentence to impose. For example, we might suspect that employment status (employed versus unemployed) is a greater factor in the decision whether to incarcerate where male offenders are concerned than where female offenders are concerned.

It is not difficult to accommodate interactions in regression equations.[127] Let us assume the relatively simple case of two predictor variables. Without any interaction terms, the single equation model can be written as

$$Y = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + U$$

where Y is the dependent variable; X_1 and X_2 are the predictor variables; U is the error term (i.e., unexplained variance); β_1 and β_2 are the parameters (regression coefficients) that describe the importance of X_1 and X_2, respectively; and β_0 is a constant that represents the Y-intercept of the regression line. We can test for an interaction between the two predictor variables (X_1 and X_2) by including a third predictor variable that is the product of X_1 and X_2:

$$Y = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_{12} X_1 X_2 + U$$

where $X_1 X_2$ is the interaction term and β_{12} is the parameter that describes its importance.

Conceptually, testing for and accommodating interactions is simple. As a practical matter, it is a tedious, time-consuming enterprise. As the number of predictor variables increases, the number of possible interactions grows dramatically. Not only can every predictor interact with every other predictor, every predictor can interact with all possible combinations of two or more other predictors. These higher-order interactions often are difficult to interpret substantively. Nevertheless, although the price of testing for interactions is high, so is the price of ignoring them. The unique explanatory power of omitted interaction terms is relegated to the error term (U), where it is manifested as lower-than-necessary variance explained (R^2). The loss

126. See, e.g., L. Tiffany et al., supra note 9, at 377, 387 (interaction between race and prior record is such that race is a factor in sentencing only among offenders who have no prior convictions).
127. See J. Cohen and P. Cohen, supra note 62, at 291-339; F. Kerlinger and E. Pedhazur, supra note 45, at 245-251.

of explanatory power diminishes our understanding of the phenomena being studied.[128]

In no reported instance did the guidelines researchers employ interaction terms in their regression models.[129] It is quite possible that the inclusion of interaction terms would have resulted in better-fitting models (i.e., higher R^2 statistics).

Functional forms. Throughout the preceding discussion, we have assumed that the predictor variables are linearly related to the dependent variable. This is not necessarily the case. Relationships may take a variety of curvilinear forms.

Let us consider the simple case of one predictor linearly related to the dependent variable:

$$Y = \beta_0 + \beta_1 X_1 + U$$

where the terms Y, X, U, β_0, and β_1 are defined as they were in the previous section. With a linear relationship, one unit of change in the predictor variable (X_1) produces β_1 units of change in the dependent variable (Y). This is true for every value of X_1 and Y. If we were to graph the predicted values of the dependent variable (Y) for every value of the predictor variable (X_1), they would form a straight line, which would intersect the Y axis at β_0 and have a slope of β_1.

128. Procedures have been developed by which a researcher can test systematically for all possible interactions. See J. Sonquist and J. Morgan, The Detection of Interaction Effects (1964); J. Sonquist, Multivariate Model Building: The Validation of a Search Strategy (1970). These procedures, called Automatic Interaction Detection (AID), are described briefly in H. Blalock, supra note 38, at 539-540. This approach is not without its own difficulties. If there are more than a handful of variables, the number of possible interactions is sufficiently large that some are likely to reach statistical significance when in fact they are the result of sampling error. Nevertheless, this approach has much to recommend it when there are not adequate theoretical grounds for specifying interaction terms.

129. See L. Wilkins et al., supra note 2; J. Calpin et al., supra note 73.

In the social sciences, linear models are far more commonly used than other functional forms.[130] There are a number of reasons for this. First, linear models are the simplest mathematical models. Second, many nonlinear functional forms approximate linearity over part of their ranges.[131] Finally, the statistical properties of some nonlinear forms are unknown because they have not been investigated thoroughly.[132]

Many curvilinear forms, however, are not unduly difficult to estimate. This is particularly true where the estimation can be accomplished by a transformation of one or more of the variables.[133] In that case, the researcher simply transforms the variable(s) and substitutes the transformed variable(s) into the equation. One commonly used functional form is known as the log-log form. It is estimated by taking the natural logarithms of all variables and substituting them into the regression equation. With one predictor variable, this can be symbolized by

$$Y^* = \beta_0 + \beta_1 X_1^* + U^*$$

where the asterisks indicate logarithmically transformed variables. Another way to write the log-log form is

$$\log Y = \beta_0 + \beta_1 \log X_1 + \log U.$$

The log-log model specifies that the effect on the dependent variable (Y) of any given change in the predictor variable (X_1) varies according to the magnitude of the predictor variable.[134]

Instead of logarithms, we may transform variables by taking their reciprocals.[135] Polynomial equations are sometimes used to specify nonmonotonic relationships, where the direction of the relationship reverses one or more times.[136] Other transformations such as logit and probit are particularly helpful when the dependent variable is

130. See Hanushek and Jackson, at 97.
131. See id.
132. See id.
133. See id.; J. Johnston, supra note 52, at 47-50.
134. See Hanushek and Jackson, at 97-99; J. Johnston, supra note 52, at 51-52; J. Neter and W. Wasserman, supra note 62, at 123-127. It is also possible to transform only some of the variables in a model. See J. Johnston, supra note 52, at 50-51; J. Neter and W. Wasserman, supra note 62, at 127-128.
135. See J. Johnston, supra note 52, at 52-55; J. Neter and W. Wasserman, supra note 62, at 128-130.
136. See Hanushek and Jackson, at 99-100; J. Cohen and P. Cohen, supra note 62, at 213-241; F. Kerlinger and E. Pedhazur, supra note 45, at 208-221; J. Neter and W. Wasserman, supra note 62, at 273-293.

dichotomous (e.g., the decision whether to incarcerate, coded as incarceration/nonincarceration).[137]

It is not difficult to imagine that some relationships in the sentencing process are nonlinear. For example, the importance of the number of prior convictions may vary according to how many the offender has. One prior conviction may be relatively unimportant, rarely spelling the difference between probation and prison, while two or three prior convictions may weigh considerably more heavily in the judge's mind. Eventually, as the number of prior convictions increases, the relative importance of an additional conviction may decline. The difference between twelve and thirteen convictions is likely to be very unimportant.

The selection of a functional form is a difficult but important task. Models must live up to the nature of the reality they are intended to represent. Simple models are to be valued, except when they are too simple. If it cannot be demonstrated that simple linear models adequately account for judges' sentencing decisions, more complex models should be developed. Some sentencing researchers already have attempted this.[138] There is, however, no evidence in the CJRC reports that nonlinear functional forms were considered or estimated in any of the three sites.[139] The loose fit of their linear models to the data offers little comfort that they suffice. We cannot say whether a different functional form would have permitted the development of more accurate models, but once again the question warrants serious investigation when fundamental rights depend upon the answer.

Data Collection

Inaccuracy and incompleteness of data limit the empirical researcher's ability to understand and explain social phenomena. In order for accurate models to be developed, the data with which they are estimated must be accurate and must include the relevant information. Sentencing research has its full share of data collection problems.[140]

The goal of data collection in sentencing research, insofar as we seek to understand the judge's sentencing behavior, is to collect all of

137. See Hanushek and Jackson, at 179-210; J. Neter and W. Wasserman, supra note 62, at 320-335; H. Theil, supra note 101, at 628-635; S. Fienberg, The Analysis of Cross-Classified Categorical Data 77-107 (1977); S. Haberman, 1 Analysis of Qualitative Data 292-346 (1978); J. Grizzle, C. Starmer, and G. Koch, Analysis of Categorical Data by Linear Models, 25 Biometrics 489 (1969). The treatment of dichotomous dependent variables is discussed below, when we take up the problem of measuring sentences. See pp. 77-78.

138. See, e.g., W. Rhodes, supra note 9.

139. See L. Wilkins et al., supra note 2; J. Calpin et al., supra note 73.

140. See T. Clear and J. Hewitt, Site-Based Sentencing Research, 6 (6) Rev. Pub. Data Use 33 (November 1978).

the information that the judge possessed when the decision was made. Of course, it is impossible to discover and assemble all relevant information that was available to the judge. Much of it goes unrecorded. Some of it is unobservable. Moreover, what we would really like to know is how the judge perceived the information. When we seek to understand why a judge imposed a particular sentence, it does little good to know that the offender had two prior felony convictions if the judge believed there were four prior felony convictions.

There are several sources of data for sentencing research. The most commonly used is official case records. These usually include charging documents, records of court proceedings, police arrest records (called "rapsheets"), applications for pretrial release, presentence reports, and miscellaneous documents such as pretrial motions, psychiatric evaluations, and letters from friends and family. A wealth of information is available from these documents, but there is no guaranty that the judge was aware of the information, or that his or her perception of the information matched the researchers'. Furthermore, the information often is inaccurate or misleading. For example, in Denver the presentence reports usually classify Mexican-Americans as "Caucasian" rather than "Hispanic" or an equivalent classification. The guidelines researchers recorded this information as they found it on the presentence reports, with the result that Mexican-Americans were not distinguished from Anglo-Americans for purposes of studying the effects of race on sentence. This might be expected to artificially deflate estimates of racial effects.[141] A better solution would have been to code Spanish-surnamed offenders as a separate group.

The judge has access to much information that is not captured in the case files. Evidence, argument, and observation of persons in the courtroom may influence the judge's decisions. In order to capture any of this information, it is necessary for the researcher to observe court proceedings directly or interview the judge afterwards. Both of these methods present difficulties, including exorbitant cost and the consumption of large amounts of time. For these reasons, they are rarely used. It is possible, however, that these more costly methods will be necessary if we are to gain the understanding needed for successful modeling of the sentencing process.

Sampling. Most social science data collection proceeds by way of sampling.[142] This is because the relevant population of things to be studied usually is too large to be considered in its entirety. Unless one has the resources and inclination to examine every instance of the

141. If minorities in general are discriminated against, the comparison of one minority group with a combined group comprising both the majority group and another minority group will understate the degree of discrimination. But see Chapter 3, note 46 (in Philadelphia, Spanish-surnamed persons are treated more like whites than like blacks).

142. See generally J. Mueller et al., supra note 73, at 363-381; W. Cochran, Sampling Techniques (2d. Ed. 1963); R. Jessen, Statistical Survey Techniques (1978).

phenomenon of interest (i.e., take a census of the population), one must sample the population.

The sampling frame is an operational definition of the relevant population. It identifies the entities that are eligible for sampling. In sentencing research, there are several problems that are encountered when one seeks to define the sampling frame.

The first difficulty pertains to the identification of the kind of items that will make up the sample. It is closely tied to the unit of analysis, discussed above.[143] The guidelines researchers selected "the case" as the unit of analysis.[144] The next question is, what constitutes a case? In many jurisdictions, a case (and therefore a case file) may include multiple charges and multiple defendants. This is true in Denver, for example, where the CJRC researchers elected to sample case files. When multiple-defendant cases were encountered, the researchers selected only one defendant about whom to collect data.[145] This procedure results in the systematic exclusion, and therefore under-representation, of codefendants. It is not unlikely that multiple defendant cases differ in some important respects from single defendant cases. The resulting sample must be considered biased from this point of view.

A better solution would have been to treat the defendant as the item to be sampled. Although this entails somewhat more work when multiple defendants are included in a single case file, it avoids biasing the sample against codefendants and in favor of single defendants. This can be done by assigning consecutive numbers to each defendant regardless of whether found in the same or different case files, and then selecting randomly from the assigned numbers. With some foresight, this can be done efficiently in one step.

Multiple charges present a different problem. Defendants are frequently charged with two or more offenses. Sometimes multiple charges arise out of the same transaction, as when two persons break and enter a home, steal a television set from inside, and are charged with burglary, theft, and conspiracy. Less often, multiple charges relate to entirely different criminal episodes. In both situations, defendants may be convicted of one, some, or all of the charges.[146] The difficulty arises when we must choose which charges to record and analyze.

We think that, on balance, the best solution is to record and analyze charges in order of the legislative grading of the seriousness of the offense. The basis on which the guidelines researchers selected charges for recording and analysis is unclear. Their codebooks, which served as instructions for the data collectors, are worded in terms of "first offense," "second offense," and so forth. Of course, the order in which

143. See pp. 30-40.
144. See L. Wilkins et al., supra note 2, at 10-11.
145. Id.
146. See Chapter 4, notes 27 and 62.

the charges appear in the case file is not necessarily the same as it would be if they were ranked by seriousness.

One way to minimize the loss of information caused by selecting charges for recording and analysis is to employ a variable that tells the number of charges of which the defendant was convicted. This was done by the CJRC researchers. In some analyses this variable proved to be a relatively good predictor of sentence length.[147]

The next decision about the sampling frame pertains to the relevant period of time from which to sample cases. The time frame must be long enough ago that all or nearly all of the cases have been disposed of, but not so long ago that significant changes in sentencing laws or practices will interfere with the researcher's ability to generalize to the present. This turned out to be problematic for the Denver guidelines construction sample, which was used by the CJRC researchers to support their conclusion that empirically based sentencing guidelines are feasible. Our independent review of the case files indicates that most of the cases included in this construction sample were decided under the previous criminal code, which was revised substantially in 1972.[148] Thus, the guidelines models were based on decisions made under one set of laws and applied to decisions made under another, quite different set of laws. We do not know whether the guidelines were materially affected, but this problem does underscore the importance of selecting an appropriate time frame.

The time during which the sampled cases were decided is important for another reason as well. Seasonal variations in crime rates have been documented.[149] There is reason to suspect that there also are seasonal variations in sentencing. For example, far fewer cases are disposed of during the summer months than at other times of the year.[150] This may make for different caseload pressures, which may affect plea bargaining and therefore sentencing. Another seasonal variation may occur just before Christmas, when the holiday spirit may influence sentencing the way it seems to influence so many other activities.

This reasoning suggests that it would be wise to sample cases from all times of the year, at least until the absence of seasonal variations has been demonstrated. Although the Denver and Newark guidelines construction samples include cases from all seasons, the Chicago construction sample included only cases in which sentencing occurred between October 25 and December 17, 1976.[151] The representativeness of the sample must for that reason be considered questionable.

147. See L. Wilkins et al., supra note 2, at 13. Cf. Tables 2-7 through 2-10, pp. __-__ (number of offenses at conviction predicts sentence length but not type of sentence).
148. Colo. Rev. Stat. §18-1-1 through 18-15-108 (1973).
149. See E. Sutherland and D. Cressey, Criminology 118-119 (10th Ed. 1978); G. Falk, The Influence of the Seasons on the Crime Rate, 43 J. Crim. L. C. and P. S. 199 (1952).
150. This is attributable in large part to the fact that many judges and lawyers take their vacations during the summer months.
151. J. Calpin et al., supra note 73.

Another question that arises in specifying the sampling frame is, What constitutes a conviction? For example, the Denver construction data included deferred judgments, which are not convictions under Colorado law.[152] In Colorado, a defendant may, with the approval of the court, enter a plea of guilty on which the judgment of conviction is withheld for up to two years contingent upon the defendant's reporting to a probation officer and avoiding additional (detected) criminal misconduct. If, at the end of two years, the defendant has lived up to the agreement, the plea is withdrawn and the charges are dismissed with prejudice. If, on the other hand, the defendant fails to report or is apprehended for subsequent crimes, the judgment of conviction can be entered and a sentence imposed.

Although deferred judgments are not technically convictions in Colorado and some other states, in practice it is clear that these procedures are used as lesser sanctions for guilty and convictable defendants.[153] Thus, their exclusion from a sample of sentencing events would in some respects be a legal fiction. Nevertheless, we believe there is good reason to exclude deferred judgments from analyses of judicial sentencing behavior where the purpose is to create sentencing guidelines. In practice, a deferred judgment cannot be obtained (in Denver, at least) without the consent of the prosecutor. Although the judge may reject an application for deferred judgment, this is rarely done in Denver. In effect, it is the prosecutor who decides whether to grant a deferred judgment.[154] The prosecutor may decide on somewhat different grounds from those the judge uses when he or she imposes a sentence. The commingling of deferred judgments and convictions may serve to reduce the explanatory power of resulting models.

Once the sampling frame has been chosen, a method of sampling must be selected. If we wish to make statistical inferences about the population of cases, sampling must be done randomly so that every case has an equal chance of being selected.[155] Where sampling was used, the CJRC researchers report that it was done randomly.[156] Although the exact procedures are not spelled out in their reports, they commonly involve the use of a random number table or a computer-generated list of quasi-randomly selected numbers.

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152. See Colo. Rev. Stat. §16-7-403 (1979 supp.). This procedure was used in Denver before the Colorado legislature authorized it by statute in 1975. See also Chapter 4, note 8.

153. See Chapter 4, notes 8, 9, and accompanying text. Some have speculated that a deferred judgment may be such a good deal that even some innocent defendants accept it.

154. See Chapter 4, note 8.

155. A random sample can be stratified so that different subpopulations are sampled in different proportions, but within each subpopulation every case must have an equal probability of selection. Moreover, if the subsamples are to be combined in analysis, they must be weighted according to the inverse of their sampling ratios. See generally the works cited in note 141 supra.

156. L. Wilkins et al., supra note 2, at 10; J. Calpin et al., supra note 73.

One of the most important sampling issues is that of sample size. In general, the larger the sample the more confidence we can place on the parameter estimates. In other words, the standard errors of our parameter estimates decrease as the sample size increases.[157] When choosing a sample size, the researcher must consider the analyses to which the data will be subjected and the resources available for data collection. Other things being equal, the requisite sample size increases as the number of variables in the analysis increases. This is because each variable uses up a degree of freedom in estimating parameters. The standard error of a statistic depends partly on the number of degrees of freedom left over.

The sample on which the Denver regressions reportedly were based, and on which the CJRC researchers relied in concluding that empirically based sentencing guidelines are feasible, proved to be too small. Before cases were deleted because of missing observations, the sample consisted of 200 cases. One popular rule of thumb holds that there should be at least 30 cases per predictor variable. In view of the number of predictor variables (as many as 14 in some analyses), this sample was inadequate for the regression analyses it was meant to sustain. With fewer than ten cases per variable, the standard error of the parameter estimates is very large. Furthermore, the variance explained (R^2) tends to be inflated because the regressions capitalize on chance variations in the sample.[158] The Chicago (485 cases) and Newark (1,250 cases) guidelines construction samples were considerably larger, and therefore did not present the same difficulty.

Missing observations. Sample size is inextricably linked to the problem of missing data. A case for which data are missing cannot contribute to the parameter estimates. Therefore, missing observations effectively reduce the sample size. The number of missing observations must be anticipated when sampling decisions are made.

There are two general strategies for deleting cases that have missing observations. According to one strategy, a case is deleted only as to those variables that have missing values. In other words, the case is used to estimate some parameters but not others. This strategy is called pairwise deletion. Although pairwise deletion maximizes information (and sample size), it suffers from one major drawback: tests of significance cannot be computed accurately because the sample size differs according to the parameter being estimated.[159]

We think that, in most situations, the preferable strategy for dealing with missing observations is listwise deletion, which results in the complete deletion of all cases having missing data for any variable

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157. See Hanushek and Jackson, at 56-65.

158. See note 62 supra.

159. See N. Nie et al., supra note 80, at 353; B. Hartel, Minimizing Error Variance Introduced by Missing Data Routines in Survey Analysis, 4 Soc. Methods and Research 459, 469 (1976).

in the analysis.[160] Thus, all parameters are estimated on the basis of the same number of cases. The disadvantage of listwise deletion is, of course, that it discards information and results in a smaller number of cases (n) being used in the analysis. Moreover, if the incidence of missing data is associated with other variables, listwise deletion may bias the sample. For example, we suspect that presentence reports are waived with disproportionately high frequency by offenders who are not released pending adjudication and who tend to have committed serious crimes and have serious prior records. We think they prefer to avoid the delay associated with presentence investigations so that they can complete the almost-inevitable term of imprisonment as soon as possible. Inasmuch as presentence reports are the exclusive source of certain data items, if this is true it suggests that samples may be biased by underrepresentation of the most serious cases. The problem is exacerbated by listwise deletion.

The guidelines researchers report that they employed listwise deletion in their multiple regression analyses.[161] According to their report, the multiple regression analyses of the Denver guidelines construction data were performed on 120 cases (i.e., all those for which information about the statutory class of the offense at conviction was available).[162] We attempted to replicate the research reported by the CJRC researchers. Starting with the same 120 cases, employing listwise deletion, and otherwise following the procedures they report, we were consistently left with fewer than 50 cases from which to estimate the Denver guidelines regression models.[163]

This occurred because of frequent missing observations in the data. The statistical routines applied to the data are straightforward and consistent in their case deletion algorithms. We applied the analytical procedures to the data in the usual manner. The discrepancy in sample sizes--between that reported by the original researchers and that reported here--might be due to the use of data estimates or other adjustments made by the original researchers.[164] But we do not find any such adjustments, or other explanations for the discrepancy, reported in the published record of the original research. Either the resulting sample size after listwise deletion was less than 50 cases and was incorrectly reported by the CJRC researchers, or the CJRC researchers did something to remedy the small sample size, in which case the remedy should have been described in their reports. By recoding some of the variables (a step the guidelines researchers do not report having taken) we were able to increase the usable sample size to slightly more than 60 cases.

Unable to establish a set of more than 65 usable cases even by recoding variables, we concluded that further attempts to fully replicate the regression equations would be fruitless. With fourteen predictor variables in the equation, the cases-to-variables ratio would be less than five-to-one, the parameter estimates would not be reliable, and the percentage variance explained statistic (R^2) would be exaggerated by capitalizing on random variation in the small amount of usable data. In short, because of problems of missing data in the already small sample of cases studied in Denver, we were unable to replicate with any assurance

of reliable results the regression equation on which the Denver guidelines models were reported to have been based.

160. See Y. Haitovsky, Missing Data in Regression Analysis, 30 Royal Stat. Soc'y J. 67 (Series B, 1968) (concluding that listwise deletion is preferable to pairwise deletion except where the proportion of missing observations is high or where the distribution of missing values is highly nonrandom).

161. L. Wilkins et al., supra note 2, at 85.

162. Id. at 13, 58, 85.

163. On the other hand, the number of cases was reduced below 30 when length of incarceration was taken as the dependent variable and cases involving sentences to probation were excluded.

164. See discussion of techniques for estimating missing data at notes 165-169 and accompanying text.

One solution to the problem of missing data is to substitute estimated data. The simplest way to do this is to replace the missing value with the mean value of all the available data. This strategy, however, results in systematic understatement of the relationships between variables.[165] Instead of using the mean value of all the available data, one can use the means of groups of cases defined by the variables. This is equivalent to the use of regression techniques to estimate missing values, which is discussed below. The use of group means systematically overstates the relationships between variables.[166] Yet another strategy is to substitute a range of values distributed around either the sample mean or group means.[167] This procedure not only biases the estimates in the manner just described; it also reduces their reliability.

A more sophisticated approach is to estimate missing data by creating regression equations that yield expected values on the basis of other variables.[168] There are computer programs available to accomplish this.[169] Although it is not without appeal, this method involves a certain amount of statistical question-begging. It assumes that the relationships in the missing data are the same as those in the available data, but that is part of the research question. We may find ourselves using estimated data to predict variables that were used to estimate the missing data. This would artificially inflate the predictive strength of the model.

The main purpose of estimating missing data is to maintain the highest possible sample size. Although under some circumstances it may be appropriate to estimate missing data, we believe that in the context of sentencing research, particularly if the research is conducted for the

165. See B. Hertel, supra note 159, at 461-465.
166. See id. at 465-468.
167. See id. at 469-471.
168. See id. at 471-472; S. Buck, A Method of Estimation of Missing Values in Multivariate Data Suitable for Use with an Electronic Computer, 22 Royal Stat. Soc'y J. 302 (Series B, 1960).
169. See, e.g., M. Brown, BMDP-77: Biomedical Computer Programs—P-Series 353-361 (1977).

purpose of making policy, the risks far outweigh the benefits. In short, there is no substitute for real data. The difficulties of obtaining accurate and complete data and the unavailability of a satisfactory method by which to substitute estimated data for real data combine to argue powerfully against the use of empirically based sentencing guidelines of the sort that is being evaluated here.

The Measurement of Variables

Until now we have concerned ourselves very little with how variables are measured, but one of the sources of error in modeling is measurement error.[170] The most sophisticated mathematical models are of little value if we are unable to measure accurately the variables included in them.

Level of measurement - There are generally said to be four levels of measurement: nominal, ordinal, interval, and ratio. Nominal-level measurement distinguishes between categories but implies no particular order. In sentencing research, nominal-level variables include sex, race, pretrial release status, method of adjudication (guilty plea, bench trial, jury trial), type of defense counsel (privately retained, privately appointed, publicly appointed), and the like. Ordinal scales imply an element of order but nothing about the magnitude of differences between categories. Injury of a victim would be an ordinal variable if the categories were no injury, injury not requiring medical treatment, injury requiring medical treatment, and death. The difference between no injury and injury not requiring medical treatment is not necessarily of the same magnitude as the difference between injury requiring medical treatment and death. As with most ordinal scales, there is no obvious, meaningful way to quantify differences.

The distinguishing feature of interval scales is that equal intervals represent equal magnitudes of difference. Ratio scales are interval scales that have a meaningful zero point. Income (measured in dollars) and sentence length (measured in months) are examples of ratio scales. In practice, interval and ratio scales rarely are distinguished. Most variables in sentencing research, however, are nominal or ordinal.

Strictly speaking, regression-based techniques, including those discussed in this chapter, assume interval-level measurement.[171] This is because regression techniques are based on the magnitude of variations from the mean (or average) value. Thus, the necessity of using categorical (nominal and ordinal) data in sentencing research, and most social research, presents a problem.

One solution to the problem of categorical data is to use statistical techniques that do not involve assumptions about interval properties. In recent years there has been a great deal of progress in the development

170. See Hanushek and Jackson, at 8-11, 27, 59. See generally J. Mueller et al., supra note 73, at 10-32; H. Blalock, supra note 38, at 11-26; S. Stevens, Mathematics, Measurement, and Psychophysics, in S. Stevens, Handbook of Experimental Psychology 1-49 (1951); H. Blalock, Measurement in the Social Sciences (1974).
171. See S. Stevens, supra note 170.

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of sophisticated modeling techniques for categorical data.[172] A few researchers have applied these techniques to sentencing research,[173] but the complexity of the techniques has prevented them from becoming popular thus far. Moreover, the analysis of categorical data qua categorical data requires classification and subclassification of the data into groups representing different combinations of attributes. This process results in diminished group sizes, which in turn makes statistical inference more difficult. The upshot is that more data are required for these techniques than are required for regression-based techniques, which employ statistical adjustments rather than cross-classification. Perhaps the use of advanced modeling techniques for categorical data eventually will prevail in sentencing research, but for the time being their complexity and expense preclude widespread application.

Regression-based techniques can accommodate categorical data in two different ways. One way is simply to violate the assumption of interval measurement by treating ordinal data as interval. There is debate among statisticians about the acceptability of this solution.[174] Empirical evidence suggests that, in practice, violation of this assumption makes no important difference in the results.[175] Regression is said to be robust in this regard; that is, it can withstand violation of the assumption of interval data without serious detriment to the accuracy of the results. The debate is complex and controversial. We believe,

172. See H. Reynolds, The Analysis of Cross-classifications (1977); S. Fienberg, The Analysis of Cross-Classified Categorical Data (1977); Y. Bishop, S. Fienberg, and P. Holland, Discrete Multivariate Analysis: Theory and Practice (1975); S. Haberman, Analysis of Qualitative Data (1978); L. Goodman, Analyzing Qualitative/Categorical Data (1978). Ordinal analogs to regression and path analysis also have been developed. See R. Morris, Multiple Correlation and Ordinally Scaled Data, 48 Social Forces 299 (1970); R. Hawkes, The Multivariate Analysis of Ordinal Measures, 76 Amer. J. Soc. 908 (1971); T. Wilson, On Interpreting Ordinal Analogies to Multiple Regression and Path Analysis, 53 Social Forces 196 (1974); R. Smith Continuities in Ordinal Path Analysis, 53 Social Forces 200 (1974); T. Wilson, Reply to Somers and Smith, 53 Social Forces 247 (1974); J. Kim, Multivariate Analysis of Ordinal Variables, 81 Amer. J. Soc. 261 (1975).

173. See, e.g., P. Burke and A. Turk, supra note 9.

174. See B. Baker, C. Hardyck, and L. Petrinovich, Weak Measurements vs. Strong Statistics: A Critique of S. S. Stevens' Proscriptions on Statistics, 26 Educ. and Psych. Measurement 291 (1966); S. Labovitz, The Assignment of Numbers to Rank Order Categories, 35 Amer. Soc. Rev. 515 (1970); R. Boyle, Path Analysis and Ordinal Data, in H. Blalock, Causal Models in the Social Sciences (1971); T. Wilson, Critique of Ordinal Variables, in H. Blalock, supra this note; R. Henkel, Part-Whole Correlations and the Treatment of Ordinal and Quasi-Interval Data as Interval, 18 Pac. Soc. Rev. 3 (1975); S. Labovitz, Comment to Henkel's Paper, 18 Pac. Soc. Rev. 27 (1975); R. Henkel, Reply to Labovitz's Paper, 18 Pac. Soc. Rev. 37 (1975); H. Reynolds, supra note 172, at 98-100.

175. See H. Reynolds, supra note 172, at 98-100.

however, that so long as the researcher is wary of grossly unequal-interval measures, the advantages (power, simplicity, and economy) of regression techniques outweigh the risks of erroneous results. Nevertheless, it must be recognized that this is a subject on which knowledgeable researchers disagree.

The second method of accommodating categorical data in regression analysis is the use of so-called dummy variables.[176] A dummy variable is simply a dichotomous variable that denotes group membership. For example, we can dummy-code race by assigning a "1" to blacks and a "0" to non-blacks. If we wish to distinguish among more than two groups, we can use additional dummy variables. These variables can be used in regression analysis much like true interval variables to estimate the effects of group membership on the dependent variable. This is a widely used and accepted solution to the problem of using categorical data in regression analysis. It is even possible to dummy-code ordinal variables in such a way that their ordinal properties are preserved.[177]

It seems to us that in sentencing research of the kind being reviewed here, ordinal data should be treated as interval for regression purposes whenever there is no particular reason to suspect that they fail to approximate interval qualities, especially if there are many categories. If, on the other hand, the categories cannot be ordered, or there are too few categories, or grossly unequal differences are suspected between ordered categories, dummy coding should be used. The guidelines researchers freely used ordinal data as interval and dummy-coded various dichotomies.[178] In our opinion, these solutions were appropriate.

The measurement of sentence. Naturally, proper measurement of the dependent variable is critical in a study of sentencing practices. This is not as simple as it might appear at first glance.

Sentences of all kinds can be thought of as having different degrees of severity. In this sense, we can measure a single dependent variable: sentence severity. In an analysis that employs some measure of sentence severity as the dependent variable, it is the degree of severity, and not the actual sentence, that is predicted. All sentences are assumed to vary along a single dimension.

176. The use of dummy variables and similar methods of coding categorical data in regression analysis are discussed in F. Kerlinger and E. Pedhazur, supra note 45, at 116-151; J. Cohen and P. Cohen, supra note 62, at 171-211; D. Suits, Use of Dummy Variables in Regression Equations, 52 Amer. Stat. Assoc. J. 548 (1957); J. Cohen, Multiple Regression as a General Data-Analytic System, 70 Psych. Bull. 426 (1968).

177. See M. Lyons, Techniques for Using Ordinal Measures in Regression and Path Analysis, in G. Bohrnstedt and T. Carter, Sociological Methodology 1971.

178. See L. Wilkins et al., supra note 2, at 13 and note 31.

Several researchers have developed and used scales of sentence severity.[179] The most widely used and adapted sentence severity scale is the one developed by the Administrative Office of the United States Courts.[180] All involve the arbitrary assignment of numerical values to suspended sentences, probationary sentences, prison sentences of various lengths, and so forth, according to the researcher's conception of their relative severity. Validation of these scales presents serious difficulties, but this has not prevented their use.

If the object is to model the sentencing process for policy making purposes, we need to predict actual sentences and not just the relative severity of sentences. If we wish to use only one dependent variable, we must measure sentences by the length of incarceration and assign zeros to nonincarcerative sentences. The alternative is to use two dependent variables: type of sentence and length of incarceration. The disadvantage of the bifurcated approach to measuring sentences is that it doubles the analytical work because all models must be estimated once with each dependent variable.

The CJRC researchers discussed at some length the bifurcated nature of the sentencing decision.[181] They argued that the judge first decides whether to incarcerate and then decides how long to incarcerate. The regression models, however, appear to have been constructed in disregard of their argument about the bifurcated nature of the sentencing decision. Although some models used only the decision whether to incarcerate as the dependent variable, when length of incarceration was predicted, all nonincarcerative sentences were assigned zeros and included in the analysis.[182] The guidelines regression models predicted both decisions using the same variables and the same weights.

If sentencing is essentially a bifurcated decision, it is quite possible that the two decisions are determined by different factors or by the same factors weighted differently. Speculation about how judges sentence suggests that this may be true. For example, although a judge may be more inclined to incarcerate an offender who is unemployed than one who currently has a job and supports his family, once having decided to incarcerate both offenders the judge is unlikely to be influenced by their employment status when deciding what term of imprisonment to impose. Indeed, there is no particular reason to think that judges consider the same factors weighted equally when they decide whether to incarcerate as they do when they decide how long to incarcerate.

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179. See S. Diamond and H. Zeisel, Sentencing Councils: A Study of Sentence Disparity and Its Reduction, 43 U. Chi. L. Rev. 109, 120-123 (1975); L. Tiffany et al., supra note 9, at 370-371; L. Sutton, Federal Criminal Sentencing: Perspectives of Analysis and a Design for Research 21-23 (Utilization of Criminal Justice Statistics Project, Analytic Report 16, U.S. Department of Justice, 1978).

180. See M. Hindelang, C. Dunn, A. Aumick, and L. Sutton, Sourcebook of Criminal Justice Statistics--1974 401 (1975).

181. L. Wilkins et al., supra note 2, at xvi, 1.

182. See id. at note 32.

In an attempt to test this hypothesis, we reanalyzed the Denver guidelines construction data. For one analysis we used the decision whether to incarcerate, coded dichotomously (0 = nonincarceration; 1 = incarceration), as the dependent variable. In the second analysis, we used length of incarceration, coded as the number of months until parole eligibility, as the dependent variable.[183] Cases in which no sentence to incarceration was imposed were excluded from the second analysis. The results indicated that the two decisions are predicted by different sets of variables. Those few variables that predicted both decisions were weighted differently for each decision. Nonetheless, because of the small sample sizes, we were doubtful of the reliability of these findings.

In order to test the notion that sentencing is a bifurcated decision, we conducted regression analyses using our own Denver data from the impact study.[184] We first selected the variables that best predicted the decision whether to incarcerate. Both decisions then were regressed on these predictor variables in two separate regressions. Similarly, we identified the variables that best predicted length of incarceration (excluding nonincarceration sentences) and regressed both decisions on them in two separate regressions. The results are reported in Tables 2-7 through 2-10.

From Tables 2-7 through 2-10 it can be seen that the best predictors of type of sentence are not the same as those for length of sentence. When type of sentence is regressed on the best predictors of length of incarceration, the variance explained ($R^2 = 0.274$) is lower than that obtained by regressing type of sentence on its own best predictors ($R^2 = 0.313$). When length of sentence is regressed on the best predictors of type of sentence, the variance explained ($R^2 = 0.135$) is lower than that obtained by regressing length of incarceration on its own best predictors ($R^2 = 0.208$).

Different factors seem to influence the two decisions with different weights. For example, the five best predictors of the decision whether to incarcerate are the offender's most severe prior sentence as an adult ($\beta = 0.190$), the statutory class of the offense at information ($\beta = 0.172$), whether the offender was on probation or parole at the time of the offense ($\beta = 0.133$), whether the offender was released pending the instant adjudication ($\beta = 0.112$), and whether the offender was employed at the time of sentencing ($\beta = -0.104$). The five best predictors of length of incarceration are the statutory class of the offense at information ($\beta = 0.412$), charge reductions ($\beta = 0.184$), whether the offender was tried by jury ($\beta = 0.149$), whether a victim was injured or placed in jeopardy of injury ($\beta = 0.133$), and the number of counts of which the offender was convicted ($\beta = 0.072$).

With the exception of the statutory class of the offense, the five best predictors of the decision whether to incarcerate pertain to the offender's prior record (prior sentence) and demonstrated ability to

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183. See Chapter 3, note 44.

184. See note 43 supra.

Table 2-7
Regression of Type of Sentence on
Best Predictors of
Type of Sentence
Denver

Variable	b	Beta	Standard Error of b	F	Probability
Statutory Class	0.522	0.172	0.005	93.277	0.001
Prior Adult Sentence	0.789	0.190	0.009	74.284	0.001
Juvenile Record	0.378	0.101	0.007	28.708	0.001
Revocations	0.520	0.084	0.012	17.440	0.001
Liberty Status	0.134	0.133	0.019	49.838	0.001
Job Status	-0.102	-0.104	0.019	29.797	0.001
Age	-0.258	-0.045	0.001	5.080	0.024
Injury/Jeopardy	0.570	0.100	0.010	31.804	0.001
Retained Counsel	-0.984	-0.087	0.021	21.593	0.001
Appointed Counsel	0.234	0.013	0.031	0.562	0.454
Sex	0.149	0.089	0.029	26.065	0.001
Bail Status	0.124	0.112	0.022	31.351	0.001
Constant	-0.184				

n = 2390
R^2 = 0.313
Adjusted R^2 = 0.309
F = 90.146

Table 2-8
Regression of Type of Sentence
on Best Predictors of Length of Incarceration
Denver

Variable	b	Beta	Standard Error of b	F	Probability
Statutory Class	0.660	0.215	0.010	40.888	0.001
Charge Reduction	0.157	0.045	0.011	1.874	0.171
Jury Trial	0.663	0.034	0.039	2.932	0.087
No. Counts convicted of	0.415	0.036	0.022	3.598	0.058
Age	-0.646	-0.112	0.001	31.660	0.001
Prior Adult Sentence	0.122	0.292	0.009	198.723	0.001
Liberty Status	0.167	0.165	0.020	69.732	0.001
Injury/Jeopardy	0.376	0.065	0.011	11.357	0.008
Bail Status	0.191	0.170	0.022	73.187	0.001
Constant	-0.541				

n = 2202
R^2 = 0.274
Adjusted R^2 = 0.271
F = 91.759

Table 2-9
Regression of Length of Incarceration
on Best Predictors of Length of Incarceration
Denver

Variable	b	Beta	Standard Error of b	F	Probability
Statutory Class	8.875	0.412	0.818	117.758	0.001
Charge Reduction	4.615	0.184	0.926	24.818	0.001
Jury Trial	16.049	0.149	2.467	42.318	0.001
Number Counts Convicted of	4.470	0.072	1.359	10.825	0.001
Age	0.463	0.012	0.093	0.247	0.619
Prior Adult Sentence	1.481	0.052	0.671	4.866	0.028
Liberty Status	1.681	0.026	1.463	1.320	0.251
Injury/Jeopardy	4.895	0.133	0.856	32.678	0.001
Bail Status	4.303	0.064	1.492	8.321	0.004
Constant	-50.403				

n = 1862
R^2 = 0.208
Adjusted R^2 = 0.204
F = 53.984

Table 2-10
Regression of Length of Incarceration on
Best Predictors of Type of Sentence
Denver

Variable	b	Beta	Standard Error of b	F	Probability
Statutory Class	5.557	0.275	0.541	105.597	0.001
Prior Adult Sentence	.977	0.038	0.832	1.380	0.240
Juvenile Record	0.891	0.039	0.627	2.022	0.155
Revocations	1.258	0.037	1.016	1.533	0.216
Liberty Status	-0.662	-0.011	1.664	0.158	0.691
Job Status	-4.028	-0.064	1.790	5.064	0.025
Age	0.997	0.027	0.115	0.754	0.385
Injury/Jeopardy	5.686	0.167	0.913	38.811	0.001
Retained Counsel	0.849	0.011	2.171	0.153	0.696
Appointed Counsel	0.498	0.005	2.634	0.036	0.850
Sex	-1.984	-0.015	3.376	0.345	0.557
Bail Status	2.022	0.032	1.846	1.200	0.274
Constant	-18.647				

n = 1353
R^2 = 0.135
Adjusted R^2 = 0.127
F = 17.356

function in society (liberty status, bail status, and employment status), whereas the five best predictors of length of incarceration pertain to the seriousness of the crime (statutory class and injury) and plea bargaining (charge reduction, jury trial, and number of counts convicted of). Moreover, the statutory class of the offense at information is far more important to the length decision than to the decision whether to incarcerate.

These results are interesting in their own right, but they also demonstrate that the factors that influence the decision whether to incarcerate are different from those that influence the length decision. Apparently, judges do not make both decisions according to the same criteria. The Denver guidelines' use of a single set of variables to determine both decisions must be regarded as a serious flaw.[185]

Previous research has pointed to a similar phenomenon in the federal system.[186] The implications are clear. We cannot assume that sentencing is a unitary decision. The use of a one-dimensional scale of sentence severity as the sole dependent variable in sentencing research is of questionable validity. Sentencing does indeed appear to be a bifurcated decision, and should be measured as such.

The guidelines developed in Chicago and Newark contain this same basic flaw. In each case, the same set of variables, with the same weights, is used to determine both the decision whether to incarcerate and the length of incarceration. While we did not conduct any analyses of the data in Chicago and Newark to confirm, as we did for Denver and Philadelphia, that the two decisions are distinct, we are confident from our other findings and from theoretical considerations that such analyses would disclose the same phenomenon in those two jurisdictions. At the very least, at this stage in our knowledge of the sentencing decision, we believe that sentencing researchers should presume a bifurcated decision, unless the evidence in a local jurisdiction warrants a contrary conclusion.

185. The CJRC researchers report that, "[a]t the direction of the Steering and Policy Committee, none of the preliminary models were based on the concept of the sentencing decision as a bifurcated decision-making process." L. Wilkins et al., supra note 2, at 14. Our criticism applies regardless of who decided to treat the two decisions as if they were made in the same way.

186. See L. Sutton, Variations in Federal Criminal Sentences: A Statistical Assessment at the National Level 10, 49-51 (Utilization of Criminal Justice Statistics Project, Analytic Report 17, 1978) (type of sentence is determined by offender-related variables whereas length of sentence is determined by offense-related variables.) Ironically, the guidelines researchers seem to have possessed evidence of the same phenomenon. They report that "offender variables . . . have a greater influence on the sentencing decision than do offense seriousness variables, unless the offense is an extremely serious one, in which case the . . . offense variables . . . have a greater impact." L. Wilkins, et al., supra note 2, at xvii. See also id. at 23. Of course, the most serious cases result in incarceration. Therefore, among those cases, the only sentence differences pertain to the length of incarceration.

The measurement of type of sentence alone is not without problems. Throughout this discussion, we have assumed that there are only two types of sentences: incarceration and nonincarceration. In reality, there are many different kinds of sentences. These may include suspended imposition with or without supervision, suspended execution with or without supervision, sentence to probation, fine, community corrections, intermittent jail terms, work release programs, split sentences, special rehabilitation programs, jail terms, reformatory terms, and penitentiary terms.

Distinguishing among several different types of sentences is difficult statistically. There are two basic alternatives. The first is to rate each type of sentence according to its severity and use the resulting scale as the dependent variable in regression analysis. The

difficulties of using a severity scale in this way are similar to those outlined above. Principally, they are that the researcher is no longer predicting the actual sentence but an assumed property of the actual sentence; that the resulting scale may not have interval properties; and that the scale is difficult to validate.

The second way to preserve the distinctions among several sentence types is to treat as a group the cases in which each type of sentence was imposed. The task then becomes one of finding the best predictors of group membership. This is what discriminant function analysis does.[187] Of course, by using the discriminant function the researcher discards information about the relative severity of each type of sentence. In effect, type of sentence is treated as a nominal variable. Nevertheless, discriminant function analysis may be a useful technique by which to model the decision about type of sentence.[188]

If we are willing to sacrifice the distinctions among the various types of sentences and satisfy ourselves with classifying all sentences as either incarcerative or nonincarcerative, we can use regression analysis to model the decision about type of sentence. It is easy enough to code the decision whether to incarcerate as a dummy variable (0 = nonincarceration; 1 = incarceration). The predictions that result from regressing this dummy variable on the predictor variables can be interpreted as probabilities;[189] in this case, the probability of incarceration. This was the approach used by the guidelines researchers,[190] and we have used it in the analyses presented in this report.

Unfortunately, the use of a dichotomous dependent variable creates a statistical problem.[191] Linear regression of a dichotomous dependent variable may result in predictions greater than 1.0 or less than zero (i.e., negative numbers). These, of course, cannot be interpreted as

187. See P. Hoel, supra note 38, at 181-186; J. Johnston, supra note 52, at 334-340; J. Van de Geer, Introduction to Multivariate Analysis for the Social Sciences 243-272 (1971); R. Bibb and D. Roncek, Investigating Group Differences: An Explication of the Sociological Potential of Discriminant Analysis, 4 Soc. Methods and Research 349 (1976).

188. We note in passing that the CJRC researchers used discriminant function analysis in developing the Chicago and Newark guidelines. Apparently, the discriminant function was employed primarily "as an additional check" on the results of the multiple regression analysis of the decision whether to incarcerate. No results of the discriminant function analysis are reported. See J. Calpin et al., supra note 73. Inasmuch as a two-group discriminant function is equivalent to multiple regression with a dichotomous dependent variable, this was more like a redundancy than a check. See R. Bibb and D. Roncek, supra note 187, at 370.

189. J. Neter and W. Wasserman, supra note 62, at 321.

190. J. Calpin et al., supra note 73.

191. Hanushek and Jackson, at 180-186; J. Neter and W. Wasserman, supra note 62, at 322-326.

probabilities, since probabilities cannot be negative or exceed 1.0. Therefore, if individual predictions must be interpreted, or if we seek the best-fitting model, we must find a way to restrict the predictions to the range between zero and one. This can be done by using curvilinear models such as the logit and probit models,[192] but these entail greater complexity than does linear regression.[193] Moreover, these models require the researcher to break the sample into groups that correspond to combinations of values of the predictor variables, with the resulting need for larger samples.

We have argued that in order for statistical models to serve as the basis for sentencing policy, they must constitute accurate explanations of the sentencing process. If we want to explain a dichotomous decision such as the decision whether to incarcerate, linear regression apparently will not provide the accuracy needed for policy making purposes.[194] It will be necessary to investigate other kinds of models, but these models will be more complex and therefore more costly.

Conclusion

In this chapter, we have reviewed the conceptual and practical problems of creating statistical models of past judicial sentencing behavior and using those models to structure judicial sentencing discretion. We have reported and criticized the methods by which sentencing guidelines were developed in Denver, Chicago, and Newark. In this concluding section, we summarize the most important problems in the execution of the guidelines research, suggest some implications of our findings, and identify what we believe to be some fundamental flaws in the concept of empirically based sentencing guidelines.

192. For discussions of the logit and probit models, see the works cited in note 137 supra.

193. Not only must one transform the dependent variable by, for example, taking the natural logarithm of the conditional odds ratio; one must then use weighted rather than ordinary least squares solutions to correct for the problem of heteroscedasticity. See Hanushek and Jackson, at 187-210; J. Neter and W. Wasserman, supra note 62, at 326-335; H. Theil, supra note 101, at 628-635.

194. We distinguish here, as we must, between the explanatory accuracy needed for empirical models that form the basis of sentencing policy, and empirical models that provide the controls needed to evaluate the impact of sentencing guidelines. The demands of the former are greater than those of the latter, even though one would prefer always to use the best methods possible. It would be less than candid not to share our sense of frustration at our lack of opportunity to develop more suitable models. To our regret, the sponsor of this research forbade us to expend grant funds developing alternative models. Nonetheless, we are convinced that for the purposes of measuring the impact of sentencing guidelines, our linear regression models suffice.

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The sentencing guidelines research was intended to identify the factors that influence judges' sentencing decisions, and to determine the relative importance of those factors. On the basis of these determinations, the researchers proposed to create models representing the average sentences for various combinations of offense- and offender-related characteristics. These models, taking the form of grids and scoring sheets, were to be consulted by judges prior to the imposition of sentences. This research was intended to guide judges in deciding whether to send human beings to prison and, if so, for how long. It is our premise that research intended to guide decisions about basic human liberties should be performed with the utmost care and thoroughness.

Our criticisms of the execution of the sentencing guidelines research can be summarized in five observations, which do not apply with equal force to each of the three sites, and a general conclusion.

First, we were unable to replicate the regression equations that reportedly formed the basis of the Denver sentencing guidelines feasibility research in a way that offers any assurance of reliable results. In this elementary sense, we were unable to confirm the Denver guidelines' empirical basis.

Second, the Denver, Chicago, and Newark guidelines fail to distinguish adequately between the decision whether to incarcerate and the decision concerning length of incarceration. The guidelines use the same variables with the same weights to determine both decisions. Our research shows that those two decisions are determined by different sets of factors.

Third, we have shown that, with one exception,[195] none of the regressions from which guidelines models reportedly were derived were able to account for much more than one-third of the sentencing variation in the samples of cases on which they were based. This means that even after one has considered the (linear) effects of the best predictors that can be identified, at least two-thirds of the variation in criminal sentences remains unexplained. This strongly implies that these regression models provide insufficient understanding of the factors that influence judicial sentencing and the precise way in which they figure in the calculus.

Fourth, our analyses suggest that changes in the research methods or the samples of cases would produce substantial differences in the factors that would be included in the guidelines and the weights that they would be assigned.

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195. See pages 66-67 for our reasons for rejecting the one reported R^2 greater than .30.

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Fifth, we have noted instances in which the guidelines researchers used methods that we consider inappropriate or less than optimal. We have made no effort to determine why the best available methods were not used. Although there are many possible explanations, such as a lack of time or money, or restrictions imposed by the judges or the funding agency, we regard these considerations as irrelevant to our inquiry.

These observations leave us with little confidence that the Denver, Chicago, and Newark guidelines actually represent the implicit policies or past practices of the judges in those three jurisdictions. Moreover, our analysis brings into question the conceptual basis of the guidelines research.

There appear to be two fundamental flaws in the conception of empirically based sentencing guidelines. The first, which we have not explored here in any depth, is a failure to distinguish adequately between descriptive and normative activities--between what is and what ought to be. It is not clear why past sentencing policy, if it could be discovered by means of empirical research, ought to serve as the basis of future sentencing policy. Furthermore, in this kind of enterprise, researchers unavoidably must make many research decisions that have significant policy implications. Consequently, policy questions, which properly are decided by policy makers, are converted into research questions, which are decided by the researchers according to the needs of empirical investigation. The apparently inevitable, if unintended, consequence of deriving sentencing policy from statistical models of past sentencing practices is a shift in the actual control over policy decisions from public authorities to researchers.

The second fundamental flaw in the conception of empirically based sentencing guidelines is a failure to distinguish adequately between the predictive and explanatory aspects of the research that forms the basis of the guidelines. This confusion manifests itself in several ways. The particular statistical procedures that were chosen by the guidelines researchers, although perhaps defensible for purely predictive purposes, are incapable of producing an adequate explanatory account of the sentencing process. The decision to aggregate data drawn from cases decided by different judges, which risks generating meaningless averages that are normatively unjustifiable, also reflects undue emphasis on prediction rather than explanation. Another manifestation is the reliance on data from official case files, which are necessarily incomplete and sometimes inaccurate. Conceived in this way, the most that the guidelines research could have achieved was a predictive device with little explanatory value. Without a means of identifying the

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factors that judges actually consider (and presumably ought to consider) in making sentencing decisions, empirically based sentencing guidelines offer no hope of achieving even the relatively modest goal of reducing disparity in sentences. It cannot plausibly be said that disparity is reduced by the reduction of sentencing variation within categories of cases defined by offense- and offender-related criteria that do not correspond to the factors that judges actually (or ought to) consider.

The point of this criticism is not that more suitable procedures could or should have been chosen in order to achieve the immediate aims of the guidelines researchers. Although more appropriate statistical modeling techniques could have been chosen, there is little reason to think that these would have provided the depth of understanding on which sound policy must be based. The point is that neither the techniques used nor any other available techniques seem capable of performing the task required of them in this context. In our opinion, it is not feasible to create guidelines reflecting rational public policy by means of statistical analysis of prior sentencing decisions. We believe that a wholly new approach to the problem of structuring judicial sentencing discretion is required.

It might be objected at this point that, after all, the purpose of the guidelines research was simply to get judges to adopt guidelines, however imperfect those guidelines might be, and that the empirical research was only a means to that end.[196] But presumably the judges adopted the guidelines because they believed them to embody their past policies. It is difficult to imagine their acceptance of these guidelines in the absence of such a belief. This objection, therefore, rests ultimately on the unacceptable substitution of the judgment of researchers for that of the judiciary. Moreover, moral and political concerns aside, the evidence suggests that the objection does not succeed even on its own terms. That is, the guidelines did not become a vehicle for judicially controlled reform of the sentencing process. For

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the very reason that judges relied on the guidelines' supposed empirical basis and methodological sophistication in deciding to adopt them, the judges did not regard the guidelines as a way of expressing their own normative views about sentencing and how it should be reformed. The guidelines remained substantially unchanged from their original formulation. With only minor exceptions, the planned periodic revisions of the guidelines did not occur. By and large, the judges regarded the guidelines with either skepticism, incomprehension, or blind faith.

Having argued against empirically based sentencing guidelines on conceptual grounds, we are compelled to add a note of caution in the event that similar projects are undertaken in the future. In observing that the guidelines researchers used methods that we consider inappropriate or less than optimal, we adverted to the possibility--which we did not explore--that lack of time or money, or restrictions imposed by the judges or the funding agency, may have contributed to the choise of those methods. In ordinary circumstances, where research is undertaken primarily for the purpose of advancing theoretical knowledge and only secondarily, if at all, for the purpose of affecting public policy, such constraints are frustrating but not necessarily harmful; at worst, they result in incorrect conclusions. On the other hand, where research is undertaken for the immediate purpose of creating public policy, these constraints may produce not only incorrect conclusions but misguided policy as well. If research is to form the basis of sentencing policy, it should be painstakingly thorough. Otherwise, a different method of formulating policy would be preferable.

It is important for government policymakers to recognize that research of the sort described here is costly, time consuming, and unavailing of any assurance about its findings or results. Pressures to conduct research without sufficient resources or time, or to reach particular findings and conclusions needed to further the larger programmatic goals, necessarily compromise the scientific integrity of the research. But where important public policies are at stake, the standards of quality should, if anything, be even higher than those applied to ordinary research that is undertaken for the purpose of advancing theoretical knowledge. Thus, there is a tension between the demands of science and practical necessity.

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196. It has been suggested that a similar approach--empirically based guidelines--might be applied to other kinds of decisions such as the determination of bail and the allocation of scarce medical resources. D. Gottfredson et al., supra note 2, at 151-164. These applications appear to involve the same conceptual difficulties as do sentencing guidelines. In any event, the precautions suggested here would seem to apply with equal force to efforts to structure other discretionary decisions by means of empirically derived guidelines.

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We think this necessitates special precautions. First, guidelines research of the kind evaluated here[195] should not be undertaken unless the responsible officials are able and willing to devote sufficient resources and time to the effort. Second, those officials must be willing to accept negative results, even if it means that the larger policymaking program has to be modified or abandoned. More importantly, they must communicate that willingness to the researchers. Finally, before the implementation of empirically based guidelines, the research should be subjected to the critical scrutiny of independent experts to assure that the policymakers are fully apprised of its possible weaknesses. This procedure necessarily will cause additional expense, delay, and in some instances controversy.

The special burdens of this kind of enterprise fall on the shoulders of the research community as well as the government. The tradition of self-imposed discipline in the research community, which is a correlative of academic freedom, is all the more important where the immediate purpose of research is the making of important public policy. Researchers should take care not to participate in such research unless the amounts of time and resources allocated to the effort appear to be adequate, and unless the available theoretical knowledge and research techniques offer reasonable hope of success. They ought also to insist that the policymakers they are assisting take the time and effort necessary to understand the research they are performing and, most importantly, the limitations of the techniques used and the implications of those limitations for substantive interpretation of the results.

Let us be clear about what we have not said. Nothing we have said rebukes the idea of structuring judicial sentencing discretion by means of guidelines that take account of factors in addition to the crime of which the offender is convicted. Unfettered discretion is the antithesis of law, and the need for law is especially acute where human liberties are at stake. Moreover, no matter what justifications are offered for the imposition of penal sanctions, it is appropriate for the sentencing judge to consider facts in addition to the legal category of the offense at conviction. Although the role of actuarial predictive devices is the subject of continuing and well-taken criticism, we do not necessarily take issue with the development and use of such devices as aids to decision making, so long as they are properly validated, use predictors that bear a plausible if not demonstrable causal relationship to the predicted events, and seek to predict a fact on which the decision is predicated rather than the outcome of the decision itself. To this extent, the innovative work of Gottfredson and Wilkins remains a paragon of good sense and rationality.

Neither do we disagree entirely with the use of empirical research results to inform policy decisions about sentencing. The role of empirical research, however, must be far more modest than that urged by the guidelines researchers. Where the sentencing process is concerned, modeling is in its nascent stage. Existing models are grossly deficient as explanations of judicial sentencing behavior. At this point, they raise more questions than they answer. Moreover, the principal role of even the most accurate models should be limited to informing those who make sentencing policy whether their decisions constitute departures from past practices and, if so, the likely consequences of the departures.

Finally, we do not necessarily disagree with the idea that sentencing guidelines should be judicially adopted rather than legislated or administratively promulgated. In our opinion, sentencing policy should be the product of deliberate choices made by a group of knowledgeable persons who have a public trust, and should be subject to traditional procedural protections. Although the administrative model offers some obvious advantages, there is no reason why workable and fair guidelines for sentencing could not be adopted by the judiciary under its rulemaking authority. A new approach, however, is imperative.

Chapter 3

The Effects of Sentencing Guidelines on the Exercise of Judicial Sentencing Discretion

Sentencing guidelines are an attempt to structure judicial sentencing discretion.[1] In order to reduce disparity in sentences, guidelines must influence the way in which judges exercise their discretion. In this chapter we examine the impact of sentencing guidelines on the exercise of judicial sentencing discretion. We begin by looking at the degree of compliance with sentencing guidelines. In the absence of substantial compliance, there is no reason to think that sentencing guidelines will accomplish their purpose. Next we ask whether in fact sentencing guidelines reduced disparity in sentences. Finally, we consider the possibility that sentencing guidelines caused an increase or a decrease in the overall severity of sentences, quite apart from their effect on disparity.

As we shall see, there is scant evidence of any change in sentencing that can be attributed to the use of sentencing guidelines. Sentences in Denver and Philadelphia were no more consistent with the guidelines after their implementation than before. Unexplained variation in sentences did not decrease. Racial and sexual disparities were not reduced. The average severity of sentences neither increased nor decreased once the relevant variables were controlled.

Many of the issues addressed in Chapter 2 surface again in this chapter. Once more we face the problem of measuring sentence severity. Again we find it necessary to treat sentencing as a bifurcated decision. Linear regression models are used to study the dichotomous decision whether to incarcerate, despite the possibility that curvilinear models (e.g., logit or probit) might provide more realistic representations of the probability of incarceration. We have by no means solved all of the problems detailed in Chapter 2, but our task is to measure impact, not to make policy. By using multiple measures, especially multiple measures of disparity, we reduce the likelihood of erroneous conclusions predicated on inaccurate measures. Each result individually should be regarded cautiously, but where the results of different analyses point to the same conclusion, we can be more confident.

Compliance

Sentencing guidelines are unlikely to affect the exercise of judicial discretion if judges ignore them. In Denver, Chicago, Philadelphia, and Newark, sentencing guidelines were adopted voluntarily by the local judiciaries. In Denver the criminal division judges agreed among themselves

1. See pp. 4-6, supra.

to use the sentencing guidelines. In Philadelphia the President Judge, who is elected by his colleagues, requested all judges handling criminal cases to use the guidelines, which had been developed and approved by a committee of judges. In Chicago the Presiding Judge of the Criminal Division, with virtually no formal involvement of other Chicago judges in the decision, distributed guidelines and requested cooperation. In none of the sites was any formal sanction for failure to comply authorized, threatened, or invoked, despite the fact that some judges openly refused to consult the guidelines and complete the attendant paperwork. The first order of business, then, is to investigate the degree of compliance with sentencing guidelines.

There are two kinds of compliance, which we shall refer to as procedural and substantive compliance. In order to comply with guidelines in the procedural sense, the judge must consult the guidelines, impose a sentence, and, if the sentence is different from that specified by the guidelines, justify the departure in writing.[2] There were no restrictions placed on the reasons for departing from the guidelines, although it may be assumed that they were not intended to include nonsensical or clearly illegitimate reasons.[3]

Whereas all judges were expected to follow the guideline procedures, they were not expected always to impose the sentences specified by the guidelines. To the contrary, judges were instructed that the sentencing guidelines had been constructed so that they would be appropriate only 80 to 90 percent of the time. There was a clear expectation that judges would depart from the substance of the guidelines in 10 to 20 percent of the cases. According to the researchers at the Criminal Justice Research Center, substantive compliance in excess of 80 or 90 percent would indicate judges' failure to recognize exceptional cases.[4]

In its fullest sense, the term "compliance" implies that a judge is motivated, at least in part, by the guidelines. Of course, the mere fact that a particular sentence is within the range specified by the guidelines does not prove that the judge chose that sentence because of the guidelines, especially when the guidelines were designed to accommodate previous sentencing practices. Indeed, even if judges ignored the guidelines we would expect that most sentences would fall within the ranges specified by the guidelines.

Clearly we cannot monitor judges' mental states. Rather, we can observe only the end result. Compliance must, therefore, be defined

2. L. Wilkins, J. Kress, D. Gottfredson, J. Calpin, and A. Gelman, Sentencing Guidelines: Structuring Judicial Discretion 29 (1978) [hereinafter cited as Wilkins et al.].

3. Id. The use of reasons that duplicate guidelines criteria raises interesting questions. Implicitly it indicates disagreement with the guidelines rather than recognition of an exceptional case requiring departure from the guidelines. See p. 109.

4. Wilkins et al., supra note 2, at 28; cf. D. Gottfredson, L. Wilkins, and P. Hoffman, Guidelines for Parole and Sentencing 133-134 (1978).

operationally as congruence between the sentence recommended by the guidelines and the sentence actually imposed by the judge. In other words, a judge is said to have complied with the guidelines in the substantive sense if a sentence that did not depart from the guidelines, regardless of whether the judge consulted or was influenced by the guidelines. On the other hand, a judge is said to have complied with guidelines in a procedural sense if either (1) the sentence actually imposed falls within the range provided by the guidelines, or (2) the sentence actually imposed fell outside of the guideline range and the judge gave a written justification for the departure.

Without any direct way to measure the extent to which guidelines motivated judges we must make inferences based on compliance as we have defined it operationally. But measures of compliance are ambiguous. Agreement between the guidelines and actual sentences may be attributable either to the guiding effects of the guidelines or to their predictive power. One way to approach this problem is to ask how many sentences would have fallen outside of the guideline ranges but for the existence of the guidelines. All other things being equal, if judges consciously adhere to the guidelines we would expect fewer sentences to fall outside the guidelines after they are implemented than before.

The CJRC researchers anticipated that 80 to 90 percent of sentences would fall within the guidelines.[5] If their expectations are borne out, however, there will be a serious question about the efficacy of the guidelines, since an observed compliance rate of 80 to 90 percent would be expected even if judges failed to consult them. On the other hand, there is merit to the contention that too much compliance is undesirable; slavish adherence to the guidelines surely would lead to unjust results in exceptional cases. Therefore, the evaluative criterion of increased compliance rates should be bounded so that more is better only up to a point. The general expectation may be stated as follows: If sentencing guidelines are to succeed in structuring judicial discretion so as to reduce disparity, ceteris paribus, more sentences should fall within the guideline ranges after their implementation than did before, but not so many as would suggest a failure to distinguish exceptional cases where departures are warranted.

The assumption that all other things are equal is significant and problematic. If a shift occurs in the kinds of cases in which sentences are imposed, the number of sentences falling within the guidelines may be affected by this change alone. There is no reason to think that guidelines designed to encompass 80 percent of the sentences in one group of cases will likewise encompass 80 percent of the sentences in another group of cases that differs from the first group in some salient way. This sharply limits the inferences that can be made from observed changes (or lack of change) in compliance rates. We must always ask what would have happened but for implementation of the guidelines. Without a suitable experimental control, there is no way to know.

5. Wilkins et al., supra note 2, at 29.

Nevertheless, it is useful to know compliance rates. To begin with, they are intrinsically interesting; they tell us how well the guidelines describe the sentencing process. Guidelines that describe the outcomes of the vast majority of cases may enable their users, perhaps including prosecutors and defense attorneys, to predict more accurately the outcomes of individual cases. Moreover, compliance rates do tell us something about the impact of guidelines on the exercise of judicial sentencing discretion. Radical change in the kinds of cases before the judiciary for sentencing is unlikely. It is even more unlikely that such change would have escaped the attention of the judges, prosecutors, and defense attorneys we interviewed. At the extremes, then, we can make inferences from observed compliance rates. Low compliance rates suggest that guidelines have little if any effect on the exercise of judicial discretion. Extremely high compliance rates raise the spectre of thoughtless adherence.

Before delving into the observed compliance rates, we should appreciate their complexities and subtleties. The distinction between procedural and substantive compliance has already been noted. A judge may depart from the guidelines, either procedurally or substantively, in three ways. First, the judge may impose a sentence to incarceration where the guidelines recommend probation, or vice versa. Second, the judge may impose a minimum sentence that is higher or lower than the range of minimum sentences provided by the guidelines. Third, the judge may impose a maximum sentence that is higher or lower than the range of maximum sentences specified in the guidelines. Thus, judges may depart from the guidelines in the direction of severity or leniency with respect to three components of the sentence.

We shall refer to sentences that are more severe than the recommended guideline sentences as severe deviations. Those that depart from the guidelines in the lenient direction are referred to as lenient deviations. The terms "lenient" and "severe" are, of course, strictly comparative and not judgmental. Thus, where the guidelines specify a sentence to probation and the judge imposes a sentence to imprisonment, we call this a severe deviation. For the decision whether to incarcerate, the proportion of severe deviations is defined as the number of sentences to incarceration imposed in cases where the guidelines specified probation, divided by the total number of cases in which the guidelines specified probation. (The percentage of severe deviations is simply the proportion of severe deviations multiplied by 100.) In other words, it is the number of severe deviations expressed as a proportion of the number of cases in which a severe deviation was possible. With respect to the decision whether to incarcerate, a severe deviation is impossible if the guidelines specify incarceration, since no more severe penalty is available. The proportion of lenient deviations is defined analogously as the number of sentences to probation in cases where the guidelines recommended incarceration, divided by the total number of cases in which the guidelines specified incarceration. The advantage of expressing the number of each kind of deviation relative to the number of opportunities

for that kind of deviation is that the result is independent of the distribution of guideline sentences.6

The proportions of lenient and severe deviations are useful indicants of the relationship between sentencing guidelines and actual sentences. An imbalance between lenient and severe deviations implies that judges systematically disagree with the guidelines. If, for example, the proportion of severe deviations far exceeds the proportion of lenient deviations, we may conclude that either the guidelines are too lenient or the judges too severe.

The definition of the compliance rate (or percentage) is straightforward. It is simply the number of compliant sentences divided by the total number of cases to which the guidelines apply. Interpretation of compliance rates, however, requires care. To the uninitiated, 80-percent compliance on the decision whether to incarcerate seems impressively high. It must be remembered, however, that the decision whether to incarcerate is dichotomous. There are only two possible outcomes: probation and imprisonment. Typically, one outcome is more frequent than the other. Suppose, for example, that 60 percent of sentences are to imprisonment. If the guidelines always recommend imprisonment, the compliance rate will be 60 percent even if judges completely ignore the guidelines. Suddenly an 80-percent compliance rate seems less impressive. If we think of the noncompliant cases as prediction errors, then using sentencing guidelines to predict sentences reduces the number of mistaken predictions by half (from 40 per 100 to 20 per 100). This 50-percent proportional reduction in error (called PRE, and computed as (original error rate - new error rate)/original error rate), e.g., (.40-.20)/.40 = .50) is in some ways a more useful statistic than the compliance rate. At the least, it reminds us that part of the compliance percentage is due to the dichotomous nature of the decision and the skewed distribution of sentences. Ninety-percent compliance when 80 percent of sentences imposed are to imprisonment (PRE = (.20 - .10)/.20 = .50) is no more impressive than 80-percent compliance when 60 percent of sentences are to imprisonment; in both cases the PRE is 50 percent.

Length of incarceration presents two additional problems. The first arises from the fact that the guidelines specify ranges rather than precise terms. In some instances these ranges are fairly broad. Of course, wide ranges provide less guidance than do narrow ranges. To analogize to archery, the judge is shooting at a large target. The larger the target, the easier it is to get a high score. When interpreting compliance scores, it is useful to consider the size of the target.

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6. A numerical example illustrates the need for this kind of measure. Suppose that, in a sample of 100 cases, there are two lenient deviations. If we chose the total sample size as our base, the percentage of lenient deviations would be 2 percent. Now suppose further that the guidelines recommended probation in 96 of the 100 cases. Lenient deviations were possible in only 4 cases; they occurred in 50 percent of the cases where they could have occurred. The 50-percent statistic is much more informative than the 2-percent statistic.

The second difficulty posed by the measurement of compliance on the decision about length of incarceration results from the fact that sentences may depart from the guidelines by differing amounts. A sentence may deviate wildly or trivially from the guidelines. A simple counting of the number of deviant sentences may obscure important differences. There are ways to handle this problem (such as the method of least squares), but they entail complexities the costs of which exceed their benefits for the present purposes. For now, it is sufficient to remain mindful of the problem when interpreting compliance rates.

The decision whether to incarcerate. In Denver, the sentencing guidelines worksheets, which display the recommended sentences, frequently were absent from the case files. Among the 2,397 postguidelines convictions in our data set, guideline worksheets were found in only 542 cases. Although in some cases the guideline worksheet was absent because the guidelines did not apply or because the presentence report was waived,7 in most cases the absence of the worksheet was due to clerical problems. It cannot safely be assumed that the 542 cases in which the guideline worksheet was present are representative of all cases to which the guidelines applied, since there is no way to know whether misfiling was essentially random.

With this caveat in mind, let us review compliance with respect to the decision whether to incarcerate in the Denver District Court. Table 3-1 displays the basic information.

Table 3-1
Compliance With Sentencing Guidelines
According to Worksheets Found
Denver

		Guideline Sentence: Out	Guideline Sentence: In	Total
Actual Sentence	Out	182*	71	253
		(64.3)	(27.4)	
		(71.9)	(28.1)	(46.7)
	In	101	188	289
		(35.7)	(72.6)	
		(35.0)	(65.0)	(53.3)
	Total	283	259	542
		(52.2)	(47.8)	

*The first entry in each cell is the number of cases. The second entry (in parentheses) is the column percentage. The third entry (also in parentheses) is the row percentage. "Out" and "In" refer to nonimprisonment and imprisonment.

In those cases for which guideline worksheets were found in the case files, judges complied with the guidelines (substantively) in 68.3

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7. See note 9 infra.

percent ((182 + 188)/542). This compliance rate is noticeably lower than the oft-quoted 80-to-90-percent figure. The proportional reduction of error is 32.1 percent ((.467 - .317)/.467). In other words, the number of mistaken predictions made by simply predicting the modal category (incarceration) is reduced by 32.1 percent if we use the guidelines to predict the decision whether to incarcerate. The percentage of lenient deviations (71/259 = 27.4%) is less than the percentage of severe deviations (101/283 = 35.7%).

In order to overcome the difficulties posed by the omission of guideline worksheets from the Denver case files, guideline sentences were computed independently on the basis of information about the offense and offender found in the case file. There was only one piece of information utilized in computing guideline sentences that was consistently unavailable: whether the offender knew the victim before the commission of the offense. Fortunately, it appears from those worksheets found in the case files that in 90.3 percent of the cases (n = 542) the victim was unknown to the offender. This permitted calculation of the guideline sentences without using victim knowledge, in effect assuming that none of the offenders knew their victims. The offense score calculated in this manner should be inaccurate by one point in approximately 10 percent of the cases. The one-point inaccuracy is likely to make the difference between a guideline recommendation of probation and one of incarceration in only a fraction of that 10 percent.[8] Therefore, with minimal cost in accuracy, we were able to compute guideline sentences for 1,451 postguidelines cases. The results are displayed in Table 3-2.

Table 3-2
Compliance on the Decision Whether to Incarcerate According to Independent Computation of Guideline Sentences
Denver

		Guideline Sentence: Out	In	Total
Actual Sentence	Out	357 (60.3) (65.9)	185 (21.5) (34.1)	542 (37.4)
	In	235 (39.7) (25.9)	674 (78.5) (74.2)	909 (62.7)
	Total	592 (40.8)	859 (59.2)	1451

According to our independent computations, the Denver compliance rate was 71.1 percent, as compared to 68.3 percent according to the worksheets.

8. Not all inaccuracies are consequential. It is only the borderline cases that are affected by one-point errors. Cases that do not fall near the dividing line on the guideline grids between "out" and "in" cells are untainted.

These computations also show a greater preponderance of severe as compared with lenient deviations than did the worksheets. The results, however, are not remarkably different from those in Table 3-1. The most notable difference is in the proportional reduction in error, which is 22.5 percent using computed guideline sentences and 32.0 percent using worksheet guideline sentences. This difference is due almost entirely to the greater skewness of the actual sentences in the larger (and presumably the more representative) sample.[9]

In order to check the accuracy of our computations, we crosstabulated the computed guideline sentences with the worksheet guideline sentences. The results are presented in Table 3-3. There is 90-percent agreement between the two sources of guideline sentences. Most of the inaccuracy stems from incorrect predictions of worksheet guideline sentences to probation (only 85.6 percent agreement). Although our independent computation of guideline sentences is not free of errors, we believe it is sufficiently accurate to justify its use.

The augmented sample size permits us to subclassify according to the type of crime and the statutory class of the offense at conviction. In

9. In the 1,451-case sample, 62.7 percent were incarcerated; in the 542-case sample, 53.3 percent were incarcerated. The different incarceration rates result partly from the exclusion of drug cases from the 1,451-case sample. Limitations of the data made it unfeasible to calculate guideline sentences in drug cases, which have lower-than-average incarceration rates by and large.

The higher incarceration rate also reflects the fact that guideline worksheets were not completed by the probation staff in cases where the presentence report was waived. (This is an important limitation of using the probation staff to compute guideline sentences.) The presentence report typically is waived by offenders who are detained pending disposition, who are fairly certain to receive a sentence to imprisonment, and who want to finish serving their terms as soon as possible. Therefore, guideline worksheets are least likely to be completed and filed in those cases that are most likely to result in a prison term.

There is no reason to expect compliance in cases where the presentence report is waived unless the judge computes the guideline sentence instead of the probation staff. According to the judges we interviewed, that is not usually done. To the extent that presentence reports are waived, low compliance rates may be attributed to the problems of administering the guidelines rather than problems intrinsic to the guidelines or the judicial temperament.

It should be noted that cases in which the presentence report was waived remain underrepresented in the 1,451-case sample, though less underrepresented than in the 542-case sample. It is difficult to obtain the information needed to compute the guideline sentence without a presentence report. The fact remains, however, that worksheets were absent from the files even where the presentence report was present and the worksheet presumably had been completed and sent to the judge. Our independent computations enable us to measure compliance in many of these cases.

Table 3-3
Crossclassification of Computed Guideline Sentence (Type) by Worksheet Guideline Sentence (Type)
Denver

		Computed Guideline Sentence: Out	In	Total
Worksheet Guideline Sentence	Out	172 (94.5) (85.6)	29 (14.3) (14.4)	201 (52.2)
	In	10 (5.5) (5.4)	174 (85.7) (94.6)	184 (47.8)
	Total	182 (47.3)	203 (52.7)	385*

ϕ = 0.802
χ^2 = 247.501
$p < .001$

*The difference between the 542 cases reported in Table 1 and the 385 cases reported here is attributable to the deletion of cases for which data were missing.

Table 3-4, Denver compliance and deviation rates are shown by type of crime. Among the seven types of crime shown in Table 3-4, compliance rates range from 65.5 percent for fraud/forgery to 84.8 percent for robbery. The compliance rates for fraud/forgery, burglary, and theft are below average, while the compliance rates for homicide, assault, sexual assault, and robbery are above average. Burglary and robbery have relatively low percentages of lenient deviations. Only sexual assault has a percentage of severe deviations less than 30.0. In most crime categories, the judges tend to be more severe than the guidelines.

Table 3-4
Compliance on the Decision Whether to Incarcerate According to Independently Computed Guideline Sentences (Selected Crimes)
Denver

Type of Crime	Percent Compliance	(n)	Percent Lenient Deviations	(n)	Percent Severe Deviations	(n)	Percent Incarcerated	PRE
Homicide	73.5	(49)	23.4	(47)	100.0	(2)	77.6	-18.2
Assault	71.6	(204)	23.9	(109)	33.7	(95)	56.4	34.8
Sexual Assault	76.5	(34)	25.9	(27)	14.3	(7)	61.8	38.5
Robbery	84.8	(211)	10.9	(193)	61.1	(18)	86.7	-14.3
Burglary	66.9	(390)	14.4	(174)	48.2	(216)	64.9	5.8
Theft	68.5	(445)	31.0	(242)	32.0	(203)	52.1	34.3
Fraud/Forgery	65.5	(87)	47.1	(41)	41.3	(46)	56.3	21.1

The Denver sentencing guidelines consist of separate grids for each felony and misdemeanor classification. In Table 3-5 the compliance and deviation rates are presented for each such classification. The compliance rates range from 67.3 percent for Class One Misdemeanors to 85.5 percent for Class Three Felonies (the most serious class to which guidelines apply). There is an imbalance in the percentage of lenient and severe deviations in Class Three and Four Felonies.

Table 3-5
Compliance on the Decision Whether to Incarcerate According to Worksheets Found
(By Statutory Classification)
Denver*

Statutory Classification	Percent Compliance	(n)	Percent Lenient Deviations	(n)	Percent Severe Deviations	(n)	Percent Incarcerated	PRE
Felony 3	85.5	(159)	1.6	(126)	63.6	(33)	91.2	-64.3
Felony 4	68.5	(422)	17.8	(270)	55.9	(152)	72.7	-15.7
Felony 5	71.5	(333)	22.6	(177)	35.3	(156)	57.6	32.6
Misdemeanor 1	67.3	(162)	30.1	(83)	35.4	(79)	53.1	30.3
Misdemeanor 2	68.6	(325)	34.6	(188)	27.0	(137)	49.2	36.3
Misdemeanor 3	72.0	(50)	33.3	(15)	25.7	(35)	38.0	26.3

*Excludes Class One and Two Felonies and Petty Offenses, to which the guidelines are inapplicable, and drug crimes, which have no statutory classification.

The most striking aspect of Tables 3-4 and 3-5 is that for certain groups of cases, the proportional reduction of error is negative. This indicates that more errors would be made by using the guidelines to predict sentences than by always predicting the more frequent outcome. It is the most serious categories of cases (homicides and robberies; Class Three and Four Felonies) that show negative PRE statistics. Thus, guidelines' efficacy with respect to the decision whether to incarcerate is dubious in the most serious cases. This is discussed at greater length below.10

The independently computed guideline sentences have another virtue in addition to augmenting the sample size. They can be computed for cases that preceded the implementation of guidelines. In a manner of speaking, we can compare "compliance" rates before guidelines were implemented with those after guidelines were implemented, in order to see if more sentences fall within the guidelines afterwards than before. Of course, "compliance" here denotes only agreement between the guidelines and the actual sentence. The guidelines obviously did not influence judges' decisions before they were implemented. The pre-existing relationship between guideline sentences and actual sentences in Denver is summarized in Table 3-6.

10. See pp. 96-99.

Table 3-6
Simulated Compliance Rates in Preguidelines Cases on the Decision Whether to Incarcerate
Denver

		Guideline Sentence: Out	Guideline Sentence: In	Total
Actual Sentence	Out	179 (58.7) (74.0)	63 (19.3) (25.0)	242 (38.3)
	In	126 (41.3) (32.3)	264 (80.7) (67.7)	390 (61.7)
	Total	305 (48.3)	327 (51.7)	632

The simulated compliance rate for preguidelines cases ((179 + 264)/632 = 70.1 percent) is only one point lower than the postguidelines compliance rate (71.1 percent). This difference does not reach statistical significance ($Z = 0.461$; $p = .323$). Likewise, the percentages of severe and lenient deviations are strikingly similar (lenient deviations: 19.3 percent before, 21.5 percent after; severe deviations: 41.3 percent before, 39.7 percent after). The preguidelines proportional reduction in error is 21.9 percent as compared to 22.5 percent postguidelines. Sentences in Denver have not changed relative to the guidelines, insofar as the decision whether to incarcerate is concerned. Of course, it must be remembered that the computed guidelines are not error-free, but there is no particular reason to suspect that they are more prone to error before than after the implementation of guidelines, or vice versa.

It is possible that by grouping cases according to whether they preceded or followed the implementation of sentencing guidelines, without regard to the amount of time by which they preceded or followed guidelines, we have obscured the discontinuous effects of guidelines or failed to recognize a preexisting trend that would necessitate a different interpretation. For example, the compliance rate might have increased immediately after the implementation of guidelines but leveled off thereafter at a lower percentage. On the other hand, finer analysis might show that before guidelines there had been a trend toward less "compliance," and that guidelines put an end to the downward trend. In order to explore these possibilities, Denver compliance rates were computed for six quarters before the implementation of guidelines and 10 quarters after. The results are graphed in Figure 3-1. (See also Appendix B, Table C-1.)

From Figure 3-1 it is clear that there was no general upward or downward trend in "compliance" percentages before the implementation of guidelines. Moreover, there is no evidence of any immediate or delayed effects after guidelines were implemented. This is shown best by the

three-quarter moving average, which smooths out variation in compliance rates.[11] The moving average is calculated by taking a weighted average of the compliance percentages for every quarter, the quarter immediately preceding it, and the quarter immediately following it. For example, the three-quarter moving average for the second quarter is the weighted average of compliance percentages for the first, second, and third quarters. Of course, three-quarter moving averages cannot be computed for the first and last quarters because they are not preceded and followed, respectively, by any quarter in the sample.

Besides the absence of a relationship between time and compliance rates, two other aspects of Figure 3-1 are noteworthy. First, there is relatively little variation in compliance rates from quarter to quarter. The lowest percentage was 66.0 in the sixth quarter, and the highest was 76.0 in the second quarter. The second noteworthy feature of these results is the rather striking decrease in the variation of compliance percentages that occurred at about the eighth quarter, which was the second quarter following the implementation of guidelines. Most of the variation in compliance rates occurred before the implementation of guidelines. Note, however, that it is not variation in sentences that decreased after guidelines were implemented, but variation in "compliance," i.e., the extent to which actual sentences fell within the guideline ranges. If the observed decrease in the variation of quarterly compliance rates were due to judges' adherence to the guidelines, we should also observe an overall increase in compliance.

The data presented in Figure 3-1 are inconsistent with this interpretation. Rather, the decrease in the variation of quarterly compliance rates appears to be a statistical artifact attributable to an increase in the numbers of cases sentenced. (See Appendix B, Table C-1.) If we were to think of the compliance percentages as parameters estimated from sample data (actually, our data include the entire population of cases), the reliability (i.e., precision) of the estimates would be expected to increase as the sample size increases. This expectation is borne out by Figure 3-1, which shows decreasing variation in the estimates of compliance later in the time series as the number of cases tends to increase.

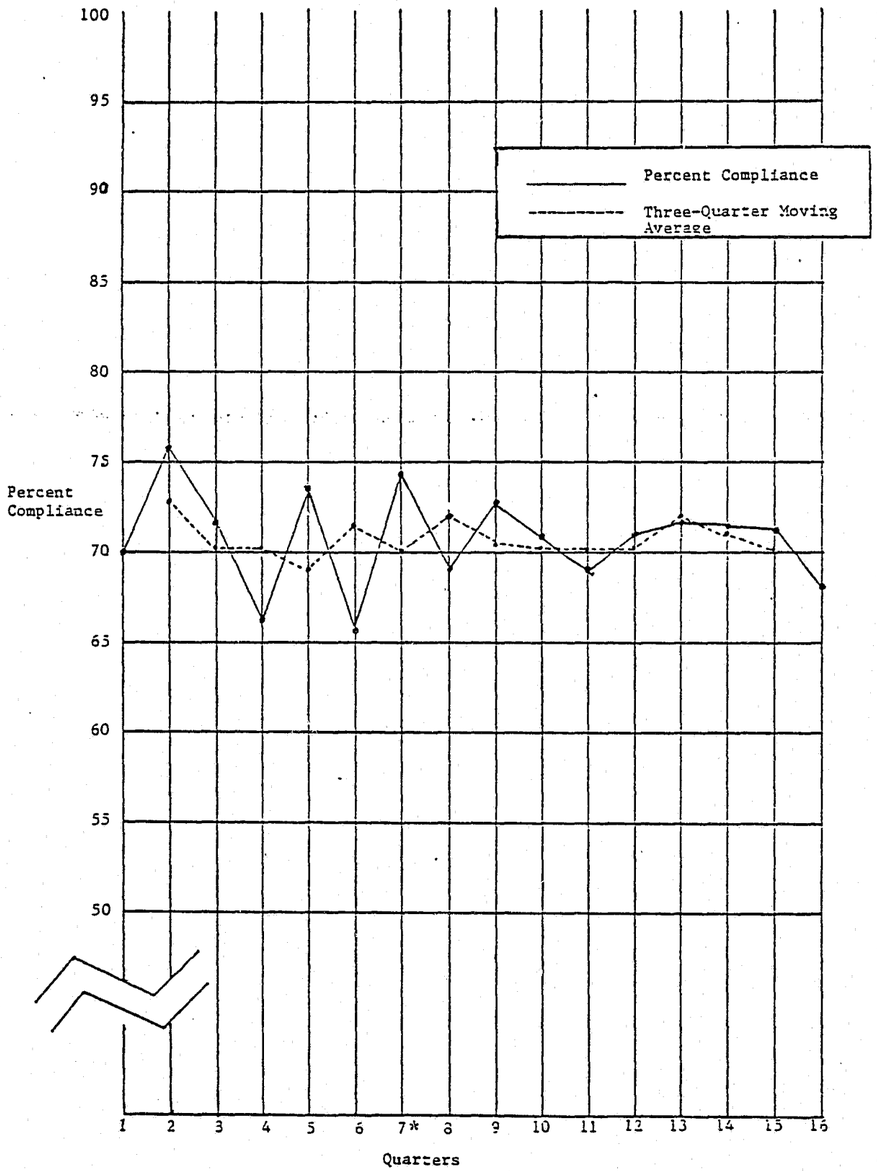
In Philadelphia, as in Denver, there are two sources of information about judicial compliance with sentencing guidelines. Information found on the guidelines worksheets was recorded for each postguidelines case, subject to availability. Analyses of these data are not reported here, however, because of the high frequency of errors observed in the computations. (In Philadelphia, individual judges were responsible for computing guideline sentences, whereas in Denver all computations were done by one member of the probation staff.) Instead, all analyses of compliance in Philadelphia are based on our independent computations using facts gathered from case files and computerized criminal history records. Although there are bound to be discrepancies between the information contained in these records and that available to the judge at sentencing, the resulting inaccuracies appear to be far less frequent than those found on the guidelines worksheets.

The basic information about compliance on the decision whether to incarcerate during the first six months following implementation of the Philadelphia guidelines is shown in Table 3-7.

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11. See generally P. Hoel, Elementary Statistics 287-294 (2d ed. 1966).

Figure 3-1
Compliance on the Decision Whether
To Incarcerate According to Computed
Guideline Sentences
Denver



*Sentencing guidelines implemented

Table 3-7
Compliance With Sentencing Guidelines
on Decision Whether to Incarcerate
(Postguidelines Cases Only)
Philadelphia

		Guideline Sentence: Out	In	Total
Actual Sentence	Out	176 (74.3) (77.2)	61 (25.7) (30.3)	237 (55.2)
	In	52 (27.1) (22.8)	140 (72.9) (69.7)	192 (44.8)
	Total	228 (53.1)	201 (46.9)	429

Philadelphia judges complied with the guidelines on the decision whether to incarcerate in 73.7 percent (i.e., (176 + 140)/429) of the cases. This compliance rate is slightly higher than Denver's. The proportional reduction of error is 41.1 percent, which is considerably higher than the comparable figure for Denver. The percentage of lenient deviations (30.3) is higher than the percentage of severe deviations (22.8), indicating that the judges tend to be more lenient than the guidelines.

Philadelphia's compliance and deviation rates are shown for selected crime categories in Table 3-8. Of the four crimes listed, burglary has the highest compliance rate (79.8 percent) while theft has the lowest (63.0 percent). The percentage of severe deviations is higher than the percentage of lenient deviations for robbery; the reverse is true for the other three crime categories.

Table 3-8
Compliance on the Decision Whether to Incarcerate
by Type of Crime (Selected Crimes)
Philadelphia

Crime	Percent Compliance	(n)	Percent Lenient Deviations	(n)	Percent Severe Deviations	(n)	Percent Incarcerated	PRE
Assault	76.1	(88)	33.3	(36)	17.3	(52)	37.5	36.4
Burglary	79.8	(109)	33.3	(48)	9.8	(61)	34.9	42.1
Robbery	69.1	(110)	26.6	(79)	41.9	(31)	64.5	12.8
Theft	63.0	(54)	50.0	(4)	36.0	(50)	37.0	0.0

In Table 3-9 compliance and deviation rates are shown for each statutory classification. In the following discussion, we shall ignore

Capital Offenses and Class Three Misdemeanors because of their infrequency. Compliance rates range from 66.0 percent for Class Three Felonies to 76.0 percent for Class Two Felonies to . The percentage of lenient deviations is higher than the percentage of severe deviations in every statutory class of offense.

Table 3-9
Compliance on the Decision Whether to Incarcerate
by Statutory Class
Philadelphia

Statutory Class	Percent Compliance	(n)	Percent Lenient Deviations	(n)	Percent Severe Deviations	(n)	Percent Incarcerated	PRE
Capital	100.0	(1)	0.0	(1)	--	(0)	100.0	--
Felony 1	75.3	(215)	26.6	(143)	20.8	(72)	55.8	44.2
Felony 2	76.0	(100)	33.3	(45)	16.4	(55)	39.0	38.5
Felony 3	66.0	(50)	57.1	(7)	30.2	(43)	32.0	-6.3
Misdemeanor 1	68.8	(32)	66.7	(3)	27.6	(29)	28.1	-11.1
Misdemeanor 2	70.4	(27)	100.0	(2)	24.0	(25)	22.2	-33.3
Misdemeanor 3	75.0	(4)	--	(0)	25.0	(4)	25.0	0.0

The most striking aspect of Table 3-9 is that in Philadelphia the proportional reduction of error is negative for certain classes of offenses, as it was in Denver. But it is the least serious crimes (Class Three Felonies and Class One and Two Misdemeanors) for which the PRE is negative, whereas in Denver, the PRE is negative for the most serious crimes. Guidelines predict the decision whether to incarcerate only in the most serious cases in Philadelphia and the least serious cases in Denver. For the other cases, guessing the more frequent outcome will result in more accurate predictions than are obtained from the guidelines.

This initially surprising finding has a sensible explanation. The PRE depends partly on the percentage of cases in which incarceration is imposed. It is defined as

$$PRE = \frac{E_m - E_g}{E_m}$$

where E_m is the number of prediction errors that result from guessing the more frequent (modal) result and E_g is the number of errors that result from using the guidelines to predict sentences. This implies that the PRE will be negative whenever the guidelines result in more errors than does guessing the more frequent category. The lower E_m is, the lower E_g must be in order to show a positive PRE. E_m is at its highest when exactly half of the cases involve sentences to incarceration. The more the percentage incarcerated departs from 50 in either direction, the more difficult it will be to design guidelines that improve our ability to predict the decision whether to incarcerate. If,

in a given class of cases, 90 percent are incarcerated, the guidelines would have to predict correctly more than 90 percent of the cases in order to improve upon the results we would obtain by predicting incarceration in every case. The Denver and Philadelphia sentencing guidelines are not such accurate predictors of judicial sentencing decisions.

This problem is not unique to sentencing guidelines. It has been recognized and discussed in the context of predicting dangerousness in mental patients and predicting recidivism in parolees.[12] It has been found that the less frequent the event being predicted, the more difficult it is to develop a satisfactory prediction device. Attempts to predict rare events generate many more erroneous predictions of the event's occurrence (called "false positives") than correct predictions of its occurrence (called "true positives"). The findings below indicate that this is also true of predictions of judicial decisions whether to incarcerate.

In order to demonstrate the relationship between the incarceration rate and the guidelines' predictive efficiency, the PRE statistics for each statutory class, in both Denver and Philadelphia, are plotted in Figure 3-2 as a function of the proportion incarcerated. The following quadratic regression equation was fitted to these data points using the ordinary least squares criterion:

$$Y = (-1.676) + (7.739)X + (-7.361)\ X^2$$

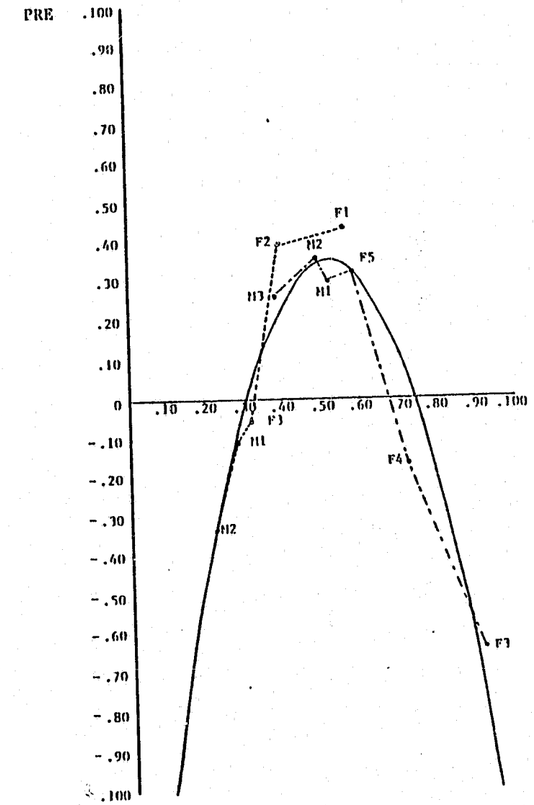
where Y is the PRE and X is the proportion incarcerated. This equation fits the data quite closely ($R^2 = .912$). Note that there are only 11 data points (5 statutory classes in Philadelphia and 6 in Denver), so the parameter estimates are unreliable. Nevertheless, this regression line is highly instructive. It is depicted in Figure 3-2 along with the actual data points.

From Figure 3-2 it can be seen that the apparently opposite relationships between the seriousness of the offense and the PRE are consistent with an explanation based on the percent incarcerated. In Denver the least serious offenses have incarceration rates close to 50 percent, while in Philadelphia it is the most serious offenses that have incarceration rates close to 50 percent. In Denver the most serious crimes have incarceration rates considerably in excess of 50 percent. In Philadelphia the least serious offenses have incarceration rates considerably below 50 percent. The Denver and Philadelphia PREs form opposite tails of a parabolic curve.

The parabolic shape and dimensions of the regression curve in Figure 3-2 have important implications for future decisions to develop

12. See A. von Hirsch, Prediction of Criminal Conduct and Preventive Confinement of Convicted Persons, 21 Buffalo L. Rev. 717, 730-739 (1972); L. Wilkins, Evaluation of Penal Measures 60-73 (1969).

Figure 3-2
Proportional Reduction of Error (PRE)
by Incarceration Rate
Denver and Philadelphia



Percent
Incarcerated

Denver
Philadelphia

sentencing guidelines. Figure 3-2 suggests that, at the current level of technology, sentencing guidelines' predictive efficacy is limited to classes of cases in which incarceration rates are close to 50 percent, say from 40 to 60 percent. For classes of cases with incarceration rates substantially in excess of 50 percent, a presumption in favor of incarceration would give greater predictive accuracy than do the guidelines.[13] For classes of cases in which incarceration rates are substantially less than 50 percent, a presumption against incarceration would achieve a similar result. In other words, the guidelines' predictive utility is greatest at 50-percent incarceration, and it actually becomes negative as the incarceration rate departs from 50 percent.

As with Denver, guideline sentences can be computed for Philadelphia cases preceding the implementation of guidelines in order to compare "compliance" before and after guidelines. Again, "compliance" means simply congruence between the guidelines and the actual sentences. The basic information about "compliance" in preguidelines cases in Philadelphia is presented in Table 3-10.

Sentencing guidelines accurately predict the decision whether to incarcerate in 67.0 percent of the preguidelines cases. Thus, the predictive accuracy of the guidelines increased from 67.0 to 73.7 percent following their implementation. In terms of PRE, the predictive efficiency of the guidelines increased from 26.2 percent in preguidelines cases to 41.1 percent in postguidelines cases. The difference in compliance rates is statistically significant ($Z = 2.483$; $p = .007$). The increase in "compliance" after the implementation of guidelines may reflect their influence on judges' decisions, but it is also consistent with an alternative explanation. The researchers who created the Philadelphia guidelines reported that a trend toward more severe sentences from 1975, the year from which the guidelines construction data were collected, to 1977, the year from which the guidelines validation data were collected, necessitated an enhancement of the penalties prescribed in the guidelines. Given that the guidelines were adjusted to reflect a preexisting trend toward more severe sentences, one would expect greater agreement between guideline sentences and actual sentences after the adjustment than before.

In order to check for preexisting trends and discontinuous effects that might be obscured by the simple before-after comparison, the compliance rates for each quarter are plotted over time in Figure 3-3. (See also Appendix B, Table C-2.)

13. There may, of course, be legal, philosophical, and practical reasons not to create such a presumption. At least one writer has urged the adoption of a uniform presumption against incarceration. M. Lasker, Presumption Against Incarceration, 7 Hofstra L. Rev. 407 (1979).

Table 3-10
Simulated Compliance Rates in Preguidelines Cases on the Decision Whether to Incarcerate
Philadelphia

		Guideline Sentence: Out	Guideline Sentence: In	Total
Actual Sentence	Out	351 (70.5) (69.1)	157 (37.2) (30.9)	508 (55.2)
	In	147 (29.5) (35.7)	265 (62.8) (64.3)	412 (44.8)
	Total	498 (54.1)	422 (45.9)	920

It is apparent from Figure 3-3 that although the compliance rates were relatively high during the two quarters (14 and 15) that followed the implementation of guidelines, they were well within the range of previously observed values. There were no obvious preexisting trends that would affect our interpretation of the before-after comparison. It remains to be seen whether compliance will continue to increase after quarter 15. At this point, the results are inconclusive. There is the barest suggestion of an increase in the predictive accuracy of guidelines. It remains possible that the Philadelphia sentencing guidelines will enhance the predictability of the decision whether to incarcerate.

Length of incarceration. In those cases where the guidelines recommend incarceration, they further specify a range of recommended minimum terms and another range of recommended maximum terms. The width of these ranges varies considerably within and between guideline grids.

The Denver guidelines specify ranges around both the minimum and maximum terms for felonies, but only a single range for misdemeanors, which are punishable by a flat jail term. In felony cases, however, the judge's choice of sentence length is seriously constrained by statute. The statutory sentencing structure in Colorado (until July 1979) is summarized in Table 3-11.

In Class Three Felonies, the minimum term of incarceration cannot be less than five years. In Class Four and Five Felonies, the minimum term must be one day. It is widely agreed by judges, prosecutors, and defense attorneys that the minimum term, which determines parole eligibility, is far more important than the maximum term, which in all but a handful of cases is irrelevant to the amount of time served by the offender. Thus, Colorado law places few constraints upon the judge's decision whether to incarcerate,[14] but many constraints on the decision about length of incarceration. (Put differently, the length of incarceration is subject

14. See Colo. Rev. Stat. §16-11-101 (1973).

Figure 3-3
Compliance on the Decision
Whether to Incarcerate
Philadelphia

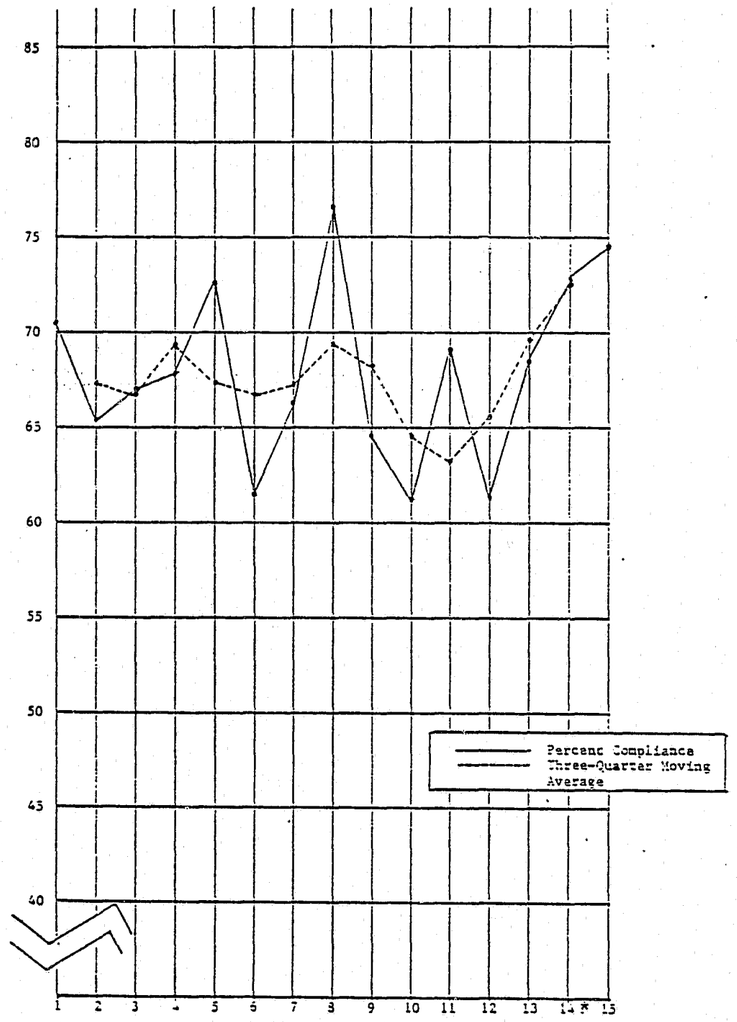


Table 3-11
Statutory Sentencing Structure
Colorado*

Statutory Classification	Minimum Term	Maximum Term
Felony One**	Life	Death
Felony Two**	10 years	50 years
Felony Three	5 years	40 years
Felony Four	One day***	10 years
Felony Five	One day***	5 years
Misdemeanor One****	6 months	2 years
Misdemeanor Two****	3 months	12 months
Misdemeanor Three****	0	6 months

*Source: Colo. Rev. Stat. §§18-1-105, 106 (1975 Supplement).
**No applicable guidelines for conviction of these offenses.
***If offender is sentenc d to imprisonment, a minimum term of one day is mandatory. The usual practice is for the parole board to release the offender serving such an indeterminate term within approximately nine months regardless of the maximum term.
****Misdemeanors are punishable by a flat term between the "minimum" and "maximum" shown.

to control by the prosecution and defense through charge bargaining, but the decision whether to incarcerate remains with the judge unless sentence bargaining also transpires.[15] The guidelines, of course, reflect these constraints. For Class Three Felonies, grid cells calling for probation are adjacent to grid cells calling for five-year minimum terms. For Class Four and Five Felonies, all "in" cells specify a one-day ("indeterminate") minimum. These peculiarities, necessitated by statute, impair not only the appearance but also the utility of the guidelines; frequently the only significant decisions are the charge to which the defendant pleads guilty and whether to incarcerate. The length of incarceration often is a foregone conclusion.

There is one other important decision with respect to terms of imprisonment. That is the decision where to incarcerate the offender. Misdemeanants must serve their time at the county jail,[16] but at one time the judge had the option of sending felons either to the state

15. For further discussion of this, see Chapter 4.
16. Colo. Rev. Stat. §18-1-106 (1973) (except where the sentence is to be served concurrently with a felony sentence).

penitentiary or to the state reformatory.[17] The penitentiary is considered by all to be a harsher environment. Moreover, sentences to the reformatory automatically were assigned a one-day minimum term regardless of the statutory classification of the offense.[18] Thus, in felony cases where a sentence to imprisonment is imposed, the most significant decision from the offender's perspective is the decision where to incarcerate. The judges we interviewed indicated that they base their decisions on the offender's age, propensity for violence, criminal experience, and potential for rehabilitation. The reformatory is viewed as a place for nondangerous offenders for whom there remains hope of a successful return to society, especially young persons who would be victimized by inmates of the penitentiary. One Denver judge put it this way:

> If you send a kid to [the penitentiary], you know that you're destroying him. That's the first thing that goes through my mind. Am I going to destroy this kid or not?

Given the significance of the decision where to incarcerate felons, including its effect on the minimum term of incarceration, it is surprising that the Denver sentencing guidelines offer no guidance to the judge in choosing between the penitentiary and the reformatory. This must be regarded as a serious omission in any attempt to structure judicial sentencing discretion in Colorado before 1979. In the following analysis of compliance, however, we shall evaluate the guidelines on their own terms and disregard the decision where to incarcerate except insofar as it affects the minimum term of incarceration, an aspect of the sentence which the guidelines do attempt to structure.

The analysis of compliance with respect to length of incarceration in Denver is based solely on the guideline worksheets found in the case files. Guideline sentences were not computed independently because of the likelihood that a one-point error in the offense score, attributable to the unavailability of information about whether the offender knew the victim, would cause an error in the computation of the guideline sentence.[19]

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17. Colo. Rev. Stat. §16-11-301(1) (1973) (class one felonies not included). The Department of Institutions retained authority to transfer prisoners from one facility to the other. Colo. Rev. Stat. §§17-1-202, 203 (1973). In 1979 the legislature removed the judge's discretionary power over the place of incarceration and delegated it to the (renamed) Department of Corrections. Colo. Rev. Stat. §16-11-301(1) (1979 Cumulative Supplement).

18. Colo. Rev. Stat. §§16-11-302, 303 (1973).

19. The risk of error in the computed guideline sentence with respect to the decision whether to incarcerate is considerably lower because only borderline cases are affected; where the length decision is concerned, however, there are many more borders and therefore many more borderline cases. We judged the anticipated number of errors intolerably high, and so chose not to compute guideline sentences for length of incarceration.

The results of the analysis of compliance with respect to the minimum term of incarceration in Denver are summarized in Tables 3-12 and 3-13. Overall, in those cases where incarceration was recommended by the guidelines and imposed by the judge, the minimum sentence length fell within the guideline range in 42.2 percent of the cases. This compliance rate is considerably lower than the 68.3-percent compliance rate for the decision whether to incarcerate. The actual minimum sentences were shorter than the guidelines in 23.9 percent of the cases, and longer than the guidelines in 33.9 percent. When we control for type of crime, the compliance rates vary from 23.1 percent for drug offenses (n = 13) to 75.5 percent for fraud and forgery cases (n = 8). Compliance rates for the more frequent categories of crime, such as assault, robbery, burglary, and theft, cluster around the average rate.

Table 3-12
Compliance on Minimum Length of Incarceration
According to Guideline Worksheets
(Selected Crimes)
Denver

Type of Crime	Percent Compliance	(n)	Percent Lenient Deviations	Percent Severe Deviations
Homicide	28.6	(7)	57.1	14.3
Assault	36.0	(25)	36.0	28.0
Sex Assault	40.0	(5)	60.0	-
Robbery	40.9	(44)	20.5	38.7
Firearms/Weapons	66.7	(3)	0	33.3
Burglary	45.6	(46)	10.9	43.5
Theft	39.0	(41)	31.7	29.3
Fraud/Forgery	75.0	(8)	-	25.0
Drugs	23.1	(13)	46.2	30.8

Table 3-13
Compliance on Minimum Length of Incarceration
According to Guideline Worksheets
Controlling for Statutory Classification
Denver

Statutory Classification	Percent Compliance	(n)	Percent Lenient Deviations	Percent Severe Deviations
TOTAL	42.2	(180)	23.9	33.9
Felony 3	31.6	(38)	36.8	31.6
Felony 4	59.7	(67)	3.0	37.3
Felony 5	54.6	(33)	3.0	42.4
Misdemeanor 1	6.7	(15)	73.3	20.0
Misdemeanor 2	13.6	(22)	63.6	22.7
Misdemeanor 3	40.0	(5)	20.0	40.0

The compliance rates also vary considerably according to statutory classification, from 6.7 percent for Class One Misdemeanors to 59.7 percent for Class Four Felonies. It should be noted that the statutory classifications having the two highest compliance rates are the two for which minimum terms of one day are mandatory. Thus, the overall compliance rate is as high as it is (42.2 percent) largely because of cases where compliance is legally unavoidable.

The compliance analysis for maximum terms in Denver is displayed in Tables 3-14 and 3-15. Misdemeanor convictions have been excluded from the analysis because they involve a single jail term rather than a minimum and a maximum term. Jail sentences for misdemeanors were treated as minimum sentences in the previous analysis.

Table 3-14
Compliance on Maximum Length of Incarceration
According to Guideline Worksheets
(Selected Crimes)
Denver

Type of Crime	Percent Compliance	(n)	Percent Lenient Deviations	Percent Severe Deviations
Homicide	100.0	(2)	0	0
Assault	62.5	(8)	25.0	12.5
Sex Assault	0	(3)	33.3	66.7
Robbery	38.9	(36)	38.9	22.2
Firearms/Weapons	50.0	(2)	50.0	-
Burglary	56.7	(30)	30.0	13.3
Theft	70.6	(17)	17.7	11.8
Fraud/Forgery	100.0	(6)	-	-
Drugs	71.4	(7)	-	28.6

Table 3-15
Compliance on Maximum Length of Incarceration
According to Guideline Worksheet
Controlling for Statutory Classification
Denver

Statutory Classification	Percent Compliance	(n)	Percent Lenient Deviations	Percent Severe Deviations
Felony 3	32.4	(34)	41.2	26.5
Felony 4	62.5	(48)	25.0	12.5
Felony 5	76.2	(21)	19.1	4.8

The overall compliance rates for maximum terms (55.3 percent) is higher than that for minimum sentences (42.2 percent), but still lower than the compliance rate for the decision whether to incarcerate (68.3 percent). More maximum sentences deviate in the lenient direction (27.4 percent) than deviate in the severe direction (15.5 percent). This is opposite to the preponderance of deviations from the guidelines for the minimum term, which was in the severe direction. The compliance rate is highest for Class Five Felonies, where the maximum cannot lawfully exceed five years, and lowest for Class Three Felonies, where the maximum sentence may be up to 40 years. Again, compliance is greatest where it is legally constrained; it is low where legal constraints do not obstruct judges' tendencies to deviate.

Like Denver, Philadelphia shows lower compliance rates for the decision about length of incarceration than for the decision whether to incarcerate. The compliance rate for the minimum term of imprisonment was 36.9 percent (n = 130) in postguidelines cases. The preponderance of deviations were in the lenient direction (31.5 percent lenient deviations compared to 19.2 percent severe deviations). The small sample size precludes reliable estimates of compliance rates for different types of crime or statutory classifications.

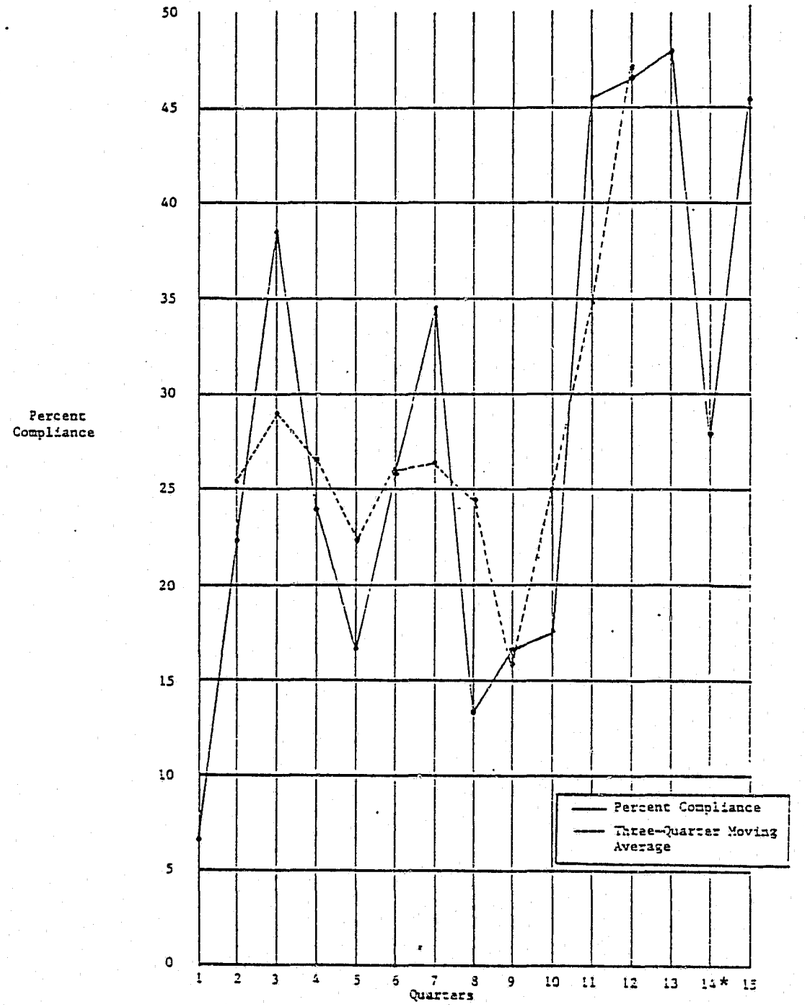
Unlike Denver, the Philadelphia data permitted the calculation of guideline minimum and maximum sentences to imprisonment for preguidelines cases. The preguidelines "compliance" rate with respect to the minimum term was 28.1 percent (n = 256). The difference between this preguidelines "compliance" rate and the postguidelines compliance rate (36.9 percent) is statistically significant (Z = 1.765; p = .038). In order to examine this increase more closely, compliance rates were computed for each quarter during the time series. These are graphed in Figure 3-4. (See also Appendix B, Table C-3.)

From Figure 3-4 it is obvious that the trend toward greater "compliance" on the decision about the minimum term of imprisonment began before the implementation of guidelines. This is consistent with the alternative explanation of increased compliance advanced above: namely, that increased compliance is the result of a pre-existing trend toward greater severity rather than of adherence to the guidelines.

Philadelphia judges complied with the guideline maximum term in 49.2 percent of the postguidelines cases (n = 130). The preguidelines "compliance" rate was 36.3 percent (n = 256). The difference is statistically significant (Z = 2.438; p = .007).

Thus, there is equivocal evidence that the Philadelphia guidelines rendered minimum and maximum terms more predictable. Nonetheless we should not lose sight of the fact that in Denver and Philadelphia, judges complied with the guidelines with respect to length of incarceration less than half of the time. The ambiguous changes in Philadelphia pale next to this overwhelming fact.

Figure 3-4
Percent Compliance on Minimum Term
Philadelphia



*Sentencing guidelines implemented

Compliance on both type and length of sentence. Total compliance, in a substantive sense, means compliance with the guidelines with respect to both the decision whether to incarcerate and, where applicable, the decision about length of incarceration. This, therefore, is the most demanding definition of compliance.

Using data collected from the guideline worksheets in Denver, we find that only 47.3 percent of the 404 cases for which information is available were compliant. In other words, judges complied fully in less than half of the cases. This finding brings into question the efficacy of the Denver guidelines as a means of controlling judicial sentencing discretion.

Total compliance in Philadelphia was only slightly higher than that in Denver. Philadelphia judges complied with the guidelines with respect to both decisions in 51.3 percent of the postguidelines cases (n = 419). Nevertheless, this represents a statistically significant increase over the 43.6-percent preguidelines compliance rate ($Z = 2.617$; $p = .004$). The suggestion of conformity to sentencing guidelines remains clouded by the alternative explanation based on a preexisting trend toward greater severity that was accommodated in the guideline grids just prior to implementation.

Reasons given in justification of deviations. One of the most promising aspects of sentencing guidelines is the requirement that reasons be given to justify departures from the guidelines. The importance of reason-giving was stressed by the guidelines' inventors.[20] It was hoped that reasons would provide grist for the development of a common law of sentencing.

The high hopes about judges' reasons for departing from the guidelines have not been realized in Denver. Of the 227 cases for which the guideline worksheet was found in the case file and the actual sentence deviated in some way from the guidelines, written reasons were provided in the appropriate space on the worksheet in only 28 cases, or 12.3 percent. Judges apparently failed to state reasons in the overwhelming majority of cases where reasons were required. Naturally, an analysis of the 28 cases in which reasons were given would be fruitless because of the low sample size.

The results were considerably different during the first six months of Philadelphia's experience with sentencing guidelines. Of 219 cases in which the actual sentence differed from the guideline sentence, reasons were given in 140 cases, or 63.9 percent. Reasons pertaining to the nature of the offense accounted for 26.7 percent; offender-related reasons accounted for 51.1 percent; and process-related reasons (e.g., plea bargains, pretrial incarceration, probation recommendations) accounted for 22.1 percent. Of the offender-related reasons, nearly two thirds pertained to prior criminal record. Approximately 10 percent of

20. Wilkins et al., supra note 2.

the reasons given were so vague as to defeat the purpose of requiring reasons to be given (e.g., "circumstances of the offense," "nature of the offense," and "criminal history"). In one case the judge declared simply that the guidelines were too lenient; in two cases they were declared too severe. In another case the judge cited the weakness of the evidence as a reason for imposing a sentence that was more lenient than that specified by the guidelines. Approximately 10 percent of the reasons given could be considered duplicative of guidelines criteria; e.g., citing a prior felony conviction as a reason for deviating from the guidelines when prior felony convictions are a guidelines criterion.

In light of these difficulties, the reason-giving requirement of sentencing guidelines cannot be considered an unmitigated success. Nevertheless, the approach seems to have potential if taken seriously by judges. The Philadelphia judges do seem able to articulate reasons for choosing extraordinary sentences. Their reasons were varied and do not seem to have been rote or mechanically applied.

Sentence Disparity

The term "sentence disparity," though widely used, has no precise, generally accepted meaning. At the most general level, it means simply differences in sentences. Of course, differences in sentences may occur for all kinds of reasons, legitimate and otherwise. One offender may be sentenced more severely than another because he or she is guilty of a more serious crime, had a greater degree of culpability, had a more serious prior criminal record, or appeared more likely to commit additional crimes. These are at least arguably legitimate reasons for different sentences. At the other extreme, one offender may receive a more severe sentence than another because of a judge's racial, ethnic, or religious bigotry. These reasons are clearly illegitimate. As a general proposition we may state that differences in offenses and offenders justify differences in sentences if they bear some rational relationship to the state's legitimate purposes in punishing criminals, so long as those differences do not involve classifications that are prohibited by our fundamental, constitutional law.

Although there is disagreement about the purposes of criminal sanctions, they are generally said to include deterrence of the offender and other potential offenders, restraint of the offender so as to limit her or his future crimes, retribution against the offender for her or his social transgressions, and rehabilitation of the offender to the status of a law-abiding citizen.[21] The philosophical underpinnings of sentencing have been the subject of considerable debate recently[22] and throughout the history of human civilization.[23] Recently the use of

21. W. LaFave and A. Scott, Handbook on Criminal Law 21-25 (1972).
22. E.g., Symposium on Sentencing, Parts I and II, 7 Hofstra L. Rev. 1-138, 243-470 (1978-79).
23. See, e.g., E. van den Haag, Punishing Criminals: Concerning a Very Old and Painful Question (1975).

incarceration for the purpose of rehabilitation has fallen into disfavor in some quarters.[24] Some contemporary commentators have criticized the punishment of one offender for the purpose of preventing others from committing crimes.[25] Other commentators have criticized the prediction of dangerousness, which is required for rational implementation of a strategy of restraint.[26] Still others are uneasy about increased reliance on the use of retributive justifications for incarceration.[27] Our purpose here, however, is not to elaborate much less resolve the debate about why we penalize criminal offenders, but simply to note the variety of more or less accepted justifications for imposing criminal sanctions, and to call the reader's attention to their implications for the legitimacy of differences in sentences.

Even the narrowest definitions of the purposes of punishment imply consideration of more than a single fact about a criminal case when the judge decides what sentence to impose. If, for example, retribution or desert were the sole purpose of criminal sanctions, we would expect the judge to consider the amount of harm or risk of harm caused by the offender, as well as the offender's culpable mental state. Thus, two persons guilty of violating the same statutory prohibition might receive different sentences even under a purely retributive theory.

As we saw in Chapter 2, the sentencing guidelines that are the subject of the present inquiry were not based on articulated decisions about the purposes to be served by imposition of sentences.[28] Rather than identify sentencing criteria deductively from a theory of the criminal sanction, the researchers and judges identified the sentencing criteria incorporated in the guidelines by empirical study of the factors that best accounted for variation in sentences. The result is a mixture of criteria without unifying principles. It should be noted, however, that each guideline criterion is relevant to at least one of the commonly

24. L. Orland, From Vengeance to Vengeance: Sentencing Reform and the Demise of Rehabilitation, 7 Hofstra L. Rev. 29 (1978).
25. E.g., R. Pugsley, Retributivism: A Just Basis for Criminal Sentences, 7 Hofstra L. Rev. 379, 391-397 (1979).
26. N. Morris, The Future of Imprisonment 31-43 (1974); A. von Hirsch, supra note 12.
27. E.g., L. Orland, supra note 24.
28. See p. 21.

accepted purposes of punishment, and most are relevant to more than one purpose.[29]

The side condition that must be satisfied in choosing sentencing criteria is that they not involve classifications that conflict with constitutional values. This is the subject matter of equal protection doctrine. Even if it could be shown that a constitutionally suspect classification, such as race or ethnicity, were rationally related to a permissible state interest, such as deterrence or rehabilitation, the Constitution would not permit its use as a criterion of sentencing.

Race[30] and ethnicity[31] are clearly impermissible classifications, but there are other classifications of less certain constitutional status. Gender-based classifications, for example, are permissible in some situations but not in others.[32] (The Equal Rights Amendment, if adopted, presumably would prohibit all governmental classifications on the basis of sex, except perhaps those adjudged benign.[33]) To a lesser extent, wealth-based classifications are disfavored by equal protection doctrine.[34]

Generally speaking, the law of equal protection disfavors, although it does not always condemn, classifications based on circumstances beyond the individual's control, even if they bear some reasonable relationship to a legitimate state interest. Usually the state bears a higher burden of proving a more compelling interest where such classifications are concerned.

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29. The Denver sentencing guidelines include the following criteria: the statutory classification of the offense at conviction; a ranking of offenses within each statutory class according to seriousness; whether the offender injured or killed the victim; whether the offender used a weapon in the commission of the crime; whether the offender sold drugs; whether the offender knew the victim prior to the commission of the crime (crimes against persons only); whether the offender was on probation, parole, or escape status at the time of the offense; the number of prior juvenile convictions; the number of prior adult misdemeanor convictions; the number of prior adult felony convictions; whether the offender's probation or parole had ever been revoked; and the number of prior adult incarcerations longer than 30 days. The criteria employed in the Philadelphia guidelines were similar except that they exclude juvenile convictions and include the amount of property loss, whether the offender completed high school, and whether the offender is employed. The guidelines used in Denver and Philadelphia are presented in Appendix E.

30. Strauder v. West Virginia, 100 U.S. 303 (1880).

31. Yick Wo v. Hopkins, 118 U.S. 356 (1886); Korematsu v. United States, 323 U.S. 214 (1944).

32. L. Tribe, American Constitutional Law, 1063-1074 (1978).

33. Brown, Emerson, Falk, and Freedman, The Equal Rights Amendment: A Constitutional Basis for Equal Rights for Women, 80 Yale L.J. 871 (1971).

34. L. Tribe, supra note 32, at 1099-1136.

Classifications based on the exercise or waiver of fundamental rights also run afoul of equal protection doctrine.[35] Thus, differences in sentences based on offenders' religious beliefs would violate equal protection. Differences in sentences attributable to the exercise of certain procedural rights, such as the right to appeal, also are forbidden.[36] The same cannot be said, however, for differential sentencing based on the exercise or waiver of trial rights. Although the law is not fully settled in this area, the U.S. Supreme Court has declared that it is permissible to induce pleas of guilty by making sentencing concessions, so long as they are not so large as to vitiate the voluntariness of the plea.[37]

The foregoing discussion suggests three categories of differences in sentences that are without legal justification: (1) differences that are based on suspect or almost-suspect classifications; (2) differences based on the exercise or waiver of rights; and (3) differences that bear no reasonable relationship to any legitimate state interest in the punishment of criminals. Sentence disparity may be defined accordingly as differences in sentences that are without legal justification. This is the definition that we have adopted for the purpose of studying the impact of sentencing guidelines on sentence disparity. Specifically, we examine the impact of sentencing guidelines on differences in sentences due to the offender's race, sex, and exercise or waiver of trial rights, as well as those differences that are attributable to no identifiable characteristic of the offense or offender.

Each kind of disparity under examination presents definitional and measurement problems. With the exception of differences attributable to the exercise of trial rights, these are discussed in the next two subsections of this chapter. Differences due to the exercise of trial rights are discussed in Chapter 4, which is concerned with the effects of sentencing guidelines on the exercise of discretion by prosecution and defense.

Racial and sexual disparity in sentences. Racial bias in sentencing has been the subject of many empirical studies. Some researchers have purported to find racial disparity, but a number of recent studies conclude that there is little persuasive evidence of racial discrimination in sentencing.[38] The crucial issue seems to be the

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35. Id. at 1002.

36. Cf. North Carolina v. Pearce, 395 U.S. 2072 (1969) (due process rationale).

37. Corbitt v. New Jersey, 439 U.S. 212 (1978).

38. See J. Hagan, Extra-legal Attributes and Criminal Sentencing: An Assessment of a Sociological Viewpoint, 8 Law & Soc'y Rev. 357 (1974); J. Gibson, Race as a Determinant of Criminal Sentences: A Methodological Critique and a Case Study, 12 Law & Soc'y Rev. 455 (1978); J. McCarthy, N. Sheflin, and J. Barraco, Report of the Sentencing Guidelines Project to the Administrative Director of the Courts on the Relationship Between Race and Sentencing (1979). But see L. Tiffany, Y. Avichai, and G. Peters, A Statistical Analysis of Sentencing in Federal Courts: Defendants Convicted After Trial, 1967-68, 4 J. Legal Studies 369 (1975) (finding racial disparities among first offenders only).

distinction between the direct and indirect consequences of race, which are analogous to de jure and de facto discrimination.

A simple example illustrates the problem. Suppose it is observed that 75 percent of black convicted offenders are incarcerated, while only 40 percent of white convicted offenders are incarcerated. It may be, however, that black convicts on the average committed more serious crimes or had more serious prior records than did whites. When we control for the seriousness of each offender's instant crime and prior criminal record, we may discover that similarly situated blacks and whites are equally likely to be incarcerated. In that event, we would have failed to demonstrate racial bias in sentencing, because legitmate sentencing criteria provide an equally satisfactory explanation of observed differences. This is not to suggest that individual judges were not motivated by racial bias. It is simply that the sentences they imposed were defensible on legitimate grounds. In the absence of evidence about judges' subjective states, there is no proof of racial bias unless observed differences cannot be explained by means of legitimate sentencing criteria.

Two additional points should be made about racial differences in sentences. First, a finding that judges do not discriminate racially in imposing sentences does not imply that there is no racial discrimination in the criminal justice system. A disproportionately large number of convicted offenders are members of minority groups. This may be caused by racially discriminatory practices of detection, prosecution, and adjudication. The effects of these practices may even be compounded by the sentencing judge when he or she takes the minority offender's prior criminal record into account. Beyond this, it is not unlikely that differential crime rates between racial groups are due at least in part to racial discrimination in society as a whole. These differences, too, may be compounded by the sentencing judge who includes employment status, educational achievement, and other indicants of so-called social stability in the decision calculus. Therefore, a finding that racial differences in sentencing are explainable on legitimate grounds does not absolve the predominantly white power structure of charges of racism.

The second point is that there is nothing to stop us from rejecting an otherwise legitimate, racially neutral criterion because its use has a racially discriminatory effect. Indeed, in some circumstances the use of such criteria is prohibited by law.[39] Some members of the committee of judges who designed the Philadelphia guidelines argued that educational achievement and employment status should be excluded as sentencing criteria because their correlation with race would result in unfair treatment of blacks and Hispanics. The essential point is that determining the acceptability of a race-related sentencing criterion may be a matter of balancing its racially discriminatory effects against the degree to which it serves the most important purposes of punishment.

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39. Griggs v. Duke Power Company, 401 U.S. 424 (1971) (employment discrimination under Title VII of the Civil Rights Act of 1964).

Perhaps it is time to declare that measures of social stability, such as employment status and educational achievement, involve discriminatory effects that outweigh their limited usefulness as predictors of future criminality or indicants of needed rehabilitation.

For measurement purposes, we may define racial disparity as variation in sentences that is attributable to the offender's race after other relevant variables have been controlled. In other words, it is the covariation between race and sentence that remains after we have eliminated covariation with other variables by crosspartitioning or statistical adjustment. But we are not only interested in the direct effects of race on sentence; we are interested in those effects of race on sentence that are mediated by other variables such as educational achievement and employment status as well. To the extent that sentencing guidelines incorporate race-related criteria, they may have unintended discriminatory effects.

The use of a causal model is one convenient way to represent the direct and indirect effects of race and other variables on sentences.[40] Such a model is pictured in Figure 3-5. Each box in the figure represents a variable relating to the offense or the offender. The arrows between boxes describe causal relationships among the variables. For the present purposes, it is assumed that one's race affects one's educational achievement (by whatever mechanism), criminal record, bail status, and so forth, each of which in turn may affect one's sentence directly or indirectly.

The reader should note that certain causal relationships have been omitted from the model. For example, it is implausible that one's sex affects one's race, or vice versa. It is assumed that no variable can affect a variable that occurs earlier in time. Thus, the arrow points from race to education rather than the reverse. At the risk of oversimplification, we have further assumed that no two variables have reciprocal effects on each other.[41] The model is therefore termed recursive, and permits of simpler statistical methods (ordinary least squares) than would be required by a nonrecursive model (two-stage least squares).[42]

It is important to recognize that the causal model itself proves nothing. It simply represents the researchers' assumptions or hypotheses about how things work. It is a method by which researchers organize their thoughts.

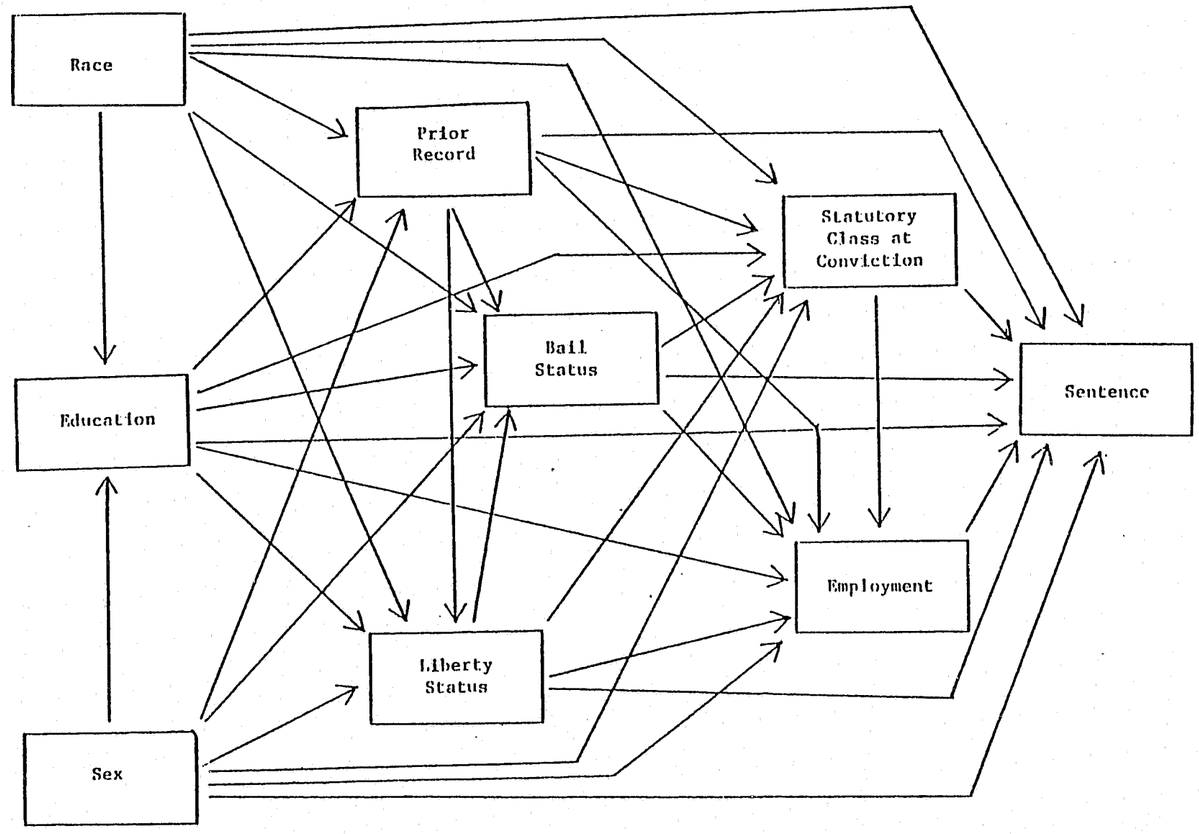
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40. See generally H. Blalock, Causal Models in the Social Sciences (1971) and D. Heise, Causal Analysis (1975).

41. It is likely, for example, that one's criminal record affects one's job status, which in turn affects one's propensity for criminal behavior. In a cross-sectional study such as this one, these reciprocal effects appear simultaneous. E. Hanushek and J. Jackson, Statistical Methods for Social Scientists 246 (1977) [hereinafter cited as Hanushek and Jackson]. Regrettably, resource limitations preclude consideration of more complex models that would accommodate reciprocal influences.

42. Hanushek and Jackson, supra note 41, at 229-243.

Figure 3-5
Causal Model of the Effects of Race
and Sex on Type of Sentence
Denver



In order to test the assumptions or hypotheses represented by the causal model, the researcher fits a set of structural equations to the model. These equations essentially describe covariation among variables. For example, variation in education may be symbolized as follows:

$$X_3 = \beta_{30} + \beta_{31} X_1 + \beta_{32} X_2 + U_3$$

where X_3 = education; X_2 = sex; X_1 = race; β_{30} is a constant (intercept); β_{31} is a coefficient that describes the relationship between X_1 and X_3 after controlling for the effects of X_2; β_{32} is a coefficient that describes the relationship between X_2 and X_3 after controlling for the effects of X_1; and U_3 is variation in X_3 that is attributable to variables excluded from the model (e.g., parental education), specification error, measurement error, and sampling error.43

A similar equation can be written for every variable in the model that has at least one arrow pointing to it. Such variables are called endogenous; variables that are not affected by any other variable in the model are called exogenous. Prior criminal record, for example, is an endogenous variable that is affected directly by race, sex, and education. The equation describing variation in prior record can be written as follows:

$$X_4 = \beta_{40} + \beta_{41} X_1 + \beta_{42} X_2 + \beta_{43} X_3 + U_4$$

where X_4 = prior record and the remaining symbols are defined as in the equation for education, above.

By solving an equation for every endogenous variable, we can describe all of the relationships depicted in the causal model. Each β coefficient represents the direct relationship between two variables when all other relevant variables are held constant by statistical adjustment. When these coefficients are used to describe the causal paths (symbolized by arrows) between variables, they are referred to as path coefficients, or more generally structural coefficients.

A set of structural equations were fit to the model in Figure 3-5 using data from the preguidelines and (separately) postguidelines cases in Denver. The results are summarized in Table 3-16. (See also Appendix B, Tables C-4 and C-5.)

The unstandardized path coefficient for the direct effects of race on the decision whether to incarcerate is 0.053. It can be interpreted as the amount of change in the dependent variable (the decision whether to incarcerate) that is attributable to the direct effects of a one-unit change in the independent variable (race). Inasmuch as race and sentence are measured dichotomously (for race, 0 = nonminority and 1 = minority;

43. Id. at 27.

Table 3-16
Path Analysis of the Effects of
Race and Sex on the Decision
Whether to Incarcerate
Unstandardized Coefficients
Denver

	BEFORE GUIDELINES			AFTER GUIDELINES		
Variable	Direct Effects	Indirect Effects	Total Effects	Direct Effects	Indirect Effects	Total Effects
Employment Status	-0.101	--	-0.101	-0.124	--	-0.124
Statutory Class	0.057	0.002	0.059	0.059	0.000	0.059
Bail Status	0.164	0.084	0.248	0.130	0.099	0.229
Liberty Status	0.186	0.043	0.229	0.133	0.056	0.189
Prior Record	0.072	0.039	0.111	0.077	0.029	0.106
Education	-0.012	-0.018	-0.030	-0.008	-0.028	-0.036
Sex	0.141	0.065	0.206	0.170	0.127	0.297
Race	0.053	0.075	0.128	0.035	0.064	0.099
	Before			After		
	Total variance in sentence accounted for by the model = 0.3085 F = 44.22 Probability = 0.0001 n = 802			Total variance in sentence accounted for by the model = 0.3018 F = 91.21 Probability = 0.0001 n = 1697		

for sentence, 0 = nonincarceration and 1 = incarceration), the path coefficient between sentence and race can be interpreted as follows: with all other variables in the equation held constant, minorities had a 5.3 percent greater chance of incarceration than did nonminorities. It should be noted, however, that the path coefficient is small and not statistically significant. In other words, if we regard all of the convictions in the Denver District Court within the period covered by our data collection effort until the implementation of guidelines as a sample, which it literally is not, the result obtained is sufficiently likely to have occurred by chance that we should regard it as sampling error. Thus, there is little evidence of racial disparity in sentences before the implementation of sentencing guidelines.

The unstandardized path coefficient for sentence and race after the implementation of guidelines was 0.035. Like the coefficient for the preguidelines cases, it does not reach statistical significance. The direct effects of race on the decision whether to incarcerate were negligible both before and after the implementation of sentencing guidelines.

The indirect consequences of race--those effects of race on sentence that are mediated by other variables in the model--are quite small and do not appear to have changed to any meaningful degree after the implementation of guidelines. The indirect effects of race on sentence were 0.075 before guidelines and 0.064 after guidelines. The total effects of race on sentence (direct plus indirect effects) were 0.128 before guidelines and 0.099 after guidelines. In other words, minorities in Denver were incarcerated 12.8 percent more frequently than nonminorities before guidelines and 9.9 percent more frequently than whites after guidelines, but most of this racial effect can be ascribed to minority defendants' more serious prior records, lower educational achievement, and so forth. The individual effects are sufficiently small that we can have little confidence in the parameter estimates.

The direct effects of gender on the decision whether to incarcerate are greater than those for race. The unstandardized path coefficient for sentence and sex was 0.141 before guidelines and 0.170 after. In other words, when all other variables in the model are held constant, males had a 14.1 percent greater chance of incarceration than females before guidelines; this increased to a 17.0 percent after guidelines. Both the before-guidelines and the after-guidelines coefficients were significant at the .01 level of probability. The difference between 14.1 percent and 17.0 percent, however, is not statistically significant ($t = 0.457$). Thus, there is evidence of sexual discrimination (against males) in sentencing in Denver, but guidelines do not appear to have reduced it.

The indirect effects of sex on sentence in Denver were smaller than the direct effects. From Table 3-16, however, it appears that the indirect effects of sex in the postguidelines cases were nearly double those of the preguidelines cases. The total effects of sex on the decision whether to incarcerate were 0.206 before guidelines and 0.297 after guidelines. Although the direct effects of sex predominated over the indirect effects, most of the increase over time is attributable to increased indirect effects. After guidelines, males were 29.7 percent more likely to be incarcerated than females; males were 17.0 percent more likely to be incarcerated than similarly situated females.

A similar causal analysis was performed using length of incarceration as the ultimate dependent variable. The causal model, which is similar to that for the decision whether to incarcerate, is depicted in Figure 3-6. The results of this analysis are summarized in Table 3-17. (See also Appendix B, Tables C-6 and C-7.)

Before sentencing guidelines, the unstandardized path coefficient for the direct effects of race on sentence length was -2.584. This parameter can be interpreted as follows: before sentencing guidelines, when all other variables in the model are held constant, minorities who were incarcerated received sentences that were, on the average, 2.584 months

Figure 3-6
Causal Model of the Effects of Race
and Sex on Length of Incarceration
Denver

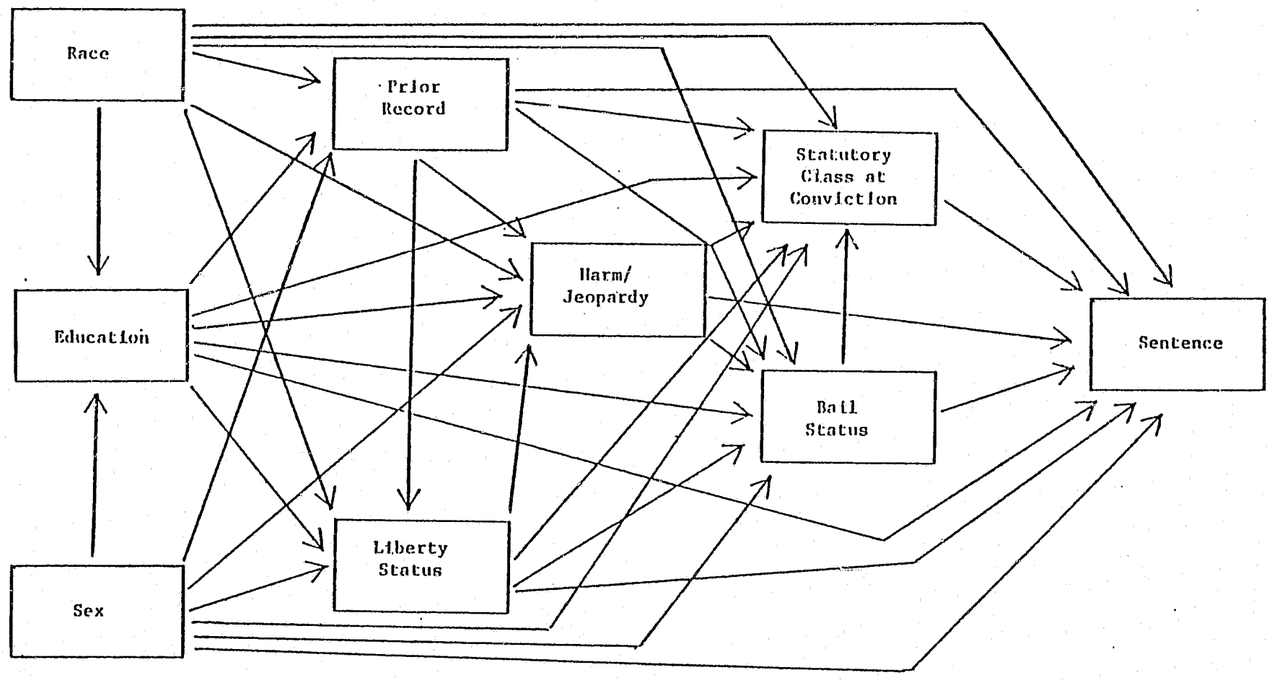


Table 3-17
Path Analysis of the Effects of
Race and Sex on Sentence Length
Using Unstandardized Path Coefficients
Denver

	BEFORE GUIDELINES			AFTER GUIDELINES		
	Direct Effects	Indirect Effects	Total Effects	Direct Effects	Indirect Effects	Total Effects
Statutory Class	5.276	—	5.276	7.537	--	7.537
Bail Status	4.818	3.303	8.121	4.512	4.605	9.117
Harm/Jeopardy	6.270	2.212	8.482	7.127	2.907	10.034
Liberty Status	0.026	-0.866	-0.840	0.236	0.916	1.152
Prior Record	-0.468	-0.242	-0.709	2.962	-1.187	1.775
Education	-0.526	0.167	-0.359	0.863	-0.755	-0.108
Sex	1.912	3.284	5.196	-6.073	9.260	3.187
Race	-2.584	-0.774	-3.358	1.342	0.572	1.914
	Before			After		
	Total variance in sentence accounted for by the model = 0.1298 F = 9.21 Probability = 0.0001 n = 503			Total variance in sentence accounted for by the model = 0.1973 F = 33.50 Probability = 0.0001 n = 1099		

shorter (in terms of parole eligibility)[44] than those given to nonminorities. In other words, minorities stood a slightly higher chance of incarceration than did nonminorities, but if incarcerated, they were likely to receive shorter sentences than were nonminorities. Note,

44. All sentence lengths in Denver are measured in terms of parole eligibility, which was calculated by subtracting the statutorily authorized good-conduct credits from the minimum term. For state institutions, good-conduct credits approximate one day for one day. Colo. Rev. Stat. §§17-20-104, 107 (1973). Prisoners given indeterminate terms (no minimum) are technically eligible for parole immediately but usually do not receive their first hearing until nine months have been served. See Colo. Rev. Stat. §17-22-103. Therefore, they were treated as if eligible for parole in nine months. Convicts given life sentences are eligible for parole after ten years. Colo. Rev. Stat. §17-2-207. Natural life terms imposed pursuant to the habitual offender statute (Colo. Rev. Stat. §16-13-101) were arbitrarily assigned a value of twenty years. Indeterminate-to-life terms imposed under the sex offender statute (Colo. Rev. Stat. §16-13-203) were arbitrarily assigned a value of five years. There is no parole from county jails, but two days are credited for every month served. Colo. Rev. Stat. §17-26-109.

however, that this coefficient does not nearly reach statistical significance, and should therefore be regarded as not reliably different from zero.

After the implementation of sentencing guidelines, the unstandardized path coefficient for the direct effects of race on sentence was 1.342. In other words, after the implementation of guidelines, all other things being equal, minorities who were incarcerated were given, on the average, sentences that were 1.342 months longer than those given to nonminorities. This coefficient also is small and far from statistically significant, and so should be considered unreliable. Therefore, there is no substantial evidence of racial disparities with respect to length of incarceration in Denver, either before or after guidelines. (The difference between the two coefficients is not statistically significant; t = 1.116.)

The indirect consequences of race were even smaller than the direct consequences. Before guidelines, the indirect effects of race on sentence length were -0.774; after guidelines they were 0.572. These must be regarded as negligible.

The unstandardized path coefficient for the direct effects of sex on sentence length before guidelines was 1.912, which means that males were given sentences that were, on the average, 1.912 months longer than those given to similarly situated females. After guidelines the path coefficient for sentence length and sex was -6.073. Although there was a change in both the direction and the magnitude of the effect, neither coefficient reaches statistical significance, nor is the difference between them statistically significant (t = 1.172). This may reflect the highly skewed distribution of the sex variable; only 10.5 percent of the cases in our sample (n = 4,712) involved women.[45] Nevertheless, it is noteworthy that, of those incarcerated after guidelines, women were given sentences that averaged 6.073 months longer than those given to similarly situated men.

The indirect consequences of sex on sentence length were larger than the direct effects. Before guidelines the indirect effects of sex on sentence length were 3.284; after guidelines they were 9.260. Males tended to receive longer sentences, mostly because they committed more serious crimes as measured by injury to the victim and statutory class of the offense at conviction. (See Appendix B, Table C-6.) It is conceivable that the guidelines were responsible for this increased difference by making sentences more consistently a function of crime seriousness, but any such conclusion would be highly speculative given the instability of the coefficient estimates and the tenuousness of the logical nexus between cause and effect.

45. With so few women, one or two especially long sentences might account for most of the observed effects.

In summary, there is no persuasive evidence of racial disparity, direct or indirect, in sentences in the Denver District Court during the 18 months preceding the implementation of sentencing guidelines. It appears that sentencing guidelines had no appreciable effect on the relationship beween race and sentence. There is, on the other hand, some evidence of sexual disparities in sentences. Males have a higher probability of incarceration than do similarly situated females, but this probability does not appear to have changed substantially as a result of the implementation of guidelines. Of those incarcerated, men were given slightly longer sentences than were similarly situated women before guidelines, but women were given sentences that were six months longer on the average than those given to similarly situated men after guidelines. It is not easy to imagine how this change could be due to sentencing guidelines, however, and it is probably best regarded as the product of chance or extraneous social causes.

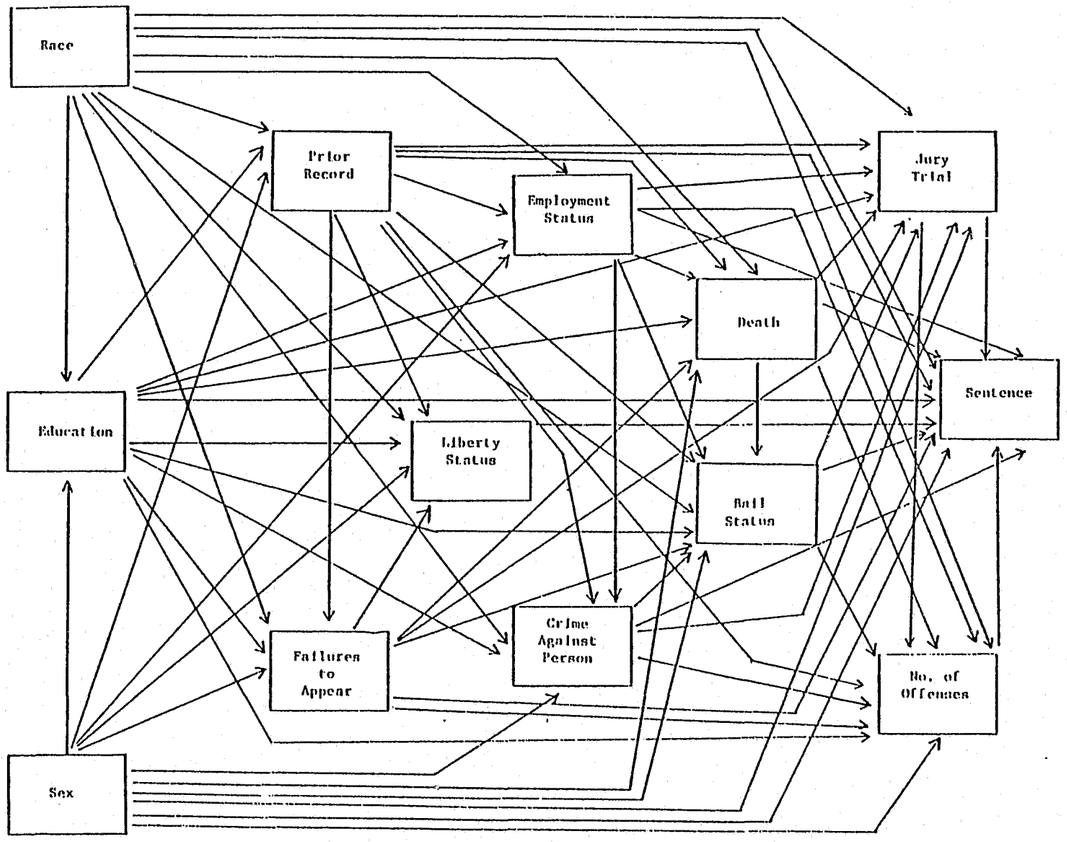
Similar analyses were conducted for Philadelphia with somewhat different results. The causal model for the effects of race and sex on the decision whether to incarcerate is depicted in Figure 3-7. The results of the path analysis are summarized in Table 3-18. (See also Appendix B, Tables C-8 and C-9.)

Table 3-18
Path Analysis of the Effects of
Race and Sex on the Decision Whether to Incarcerate
Unstandardized Path Coefficients
Philadelphia

	BEFORE GUIDELINES			AFTER GUIDELINES		
Variable	Direct Effects	Indirect Effects	Total Effects	Direct Effects	Indirect Effects	Total Effects
No. of Offenses	0.200	—	0.200	0.050	—	0.050
Jury Trial	0.135	0.106	0.241	0.072	0.044	0.116
Bail Status	0.118	0.010	0.128	0.146	0.007	0.153
Death of Victim	0.283	0.025	0.308	0.312	0.032	0.344
Crime Against Person	0.179	0.187	0.366	0.078	0.109	0.187
Employment Status	0.019	-0.001	0.018	-0.022	0.084	0.062
Liberty Status	0.021	—	0.021	0.047	—	0.047
Failure to Appear	0.047	0.067	0.114	0.102	0.058	0.160
Prior Record	0.043	0.021	0.064	0.040	0.051	0.091
Highest Grade	-0.005	-0.020	-0.025	-0.017	-0.012	-0.029
Sex	0.200	0.082	0.282	0.108	0.098	0.206
Race	0.070	0.081	0.151	0.118	0.117	0.235

Before	After
Total variance in sentence accounted for by the model = 0.3599	Total variance in sentence accounted for by the model = 0.4001
F = 43.71	F = 24.62
n = 946	n = 456
Probability = 0.0001	Probability = 0.0001

Figure 3-7
Causal Model of the Effects
of Race and Sex on Type of Sentence
Philadelphia



Assuming, of course, that our path model approximates the causal structure of the sentencing decision, the results indicate significant racial disparities in Philadelphia. The direct effects of race on the decision whether to incarcerate were 0.070 (p = .023) before guidelines and 0.118 (p = .011) after guidelines. The probability (p) levels indicate that these estimates are reliably greater than zero. According to our estimates, before guidelines black convicts had a 7-percent higher probability of incarceration than did non-blacks who were similarly situated with respect to the other variables in the model.[46] After guidelines, they had an 11.8-percent greater probability of incarceration. These effects are sufficiently small that they could be attributable to a relatively small number of judges who discriminate consistently rather than a pervasive practice among a majority of judges. The difference between the before- and after-guidelines coefficients is small and nonsignificant (t = 0.332). Moreover, there would be no reason to attribute it to the guidelines. In any event, sentencing guidelines did not reduce the direct effects of race on judges' decisions whether to incarcerate.

The indirect effects of race on sentence were approximately equal to the direct effects. Before guidelines, the indirect effects were 0.081; after guidelines, they were 0.117. The apprehension, voiced by some members of the committee of judges that designed the guidelines,[47] that the inclusion of education and employment status in the guidelines would have a racially discriminatory effect has not materialized on a widespread basis. The indirect effect of race through education (-0.012 before; 0.004 after) and employment status (0.001 before; 0.009 after) are negligible. A large proportion of these indirect effects is attributable to the fact that blacks were more often convicted of crimes against persons than were non-blacks, an obviously legitimate sentencing consideration. All things considered, blacks were 15.1 percent more likely to be incarcerated than were non-blacks before guidelines, and 23.5 percent more likely to be incarcerated after guidelines.

The direct effects of the offender's sex on sentence showed a decrease after the Philadelphia guidelines were implemented. Before guidelines, the direct effects of sex were 0.200, which is significant at the .001 level. In other words, men were 20 percent more likely to be incarcerated when all other variables in the model were held constant. After guidelines, the direct effects were 0.108, which is not statistically significant (p = .189). The different between the pre- and postguidelines coefficients, however, is not statistically significant

46. For purposes of these analyses, race was dichotomized between blacks (assigned a value of "1") and all others, including whites and Spanish-surnamed persons (assigned a value of "0"). This was done after preliminary analyses showed that Spanish-surnamed persons are treated more like whites than like blacks. In fact, Spanish-surnamed persons were treated most leniently of the three groups, while blacks were treated most harshly. The small number of Spanish surnamed persons in the sample made it unfeasible to create a third category for them alone.

47. See p. 113.

(t = 0.953). Therefore, the evidence of decreased sexual disparities in the decision whether to incarcerate is very weak.

The indirect effects of sex on sentence in Philadelphia were smaller than the direct effects and showed little change from the pre- to postguidelines samples. Before guidelines, the indirect effects of sex on sentence were 0.082; after guidelines they were 0.098.

A similar causal analysis was performed on the Philadelphia data using length of incarceration as the dependent variable.[48] The causal model for length of incarceration is shown in Figure 3-8, and the numerical results are summarized in Table 3-19. (See also Appendix B, Tables C-10 and C-11.)

Before the implementation of sentencing guidelines the direct effects of race on length of incarceration were -3.920. In other words, when all other variables in the model are held constant, blacks received prison terms that were, on the average, 3.920 months shorter than those given to non-blacks. After guidelines, the direct effects of race on sentence length were 11.444; i.e., blacks received sentences that were 11.444 months longer than those given to non-blacks, all other variables held constant. Neither of these estimates, however, is statistically significant. The difference between them is not significant either (t = 0.712). In other words, the observed relationships likely resulted from sampling error and do not necessarily reflect true relationships.

The indirect effects of race on length of incarceration were 5.709 before guidelines (note that the indirect effects are opposite to the direct effects) and 7.229 after guidelines. This increase is due in large part to unquestionably legitimate considerations, namely victim injury or death, and whether the crime was against persons or property. Blacks in the postguidelines sample tended to commit more serious crimes and consequently received more severe sentences. Moreover, the increase in indirect effects was so small that it must be regarded as within the realm of sampling error.

The total (direct plus indirect) effects of race on sentence length in Philadelphia was negligible (1.789 months) before guidelines but quite large (18.673 months) after guidelines. In other words, all things considered, before guidelines blacks received prison terms that were approximately equal to those given to non-blacks, but after guidelines blacks were given prison terms that were almost 19 months longer than those given to non-blacks. Again, approximately half of these total

48. Specifically, the minimum term of incarceration was used as the dependent variable. In Pennsylvania, no time is credited for good behavior. Pa. Stat. Ann. tit. 61, §271 (1978 Supp.). Therefore, prisoners are eligible for parole only after serving the full minimum term. Conceptually, although not operationally, the dependent variable used for the Philadelphia analyses is the same as that used in the Denver analyses: parole eligibility.

Figure 3-8
Causal Model of the Effects
of Race and Sex on Length of Incarceration
Philadelphia

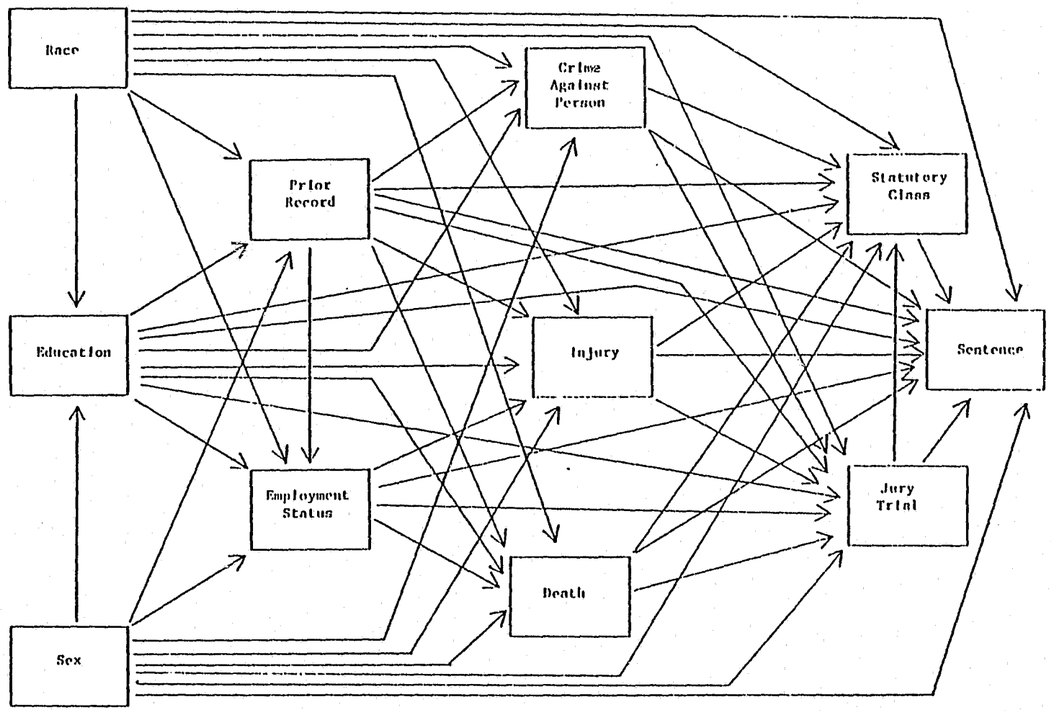


Table 3-19
Path Analysis of the Effects of
Race and Sex on Length of Incarceration
Unstandardized Path Coefficients
Philadelphia

	BEFORE GUIDELINES			AFTER GUIDELINES		
Variable	Direct Effects	Indirect Effects	Total Effects	Direct Effects	Indirect Effects	Total Effects
Statutory Class	-11.130	--	-11.130	-10.632	--	-10.632
Jury Trial	101.635	0.000	101.635	195.330	3.168	198.498
Death of Victim	108.984	26.726	135.710	273.270	27.403	300.673
Injury	3.605	2.003	5.608	3.004	11.580	14.584
Crime Against Person	-5.811	12.378	6.567	-14.193	17.980	3.787
Employment Status	-1.873	-9.289	11.162	14.112	-41.308	-27.196
Prior Record	0.164	-1.517	-1.353	-2.942	-1.088	-4.030
Highest Grade	2.457	-0.851	1.606	1.697	-1.319	0.378
Sex	18.173	23.422	41.595	30.285	-22.163	8.122
Race	-3.920	5.709	1.789	11.444	7.229	18.673

Before	After
Total variance in sentence explained by variables in model = 0.2313 F = 12.52 n = 427 Probability = 0.0001	The variance in sentence explained by variables in model = 0.4266 F = 14.96 n = 212 Probability = 0.0001

racial effects appear due to legitimate sentencing considerations. The increase in total effects may reflect nothing more than sampling error, but it is consistent with the increase in the direct effects of race on the decision whether to incarcerate, reported above. Since we cannot entirely rule out sampling and specification error, these results should be taken as suggestive rather than conclusive. In any event, there is little reason to suppose that sentencing guidelines are responsible. The only increased racial differences that guidelines could be expected to cause are increases in the indirect effects of race on sentence resulting from correlations between race and the guidelines variables. These increases did not materialize in any substantial way.

The direct effects of sex on the length of incarceration were more pronounced than the direct effects of race. Before guidelines, the direct effects of sex were 18.173 months. In other words, when all other variables in the model are held constant, males received prison terms that were 18.173 months longer than those given to females before guidelines. This difference increased to 30.285 months after guidelines. Despite the large differences between males and females, neither coefficient is reliably greater than zero. This reflects, at least in part, the fact that there were so few females in our sample.

The indirect effects of sex on sentence length were 22.422 months before sentencing guidelines. After guidelines, the indirect effects of sex on sentence length were -22.163 months. This confirms the instability of these estimates. The large, negative postguidelines estimate apparently reflects a small number of homicide cases involving female defendants. (The indirect effects of sex through the "death" variable were -31.571.)

To summarize, there is little evidence that sentencing guidelines reduced racial or sexual disparities in Denver or Philadelphia. In Denver there was little evidence of racial disparity even before the implementation of sentencing guidelines. There is weak evidence of an increase in the direct effects of race on sentence in Philadelphia, but it is unlikely that sentencing guidelines were the cause of this increase. On the other hand, it does not appear that the inclusion in sentencing guidelines of employment and educational criteria has had a generally discriminatory effect. There is simply little evidence of any change in racial or sexual disparity as a result of the introduction of sentencing guidelines.

Disparity as unexplained variation. One of the definitions of disparity formulated above[49] is differences in sentences not attributable to any identifiable characteristic of the offense or the offender.[50] In a statistical sense, this kind of disparity may be thought of as variation that remains unexplained after all relevant variables have been considered. Unexplained (residual) variance has several components, which include measurement error, specification error, and sampling error.[51] It is therefore not solely a measure of inconsistency in sentences. If, however, we are willing to assume that the other sources of error remain relatively constant over time, we can use residual variance as a rough measure of disparity of the inconsistency kind.

There is another, perhaps better, way to view the problem. Sentencing guidelines were intended to reduce unjustified variation in sentences, which includes variation not attributable to identifiable characteristics of the offense or the offender. In order to accomplish their purpose, the guidelines' originators formulated an elaborate, numerical policy statement to the effect that, in general, sentences should be determined by the factors included in the guidelines. In other words, it was intended that sentences become more consistently the product of the factors included in the guidelines. The research question is as follows: were sentences after guidelines more explainable on the basis of the guidelines criteria than they were before guidelines?

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49. See p. 112.
50. Note that this definition includes both between-judge and within-judge variation. We have chosen not to separate these two sources of variation. The source of variation is relevant only if we observe a change, which, it turns out, we do not.
51. Hanushek and Jackson, supra note 41, at 27.

This research question bears a strong resemblance to that addressed above by the comparison of "compliance" before and after guidelines.[52] Indeed, the analysis here is quite similar to the compliance analysis reported above, but the two different methods give us greater opportunity to discover the effects of sentencing guidelines on the exercise of judicial sentencing discretion.

In order to determine whether sentences after the implementation of guidelines were more consistently the product of the guidelines criteria, we regressed sentence, measured first dichotomously as incarceration or nonincarceration, and subsequently as length of incarceration, on the guidelines variables. This was done separately for the preguidelines and postguidelines cases. To the extent that sentencing guidelines have reduced disparity, the amount of variation in sentence that is explained by the guidelines variables should be greater for the postguidelines cases than for the preguidelines cases. The results are summarized for Denver in Tables 3-20 and 3-21.

Table 3-20
Regression of Type of Sentence on
Guidelines Variables
Preguidelines Cases
Denver

Variable	b	Beta	Standard Error of b	F	Probability
Statutory Class/Rank	0.248	0.226	0.004	34.500	0.001
Injury	-0.630	-0.006	0.043	0.021	0.885
Weapon	0.558	0.052	0.045	1.519	0.218
Liberty Status	0.142	0.127	0.043	10.800	0.001
Juvenile Convictions	0.159	0.199	0.030	28.287	0.001
Misdemeanor Convictions	0.958	0.139	0.027	12.964	0.001
Felony Convictions	0.107	0.225	0.029	14.051	0.001
Revocations	0.531	0.079	0.030	3.210	0.074
Prior Incarcerations	-0.267	-0.059	0.027	0.984	0.322
Intercept	0.116				

n = 578
R^2 = 0.271
Adjusted R^2 = 0.259
F = 23.429
Probability = 0.0001

The total variance explained by the guidelines variables in preguidelines cases was 0.271, or 27.1 percent. For the postguidelines cases, the total variance explained dropped slightly, to 0.256, or 25.6 percent. If anything, sentences became less consistently the product of the guidelines variables.

52. See pp. 82-109.

Table 3-21
Regression of Type of Sentence on
Guidelines Variables
Postguidelines Cases
Denver

Variable	b	Beta	Standard Error of b	F	Probability
Statutory Class/Rank	0.257	0.237	0.003	83.246	0.001
Injury	0.491	0.050	0.026	3.619	0.057
Weapon	0.672	0.063	0.029	5.258	0.022
Sale of Drugs	-0.219	-0.028	0.189	1.346	0.246
Liberty Status	0.151	0.145	0.027	32.314	0.001
Juvenile Convictions	0.104	0.130	0.120	28.535	0.001
Misdemeanor Convictions	0.216	0.032	0.018	1.446	0.229
Felony Convictions	0.896	0.212	0.015	35.121	0.001
Revocations	0.127	0.021	0.018	0.488	0.485
Prior Incarcerations	0.293	0.074	0.015	3.972	0.046
Intercept	0.121				

n = 1,334
R^2 = 0.256
Adjusted R^2 = 0.250
F = 45.407
Probability = 0.0001

A test of heterogeneity of regression was performed in order to determine whether the difference between the pre- and postguidelines regression is statistically significant.[53] The results are presented in Table 3-22.

Table 3-22
Test of Heterogeneity of Regression
Type of Sentence Regressed on Guidelines Variables
Denver

Source	Degrees of Freedom	Sum of Squares	Mean Square	F	Probability
Coincidence	11	3.01143	0.27377	1.54967	0.108
Intercepts	1	0.65588	0.65588	3.71260	0.054
Parallelism	10	2.35555	0.23556	1.33338	0.207
Residual (Separate)	1890	333.88241	0.17666		
Residual (Total)	1901	336.89384			

Number of cases = 1912
Number of groups = 2
Number of predictors = 10

53. The test of heterogeneity of regression, or Chow test, is described at p. 37.

The test of heterogeneity of regression tells us, in effect, that the vectors of coefficients for the guidelines variables do not differ significantly before and after the implementation of guidelines. In nonstatistical terms, the decision whether to incarcerate was no more or less predictable, on the basis of the guidelines criteria, after sentencing guidelines than before. Sentencing guidelines appear to have had no significant effect on sentence disparity, as measured by unexplained variation in the decision whether to incarcerate, in the Denver District Court.

A similar analysis was performed using length of incarceration as the dependent variable. The results of regressing length of incarceration on the guidelines variables separately for preguidelines and postguidelines Denver cases are reported in Tables 3-23 and 3-24.

Table 3-23
Regression of Length of Incarceration
on Guidelines Variables
Preguidelines Cases
Denver

Variable	b	Beta	Standard Error of b	F	Probability
Statutory Class/Rank	1.137	0.190	0.349	10.637	0.001
Injury	-0.674	-0.012	3.194	0.044	0.834
Weapon	2.662	0.048	3.380	0.620	0.432
Liberty Status	0.923	0.017	2.993	0.095	0.758
Juvenile Convictions	-3.185	-0.079	2.171	2.152	0.143
Misdemeanor Convictions	-4.821	-0.132	2.016	5.718	0.017
Felony Convictions	0.144	0.006	1.931	0.006	0.938
Revocations	0.261	0.008	1.977	0.017	0.896
Prior Incarcerations	1.280	0.060	1.807	0.502	0.479
Intercept	7.919				

n = 354
R^2 = 0.077
Adjusted R^2 = 0.053
F = 3.193
Probability = 0.0010

The proportion of variance in sentence length explained by the guidelines variables nearly doubled, from R^2 = 0.077 before guidelines to R^2 = 0.134 after guidelines. In both equations, however, the proportion of variance explained is very low. This suggests that guidelines variables have very little predictive or controlling value where length of incarceration is concerned.[54]

54. See pp. 51-52.

Table 3-25
Test of Heterogeneity of Regression
Length of Incarceration Regressed on Guidelines Variables
Denver

Source	Degrees of Freedom	Sum of Squares	Mean Square	F	Probability
Coincidence	11	10026.64390	911.51308	1.21835	0.269
Intercepts	1	186.93920	186.93920	0.24987	0.617
Parallelism	10	9839.70470	983.97047	1.31520	0.217
Residual (Separate)	1175	879081.74301	748.15467		
Residual (Total)	1186	889108.38694			

Number of cases = 1197
Number of groups = 2
Number of predictors = 10

The results of the test of heterogeneity of regression using length of incarceration as the dependent variable are similar to those already reported. The vector of coefficients for the guidelines variables for preguidelines cases is not significantly different from that for postguidelines cases. In other words, the increased predictability of sentence length should be regarded as well within the realm of what would ordinarily be considered sampling error. The evidence that the Denver sentencing guidelines reduced disparity in sentence length, as measured by parole eligibility, is scant.

Similar regression analyses were performed on the Philadelphia data. The regressions of type of sentence on the guidelines variables in the pre- and postguidelines cases are reported in Tables 3-26 and 3-27.

The proportion of variance in the decision whether to incarcerate explained by the guidelines variables increased from R^2 = 0.216 to R^2 = 0.276. The test of heterogeneity of regression, reported in Table 3-28, reveals that the guidelines variables do not predict the decision whether to incarcerate significantly better for the postguidelines sample than for the preguidelines sample (F = 0.2012 with 10, 1,378 degrees freedom).[56]

56. The R^2 statistic is sample-specific. That is, its magnitude depends partly on the sample variances of the predictor variables. Hanushek and Jackson, supra note 41, at 59. Comparisons of R^2 statistics from different samples therefore can be misleading. This may explain why the difference between the R^2 statistics for the pre- and postguidelines regressions (R^2 = 0.216 before; R^2 = 0.276 after) seems relatively large but turns out to be insignificant.

Table 3-25
Test of Heterogeneity of Regression
Length of Incarceration Regressed on Guidelines Variables
Denver

Source	Degrees of Freedom	Sum of Squares	Mean Square	F	Probability
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The results of the test of heterogeneity of regression using length of incarceration as the dependent variable are similar to those already reported. The vector of coefficients for the guidelines variables for preguidelines cases is not significantly different from that for postguidelines cases. In other words, the increased predictability of sentence length should be regarded as well within the realm of what would ordinarily be considered sampling error. The evidence that the Denver sentencing guidelines reduced disparity in sentence length, as measured by parole eligibility, is scant.

Similar regression analyses were performed on the Philadelphia data. The regressions of type of sentence on the guidelines variables in the pre- and postguidelines cases are reported in Tables 3-26 and 3-27.

The proportion of variance in the decision whether to incarcerate explained by the guidelines variables increased from $R2 = 0.216$ to $R^2 = 0.276$. The test of heterogeneity of regression, reported in Table 3-28, reveals that the guidelines variables do not predict the decision whether to incarcerate significantly better for the postguidelines sample than for the preguidelines sample ($F = 0.2012$ with 10, 1,378 degrees freedom).56

56. The R^2 statistic is sample-specific. That is, its magnitude depends partly on the sample variances of the predictor variables. Hanushek and Jackson, supra note 41, at 59. Comparisons of R^2 statistics from different samples therefore can be misleading. This may explain why the difference between the R^2 statistics for the pre- and postguidelines regressions ($R^2 = 0.216$ before; $R^2 = 0.276$ after) seems relatively large but turns out to be insignificant.

Table 3-26
Type of Sentence Regressed on
Guidelines Variables
Preguidelines Cases
Philadelphia

Variable	b	Beta	Standard Error of b	F	Probability
Type of Offense	0.033	0.163	0.006	27.816	0.001
Injury	0.091	0.162	0.019	22.707	0.001
Type of Weapon	0.116	0.114	0.034	11.419	0.001
Value of Property	0.125	0.093	0.041	9.210	0.003
Victim	-0.046	-0.046	0.032	2.041	0.153
Prior Incarceration	0.090	0.154	0.025	12.678	0.001
Liberty Status	0.113	0.104	0.041	7.718	0.006
Prior Convictions	0.059	0.142	0.020	8.350	0.004
Prior Felonies Against Person	0.076	0.059	0.043	3.071	0.080
Employed/Graduate	0.053	0.053	0.029	3.221	0.073
Intercept	0.052				

n = 943
$R^2 = 0.216$
Adjusted $R^2 = 0.208$
F = 25.67
Probability = 0.0001

Table 3-27
Type of Sentence Regressed on
Guidelines Variables
Postguidelines Cases
Philadelphia

Variable	b	Beta	Standard Error of b	F	Probability
Type of Offense	0.047	0.226	0.009	26.833	0.001
Injury	0.099	0.172	0.028	12.884	0.001
Type of Weapon	0.109	0.107	0.047	5.507	0.019
Value of Property	0.200	0.141	0.061	10.733	0.001
Victim	0.013	0.013	0.047	0.080	0.777
Prior Incarceration	0.112	0.165	0.040	7.909	0.005
Liberty Status	0.140	0.128	0.054	6.819	0.009
Prior Convictions	0.065	0.147	0.027	5.893	0.016
Prior Felonies Against Person	0.101	0.074	0.066	2.393	0.123
Employed/Graduate	0.048	0.048	0.041	1.396	0.238
Intercept	-0.061				

n = 457
$R^2 = 0.276$
Adjust $R^2 = 0.261$
F = 16.96
Probability = 0.0001

Table 3-28
Test of Heterogeneity of Regression
Type of Sentence Regressed on Guidelines Variables
Pre- and Postguidelines Cases
Philadelphia

Source	Degrees of Freedom	Sum of Squares	Mean Square	F	Probability
Coincidence	11	1.5035	0.1367	0.7072	0.733
Intercepts	1	1.1148	1.1148	5.7672	0.016
Parallelism	10	0.3887	0.0389	0.2012	0.996
Residual (Separate Regressions)	1,378	266.3838	0.1933		
Residual (Pooled Regressions)	1,389	267.8873			

Number of cases = 1400
Number of groups = 2
Number of predictors = 10

Although there is no significant difference in the slopes of the regression lines, there is a significant difference (p = .016) in their intercepts. This indicates that the overall severity of sentences (but not their predictability) changed relative to the guidelines variables. Specifically, the intercept decreased from 0.052 before guidelines to -0.061 after guidelines. The issue of changes in the overall severity of sentences is discussed more fully below.[57]

Another set of regressions were performed in which sentence length was regressed on the guidelines variables.[58] The results are reported in Tables 3-29 and 3-30.

The proportion of variance in sentence length explained by the guidelines variables changed very little after the implementation of guidelines. Before guidelines, $R^2 = 0.113$; after guidelines, $R^2 = 0.115$. Moreover, it is obvious that the guidelines predictors do a very poor job of accounting for variation in sentences. Less than 12 percent of the variance in sentence length is accounted for by the model. This is further evidence of the guidelines' weak empirical basis.[59]

The test of heterogeneity of regression, reported in Table 3-31, indicates that there was no significant increase in the predictability of sentence length as a function of the guidelines variables (F = 0.5072 with 10, 608 degrees of freedom).

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57. See pp. 137-150.
58. For the reasons outlined above, the minimum term of imprisonment was used as the dependent variable in these regressions. See note 48, supra.
59. See Chapter 2.

Table 3-29
Length of Incarceration Regressed on
Guidelines Variables
Preguidelines Cases
Philadelphia

Variable	b	Beta	Standard Error of b	F	Probability
Type of Offense	5.332	0.125	2.148	6.162	0.013
Injury	29.132	0.303	5.206	31.309	0.001
Type of Weapon	0.692	0.004	10.350	0.004	0.947
Value of Property	5.442	0.021	12.726	0.183	0.669
Victim	15.301	0.077	10.164	2.266	0.133
Prior Incarceration	5.828	0.061	6.675	0.762	0.383
Liberty Status	12.121	0.062	11.262	1.158	0.282
Prior Convictions	-4.073	-0.052	6.072	0.450	0.503
Prior Felonies Against Person	5.682	0.026	11.205	0.257	0.612
Employed/Graduate	-16.718	-0.087	9.020	3.435	0.065
Intercept	-53.914				

n = 422
$R^2 = 0.113$
Adjusted $R^2 = 0.094$
F = 5.24
Probability = 0.0001

Table 3-30
Length of Incarceration Regressed on
Guidelines Variables
Postguidelines Cases
Philadelphia

Variable	b	Beta	Standard Error of b	F	Probability
Type of Offense	8.395	0.185	3.471	5.851	0.017
Injury	34.833	0.348	8.226	17.929	0.001
Type of Weapon	-24.652	-0.128	15.169	2.641	0.106
Value of Property	17.914	0.066	19.999	0.802	0.372
Victim	15.183	0.077	16.006	0.900	0.344
Prior Incarceration	-0.918	-0.009	10.873	0.007	0.933
Liberty Status	-15.352	-0.079	15.697	0.956	0.329
Prior Convictions	3.081	0.039	8.383	0.135	0.714
Prior Felonies Against Person	6.370	0.029	17.051	0.140	0.709
Employed/Graduate	-4.971	-0.026	13.098	0.144	0.705
Intercept	-57.212				

n = 208
$R^2 = 0.115$
Adjusted $R^2 = 0.075$
F = 2.56
Probability = 0.0062

Table 3-31
Test of Heterogeneity of Regression
Length of Incarceration Regressed on Guidelines Variables
Pre- and Postguidelines Cases
Philadelphia

Source	Degrees of Freedom	Sum of Squares	Mean Square	F	Probability
Coincidence	11	43,312	3,937	0.4651	0.925
Intercepts	1	383	383	0.0452	0.832
Parallelism	10	42,929	4,293	0.5072	0.885
Residual (Separate Regressions)	608	5,146,392	8,464		
Residual (Pooled Regressions)	619	5,189,704			

Number of cases = 630
Number of groups = 2
Number of predictors = 10

In summary, neither Denver nor Philadelphia showed any significant reduction in disparity, defined as unexplained variation in sentences, following the implementation of sentencing guidelines. More technically, the preguidelines and postguidelines samples are homogeneous with respect to the regression of sentences on the variables included in the guidelines. On the basis of the evidence assembled here, it appears that sentencing guidelines have failed to reduce disparity in Denver and Philadelphia. This failure is not surprising, given the relatively low levels of compliance reported above.60

It should be kept in mind, however, that our data include only the first six months following implementation of the Philadelphia guidelines. It is possible that the Philadelphia guidelines have had delayed effects, and that these effects will manifest themselves in later studies.

Sentence Severity

The sentencing guidelines in Denver, Chicago, Philadelphia, and Newark were designed to replicate past sentencing practices. They were intended not to affect the overall severity of sentences.61 As we saw in Chapter 2, however, the vagaries of statistical modeling are replete with opportunities for error. It is possible, therefore, that the

60. See pp. 87-108.
61. See L. Wilkins et al., supra note 2, at xvii-xviii.

overall severity of sentences was inadvertently increased or decreased as a result of the implementation of sentencing guidelines. In this section we shall explore that possibility.

We shall continue to define and measure sentence severity not as a unitary concept or dimension but as a bifurcated concept. This follows from the observation made in Chapter 2 that the decision wnether to incarcerate and that about length of incarceration are the product of different considerations.62 There is every reason to believe that judges, as well as offenders, see those two decisions as qualitatively different. This argues against the use of a single scale of severity. Moreover, as we shall see, the requirements of statistical control militate against the use of a single dependent variable.63 Consequently, two dependent variables are used: incarceration vel non and length of incarceration. Once again, length of incarceration will be measured by the amount of time until the prisoner is eligible for parole release.

Although there is something to be learned from simply observing the frequency and average length of incarceration over time, we must interpret these statistics with caution. Suppose, for example, that we observe an incarceration rate (number of persons incarcerated divided by the number of persons sentenced) of 60 percent in Time 1 and 70 percent in Time 2. We should hesitate before concluding that judges have become more severe. It is possible that for some reason, such as changes in the prosecutor's charging practices, the cases in Time 2 involved more serious crimes than those in Time 1. If this were true, an individual convict in Time 2 might be no more likely to be incarcerated than he or she would have been in Time 1. It would therefore be misleading to conclude that judges had become more severe from Time 1 to Time 2.

In order to avoid misconstruing differences in sentence severity that are attributable to differences in the cases rather than different sentencing practices, we must control for the variables that are related to sentence severity (except time, of course). This can be done either by cross-partitioning or by statistical adjustment. We have opted for statistical control so as to avoid the problems of handling a large number of subsamples containing relatively few cases.

Statistical control can be obtained by using predicted values from regression equations. In order to do so, one must identify the relevant variables, estimate parameter coefficients, and obtain for each case a predicted value of the dependent variable by substituting into the regression equation the values of the independent variables. The residual is then obtained by subtracting the predicted value from the

62. See p. 72-76.
63. Since the two decisions are determined by different variables, different regression models will be needed in order to control for the relevant variables.

observed value for each case, and can be interpreted as the value of the dependent variable after all of the independent variables have been controlled by statistical adjustment.64

For preguidelines cases in the Denver District Court, the incarceration rate, without controlling for case characteristics, was 64.1 percent (n = 1201). For postguidelines cases, 63.5 percent of convicted offenders (n = 2370) were incarcerated. Thus, there was no meaningful change in the overall incarceration rate. The unadjusted quarterly incarceration rates are graphed in Figure 3-9. (See also Appendix B, Table C-12.)

Despite the absence of pronounced differences or trends in incarceration rates (see Figure 3-9), it is possible that judges became more lenient or severe but that this change was suppressed by compensating changes in the types of cases in which sentences were imposed. Therefore, it is incumbent on us to examine incarceration rates after controlling for the factors that determine sentences. In order to do so, sentence, measured as a dichotomy (incarceration/ nonincarceration), was regressed on the variables found to be most predictive of the decision whether to incarcerate.65

A test for heterogeneity of regressionwas performed to determine whether, all other things being equal, the probability of incarceration was higher or lower after sentencing guidelines than before. Again, time was entered as a dummy variable (before/after), along with a vector of coefficients formed by multiplying the time variable by the other predictor variables. If sentences for similarly situated offenders were, on the average, higher or lower after guidelines than before, we would expect to see a significant difference in the intercepts of the two regression lines that represent the preguidelines and postguidelines cases. The results are displayed in Table 3-32.

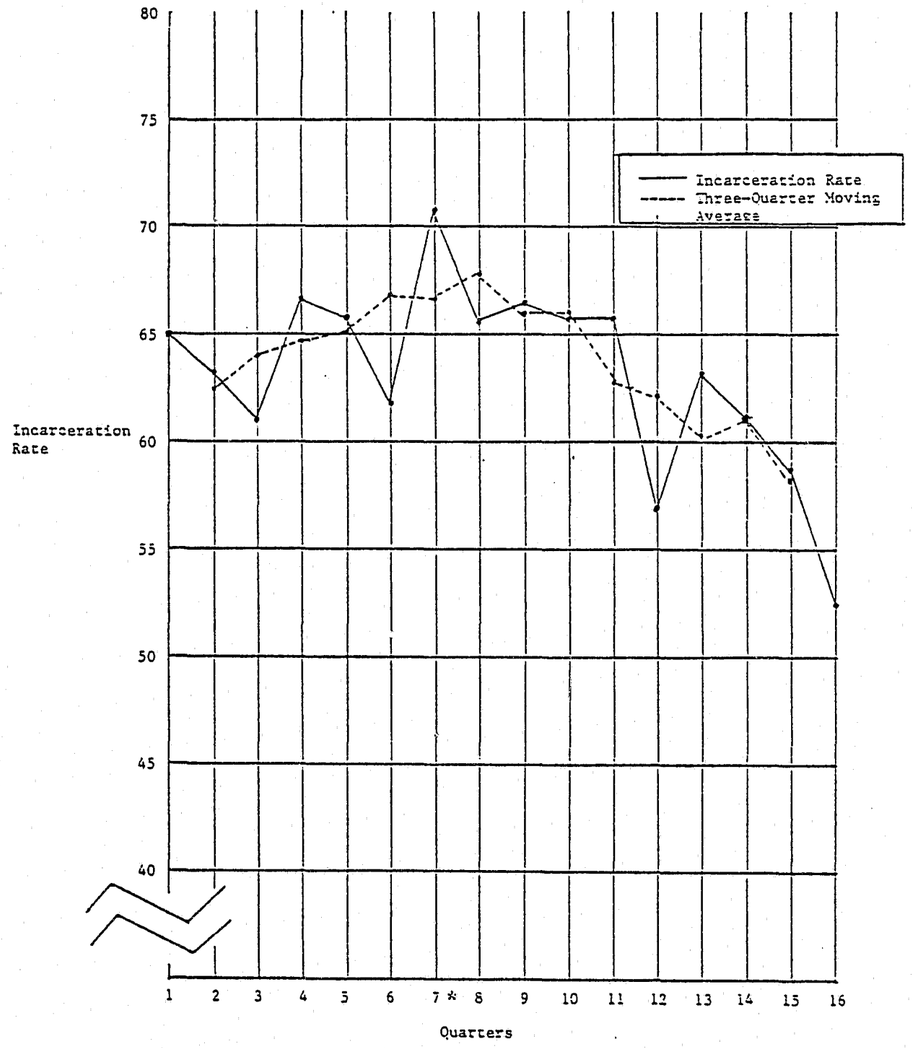
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64. See generally F. Kerlinger and E. Pedhazur, Multiple Regression in Behavioral Research 415-417 (1973). Of course, the degree of control that is obtained depends upon how well the model is specified. A poor-fitting model will result in inadequate control. Nevertheless, if no differences are found, specification errors probably are inconsequential. As a practical matter, only where differences are found need one be concerned that they reflect inadequate specification of the model rather than true differences between samples.

65. The variables found to be the best predictors of the decision whether to incarcerate in Denver were the statutory class of the offense at information (filing); prior adult sentences; juvenile record; prior probation or parole revocations; whether the offender was on bail, probation, or parole at the time of the offense; employment status; age; whether a person was put in jeopardy of personal harm; type of counsel; sex; and whether the offender was released on bail. For detailed regression results, see Appendix B, Tables C-13 through C-15.

Figure 3-9

Unadjusted Quarterly Incarceration Rates
Denver



*Sentencing guidelines implemented

Table 3-32
Test of Heterogeneity of Regression
Type of Sentence Regressed on Best Predictors
Denver

Source	Degrees of Freedom	Sum of Squares	Mean Square	F	Probability
Coincidence	13	1.85343	0.14258	0.86264	0.593
Intercepts	1	0.10967	0.10967	0.66353	0.415
Parallelism	12	1.74382	0.14532	0.87922	0.568
Residual (Separate)	2187	361.47510	0.16528		
Residual (Total)	2200	363.32859			

Number of cases = 2213
Number of groups = 2
Number of predictors = 12

There is no significant difference between the intercepts of the preguidelines and postguidelines regression lines ($F = 0.66353$; $p = .415$). In other words, when the relevant variables are controlled, the frequency of incarceration was not significantly higher or lower in the postguidelines sample than in the preguidelines sample.

In order to permit observation of trends, the semimonthly mean residuals are plotted over time in Figure 3-10. There are no obvious trends that would contradict our interpretation of the test of heterogeneity reported above.

Similar analyses were performed using length of incarceration as the dependent variable. First, unadjusted mean sentence lengths were computed for the preguidelines and postguidelines cases. Before sentencing guidelines, the average length of incarceration in Denver, as measured by time until parole eligibility, was 17.0 months; after sentencing guidelines, it was 21.1 months.

In order to permit observation of trends, the mean length of incarceration was computed for each quarter in the time series. These are graphed in Figure 3-11. (See also Appendix B, Table 16.)

It can be seen from Figure 3-11 that the relationship between time (in quarters) and sentence length is not monotonic, but there is a general upward trend that began well before the implementation of guidelines and appears to have continued uninterrupted thereafter. Therefore, it is unlikely that the difference in average sentence length for pre- and postguidelines cases was caused by the implementation of sentencing guidelines.

This interpretation is reinforced by testing the difference in the intercepts of regression equations for sentence length regressed on its

Figure 3-10
Scatterplot of Semi-monthly Mean Residuals
Type of Sentence Regressed on Best Predictors
Denver

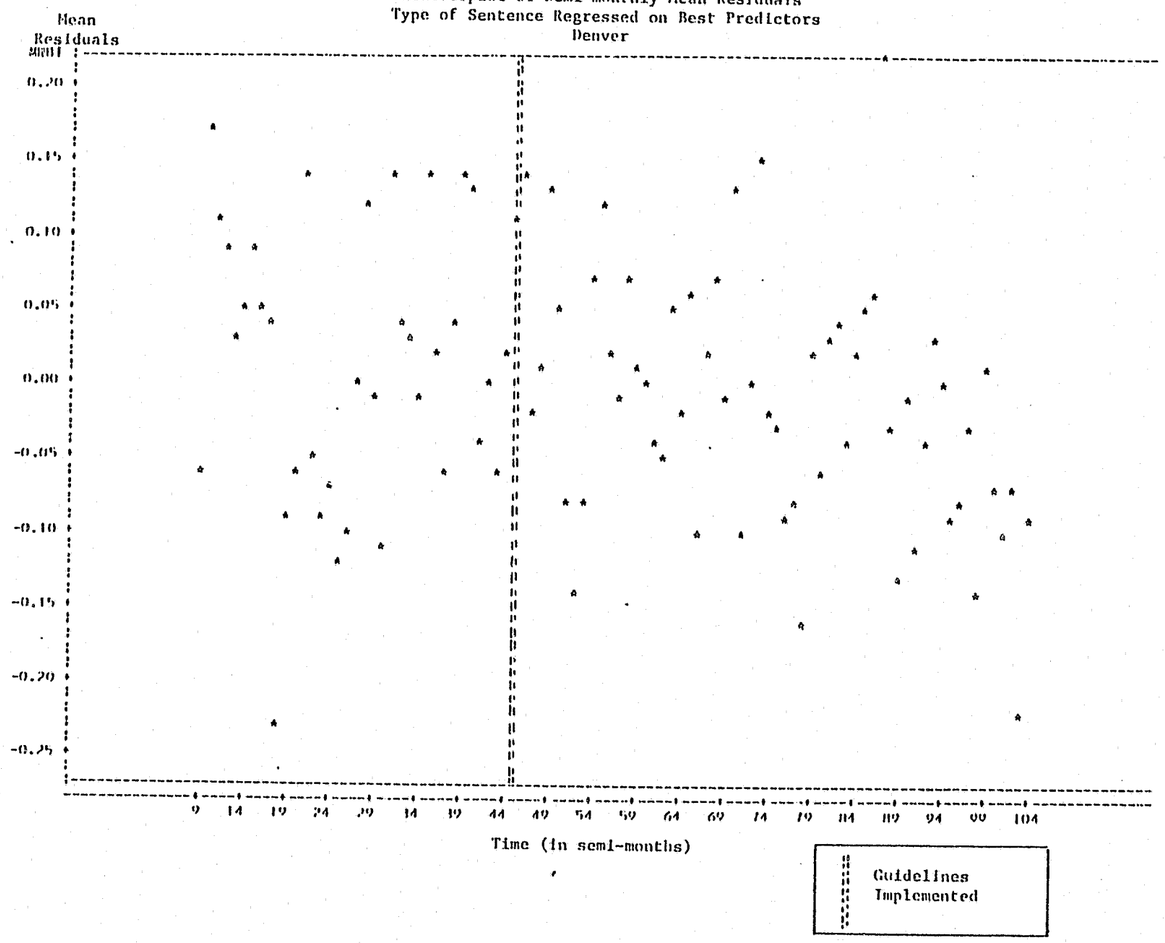
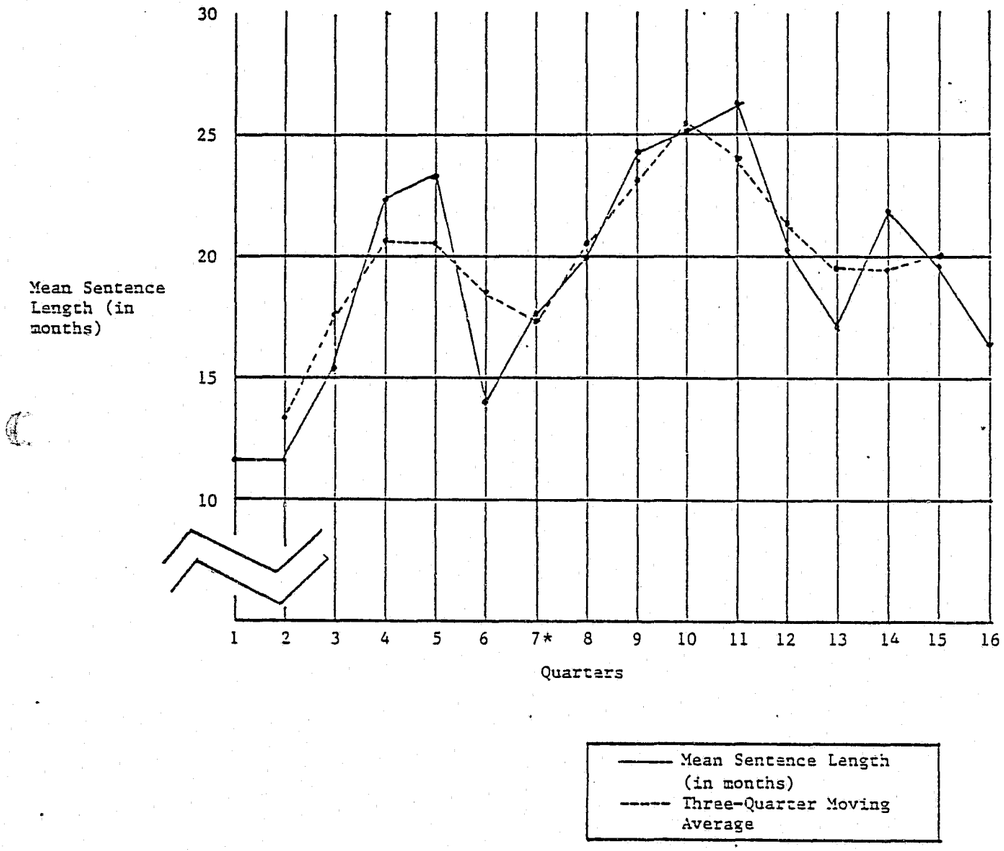


Figure 3-11
Unadjusted Mean Quarterly Sentence Lengths
Denver



*Sentencing guidelines implemented

best predictors for pre- and postguidelines cases.[66] The results of the test for heterogeneity of regression are summarized in Table 3-33. (See Appendix B, Tables C-17 through C-19 for detailed regression results.) The test shows no significant differences in the intercepts of the regression lines before and after guidelines. In Denver, in other words, when the relevant variables were controlled, sentences after guidelines were not significantly longer or shorter than those before guidelines.

Table 3-33
Test of Heterogeneity of Regression
Length of Incarceration Regressed on Best Predictors
Denver

Source	Degrees of Freedom	Sum of Squares	Mean Square Error	F	Probability
Coincidence	10	13479.187	1347.919	1.716	0.072
Intercepts	1	1318.289	1318.289	1.679	0.195
Parallelism	9	12160.898	1351.211	1.721	0.080
Residual (Separate)	1328	1042928.083	785.337		
Residual (Total)	1338	1056407.270			

Number of cases = 1348
Number of groups = 2
Number of predictors = 9

From the regression equation for the combined pre- and postguidelines sample (see Appendix B, Table C-19) we obtained predicted scores and, by subtraction from the actual scores, residuals. These residuals, which represent variation in sentence length after the predictor variables have been controlled, were averaged semi-monthly and plotted over time in Figure 3-12. Despite the lack of a statistically significant difference in the intercepts of the preguidelines and guidelines regression equations, a slight upward trend in sentence length is evident in the scatterplot. This trend, however, seems to have begun before the implementation of sentencing guidelines. There appears to be no interruption in the time series.

A parallel analysis was conducted to determine whether the Philadelphia sentencing guidelines affected the overall severity of sentences. During the 39 months preceding implementation of the Philadelphia guidelines, the average incarceration rate was 46.8

66. The best predictors of sentence length in Denver were the statutory class of the offense at information (filing); charge reductions; whether the case went to jury trial; the number of counts of which defendant was convicted; age; prior adult sentences; whether the offender was on bail, probation, or parole at the time of the offense; whether a person was put in jeopardy of personal harm; and whether the defendant was released on bail.

percent. The average incarceration rate during the six months following the implementation of guidelines was 48.5 percent. The difference is not statistically significant (Z = 0.6991; p = .2420).

Quarterly incarceration rates are graphed in Figure 3-13, from which it can be seen that the two postguidelines quarterly incarceration rates are within the range of preguidelines quarterly incarceration rates. (See also Appendix B, Table C-20.) The three-quarter moving averages indicate a slight upward trend throughout the time series. In the absence of statistical controls, there is no evidence of a change in the incarceration rate as a result of judges' use of sentencing guidelines.

Again, it is possible that changes in the kinds of cases coming before the judiciary have masked the effects of sentencing guidelines on the overall severity of sentences. In order to test this possibility, type of sentence was regressed on its best predictors.[67] The research hypothesis is that once the relevant variables have been controlled, there will be a difference in the average incarceration rate between the pre- and postguidelines samples. This is equivalent to positing a difference in the intercepts of the pre- and postguidelines regression lines. Results of the test of heterogeneity of regression are presented in Table 3-34. (See also Appendix B, Tables C-21 through C-23.)

Table 3-34
Test of Heterogeneity of Regression
Type of Sentence Regressed on Best Predictors
Pre- and Postguidelines Cases
Philadelphia

Source	Degrees of Freedom	Sum of Squares	Mean Square	F	Probability
Coincidence	9	1.5795	0.1755	1.0792	0.375
Intercepts	1	0.0630	0.0630	0.3874	0.534
Parallelism	8	1.5166	0.1896	1.1659	0.316
Residual (Separate regressions)	1,577	256.4562	0.1626		
Residual (Pooled Regressions)	1,586	258.0357			

Number of cases = 1595
Number of groups = 2
Number of predictors = 8

67. The best predictors of the decision whether to incarcerate were the number of offenses convicted of; whether the crime was against persons or property; whether a victim was killed; whether the defendant went to jury trial; whether the defendant failed to appear for court proceedings; the defendant's sex; the prior record of the defendant; and whether the defendant was released on bail or personal recognizance pending adjudication.

Figure 3-12
Scatterplot of Semi-monthly Mean Residuals
Length of Incarceration Regressed on Best Predictors
Denver

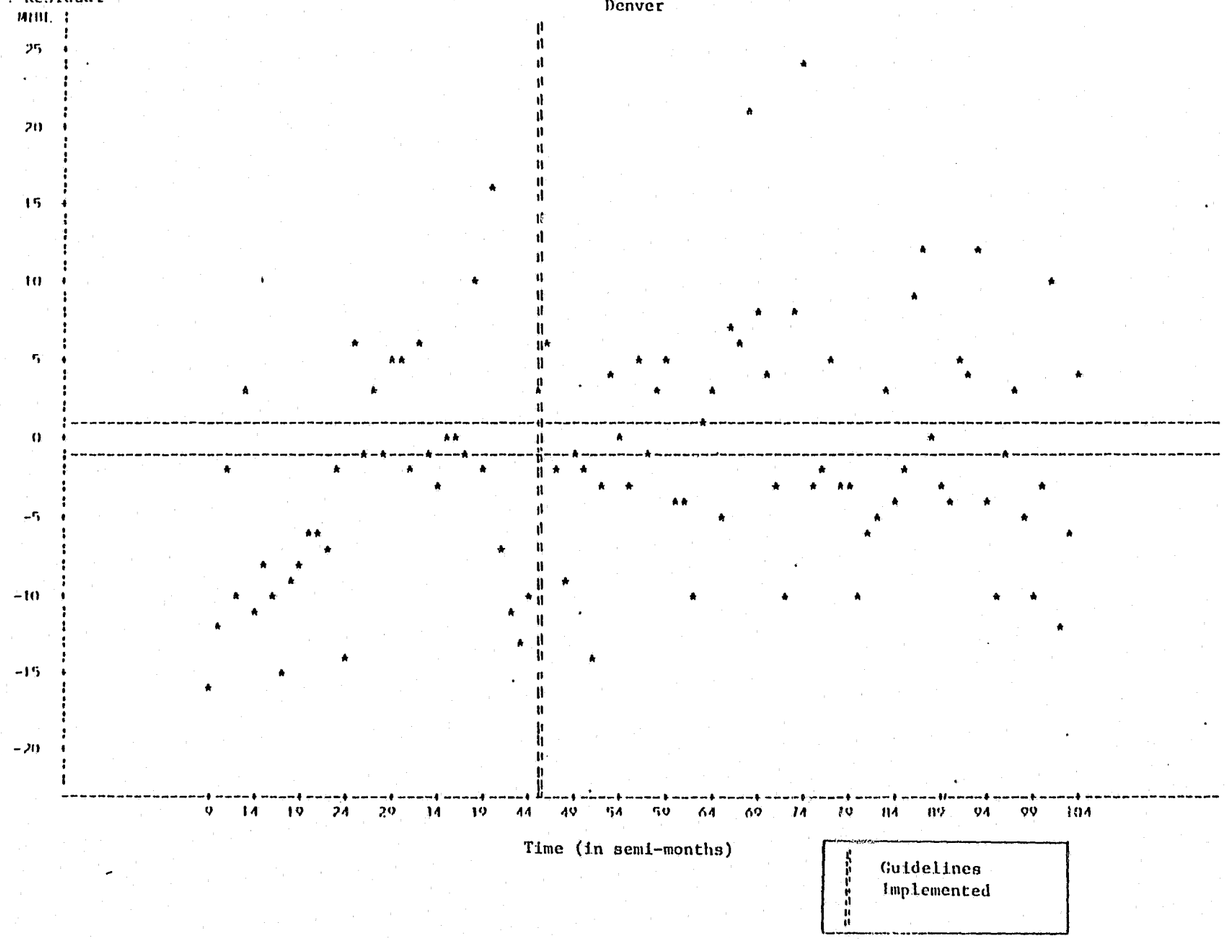
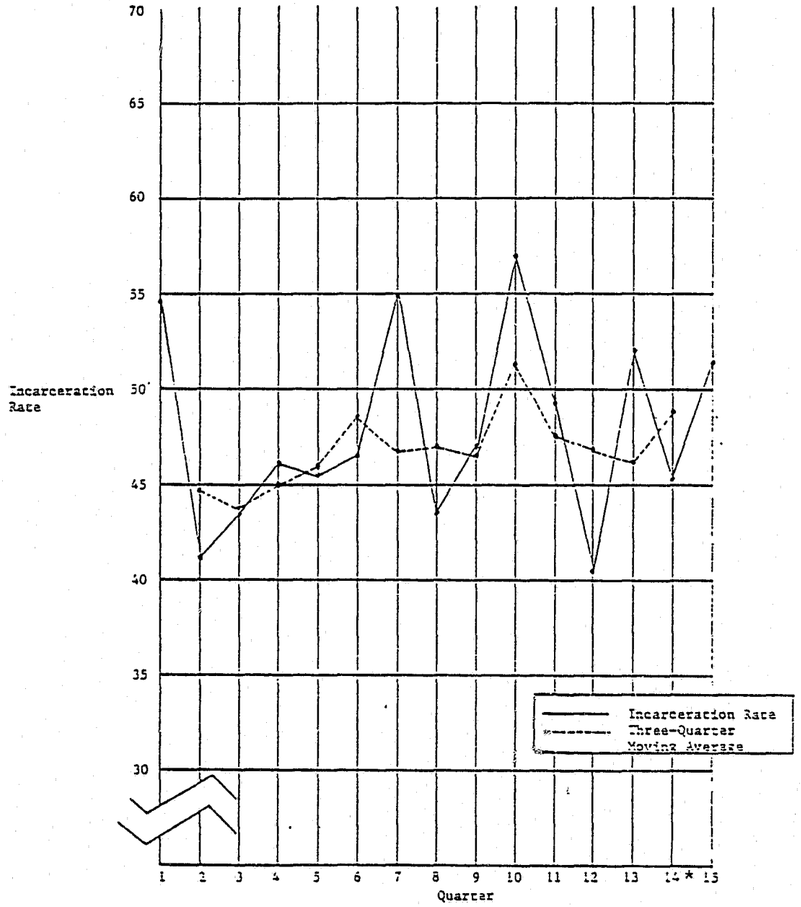


Figure 3-13
Unadjusted Quarterly Incarceration Rates
Philadelphia



*Sentencing guidelines implemented

The test of heterogeneity of regression indicates that there was no significant change in either the intercept or the slope from the pre- to postguidelines sample. In other words, once the relevant variables are controlled, neither more nor fewer offenders were sentenced to imprisonment after guidelines than before.

The average minimum term during the 39 months preceding implementation of the Philadelphia guidelines was 22.6 months. During the six months following implementation of the guidelines, the average minimum term was 22.9 months. This difference of 0.3 months is so small as to be meaningless. Figure 3-14 shows the average quarterly minimum terms plotted over time. (See also Appendix B, Table C-24.) From Figure 3-14 it is clear that the average minimum terms during the two quarters following the implementation of guidelines were well within the range of the quarterly preguidelines averages. The three-quarter moving averages indicate a slight trend toward longer sentences over the entire time series.

In order to measure sentence length while controlling for the effects of variables other than time, minimum term of imprisonment was regressed on its best predictors.[68] The intercepts of the pre- and postguidelines regression lines were compared by testing for heterogeneity of regression. The results are reported in Table 3-35. (See also Appendix B, Tables C-25 through C-27.)

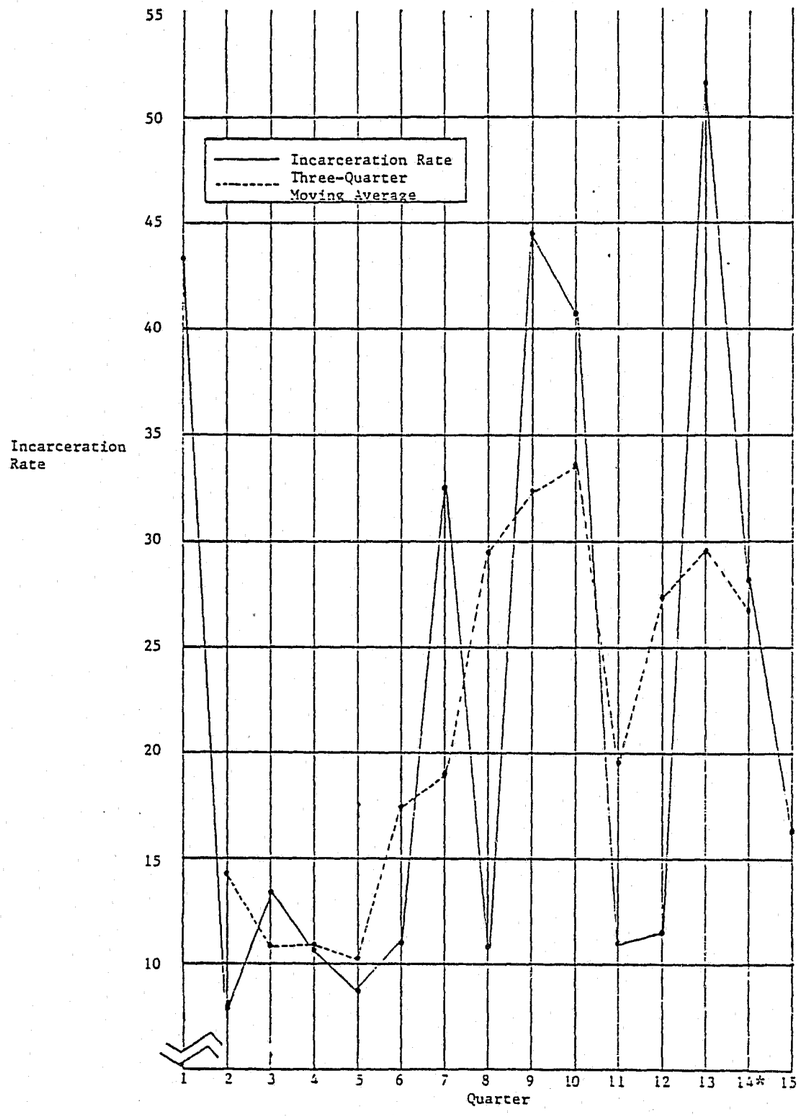
Table 3-35
Test of Heterogeneity of Regression
Length of Incarceration Regressed on Best Predictors
Pre- and Postguidelines Cases
Philadelphia

Source	Degrees of Freedom	Sum of Squares	Mean Square	F	Probability
Coincidence	6	70,467	11,745	1.6634	0.127
Intercepts	1	5,783	5,783	0.8190	0.366
Parallelism	5	64,684	12,937	1.8322	0.104
Residual (Separate Regressions)	814	5,747,444	7,061		
Residual (Pooled Regressions)	820	5,817,911			

Number of cases = 826
Number of groups = 2
Number of predictors = 5

68. The best predictors of sentence length were whether a victim was killed; whether the defendant went to jury trial; the statutory class of the offense at conviction; whether the offense was against persons or property; and whether a victim was injured.

Figure 3-14
Unadjusted Mean Quarterly Sentence Lengths
Philadelphia



*Sentencing guidelines implemented

The test of heterogeneity of regression indicates that there is no significant difference in either the intercepts or the slopes of the regression lines of the pre- and postguidelines samples. From this we conclude that when the variables that affect sentence length are held constant, sentences to imprisonment were no longer or shorter after guidelines than before.

All indications in Denver and Philadelphia point to the same conclusion. Sentencing guidelines did not affect the overall severity of sentences.

Conclusions

As we said at the beginning of this chapter,[69] no single measure or test should be taken as conclusive. Each has its limitations and most are subject to more than one interpretation. Nevertheless, these diverse measures almost unanimously converge on a single conclusion: sentencing guidelines have had no detectable, objectively manifested impact on the exercise of judicial sentencing discretion.

Judges frequently departed from the sentences recommended by the guidelines. Denver judges rarely provided written reasons for their departures. There was no clear increase in the proportion of sentences that fell within the guidelines' prescriptions. Racial and sexual disparities, where extant before guidelines, showed no signs of reduction. Neither was there a significant diminution of statistically unexplained variation in sentences. Sentencing guidelines did not enhance the predictability of sentences.

Two caveats are in order. First, it should be recalled that this study includes no data on the impact of the Philadelphia sentencing guidelines after the first six months of their use. The possibility of delayed effects still exists. Indeed, there is good reason to think that changes in sentencing practices will be gradual. The guidelines may have their greatest effect as new judges are rotated onto the criminal bench. Perhaps Philadelphia's experience will turn out to be different from Denver's.[70]

The second caveat is that the lack of impact evident in Denver and Philadelphia does not necessarily mean that sentencing guidelines are inherently incapable of accomplishing their purposes. The ineffectuality of the Denver and Philadelphia guidelines may be explained by a number of circumstances having no necessary relationship to sentencing guidelines. For example, the guidelines of Denver and Philadelphia were adopted voluntarily by the local judiciaries. There were no sanctions for failure to consult the guidelines or for failure to follow their recommendations. Guidelines promulgated by an administrative body pursuant to legislative mandate, or by a state supreme court under its constitutional rulemaking authority, and backed by legal remedies for

69. See p. 82.
70. But see Chapter 4.

CONTINUED

2 OF 5

noncompliance, might have significant effects on the exercise of judicial sentencing discretion.

On the other hand, it may be that environmental constraints common to many courts prevented the guidelines from having their intended effects. Plea bargaining, the process by which most cases are disposed of, may limit the effectiveness of sentencing guidelines. This possibility is considered in the next chapter.

Chapter 4

Sentencing Guidelines and the Exercise of Discretion by Prosecution and Defense

The overwhelming fact of American criminal justice is that the vast majority of criminal convictions are the result of bargained-for pleas of guilty.[1] Although virtually any concession within the power of the government to grant could be the subject of negotiation, most defendants are concerned primarily if not exclusively with the sentence. Charge reductions frequently induce guilty pleas, but it is usually the consequent sentence reduction that is sought by the defendant. The inextricable link between plea bargaining and sentencing makes it folly to address one without considering the other. In a very real sense, plea bargaining _is_ sentencing.

Nominally, it is the judge who decides what sentence to impose. In plea-bargained cases, however, the defense and often the prosecution[2] must consent to the procedure if not the outcome. It is within their discretion to withhold consent and thereby prevent the formation of a plea agreement. To the extent that the judge is interested in disposing of the case by plea of guilty rather than trial, he or she is dependent upon the parties' exercise of discretion. Similarly, to the extent that

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1. D. Newman, _Conviction: The Determination of Guilt or Innocence Without Trial_ 3-6 (1966); H. Miller, W. McDonald, and J. Cramer, _Plea Bargaining in the United States_ 19 (1978). Our data indicate that 92.1 percent of convictions in the Denver District Court are obtained by plea of guilty or _nolo contendere_; bench trials account for 0.9 percent of convictions, jury trials for 7.0 percent. The picture in Philadelphia is somewhat different: 59.2 percent of convictions are obtained by plea of guilty or _nolo contendere_. Bench trials account for 37.5 percent of convictions, jury trials for only 3.2 percent. Concerning the frequency of bench trials in Philadelphia, _see_ note 72 _infra_. Judges, prosecutors, and defense attorneys in Denver told us that unbargained-for pleas of guilty or _nolo contendere_ are almost unheard of. In Philadelphia there are a substantial number of "open pleas of guilty," but most of these actually involve some sort of bargain, tacit or otherwise. _See_ p. 172.

2. There is no constitutional right to waive a jury trial. Singer v. United States, 380 U.S. 24 (1965). The Pennsylvania legislature has declared that "the Commonwealth shall have the same right to trial by jury as does the accused." 42 Pa. Cons. Stat. Ann. §5104(c) (1980 Supp.). Although the defendant has the right to waive a jury trial in Illinois [Ill. Ann. Stat. ch. 38, §103-6 (1970); People v. Spegal, 5 Ill. 2d 211, 125 N.E. 2d 468 (1965)], and apparently in Colorado except for capital offenses [Colo. R. Crim. P. 23(a)(5)], the need to maintain smooth working relationships among the judge, prosecutor, and public defender creates pressure to operate on a consensual basis. Moreover, if charge reductions are needed to effectuate a plea agreement, the prosecutor's consent is convenient if not essential. _See_ p. 181.

the prosecutor desires the certainty of conviction of a guilty plea rather than the risk of acquittal inherent in the trial process, he or she depends upon the discretionary decisions of the judge and the defense. Finally, to the extent that the defendant wishes to exchange constitutional rights for leniency, he or she depends upon the discretionary decisions of the judge and prosecutor. The three major participants in criminal proceedings move about in a web of mutual discretion.

Knowing the interdependence of judge, prosecutor, and defendant, one should not expect to alter the discretionary decisions of the judge without also affecting those of prosecution and defense. Sentencing guidelines are an attempt to structure, and thereby alter, the sentencing discretion of the judge. To the extent that they are successful, we should expect them to influence the actions of prosecutor and defendant. Conversely, the judge's dependence on the discretionary actions of prosecutor and defendant, if not adequately taken into account, may frustrate the reformers' attempt to structure judicial sentencing discretion through guidelines. To the prosecutor, defendant, and perhaps the judge, sentencing guidelines may become an inconvenience requiring circumvention. Or, the guidelines may become completely irrelevant to their daily work. Worst of all, they may distort the criminal process in ways that were not intended or even contemplated by the reformers.

In this chapter we shall discuss the effects of sentencing guidelines on the balance (or imbalance) of discretionary power among judge, prosecutor, and defendant. We have already seen that the Denver and Philadelphia guidelines had little demonstrable impact on the exercise of judicial sentencing discretion. Shortly we shall see that their impact on prosecution and defense was virtually nil. Nevertheless, it is useful to focus on the plea bargaining process because it provides a large part of the explanation for the guidelines' failure to achieve their goals.

The Elements of Discretion

Before proceeding further, we think it useful to review some of the discretionary decisions of prosecution and defense that determine the eventual penalty. Sentencing guidelines have the potential to affect all of these decisions. Correlatively, each of these decisions has significance for the successful introduction of sentencing guidelines. Although the relevant decisionmaking begins not in the court but on the street, we shall, for the most part, arbitrarily limit ourselves to the actions of prosecutors, defense attorneys (and defendants), and judges. This limitation is adopted for the sake of convenience and economy, and is not meant to imply that other decisions, such as those of the police, victims, and witnesses, are unimportant.

The threshold question in a criminal case is whether to prosecute. This decision is made initially by the police, in the sense that their failure to press charges is dispositive. Once the matter is in the prosecutor's hands, he or she may refuse to file all or some of the

charges in the magistrate's court.[3] It is the prosecutor who determines how many and exactly which charges should be brought, including applicable habitual,[4] violent,[5] or sex offender[6] charges. Once the charges have been filed, the prosecutor generally retains the power to dismiss the case.[7] Short of outright dismissal, prosecutors frequently have the power to condition dismissals on successful completion of a period of probation supervision.[8] These programs frequently are viewed

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3. In Philadelphia, however, the police file the charges in the Municipal (magistrate's) Court. See p. 168.

4. E.g., Colo. Rev. Stat. §16-13-101 (1973) (enhanced penalties for felons having two or more prior felony convictions). In Colorado the prior convictions must be alleged in the information or indictment and proved. Colo. Rev. Stat. §16-13-103.

5. E.g., Colo. Rev. Stat. §16-11-309 (1977 Supplement) (prescribing mandatory minimum terms of imprisonment upon conviction of certain violent crimes). In Colorado a special count must be included in the information or indictment in order for the mandatory minimum to apply. Colo. Rev. Stat. §16-11-309(4).

6. E.g., Colo. Rev. Stat. §16-13-201 et seq. (1973) (prescribing indeterminate-to-life commitment of dangerous sex offenders). This proceeding can be commenced by motion of the prosecutor, the defendant, or the court. Colo. Rev. Stat. §16-13-205.

7. In many states the prosecutor cannot dismiss a case without stating the reasons for dismissal and obtaining consent of the court. E.g., Colo. R. Crim. P. 48(a). As a practical matter this constraint amounts to little because, although the judge can force prosecution of the case, he or she cannot force diligent prosecution. The judge's consent is tantamount to mere formality, and the prosecutor routinely gives pat reasons such as "insufficient evidence."

8. For example, in Colorado the court may, with the consent of the defendant and the prosecutor, defer prosecution of a criminal case for up to two years and place the defendant under the supervision of the probation department. Upon successful completion of the supervisory period, the charges are dismissed with prejudice. If the conditions of supervision are violated, prosecution may be resumed. The defendant's acceptance of a deferred prosecution entails a waiver of the right to speedy trial but not the right to a trial. Colo. Rev. Stat. §16-7-401 (1973). In practice, the judge rarely countermands the prosecutor, so it is the prosecutor who grants or denies deferred prosecutions. A similar program, called "accelerated rehabilitative disposition," exists in Pennsylvania. See Pa. R. Crim. P. 175-185.

In Colorado there is also a program called "deferred sentence" or "deferred judgment," in which the defendant pleads guilty, but, subject to the agreement of both parties and the court, the case is continued for a period of up to two years without entry of a judgment of conviction. Upon satisfactory completion of probationary supervision, the plea is withdrawn and the case is dismissed with prejudice. Violation of the conditions of probation can result in entry of a judgment of conviction and sentencing. The defendant is deemed to have waived trial rights. Colo. Rev. Stat. §16-7-403 (1975 Supp.). Although Denver judges report that they occasionally deny a deferred judgment despite approval by the prosecutor, these instances are rare. Moreover, disapproval by the prosecutor is determinative.

as lesser penalties than probation following a conviction, but the only difference between them is the fact of conviction. In other words, the conviction itself is a form of punishment,[9] which the prosecutor has the discretionary power to avoid.

The defendant's position is inherently weak. It is only by the credible threat of exercising legal rights that the defendant possesses any power to withstand the state's prosecutorial power. The exercise of defendant's rights is costly to the state in terms of resources, delay, and the risk of nonconviction. So long as the defendant is willing to waive speedy trial rights[10] (and many are), the use of dilatory tactics is a valuable weapon in the limited defensive arsenal. Witnesses forget, die, lose their anger, or tire of unproductive appearances in court. Stale cases can be a source of embarrassment to the court.[11] If free on bail, the defendant has the opportunity to demonstrate his or her ability to lead a straight life and thereby persuade the court or even the prosecutor that incarceration is unnecessary; if the court and prosecutor are unpersuaded, the defendant has at least had an additional period of time "on the outside."

Although any right can be bargained, trial rights are the usual subject of negotiations. Trials, especially jury trials, are expensive, time-consuming, and risky in their outcomes. For every day of trial, from four to ten cases can be disposed of by guilty plea. The parties are saved substantial amounts of trial preparation. Juror and witness fees are conserved. The state is spared the risk of an acquittal, a hung jury, a reversal on appeal, or a successful habeas attack. While the debate about the effects of court caseloads on trial rates continues,[12] it is clear that judges, prosecutors, and defense attorneys perceive a need to dispose of most cases by guilty pleas.[13] A Denver judge expressed the almost universal view this way: "Plea bargaining is an absolute necessity. You could not run a judicial system [without it]." Except in those relatively few cases that go to trial, the value of trial

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9. The idea that a conviction per se constitutes a criminal penalty is illustrated nicely by a Pennsylvania statute that authorizes a "determination of guilt without further penalty." 18 Pa. Cons. Stat. Ann. §1323 (1978-79 Supp.) (emphasis added).

10. In Colorado, Illinois, and Pennsylvania, the defendant has a qualified right to a trial within a fixed period of time. Colo. Rev. Stat. §18-1-405 (1973) (six months); Ill. Ann. Stat. ch. 38, §103-5 (1973) (120 days for persons in custody; 160 days for persons on bail or personal recognizance); Pa. R. Crim. P. 1100 (180 days).

11. See p. 178.

12. See M. Heumann, A Note on Plea Bargaining and Case Pressure, 9 Law & Soc'y Rev. 515 (1975); M. Feeley, The Process is the Punishment 244-277 (1979).

13. A majority of the U.S. Supreme Court shares this perception. E.g., Santobello v. New York, 404 U.S. 257, 260 (1971). See also the remarks of Chief Justice Burger, Address at the American Bar Association Annual Convention, N.Y. Times, August 11, 1970, p. 24, col. 4, quoted in Y. Kamisar, W. LaFave, and J. Israel, Modern Criminal Procedure 1096 (4th ed. 1974).

rights to the defendant derives from the cost that the state must bear when they are exercised. It is within the defendant's discretion to forgo a trial. The exercise of that private discretion depends upon how the prosecutor and judge exercise their official discretion. The question is, What can the defendant get for a waiver, or, more accurately, what does the defendant think he or she is getting for a waiver?

The term "plea bargain" refers to a process as well as an outcome. There is considerable variation in the process from one jurisdiction to another, and sometimes even within a single jurisdiction. It is within the discretion of the prosecutor to refuse to bargain entirely,[14] to engage only in certain kinds of bargaining,[15] or to engage in bargaining only at certain stages of the proceedings.[16] The bargain can be explicit or implicit;[17] the defendant's waiver can be induced by an express promise, a tacit understanding, or a general expectation of leniency. The inducement can take the form of a charge reduction (i.e., substitution of a lesser offense or dismissal of some of the charges) or a sentence concession.[18] These are loosely referred to as charge bargaining and sentence bargaining, respectively. The distinction is one of form rather than substance. As we have said, ultimately the defendant cares about the sentence. Charge manipulations are a means of bounding the judge's sentencing discretion. Nevertheless, the means by which the sentence is arrived at is central to our inquiry. What are the processual implications of sentencing guidelines? What are the

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14. The Attorney General of Alaska has ordered prosecutors in that state not to plea bargain. See M. Rubinstein and T. White, Alaska's Ban on Plea Bargaining, 13 Law & Soc'y Rev. 367 (1979).

15. The District Attorney of Denver has prohibited his deputies from negotiating sentences. See p. 178.

16. Denver prosecutors are not permitted to engage in charge bargaining after a case has been set (calendared) for trial. See p. 160.

17. See A. Alschuler, The Trial Judges Role in Plea Bargaining, Part I, 76 Columbia L. Rev. 1059, 1076-1099 (1976) [hereinafter cited as The Trial Judge's Role]; L. Ohlin and F. Remington, Sentencing Structure: Its Effect Upon Systems for the Administration of Criminal Justice, 23 Law & Contemp. Prob. 495, 502-3 (1958) [hereinafter cited as Ohlin and Remington].

18. Where the judge does not participate in plea negotiations, the prosecutor's "sentence concession" can take the form of a promise not to oppose probation, a promise not to make a sentence recommendation, a promise to recommend a particular sentence, a promise that a particular sentence will be imposed, or a representation that the judge generally imposes a particular sentence in cases like the one being negotiated. Some judges who participate in plea negotiations promise specific sentences; others make qualified promises or promises in hypothetical form; others purport merely to describe their usual practices in similar cases; still others resort to vague statements that imply a concession of undetermined dimensions. For a rich, detailed description of the myriad forms and facets of plea negotiations, see The Trial Judge's Role, supra note 17.

implications of the plea bargaining process for the implementation of sentencing guidelines? We begin with an examination of plea bargaining in the courts where guidelines were implemented.

Plea Bargaining in Three Courts

Ohlin and Remington have observed that the statutory sentencing structure has an important influence on the nature of the plea bargaining process.[19] To the extent that the inducement of guilty pleas requires sentence concessions,[20] if the statutory sentencing structure gives wide discretion to the judge, there is no reason to bargain for reduced charges; the defendant may bargain directly for a sentence concession or plead guilty on the expectation, presumably based on prior practice, that the judge will impose a lesser sentence. Where the statutory sentencing structure limits the judge's discretion, the prosecutor must make concessions in the form of charge reductions in order to induce guilty pleas. This is particularly true where the legislature has specified mandatory minimum sentences for certain crimes. In general, the less the overlap among the authorized prison terms for various felony and misdemeanor classifications, the greater the need to negotiate charge reductions. The influence of statutory sentencing structure on plea bargaining practices is evident in Denver, Philadelphia, and Chicago, the three sites in which we conducted extensive interviews.

Denver. In Colorado the judge's sentencing discretion was statutorily limited in several important respects during the period 1972-79.[21] Persons convicted of a Class Two or Three felony, if

19. Ohlin and Remington, supra note 17. See especially id. at 502-3, 505.

20. There are two reasons why sentence concessions might be unnecessary. First, it is possible to induce guilty pleas with illusory concessions. Illusory bargains do occur (see pp. 168-69, but experienced defendants and even inexperienced defendants represented by reasonably experienced and conscientious counsel are unlikely to be deceived. In many cases, therefore, illusory concessions will not suffice.

The other reason why concessions may be unnecessary is that defendants may be motivated to reduce uncertainty about the outcome of the case. The certainty resulting from an explicit plea agreement may have intrinsic value to the defendant, quite apart from the concession of a lesser sentence. The defendant no longer faces the possibility of a harsh sentence. See The Trial Judge's Role, supra note 17, at 1080-82. Although the reduction of uncertainty is no doubt a factor, it does not seem to be a sufficiently strong motive to induce guilty pleas in nine out of ten cases without the incentive of genuine sentence concessions.

21. On July 1, 1979, a new "presumptive" sentencing law went into effect in Colorado. The new law makes dramatic changes in the statutory sentencing structure and has obsoleted the Denver sentencing guidelines. Accordingly, our study is limited to the period ending June 30, 1979. For a detailed account of the statutory sentencing structure in effect during that time, see pp. 100-102.

incarcerated, must receive a minimum term of at least ten or five years, respectively. Persons convicted of a Class Four or Five felony, if incarcerated, must receive an indeterminate term, i.e., no minimum[22] unless they have been previously convicted of a felony within five years of the instant offense[23] or previously convicted of a violent offense,[24] in both of which cases the person must be imprisoned for a minimum term of at least two years for a Class Four Felony and one year for a Class Five Felony. While, in many cases, the judge retains discretion whether to incarcerate and, in serious cases, has considerable discretion with respect to length of incarceration, charge reductions frequently are necessary in order to obtain sentence concessions sufficient to induce guilty pleas. As one who is knowledgeable about Colorado's statutory sentencing structure might guess, charge bargaining is by far the predominant form of plea bargaining in the Denver District Court.[25]

The predominance of charge bargaining over sentence bargaining in the Denver District Court was reported with virtual unanimity by the judges, prosecutors, and defense attorneys we interviewed. It is evident from our case disposition data as well. Method of conviction[26] is cross-classified with charge reduction[27] in Table 4-1, using pre- and postguidelines cases.

22. Colo. Rev. Stat. §16-11-101(1)(b) (1973).
23. Colo. Rev. Stat. §16-11-101(1)(d) (1977 Supp.).
24. Colo. Rev. Stat. §16-11-309 (1977 Supp.).
25. Two of the more experienced Denver judges recalled that under the previous criminal code, which allowed the judge much greater discretion in sentencing, sentence bargaining, often with the active participation of the judge, was commonplace.
26. The method of conviction is a dichotomy between pleas of guilty or nolo contendere and trials. Jury trials and bench trials are grouped together because bench trials are so infrequent in Denver (note 1 supra) and because, unlike Chicago and Philadelphia, there was little suggestion in Denver that bench trials amounted to "slow pleas" or that they were rewarded by lesser sentences than would have been imposed following a jury trial. Our informants said that those few bench trials that do occur are cases in which either the defense was technical or complicated (e.g., entrapment, insanity), or the defendant was an unsavory character against whom a jury might react more strongly than a judge.
27. Charge reduction is a simple dichotomy between no charge reduction and some charge reduction. It is defined here as a reduction in the statutory class of the most serious offense from the filing of the information (formal charging in the felony court) to conviction. This definition ignores reductions in the number of charges per se because consecutive sentences are infrequent and prohibited by law where the offenses constitute the same act or series of acts arising from a single criminal episode. Colo. Rev. Stat. §18-1-408 (1973). A reduction in the number of charges without a reduction in the statutory class of the most serious charge rarely affects the sentence, and therefore is not usually a meaningful charge reduction.

It should be noted here that reductions from a Class Four Felony to a Class Five Felony are counted as charge reductions despite the fact that in

Table 4-1
Cross-classification of Method of Conviction
by Charge Reduction
Pre- and Postguidelines Cases
Denver

		Charge Reduction: No	Yes	Total
Method of Conviction	Plea	748*	2,449	3,197
		(76.3)	(98.1)	(91.9)
		(23.4)	(76.6)	
	Trial	232	48	280
		(23.7)	(1.9)	(8.1)
		(82.9)	(17.1)	
	Total	980	2,497	3,477
		(28.2)	(71.8)	

ϕ = 0.129
χ^2 = 449.7
Probability < .001

*The first entry in each cell is the frequency. The second entry (in parentheses) is the column percentage. The third entry (also in parentheses) is the row percentage.

Of those defendants who pleaded guilty or nolo contendere during the period of our time series, 76.6 percent received a charge reduction. Of those who were convicted at trial, only 17.1 percent were convicted of reduced charges.[28] These statistics say a great deal about the plea bargaining process in Denver. An overwhelming number of the judges, prosecutors, and defense attorneys we interviewed reported that unbargained-for charge reductions are rare.[29] Therefore, it is not much of an overstatement to say that in Denver the only way a defendant can secure a charge reduction and the consequent sentence concession is to plead guilty or nolo contendere.

Although charge bargaining has predominated over sentencing bargaining in Denver since the adoption of the new criminal code in the early 1970s, in January 1978 the Denver District Attorney, with the assistance of a grant from the Law Enforcement Assistance Administration, had undertaken a project to reduce plea bargaining.[30] Specifically, courtroom prosecutors were instructed not to negotiate pleas of guilty after felony cases had been set for trial and not to engage in sentence bargaining at any stage in the proceedings. In contrast to the Alaska project,[31] no attempt was made to eliminate all plea bargaining. Charge bargaining before the setting of a trial date was expressly approved. In order to achieve goals of the project, the District Attorney provided for improved screening and investigation of cases, and, at least in theory, earlier preparation of cases for plea bargaining.

In Denver it is the prosecutor who drafts and files complaints in the County (magistrate's) Court. As a result of the plea bargaining reduction project, the initial charging, which was already accurate in most cases, became somewhat more accurate.[32] It is fair to say that the device of

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some cases such reductions do not affect the judge's sentencing discretion in any significant way. Convictions of both classes of offenses result in indeterminate (one-day) minima if the offender is sentenced to imprisonment. The only difference is that in Class Four Felonies the judge can impose a maximum of up to ten years, whereas in Class Five Felonies the maximum sentence cannot exceed five years. Colo. Rev. Stat. §18-1-105 (1975 Supp.). Since parole eligibility is determined by the minimum term, Colo. Rev. Stat. §17-2-204(1) (1973), and since convicts rarely serve the maximum term without parole, a reduction from a Class Four to a Class Five Felony usually is inconsequential. The Denver defense attorneys we interviewed said that such "plea bargains" are illusory, that they themselves would never advise a client to accept one, and that only inexperienced or unscrupulous defense lawyers would allow their clients to accept such a bad deal. Nevertheless, where the defendant is convicted of a violent offense or has been convicted of another felony within five years preceding the instant offense, a reduction from Class Four to Class Five means the difference between a two-year mandatory minimum and a one-year mandatory minimum. See notes 22 and 23 and accompanying text supra. For this reason, reductions from Class Four to Class Five count as charge reductions in Table 4-1.

28. There are two ways of receiving a charge reduction in a case that goes to trial. First, the prosecutor may reduce the charges on his own motion prior to trial. This may be done for tactical reasons, or because election may be required under Colo. Rev. Stat. §18-1-408 (1973). According to our informants, gratuitous charge reductions are rare, and tactically it usually is thought better from the prosecutor's perspective to present all of the charges to the jury.

The second way that a charge reduction can result is for the jury or court to return a verdict of guilty to a lesser offense. As the figures in Table 4-1 indicate, such verdicts are infrequent. Juries usually convict on the highest charge.

29. Prosecutors and defense attorneys agree that unbargained-for charge reductions occur only where there has been a mistaken charging decision, usually pertaining to property valuation. Unbargained-for pleas of guilty also are infrequent, according to our informants.

30. J. Sterling, L. Tiffany, and W. Beaney, Denver Plea Bargaining Reduction Project--An Evaluation 1 (1979).

31. See note 14 supra.

32. See J. Sterling et al., supra note 29, at 24-30, 39. The judges and even most of the defense attorneys we interviewed agreed that the initial charges in the District Court are accurate in terms of the actual guilt of the defendant, and usually also in terms of what can be proved. Of the few who disagreed, the most common complaints related to the use of conspiracy charges in addition to charges of choate crimes, and the filing of charges on the assumption of the most culpable mental state.

overcharging defendants in order to coerce guilty pleas is rarely if ever employed in Denver.

Also as a result of the plea-bargaining-reduction project, the prosecutors become somewhat more selective in the cases they chose to prosecute. The annual number of case filings, which had been dropping before 1978, continued to decline.[33] More cases were disposed of as misdemeanors in the County Court.[34] The conviction rate in the District Court increased, as did the number of guilty pleas to the original charge.[35] The number of late guilty pleas decreased significantly.[36]

With few exceptions, Denver District judges do not participate in plea negotiation. Our interview respondents identified only one judge who takes an active role in plea bargaining. Some judges participate passively by indicating the likelihood that a particular sentence will be imposed, but many, perhaps most, remain aloof from the bargaining process. Of course, to the extent that the sentence is dictated by a charge reduction, judicial participation is unnecessary; sentencing discretion resides with the prosecutor. To the extent that sentencing discretion, especially discretion whether to incarcerate, remains with the judge, the judge must either follow the bargained-for prosecutorial sentence recommendation or, on his or her own initiative, induce guilty pleas by sentencing differentially on the basis of the method of conviction. When prosecutors do not make sentencing recommendations, the judge must (somewhat blindly) adjust his or her sentencing practices so that the number of trials is kept tolerably low. One judge reported that he gives defendants who plead guilty to reduced charges extra sentence consideration (leniency) simply because charge reductions indicate that the prosecutor, who is the authorized representative of the State, does not think that the case is serious enough to warrant severe treatment. This remarkable deference to the prosecutor serves a practical function: the inducement of guilty pleas without explicit participation in plea bargaining.

The procedural rules relating to plea bargaining in Colorado place the judge in a position that is in some ways awkward and in other ways comfortable. Although the Colorado Rules of Criminal Procedure expressly approve plea negotiations between prosecution and defense,[37] judicial participation is flatly prohibited.[38] Moreover, Rule 11(f)(5) declares,

> Notwithstanding the reaching of a plea agreement between the district attorney and defense counsel or defendant, the judge in every case should exercise an independent judgment in deciding whether to grant charge and sentence concessions.[39]

33. Id.
34. Id.
35. Id.
36. Id.
37. Colo. R. Crim. P. 11(f)(1).
38. Colo. R. Crim. P. 11(f)(4).
39. Colo. R. Crim. P. 11(f)(5).

Rule 32(e), on the other hand, requires the judge to allow withdrawal of a guilty plea if the judge decides not to make "the charge or sentence concessions contemplated by a plea agreement."[40]

In People v. Wright,[41] the Colorado Supreme Court held that the term "sentence concessions," as it is used in Rule 32(e), includes prosecutorial sentence recommendations. Wright mandates leave to withdraw a guilty plea whenever the judge fails to follow a negotiated recommendation from the prosecutor, even if the judge has admonished the defendant that the prosecutor's recommendation is not binding on the judge and the prosecutor has made no representations to the contrary.[42] In other words, Wright makes the prosecutor's recommendation binding in the sense that the defendant cannot be held to his or her guilty plea if the judge fails to cooperate. This ruling protects the defendant who pleads guilty from being denied the benefit of the bargain. Although judges cannot afford to disappoint too many defendants and thereby destroy the incentive to plead guilty, the Wright case prevents the occasional injustices that can result under a contrary rule.[43]

All of this leaves the defendant relatively well protected against uncooperative judges, and gives even that sentencing discretion that is not constrained by the statutory sentencing structure over to the prosecutor, who is free to bargain with the defense and whose promises (even promised recommendations) must be performed by the judge if he or she wishes to dispose of cases by guilty pleas. Along with discretionary power, however, goes political responsibility. Where the prosecutor

40. Colo. R. Crim. P. 32(e).
41. 194 Colo. 448, 573 P.2d 551 (1978).
42. Wright pleaded guilty to second degree assault in return for the prosecutor's promise to recommend probation if the defendant secured employment. The prosecutor made the recommendation, but the court, after warning the defendant that it would not be bound by the recommendation, sentenced Wright to imprisonment. The court refused to permit Wright to withdraw his guilty plea. On appeal, the prosecution argued that only promises to the effect that a particular sentence will be imposed, i.e., promises that the court will follow the recommendation, constitute "sentence concessions" within the meaning of Colo. R. Crim. P. 32(e). Mere promises to recommend a sentence were excluded from the definition urged by the prosecutor. The Colorado Supreme Court disagreed, however, observing that the prosecutor only has authority to make recommendations, not sentence concessions. The Court affirmed the decision of the Colorado Court of Appeals, which had reversed the decision of the trial court, and held that the defendant must be permitted to withdraw his guilty plea regardless of whether the prosecutor had promised that the trial court would follow the recommendation. Paradoxically, the practical effect of this decision is to give prosecutors the power to make sentence concessions. As one Denver judge observed, "The [Wright case] has taken discretion away from the judge and put it into the hands of the attorneys."
43. See note 88 infra.

makes binding recommendations, the judge can deflect the political repercussions of sentence concessions to the prosecutor by accurately pointing out that it is the prosecutor who makes the concessions. Prosecutors are in a politically vulnerable position. If they bargain, they can be blamed for treating defendants who plead guilty too leniently. If they refuse to bargain, they may precipitate trials in unmanageable numbers.

The District Attorney's prohibition of sentence bargaining can be seen in this light as a refusal to take political responsibility for the sentence concessions that are needed to induce a sufficient number of guilty pleas. The hot potato has been tossed back to the judge, who is better insulated from political heat.[44] Nonetheless, the District Attorney is not entirely uncooperative. Prosecutors still engage in the less conspicuous practice of charge bargaining, which is necessary to effect many sentence concessions under Colorado's statutory sentencing structure.

There is one final aspect of the Denver case processing system that requires mention: the method by which cases are assigned to judges (as well as prosecutors and public defenders), and the opportunities for judge shopping. Almost every prosecutor and public defender in the Denver District Court is assigned to a courtroom. Most remain in a single courtroom for at least a few months, sometimes up to a year or longer. When a complaint is filed in the County Court, it is assigned a preliminary hearing date, and at the same time is assigned to a courtroom (judge) in the District Court. The assignment to courtrooms is made on a rotating basis in the order in which the cases are filed. There is little opportunity to influence the assignment. Short of bribery, the only way to manipulate the assignment is for the prosecutor to withhold filing a case until exactly the right moment. This seems impracticable, and defense attorneys agree that it is not done with any significant frequency if at all.

For most felony cases in Denver, the preliminary hearing is the first opportunity to bargain. In the vast majority of cases, it is the first time that the prosecutor becomes familiar with the file. On the day of the preliminary hearing the deputy district attorney and the assistant public defender (if one is appointed) assigned to the District courtroom to which the case is assigned appear in the County Court. Plea bargains frequently are struck at the preliminary hearing. It is there that the only significant opportunity for judge-shopping occurs.[45] If the attorneys negotiate a guilty plea to a (reduced) misdemeanor charge, either the case can be disposed of before the preliminary hearing judge, who has jurisdiction over misdemeanors, or it can be filed as a felony in the District Court, where the bargain can be consummated in front of the

44. In Colorado, district attorneys are popularly elected to four-year terms, Colo. Const. Art. VI, §13, whereas district judges are appointed under a Missouri-like plan and need only stand for retention election every six years. Colo. Const. Art. VI §§10(2), 20, 25.

45. But see note 47.

judge to whose courtroom the attorneys are assigned. Our informants reported that a substantial number of cases are disposed of in the lower court, but that the identity of the sentencing judge is not usually a major factor in the decision where to plead.[46] Of course, if the agreement contemplates a guilty plea to a felony charge, the case must be bound over to the District Court because the County Court lacks jurisdiction over felonies. Once filed in the District Court, the case generally remains assigned to the courtroom to which it was assigned upon filing.[47]

The full-time assignment of prosecutors and public defenders to courtrooms has important implications for plea bargaining and, consequently, for the role of sentencing guidelines. Usually, three deputy district attorneys and three assistant public defenders are assigned to each criminal courtroom. These lawyers handle the vast majority of the business that transpires there. Over a period of months, they and the judge work together closely. Each gets to know the others well enough to permit relatively accurate predictions about how they will respond in specific situations. Assistant public defenders have a sense of what a particular prosecutor will offer in the way of a concession. They also are able to anticipate the sentence that the judge will impose. We heard the following assertion repeatedly in Denver: "After a while you get to know what a case is worth."

The differing and putatively adversarial roles of the participants notwithstanding, their common interests serve to unite them in the pursuit of dispositions. Prosecutors and public defenders know that they do not have enough staff to try every case. Not only must they maintain the cordiality necessary to a tolerable daily work routine, but they must also survive as a production unit. One Denver judge described it this way: "I used to meet with my people once a month to go over all the trials to come. We got so we were just like brothers. We were a team, working together."

46. One or two defense attorneys said that when they were assigned to the courtroom of a particularly severe or erratic judge, they sought to dispose of cases in the County Court whenever possible . Other defense attorneys preferred to dispose of cases in the District Court because "their" judge was more lenient. One attorney was apprehensive about disposing of cases in the County Court because (former) felonies might seem very serious compared to the County Court judge's usual docket of misdemeanors. Nevertheless, most lawyers we interviewed did not regard the plea to a misdemeanor as an opportunity to judge-shop.

47. There is one exception to this generalization. When a defendant has two or more cases pending in different courtrooms, they are sometimes consolidated and disposed of as a single case. If so, the consolidated case can be disposed of in any of the courtrooms to which a constituent case has been assigned. Prosecutors and defense attorneys report that they sometimes bargain over which judge should impose sentence. Numerically speaking, however, such cases are unimportant.

Although he was unusual if not unique in that few if any of his colleagues participated so actively in plea bargaining, his description of the relationships among the members of the work group, though perhaps somewhat overemphatic, no doubt applies in varying degrees to other courtrooms.[48]

This, then, is the environment into which the first sentencing guidelines were introduced in November 1976. The chief judge and the presiding judge of the criminal division, after consulting the other judges assigned to the criminal division, had agreed to participate in a study of the feasibility of empirically based, judicially adopted, voluntary sentencing guidelines. Individual judges' reactions ranged from enthusiasm to serious reservations,[49] but no one actively opposed the participation in the feasibility study. The dominant mood seems to have been one of acquiescence and cooperation.[50]

Before implementation of the guidelines, the criminal division decided en banc that the probation department, which was assigned responsibility for computing the guideline sentences, should distribute the guideline worksheets to the judges but not to the attorneys. The judges were concerned that distribution of the guidelines to the attorneys along with the presentence report might result in argument and eventually litigation about the use of the guidelines. Sentencing guidelines were not to be kept a strict secret, but they were to be downplayed as much as possible.

The judges succeeded remarkably in their attempts to minimize the guidelines. Although all but one of the attorneys we interviewed were aware of the guidelines' existence, few were familiar with them. Judges did not refuse to show them to the attorneys, but only a few attorneys ever consulted the guidelines. None did so regularly. The common

48. See J. Eisenstein and H. Jacob, Felony Justice: An Organizational Analysis of Criminal Courts (1977), wherein the authors analyze the disposition of criminal cases in terms of courtroom workgroups. See also A. Blumberg, Criminal Justice (1970).

49. One judge recalled, "I questioned the concept initially. I had serious problems with the project. It was essentially quasi-legislative. I didn't think it was proper for judges to come down with presumptive sentences when the statutes were of a totally different nature." At least one other judge objected on the grounds that the guidelines were an infringement on his judicial prerogative.

50. One judge described his reaction this way: "There have been so many projects in Denver that when a project comes along, frankly I take it with a grain of salt. We've had people coming in here endlessly wanting to study this and that. You just lose interest."

perception was that the guidelines were not important to the judges. Moreover, most experienced attorneys, especially those assigned to courtrooms, felt confident in their ability to predict the sentence without the assistance of empirically based sentencing guidelines. The remarks of one public defender are typical: "I didn't pay a lot of attention to them. It was a matter of knowing your judge. That was the most important thing. The guidelines were pretty unimportant compared to what judge you were in front of."

A judge described it this way: "I work with the same assistant district attorneys and deputy public defenders for a period of two years, generally speaking. After the first four or five months they knew pretty much what I was going to do, and after a year they knew exactly what I was going to do. You don't work with a man that long without learning something about him.... They may have looked at the sentencing guidelines, but they had a pretty good idea of what I was going to do."

The attorneys' failure to consult the guidelines cannot simply be attributed to the fact that they were not given copies. The district attorney had obtained a blank set of guidelines for a briefing of his staff. No doubt the public defender could have done likewise. Furthermore, most of the attorneys we interviewed knew that they could gain access to the guidelines worksheet for a particular case just by asking to see the official court file in the judge's chambers.

Moreover, refusal to divulge the guidelines worksheet would have been of questionable legality. In practice, the judges received their copies of the guidelines worksheets stapled to the front of the presentence report. They were computed on the basis of the information in the presentence report by the senior probation officer, who was responsible for reviewing all presentence reports and making probation recommendations. In fact, the probation officer used the guidelines in making his probation recommendations. The guidelines were physically and conceptually a part of the presentence report. Under Colorado law, copies of presentence reports must be furnished to the prosecutor and the defense counsel.[51] Almost incredibly, the issue of disclosure of the guidelines was never litigated.

Denver attorneys' indifference to sentencing guidelines can only be explained by their perception that the guidelines were inconsequential.

51. Colo. Rev. Stat. §16-11-102(1) (1973). See generally S. Fennell and W. Hall, Due Process at Sentencing: An Empirical and Legal Analysis of the Disclosure of Presentence Reports in Federal Courts, 93 Harv. L. Rev. 1615, 1674-77 (1980).

From their vantage point, it did not seem as if the judges were influenced by the guidelines to any significant degree.[52] Our statistical analysis in Chapter 3 substantiates this perception. The guidelines appear to have had no effect on the exercise of judicial sentencing discretion. Moreover, the attorneys did not feel need of a device to predict what sentence the judge would impose; in most cases they already knew. In many cases, it was the attorneys who had effectively decided the sentence.

Philadelphia. The Philadelphia Court of Common Pleas presents a sharp contrast to the Denver District Court, both in terms of the case-processing system and to some extent in terms of the role of sentencing guidelines. To begin with, Pennsylvania's statutory sentencing structure is quite different from Colorado's. For each statutory felony and misdemeanor classification, there is a statutory maximum that the maximum term cannot exceed,[53] and a requirement that the minimum term not exceed half the maximum term.[54] Within those minimal restrictions, the judge is free to impose any term of imprisonment. In addition, the place of incarceration (county versus state penitentiary) is left partly to the judge's discretion.[55] The judge's power to make a "determination of guilt without further punishment,"[56] order restitution, sentence to payment of a fine,[57] sentence to probation,[58] and sentence to partial confinement[59] is largely unrestricted.[60] Pennsylvania judges have as much statutory sentencing discretion as any judges in the country.

Sentence bargaining predominates over charge bargaining in the Philadelphia Court of Common Pleas.[61] Although charges frequently are

52. One judge assessed the guidelines' importance from his perspective this way: "This may surprise you: I rarely discussed how I used the guidelines with other judges, and they didn't discuss it with me either. So they were there, judges were using them, but it wasn't really a big deal."

53. 18 Pa. Cons. Stat. Ann. §§1102-1105 (1973).

54. 18 Pa. Cons. Stat. Ann. §1356(b) (1978-79 Supp.).

55. Where the maximum term is five years or longer, the offender must be confined at the state penitentiary. If the maximum is less than two years, the sentence ordinarily is served in the county penitentiary. Where the maximum sentence is two years or longer but less than five years, the place of incarceration is left to the judge's discretion subject to subsequent administrative transfers. Cf. 18 Pa. Cons. Stat. Ann. §1362 (1978-79 Supp.). Thus, either directly or indirectly by setting the maximum term, the judge can influence the place of confinement.

56. 18 Pa. Cons. Stat. Ann. §1323 (1978-79 Supp.).

57. 18 Pa. Cons. Stat. Ann. §1326 (1978-79 Supp.).

58. 18 Pa. Cons. Stat. Ann. §1322 (1978-79 Supp.).

59. 18 Pa. Cons. Stat. Ann. §1324 (1978-79 Supp.).

60. 18 Pa. Cons. Stat. Ann. §1321 (1978-79 Supp.).

61. Apparently, this has been true for some time. See Ohlin and Remington, supra note 17, at note 14 and accompanying text.

dismissed in the course of a negotiated disposition, defendants commonly plead guilty to the highest charge. This seemingly odd practice can be attributed to the abundant use of multiple charges, to the fact that the statutory sentencing structure rarely requires a charge reduction in order to effect a sentence concession, and to the Philadelphia court's unique system of inducing guilty pleas. We shall consider these in order.

It is the police who file the charges in the Philadelphia Municipal (magistrate's) Court. As a result, overcharging is customary. The charges are usually excessive in number and not infrequently excessive in terms of what the evidence can support. An ordinary armed robbery typically is charged as follows: robbery (and its lesser included offenses), reckless endangerment, terroristic threats, possession of an instrument of crime (two counts), possession of an offensive weapon, and violation of the Uniform Firearms Act. For a run-of-the-mill burglary of an unoccupied home, the charges typically include burglary, theft by taking, receiving stolen property, criminal trespass, and possession of an instrument of crime. Conspiracy and attempt charges are used liberally.

Although Pennsylvania law frequently permits consecutive sentences for multiple offenses arising out of the same transaction,[62] Pennsylvania judges usually have sufficient sentencing latitude under a single charge that multiple convictions seldom are necessary. Consecutive sentences to imprisonment occur in a relatively small minority of cases in Philadelphia.[63] Multiple charges are dismissed as

62. Unlike Colorado, Pennsylvania has no general statutory prohibition against consecutive sentences for multiple offenses arising out of the same criminal episode. Pennsylvania case law in this area suffers from some confusion but generally permits the imposition of consecutive sentences where multiple victims are involved. E.g., Commonwealth v. Marker, 231 Pa. Super. Ct. 471, 331 A.2d 883 (1974); Commonwealth v. Szulczewski, 233 Pa. Super. Ct. 389, 335 A.2d 810 (1975). See also Commonwealth v. Lockhart, 223 Pa. Super. Ct. 60, 296 A.2d 883 (1972). But see Commonwealth v. McCord, 114 Pa. Super. Ct. 480, 176 A. 834 (1935); Commonwealth v. Speelman, 235 Pa. Super. Ct. 109, 341 A.2d 138 (1975); and Commonwealth v. Reynolds, 256 Pa. Super. Ct. 259, 389 A.2d 1113 (1978). Where only one victim is involved, consecutive sentences usually are permitted except where they fail to satisfy the constitutional requirement that each offense require proof of a fact that the others do not. Blockburger v. United States, 284 U.S. 299 (1931). See 11A Vale Pa. Dig. §§984, 991(1). Under the new criminal code, however, a defendant cannot be convicted of both burglary and the crime that was the object of the breaking and entry, except where the object crime is a first or second degree felony. 18 Pa. Cons. Stat. Ann. §3502(d) (1973).

63. In our total Philadelphia case record sample, consecutive sentences to imprisonment were imposed in only 33 out of 1,927 cases, or 1.7 percent.

Sentences to probation consecutive to imprisonment, however, are not infrequent. In our total Philadelphia sample, probation was imposed

much to tidy the records as to protect against consecutive prison terms. There is little doubt that the bewildering assortment of initial charges also serves to convince some defendants that their lawyers have secured advantageous bargains. The potential for illusory bargaining is obvious. Nevertheless, as long as there is sentence bargaining (or its equivalent), there is no reason to seek a charge reduction. Sentence concessions are uninhibited by the statutory sentencing structure. Conversely, a charge reduction does not often guarantee a lighter sentence.

Pennsylvania law prohibits judicial participation in plea negotiations until after an agreement has been reached between prosecution and defense.[64] Pennsylvania judges, however, are permitted to listen to a tentative agreement and indicate whether they concur in it. This amounts to legitimation of the passive form of judicial participation in plea bargaining. So long as the judge does not initiate plea discussions and the formality of a tentative agreement is observed, the judge can be an effective participant without serious risk of reversal. In this manner, a substantial minority of Philadelphia judges participate in plea negotiations. It is not uncommon for the attorneys to present their agreement to the judge off the record to obtain the judge's consent before the plea is tendered. Some judges limit their participation to objecting whenever the attorneys have reached an unacceptable bargain; their silence implies consent. As a result, defendants have some assurance that their negotiated concessions will not be vitiated by an uncooperative judge.

Pennsylvania law, like that of Colorado, requires the judge to allow withdrawal of a guilty plea if the judge decides not to follow a bargained-for prosecutorial sentence recommendation.[65] This rule provides additional protection to the defendant against evanescent concessions,[66] while simultaneously giving the prosecutor the power to supplant the judge as sentencer unless the judge is willing to countenance additional jury trials.

The Philadelphia District Attorney, somewhat like his Denver counterpart, has announced a policy restricting prosecutorial

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consecutive to incarceration in 200 out of 1,927 cases, or 10.4 percent. This is a device by which a judge can maintain control over an offender after she or he has been paroled. Upon release from the penitentiary, the offender must report to a probation officer and otherwise satisfy the conditions of probation. The judge retains the power to revoke probation and reincarcerate the offender if the conditions are violated.

64. See Pa. R. Crim. P. 319(b)(1). See also Commonwealth v. Evans, 434 Pa. 52, 252 A.2d 689 (1969) (citing with approval, A.B.A Project on Minimum Standards for Criminal Justice, Standards Relating to Pleas of Guilty §3.3(b) (Approved Draft 1968).

65. Commonwealth v. Fazenbaker, 248 Pa. Super. Ct. 433, 375 A.2d 175 (1977); Commonwealth v. Sutherland, 234 Pa. Super. Ct. 520, 340 A.2d (1975); Commonwealth v. Edwards, 226 Pa. Super. Ct. 520, 313 A.2d 275 (1973); Commonwealth v. Barrett, 223 Pa. Super. Ct. 163, 229 A.2d 30 (1972). Cf. Commonwealth v. Evans, supra note 63.

66. See note 88 infra.

participation in plea bargaining. Courtroom prosecutors must consult their superiors before agreeing to reduce charges from one grade to another, but, as we have said, charge bargaining is a very minor part of plea bargaining in the Court of Common Pleas. More significantly, the district attorney has partially restricted his deputies' authority to sentence bargain. Philadelphia prosecutors continue to engage in the process of plea negotiation, but in many instances they offer sentence recommendations that substantially exceed the sentence that would be imposed in the absence of an agreement. Under these circumstances, the bargain gives no benefit to the defendant, and therefore is no bargain at all. Consequently, defense attorneys report that often it is not worth their while to speak with the prosecutor about plea bargains. The District Attorney's plea bargaining policy, like that of his Denver counterpart,[67] can be seen as an attempt to lay political responsibility for the sentence concessions needed to induce guilty pleas squarely on the shoulders of the judiciary.

Constrained by the rule against initiating plea negotiations and (lately) the prosecutor's partial abstention from bargaining, the Philadelphia Court of Common Pleas has evolved an unusual and intriguing system for disposing of its huge caseload. The criminal bench is divided into three components. One component handles homicides exclusively. For now, we shall exclude it from consideration. After a nonhomicide case has been filed in the Court of Common Pleas, it is assigned to one of the two remaining components of the criminal bench, which are called the criminal list and the criminal calendar.[68] The criterion for assignment is the amount of the court's time and resources that appear necessary to dispose of the case. Cases involving multiple defendants, multiple witnesses, or factual complications are assigned to the criminal calendar, as are all cases in which a jury trial appears likely; almost all rape, arson, and armed robbery cases are sent to the criminal calendar. The criminal list judges dispose of relatively simple, nonserious cases in which no jury trial is demanded.

As a practical matter, it is the prosecutor who determines whether a case is assigned to the criminal list or the criminal calendar. Under Pennsylvania law, the Commonwealth has a right to demand a jury trial.[69] Moreover, the court administration relies on the prosecutor's

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67. See pp. 162-163.

68. These organizational divisions have been called by different names in the past. The criminal calendar was known as the "jury trial program" at one time and as the "major felony program" at another. The criminal list used to be called the "waiver program."

69. 42 Pa. Cons. Stat. Ann. §5104(c) (1980 Supp.) provides, "In criminal cases the Commonwealth shall have the same right to trial by jury as does the accused." To the extent that judges impose more severe sentences in jury trial cases than in guilty plea or bench trial cases, the prosecution can, by exercising its prerogative, cause a stiffer sentence to be imposed on a defendant who would be willing to save the court's time by pleading guilty or waiving a jury. See p. 196.

assertions about the complexity and seriousness of a case in deciding where to assign it.[70] Defense attorneys report that, by speaking with the prosecutor before the case is assigned, they occasionally can influence the assignment. Even after a case has been assigned to the criminal calendar, it can be transferred to the criminal list if the prosecutor consents. A jury trial demand by either party at any time results in a transfer from the criminal list to the criminal calendar.

The assignment of a case to the criminal list or criminal calendar is a crucial decision from the defendant's perspective. It is universally understood that the judges who dispose of criminal list cases are the most lenient in the Court of Common Pleas. Criminal list judges typically dispose of four or five cases per day by plea of guilty. They handle the great bulk of the court's caseload. The criminal calendar judges are well known to be more severe than their colleagues who handle criminal list cases. Consequently, the assignment of a case to the criminal list or criminal calendar is perhaps the most important sentencing decision in the Court of Common Pleas.

Although all jury trials are conducted by the criminal calendar judges, only a minority of criminal calendar cases are disposed of by jury trial. Within the criminal calendar, some judges hear jury trials almost exclusively while others hear bench trials and guilty pleas almost exclusively. The jury trial specialists are, in the opinion of almost everyone we interviewed, the most severe sentencers in the court. They are commonly described as "heavy hitters," "hammers," and "hanging judges."[71] The criminal calendar judges who handle bench trials and guilty pleas are widely perceived as more lenient than the jury trial specialists but more severe than the criminal list judges. Thus, after a case is assigned to the criminal calendar, the next most important sentencing decision is the assignment of the case to a judge within the criminal calendar section.

From the perspective of court administration, the assignment of cases within the criminal calendar is a matter of allocating scarce resources. If the assignment judge assigns what would otherwise be a bench trial or guilty plea case to a severe sentencer, the defendant is likely to opt for a jury trial in the hope of an acquittal. Such a defendant has little to lose and much to gain by insisting on a jury trial. On the other hand, if a jury trial is assigned to one of the more lenient judges, that judge will be unable to dispose of other cases during the course of the trial. One jury trial may take two days or longer, during

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70. The court administration has little choice in this matter, since the prosecutor usually is the only person who is familiar with the state's case. To a large extent, the prosecutor can determine how many witnesses will be called.

71. At least some of these judges speak freely and proudly of their severity. One such judge characterized his attitude by the following declaration: "Nobody understands terror as much as the man who uses it." He further explained, "If you pull a gun on someone, you're not entitled to a perfect judge."

which time the judge could have disposed of several cases by guilty plea or bench trial.[72] Furthermore, many defendants who otherwise would have waived jury trials would take advantage of the opportunity for a jury trial in the courtroom of a moderate sentencer. The availability of low-risk jury trials could be expected to produce an increase in the number of demands for jury trials. Without the resources to accommodate more jury trials, the assignment judge cannot afford to convert jury waivers into jury trials either by assigning would-be jury waivers to severe judges or by assigning would-be jury trials to lenient or moderate judges; nor can the court afford to occupy the more lenient judges with jury trials. Consequently, the parties' intentions with respect to the exercise of jury trial rights determines which judge a case will be assigned to. As one Philadelphia judge put it, "Sentence disparity is used as a tool to dispose of cases."

From the defendant's perspective, there is a huge premium placed on relinquishing one's jury trial rights. The statistical analysis reported below suggests that a jury trial will cost the average convicted defendant ten years' additional imprisonment.[73] Defense attorneys routinely warn their clients that, in order to secure a jury trial, they must be willing to risk being sentenced by a "hammer."

This organizational system is remarkably successful at inducing jury waivers. Over 96 percent of convictions are obtained by guilty plea or bench trial.[74] Interestingly, a significant number of cases are disposed of by so-called "open pleas of guilty." These are cases in which there is no explicit plea agreement between the judge and the parties. These guilty pleas, however, are far from unbidden. It is common for the prosecutor and defense attorney to agree that the defendant will plead guilty before a particular judge without any specific promise of a sentence concession. Upon reaching such an agreement, the attorneys inform the assignment judge of its terms. Unwilling to risk converting a guilty plea into a trial, the assignment judge usually assigns the case to the agreed-upon judge or, if that judge's calendar is too full, to another judge who is acceptable to the parties. Judge shopping is an integral part of the Philadelphia system.

In Philadelphia, sentencing takes place in a politically charged atmosphere. The local news media devote considerable attention to sentencing. The district attorney periodically inveighs against judicial leniency in sentencing. His attacks are well covered by the press. He also publishes and distributes to community groups a newsletter that contains summaries of recent cases in which he feels judges were unduly lenient. The judiciary is not oblivious to all of

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72. There appears to be relatively little pressure to plead guilty rather than take a bench trial. One judge explained, "The guilty plea colloquy takes one hour, so a guilty plea doesn't save much time over a [bench] trial." Philadelphia has an unusually high proportion of bench trials. See note 1 supra.

73. See pp. 189-196.

74. See note 1 supra.

this adverse publicity.[75] There is much nervous discussion among apprehensive judges. Unsurprisingly, there is no small amount of acrimony between the two camps.

In the mid-1970s several bills were introduced into the Pennsylvania legislature for the purpose of restricting judicial sentencing discretion, some of which provided for mandatory minimum sentences.[76] Amid growing public controversy, the Philadelphia Court of Common Pleas undertook to develop sentencing guidelines patterned after those created by the Criminal Justice Research Center for Denver, Newark, Chicago, and Phoenix. A committee of judges was appointed to oversee development of the guidelines by researchers on the Court's administrative staff.

The empirical study on which the guidelines were based revealed that the method of conviction was a major determinant of sentences. Although they obviously were familiar with the Court's unique system for disposing of cases, the judges on the guidelines committee decided not to include method of conviction as a variable in the guidelines. Some of the members of the committee believed its inclusion would be held unconstitutional.

On February 28, 1979, the new guidelines were announced at a press conference. The official date of their implementation was March 5. The judiciary's reaction to the guidelines was mixed but generally favorable. There was, however, vocal opposition among the jury trial specialists, who considered the guidelines too lenient, and some of whom considered them an infringement on their judicial prerogative.

Unlike the Denver judges, the Philadelphia judges have made no concerted effort to keep the guidelines to themselves. Copies are freely distributed to prosecutors and defense attorneys. Many judges discuss them in open court and entertain argument concerning the computations and their factual basis.[77] Although the district attorney has criticized

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75. Pennsylvania judges are elected by popular vote to ten-year terms of office. Pa. Const. Art. V, §§5(a), 15(a). Incumbents are retained by a simple majority vote in a nonpartisan election. Pa. Const. Art. V, §15(b).

76. The legislation that eventually was enacted in 1978 creates a Commission on Sentencing whose principal mandate is to develop statewide guidelines for sentencing. See 18 Pa. Cons. Stat. Ann. §§1381-1386 (1979-80 Supp.). When the statewide guidelines are promulgated, they will supplant the Philadelphia guidelines. Despite the fact that the Philadelphia guidelines' days were numbered before they were fully developed, the Court elected to complete the development process and implement the guidelines. Some judges expressed the hope that the Philadelphia guidelines would serve as a model for the statewide guidelines.

77. In Philadelphia, computation of guideline sentences is left to individual judges, some of whom delegate the responsibility to their clerks. Computational errors have been a major problem. Estimates of the frequency of such errors vary from 10 to 25 percent of the cases. Disputes about the accuracy of the facts that formed the basis of the computations also are common. There is often disagreement over the definition of guidelines variables (e.g., what constitutes a weapon or an injury).

the guidelines for being too lenient, a few prosecutors and defense attorneys report that they are used in plea negotiations.[78] Some defense attorneys show the guidelines to clients who seem to have "unrealistic" expectations about sentence concessions. These attorneys find that the guidelines sometimes are useful in persuading a client to plead guilty.[79] Nevertheless, sentencing guidelines do not seem to play a major role in the plea bargaining process. Lawyers on both sides report that the rules of the case disposition game remain in force. While there is disagreement about whether sentencing guidelines have enhanced the predictability of sentences in nonjury cases, virtually everyone agrees that the jury trial judges are uninfluenced by the guidelines, and that they remain much more severe than their colleagues who handle guilty pleas and bench trials. It is business as usual in Philadelphia.

Chicago. The plea bargaining process in the Cook County Circuit Court has been described at length in previous studies.[80] As we shall see, the extremely limited use of sentencing guidelines in Chicago makes that site less central to our inquiry than are Denver and Philadelphia. Nevertheless, an examination of plea bargaining in Chicago provides useful insights into the potential role of sentencing guidelines in the disposition of criminal cases.

When the Chicago guidelines originally were developed and implemented, Illinois had an indeterminate statutory sentencing structure which, generally speaking, allowed greater judicial sentencing discretion than did the Colorado statutes but not quite as much as the Pennsylvania statutes.[81]

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78. A relatively small number of attorneys reported using the guidelines as an argument in support of their bargaining positions, but this tactic proved far from decisive. In any event, it does not seem to have enjoyed much popularity.

79. It is not entirely clear whether this is because the guideline sentences are sufficiently severe that they make prosecutors' offers seem generous, or because the guideline sentences are sufficiently lenient that defendants are willing to plead guilty before a judge who seems to follow the guidelines rather than proceed to trial before a "hammer." The latter seems more likely, but some defense lawyers reported the former.

80. For a description and organizational analysis of the plea bargaining process in the Cook County Circuit Court with special attention to the political environment, see J. Eisenstein and H. Jacob, supra note 48. See also, The Trial Judge's Role, supra note 17; A. Alschuler, The Prosecutor's Role in Plea Bargaining, 36 U. Chi. L. Rev. 50 (1968); A. Alschuler, The Defense Attorney's Role in Plea Bargaining, 84 Yale L. J. 1179 (1975).

81. See Ill. Ann. Stat. ch. 38, §1005-8-1 through 8-3 (1973). The principal constraints on the judge's discretion were as follows: for murder, minimum no less than 14 years, maximum any term of years; Class One Felony, minimum no less than four years, maximum any term of years; Class Two Felony, minimum no less than one year, maximum no greater than 20 years; Class Three Felony, minimum no less than one year, maximum no

As a result, sentence concessions usually were the direct object of plea bargaining; charge reductions usually were unnecessary.[82]

As in the Denver District Court, prosecutors and public defenders in the Cook County Circuit Court are assigned to an individual courtroom for relatively long periods of time.[83] They and the judges develop close working relationships as a result of their daily work interactions.[84] The majority of cases in the felony courtrooms are settled by plea agreement.[85]

The Illinois rule concerning judicial participation in plea negotiations is somewhat less restrictive than is Pennsylvania law.[86] Illinois judges are not permitted to initiate plea discussions, but are expressly allowed to hear a tentative agreement and indicate whether they will concur.[87] Perhaps partly because of the difference in legal constraints, Chicago judges participate much more extensively in plea bargaining than do their Philadelphia counterparts.

As elsewhere, there are three approaches to plea bargaining. A few Chicago judges remain aloof from the bargaining process, rarely even discussing tentative agreements with the parties. Ironically, those nonparticipating judges who seek to maintain their

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greater than 10 years; Class Four Felony, minimum no less than one year, maximum no greater than three years. For felonies of classes two through four, the minimum term imposed could not exceed one-third of the maximum term imposed. Misdemeanors were punishable by jail terms that could not exceed one year for Class A Misdemeanors, six months for Class B Misdemeanors, and 30 days for Class C Misdemeanors.

82. Eisenstein and Jacob have reported that judge-shopping is common in the Cook County Circuit Court. J. Eisenstein and H. Jacob, supra note 48, at 114. They note that defense attorneys' requests that their cases not be assigned to a particular judge usually were honored by the presiding judge of the criminal division. No doubt this is due at least in part to the fact that under Illinois law the defendant has the right to one peremptory judge substitution. See Ill. Ann. Stat. ch. 38, §114-5(a) (1980-81 Supp.). In any event, judge-shopping does not appear to be an integral part of the plea bargaining process as it is in Philadelphia.

83. See Eisenstein and Jacob, supra note 48, at 106, 244-251.

84. See id. at 106-109.

85. According to Eisenstein and Jacob, id. at 233, 61.7 percent of felony cases are disposed of by plea of guilty, 11.7 percent are dismissed, 19.9 percent are disposed of by bench trials, and only 6.7 percent go to jury trial. Guilty pleas accounted for 76.3 percent of convictions, while bench trials accounted for 15.9 percent and jury trials for 7.8 percent. See id. at 270.

86. Illinois law merely forbids the judge to "initiate plea discussions," Ill. Ann. Stat. ch. 38, §402(d) (4) (1973), whereas Pennsylvania law prohibits judicial participation "in plea negotiations preceding an agreement," Pa. R. Crim. P. 319(b)(1).

87. Ill. Ann. Stat. ch. 38, §402(d) (1973).

judicial independence often find it necessary to accept whatever bargain the parties arrive at in order to dispose of enough cases by guilty plea.[88] These judges tend to be seen by the attorneys as weaker, less dominant personalities. The alternative for a nonparticipating judge is to make it known, either by explicit statement or by consistent practice, that he or she sentences differentially according to the method of conviction. One judge stated flatly that he gives (what are for him) lenient sentences to defendants who plead guilty, somewhat more severe sentences to those convicted after a bench trial, and much more severe sentences to those who insist on a jury trial. Only a few judges, however, refuse to participate in plea negotiations.

A substantial number of judges, perhaps a majority, use the passive form of participation legitimated by Illinois law, but a significant number participate actively. Although the active participants often observe the letter of the law by ensuring that the prosecutor and defense counsel already have initiated plea discussions, these judges

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88. Illinois law, unlike Colorado and Pennsylvania law, does not require the judge to permit withdrawal of a guilty plea if the prosecutor's bargained-for sentence recommendation is not adhered to by the judge. People v. Lambrechts, 41 Ill. App. 3d 729, 355 N.E. 2d 53 (1976). See also People v. Goodman, 2 Ill. App. 3d 584, 277 N.E. 2d 136 (1971) (leave to withdraw guilty plea not required where court not advised of the plea agreement when the plea was accepted); People v. McKeag, 7 Ill. 2d 586, 131 N.E. 2d 517 (1956) (withdrawal must be permitted where prosecutor breaks a promise to make sentence recommendation, even though the recommendation would not have bound the court); People v. Wright, 21 Ill. App. 3d 301, 314 N.E. 2d 733 (1974) (withdrawal must be allowed where the defendant was not told that the prosecutor's bargained-for recommendation was not binding on the court).

The facts of the Lambrechts case provide a good illustration of the injustice that can occur under this rule. When the case was called for trial, the prosecutor reported that he and defense counsel had reached a plea agreement, the terms of which included a recommendation from the state that the defendant be sentenced to 18 to 54 months' imprisonment, to be served concurrently with a one to three year sentence from another county. The following exchange ensued:

> Court: You are going to serve 18 to 54 months on this charge, concurrent with the one to three charge, no matter when you serve that? Do you understand the 18 to 54 would start in this case as soon as you are sentenced? Do you understand that?
>
> Defendant: Yes.
>
> Court: All right. The plea agreement has been stated and I assume the defendant understands it.

After admonishing the defendant that he would not be bound by the attorneys' agreement, the judge ordered a presentence report. At the subsequent sentencing hearing, the judge imposed a prison sentence of six to 20 years. The startled defendant was denied permission to withdraw his guilty plea. The conviction was affirmed by the Illinois Court of Appeals.

clearly dominate the bargaining process by the strength of their personalities and the full exploitation of their judicial powers. Consequently, they maintain a high degree of control over the results of the bargaining process. When they meet with the attorneys, they question each about the strength of the case and the background and criminal history of the offender. The judge then attempts to arrive at a resolution that is acceptable to both sides. After some dickering, the judge makes his or her "best and final offer." A prosecutor who is dissatisfied with the judge's final offer had little recourse under the indeterminate sentencing structure. If the defendant, upon hearing the proposal from defense counsel, chooses to reject it in favor of a trial, he or she does so knowing that the sentence following a verdict of guilty will be harsher than the one offered in return for a guilty plea.[89] For most defendants, it is an offer that cannot be refused.

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89. But see J. Eisenstein and H. Jacob, supra note 48, at 276-279, 282-283, concluding that method of conviction has little effect on sentence in Chicago. Their conclusion is based on the relatively small increment in variance explained (R^2) that occurred when method of conviction was entered fourth in a six-variable hierarchical regression. It is not very informative, however, to be told that, relative to type of crime and prior criminal record, method of conviction explains a small proportion of the variance in sentence. One would like to know how much difference in sentence (probability of incarceration or length of incarceration) the method of conviction makes after all (five) other relevant variables have been controlled. This would require the interpretation of regression coefficients after all variables were entered in the model.

Curiously, Eisenstein and Jacob justify their decision to present and interpret multiple correlation coefficients rather than regression coefficients on the grounds that regression coefficients for dummy variables such as method of conviction are uninterpretable. By way of example, they explain that "it makes no sense to say that a little more femaleness would lead to a lower sentence; defendants are either male or female." To the contrary, dummy regression coefficients can easily be interpreted as mean residual group differences. See J. Cohen and P. Cohen, Applied Multiple Regression/Correlation Analysis for the Behavioral Sciences 183-184 (1975); E. Hanushek and J. Jackson, Statistical Methods for Social Scientists 101-108, 124-129 (1977). See also M. Lyons, Techniques for Using Ordinal Measures in Regression and Path Analysis, in G. Bohrnstedt and T. Carter, Sociological Methodology, 1971. To take their example, the regression coefficient for the sex dummy variable could have been interpreted as the average difference in sentence (probability or length of incarceration) between males and females who are similarly situated with respect to the other variables in the model.

Returning to the issue of method of conviction, it may be that although method of conviction explains little variance compared to other variables, those who go to jury trial may receive (on the average) significantly longer sentences than do otherwise similarly situated defendants who plead guilty or go to bench trial. The regression coefficient for method of conviction and its standard error (after all other variables are entered) would allow us to determine its statistical and practical significance. Without them, Eisenstein's and Jacob's conclusion is not well supported.

The atmosphere of the Chicago felony courts is pervaded by a felt need to dispose of large numbers of cases without long delays. A crime commission study and newspaper reports about delays and backlogs have heightened judges' awareness of caseload problems. Periodically the presiding judge of the criminal division circulates tabulations of the number of cases disposed of by each judge on the criminal bench.[90] The judges regard these statistics as somewhat like the daily baseball standings in the newspapers. They compare their performances with those of their colleagues. A high disposition rate is a source of pride to those who achieve it. Less prolific judges respect their more prolific colleagues for their achievements. Disposition statistics are the subject of friendly but competitive banter among the judges.[91] A few judges admit that they feel pressure to improve their standing in the disposition reports. Not surprisingly, it is the active participants in the plea negotiation process who have the highest disposition rates,[92] while the nonparticipants tend to have the lowest disposition rates.

This was the environment into which sentencing guidelines were introduced. Researchers from the Criminal Justice Research Center (CJRC) collected data between October 1976 and March 1977, and the guidelines were implemented in two Chicago branches of the court in June 1977.[93] The CJRC researchers monitored the results in those two branches and collected additional data in the two other Chicago branches. The plan was to extend the guidelines' use to all four Chicago branches.

Meanwhile, the Illinois legislature passed a new Unified Code of Corrections, which drastically altered the statutory sentencing structure by providing for flat prison terms and reducing judicial sentencing discretion.[94] Perhaps the most widely noted feature of the new law was the creation of a new class of felonies, called "Class X," which are

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90. See J. Eisentein and H. Jacob, supra note 48, at 113.

91. It is not uncommon to hear one judge greet another by asking, "How are your dispositions this week?"

92. One judge who takes an active role in plea negotiations, and whose name regularly appears at or near the top of the disposition ratings, described his approach this way: "I give premiums [for guilty pleas]. I have Christmas sales."

93. The Cook County Circuit Court has several branches in different locations around the county, four of which serve the city of Chicago.

94. Ill. Ann. Stat. ch. 38, §§1001-1-1 through 1008-6-1 (1980-81 Supp.). The new law authorizes conditional discharge (release without supervision), probation, periodic imprisonment, imprisonment, fines, and orders to make restitution, except in cases of murder, attempted murder, and all Class X Felonies, where the offender must be imprisoned. Ill.

punishable by no less than 6 nor more than 30 years' imprisonment.[95] The legislation also included a provision for extended terms triggered by findings of aggravating circumstances.[96]

In order to accommodate the new sentencing law, the CJRC researchers revised the Chicago guidelines so that they would provide for flat prison terms that would result in the same amount of actual prison time as was served under the former law.[97] The revised guidelines were implemented in the four Chicago branches of the Circuit Court on February 1, 1978, the effective date of the new law.

In retrospect, it appears that the guidelines were hardly implemented in any real sense of the word. Very few judges ever computed the guideline sentences before imposing a sentence. The judges regarded the guidelines project as an exercise in data collection and a "study of

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Ann. Stat., ch. 38, §1005-5-3 (1980-81 Supp.). The authorized sentences to imprisonment are as follows:

Offense	Minimum	Maximum
Aggravated Murder	Natural life	Natural life
Habitual Criminal	Natural life	Natural life
Murder	20 years	40 years
Class X Felony	6 years	30 years
Class 1 Felony	4 years	15 years
Class 2 Felony	3 years	7 years
Class 3 Felony	2 years	5 years
Class 4 Felony	1 year	3 years
Class A Misdemeanor	0	1 year
Class B Misdemeanor	0	6 months
Class C Misdemeanor	0	30 days

Ill. Ann. Stat. ch. 38, §§1005-8-1, 3 (1980-81 Supp.). The legislation also established a Criminal Sentencing Commission, Ill. Ann. Stat. ch. 38, §1005-10-1 (1980-81 Supp.), whose duties include the development of "standardized sentencing guidelines" to promote greater uniformity in sentencing. Ill. Ann. Stat. ch. 38, §1005-10-2(5) (1980-81 Supp.).

95. Ill. Ann. Stat. ch. 38, §§1005-5-3(c)(2), 8-1 (1980-81 Supp.).

96. Ill. Ann. Stat. ch. 38, §1005-8-2, 5-3.2 (1980-81 Supp.).

97. The new law provides one day of good conduct credit for each day served. Ill. Ann. Stat. ch. 38, §1003-6-3 (1980-81 Supp.). Under the former law, good conduct credits were determined by the Department of Corrections, Ill. Ann. Stat. ch. 38, §1003-6-3 (1973), which was somewhat less generous. Each grid cell of the guidelines was mathematically transformed so that the new cell value would result in the same actual time served as it did under the old law. This was in some ways a curious response to a sweeping legislative mandate for change.

disparity."[98] Many dutifully completed the guideline worksheets after sentencing hearings or at the end of the week, mused at the relationship between their sentences and the guideline sentences,[99] and turned in the worksheets for compilation. After months without feedback about the results of their clerical efforts,[100] most judges lost interest and stopped completing the worksheets. Subsequently the project was modified to require the completion of the worksheets during only four weeks of each year. Still, many judges found it too burdensome or unrewarding to adhere even to those minimal requirements. It is safe to say that the Chicago guidelines had no significant effect on sentencing practices in the Cook County Circuit Court.

With few exceptions, Chicago prosecutors and defense attorneys were unaware of the existence of sentencing guidelines. It seems that the guidelines were so unimportant to the judges that they escaped the attention of the lawyers with whom they dealt closely on a daily basis.[101] We heard of two occasions when a judge referred to the guidelines during plea negotiations in an attempt to cajole one side or the other into accepting a proposed bargain, but the references were made in a humorous vein, and such instances were extremely rare. Even those few attorneys who had heard of or seen the guidelines considered them to be of no importance to their daily activities.

It is difficult to say exactly why the guidelines were so insignificant in Chicago, but there seem to be several causes. First, the guidelines were not taken seriously by the judges as a reform effort. Apparently the purposes of sentencing guidelines were not adequately communicated to the judges who were expected to use them. Furthermore, the established patterns of plea bargaining within close-knit workgroups left no obvious role for the guidelines, especially since they called for the same sentence no matter whether the defendant had been convicted by guilty plea, bench trial, or jury trial.[102]

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98. In what was apparently an attempt to assuage the judges' concerns about the project, the researchers told them that their analyses showed very little sentence disparity in Chicago. Several judges cited this finding to us during our interviews as evidence that guidelines were not needed. Ironically, the researchers' report of their findings (which seem to have little basis in fact--see pp. 56-57) may have contributed to the judges' failure to heed the guidelines.

99. Said one judge, "I liked to see how close I came. It was like a game."

100. Although the information on the worksheets was punched onto computer cards, no data analysis was performed.

101. One judge expressed concern over the possibility that the guidelines might become part of the record on appeal. Nevertheless, although there may have been some attempt to conceal the guidelines from the lawyers, the guidelines' lack of importance to the judges appears to be the predominant reason for lawyers' unawareness of them.

102. One judge observed that "ninety percent of the cases are pled out, but the guidelines are based on an analysis that has nothing to do with whether the case is pled out." He went on to say that his sentences in guilty plea cases tended to fall within the guidelines ranges, whereas his sentences in jury and bench trial cases tended to exceed the guidelines.

Brief consideration of the effects of the new determinate sentencing law on the plea bargaining process permits a useful comparison with sentencing guidelines. The new statutory sentencing structure has constrained judicial sentencing discretion to the point where charge reductions are necessary to effect sentence concessions in a substantial number of cases. Taking advantage of their newly acquired power, prosecutors now charge Class X Felonies whenever possible in order to enhance their bargaining strength.[103] Conventional wisdom holds that the result will be an increased role for the prosecutor and a reduced role for the judge in the bargaining process. This has occurred in many Chicago courtrooms. Prosecutors praise the new law, defense attorneys complain about the prosecutors' increased ability to "hammer" their clients, and many judges lament the limitations on their discretion. The only universally applauded feature of the new law is its provision for flat prison terms with known release dates.

Nonetheless, in some courtrooms there has been little change in the exchange relationships among judge, prosecutor, and defense attorney. These are the courtrooms of the judges who actively participate in and dominate plea negotiations. They have maintained their dominance partly by the strength of their personalities and their ability to make life difficult for uncooperative prosecutors. They also show an ability to manipulate the system so that it accommodates their established practices. One way to avoid the long prison terms specified for certain classes of felonies is to use periodic imprisonment and work release, as authorized by the new law. Where these are inappropriate, the judge can respond to an uncooperative prosecutor by urging the defense attorney to "take a bench trial." It is understood by this that if the defendant requests a bench trial, the judge will convict on a lesser included offense and impose the sentence favored by the judge during the plea discussions.[104] The prosecutor's opposition is an inconvenience but not a barrier. Sooner or later, he or she is likely to accede to the judge's demands. By adapting to the new law and making some decisions sub rosa, the judge can maintain approximately the same stance that he or she did under the previous law.

The Illinois determinate sentencing law is a much more comprehensive and authoritative reform effort than are judicially adopted, voluntary sentencing guidelines. Yet even legislation cannot always overcome established practices. Without the active involvement and support of

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103. For example, the charge of "home invasion" frequently is added to charges of burglary and aggravated battery because home invasion is a Class X Felony, Ill. Ann. Stat. ch. 38, §12-11 (1979), which carries a mandatory minimum sentence of six years' imprisonment. Ill. Ann. Stat. ch. 38, §§1005-5-3, 8-1 (1980-81 Supp.).

104. One judge uses an interesting variation on this theme. Illinois law requires the judge to determine that there is a factual basis for a guilty plea before entering a final judgment. Ill. Ann. Stat. ch. 110A, §402(c) (1976). The judge simply accepts a guilty plea but determines that there is no factual basis for the original charge, enters a judgment of conviction on a lesser included offense, and imposes the sentence he thinks appropriate.

judges, and perhaps even of prosecutors and defense attorneys, it seems unlikely that voluntary sentencing guidelines can accomplish their purposes.

A Quantitative Assessment

Professor Albert Alschuler has argued persuasively that recent attempts to limit judicial sentencing discretion are likely to enhance the power of the prosecutor through the mechanism of charge bargaining.[105] To the extent that the charge of which an offender is convicted precisely determines the sentence, the prosecutor and defense attorney will be able to control the sentence decision by negotiating a guilty plea to a reduced charge. Except insofar as the judge can effectively review charge-reduction decisions,[106] the judge's role in sentencing will diminish. Discretion, it is argued, will shift from the judge to the attorneys--primarily the prosecutor.

Professor Alschuler referred specifically to legislatively fixed or presumptive sentences, but although sentencing guidelines take into account several factors in addition to the charge convicted of, and to that extent are more flexible than legislatively fixed sentences, the same potential for shifting discretion from the judge to the attorneys inheres in sentencing guidelines. Suppose that judges faithfully comply with the guidelines in nearly all cases. During plea negotiations, all of the guidelines factors (weapon use, number of prior convictions, and so on) are known to the attorneys except for the charge at conviction, which is subject to their control. If the judge reliably follows the guidelines, the attorneys can determine the sentence by consulting the guidelines and agreeing on a charge reduction that will produce the desired sentence.

Denver: the effects of charge reductions on sentences. The hypothesis of shifting discretion is predicated on an increase in the strength of the relationship between charge reductions and sentences. The stronger this relationship, the more discretion should shift from the judge to the lawyers. Our Denver case data allow us to measure the strength of the relationship. As we have described, Denver is already a charge bargaining system, but there remains substantial judicial discretion whether to incarcerate and, in Class Two and Three felonies, how long to incarcerate. To the extent that guidelines made judges' decisions more predictable or predetermined, there was a potential for additional discretionary control to shift from the judge to the attorneys via charge bargaining. Of course, given the results reported in Chapter 3 (that the guidelines did not affect sentences) and the observations reported earlier in this chapter (that charge bargaining already was the form of plea bargaining), we should not be surprised to find that the relationship between charge reductions and sentences has not changed.

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105. A. Alschuler, Sentencing Reform and Prosecutorial Power: A Critique of Recent Proposals for "Fixed" and "Presumptive" Sentencing, 126 U. Pa. L. Rev. 550 (1978).

106. See note 7 and accompanying text supra.

We shall use path analysis to measure the effects of charge reductions on sentences.[107] A simplified causal model of the decision whether to incarcerate is depicted in Figure 4-1. We have eliminated from the model those variables that, on the basis of preliminary analyses, seem to have no important effect on the decision whether to incarcerate.[108] The remaining variables include the offender's prior criminal record,[109] the offender's liberty status at the time of the offense,[110] the statutory class of the offense at filing of the information in the District Court,[111] the offender's bail status,[112] the degree of charge reduction,[113] and the type of sentence.[114]

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107. For a discussion of path analysis and a description of the methods we have used, see pp. 114-116. To recapitulate, the method of ordinary least squares is used to estimate a recursive model. For a description of the data on which the analysis is based, see p. 18.

108. We also have eliminated method of conviction from the model, despite its importance in plea bargaining. The omission is for technical reasons. There is a reciprocal relationship between charge reductions and method of conviction. Indeed, it is the essence of a reciprocal relationship; it is contractual. The prosecutor's offer of a charge reduction is contingent upon the defendant's tender of a guilty plea, and the defendant's tender of a guilty plea is contingent upon a charge reduction. Models that include reciprocal relationships are nonrecursive and do not permit ordinary least squares solutions. So many of the relevant variables are interrelated that we could not fully identify a nonrecursive model. There were too many endogenous variables relative to the number of exogenous variables. Our solution, with which we are not entirely happy, is to omit method of conviction from the model and allow charge reductions to serve partly as a surrogate for it. The reader will recall that there is a fairly strong relationship between charge reductions and method of conviction. See pp. 158-160. We believe the problem of fully identifying nonrecursive models of the sentencing decision should receive attention in future research.

109. Measured by a six-point scale based on depth of prior penetration into the criminal justice system (e.g., no prior adult or juvenile contacts, prior juvenile convictions but no juvenile incarceration or adult convictions, etc.).

110. Measured as a dichotomy: 0 = not on bail, probation, or parole; 1 = otherwise.

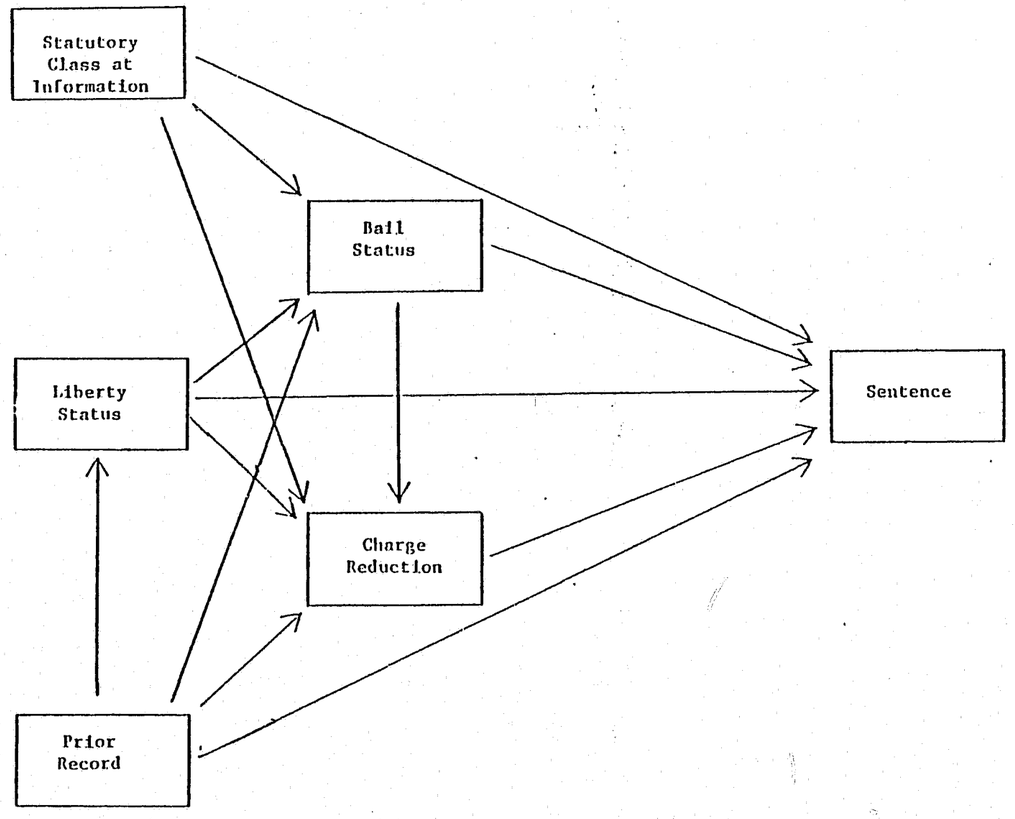
111. Measured as if it were of interval level by assigning a numerical value to each statutory class: 8 = Felony One, 7 = Felony Two; . . . 1 = Misdemeanor Three.

112. Measured as a dichotomy: 0 = released on personal recognizance or bail and not revoked; 1 = not released or released but bail was revoked.

113. Measured by the number of statutory classes by which the charge was reduced: a reduction from a Class 2 Felony to a Class 5 Felony is a three-class reduction; a reduction from a Class 5 Felony to a Class 2 Misdemeanor is a two-class reduction.

114. Measured as a dichotomy: 0 = no incarceration; 1 = incarceration. For a discussion of the use of dichotomous dependent variables with regression-based techniques, see Chapter 2, note 193 and accompanying text.

Figure 4-1
Causal Model of the Relationship Between
Charge Reductions and Type of Sentence
Denver



Parameters for the path model were estimated using the pre- and postguidelines Denver samples separately. The results are summarized in Table 4-2. (See also Appendix B, Tables D-1 and D-2.) Before sentencing guidelines, the direct effects of charge reductions on the decision whether to incarcerate were -0.052. This can be interpreted as follows: When all other variables in the model are held constant, for each charge reduction of one statutory class there is a 5.2 percent decrease in the probability of incarceration. The coefficient that represents the effects of charge reductions on sentence is significant at $p < .001$, meaning that the estimate is reliably greater than zero. Thus, before sentencing guidelines, charge reductions had a significant but nevertheless small impact on the judge's decision whether to incarcerate. After sentencing guidelines the direct effects of charge reductions on the decision whether to incarcerate were -0.050, almost identical to the preguidelines coefficient. The postguidelines coefficient also is significant at $p < .001$, but of course the difference between the pre- and postguidelines coefficients is far from statistically significant. We conclude that, as expected, there has been no change in the relationship between charge reductions and the decision whether to incarcerate.

Table 4-2
Path Analysis of the Effects of
Charge Reductions on the Decision
Whether to Incarcerate
Unstandardized Coefficients
Denver

	BEFORE GUIDELINES			AFTER GUIDELINES		
Variable	Direct Effects	Indirect Effects	Total Effects	Direct Effects	Indirect Effects	Total Effects
Charge Reduction	-0.052	--	-0.052	-0.050	--	-0.050
Bail Status	0.204	0.025	0.229	0.185	0.027	0.212
Statutory Class	0.069	0.013	0.082	0.077	0.007	0.084
Liberty Status	0.182	0.037	0.219	0.135	0.043	0.178
Prior Record	0.078	0.035	0.113	0.090	0.028	0.118

Before

Total variance in sentence accounted for by the model = 0.2813

F = 68.11
Probability = 0.0001
n = 876

After

Total variance in sentence accounted for by the model = 0.2810

F = 145.79
Probability = 0.0001
n = 1871

Before considering the effects of charge reductions on length of incarceration, we should comment on the effect of bail status on the decision whether to incarcerate. Note that the direct effects of bail status on the decision whether to incarcerate were 0.204 in the preguidelines sample and 0.185 in the postguidelines sample. Both coefficients are statistically significant (p E .001), although the difference between them is not. This indicates that pretrial release has a significant effect on the decision whether to incarcerate, so that those not released have approximately a 20-percent greater likelihood of being sentenced to imprisonment than do similarly situated defendants who are released pending adjudication. There are, however, a number of possible explanations for this. First, bail status may serve as a surrogate for variables that have been omitted from the model. In particular, bail status is related to the seriousness of the offense, inasmuch as magistrates use the seriousness of the offense as a criterion for setting the amount of bail. Those who commit the most serious offenses therefore are unlikely to be released. Bail status also reflects the defendant's economic condition, since poorer defendants are less able to post bond or even a 10-percent personal recognizance fee. Finally, we suspect that one's bail status affects one's bargaining position, and therefore one's sentence. Those who are released have every incentive to delay and probably can negotiate a better deal than those who remain in jail. It would take a more sophisticated analysis than the present one to disentangle the possible causal relationships involving bail status. Although such an analysis is beyond the scope of this study, we believe it worthy of future research.

A similar path analysis was performed on the Denver data using length of incarceration (imprisonment cases only) as the ultimate dependent variable.[115] The causal model is depicted in Figure 4-2. It includes the same variables as those used in the model of the decision whether to incarcerate, except that a harm/jeopardy variable[116] has been added because of its apparent influence on the decision about length of incarceration.[117] The results of the path analysis are summarized in Table 4-3. (See also Appendix B, Tables D-3 and D-4.)

The direct effects of charge reductions on sentence length before guidelines were -5.129. In other words, before sentencing guidelines, a charge reduction of one statutory class was associated with a 5.129-month reduction in sentence length. The coefficient for the effects of charge reductions on sentence length is significant at p .001. These results are consistent with our analysis of the Colorado sentencing

115. Length of incarceration was measured in months until parole eligibility. See Chapter 3, note 44.

116. Measured as if it were interval level by assigning numbers as follows: 0 = no persons put in jeopardy of physical injury during commission of crime; 1 = persons in jeopardy but no physical injury resulted; 2 = physical injury; 3 = death.

117. Preliminary analyses indicated that the injury/jeopardy variable affects sentence length but not the decision whether to incarcerate. See p. 76.

Figure 4-2
Causal Model of the Relationship Between
Charge Reductions and Length of Incarecaration
Denver

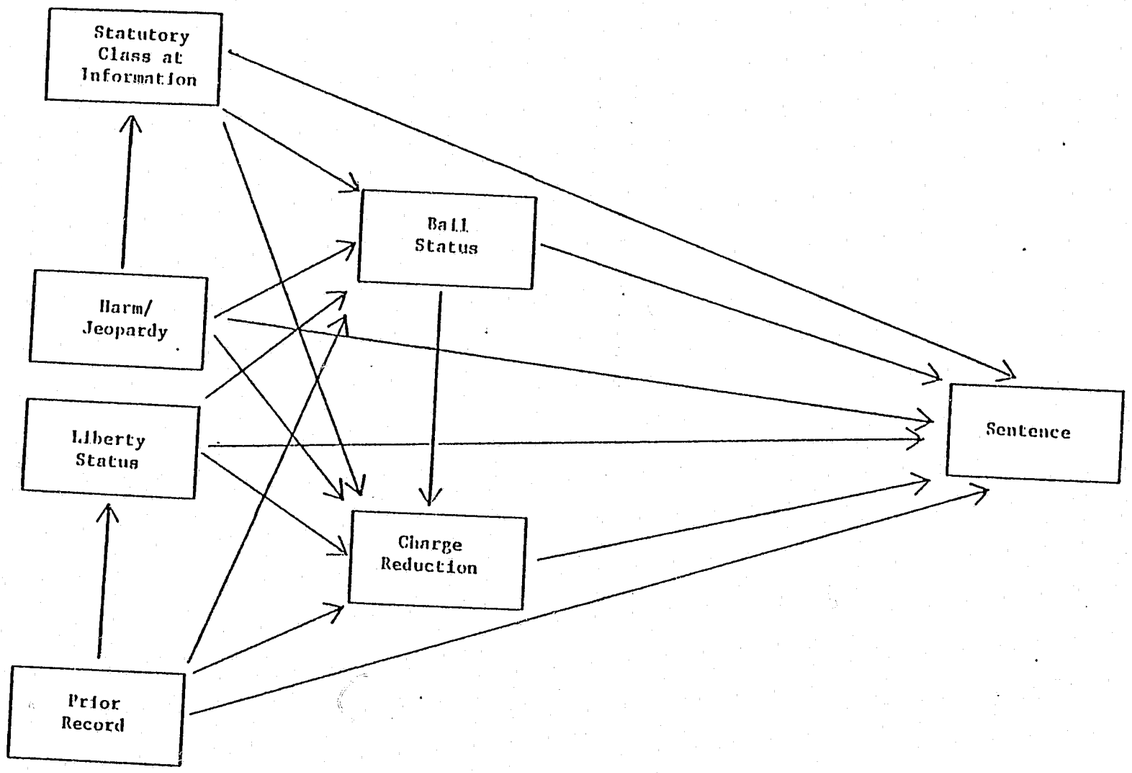


Table 4-3
Path Analysis of the Effects of Charge Reductions on Sentence Length
Unstandardized Coefficients
Denver

	BEFORE GUIDELINES			AFTER GUIDELINES		
Variable	Direct Effects	Indirect Effects	Total Effects	Direct Effects	Indirect Effects	Total Effects
Charge Reduction	-5.129	--	-5.129	-6.012	--	-6.012
Bail Status	5.209	2.170	7.379	3.718	2.447	6.165
Statutory Class	7.825	-0.279	7.546	11.248	-0.338	10.910
Harm/Jeopardy	6.262	3.139	9.401	5.656	4.703	10.359
Liberty Status	0.464	0.749	1.213	0.432	1.572	2.004
Prior Record	-0.020	0.435	0.415	2.954	0.131	3.085

Before

Total variance in sentence accounted for by the model = 0.1542

F = 16.20
Probability = 0.0001
n = 540

After

Total variance in sentence accounted for by the model = 0.2139

F = 53.23
Probability = 0.0001
n = 1181

statutes, which indicates that reductions in statutory class constrain the judge's choice of sentence length more than they restrict the judge's decision whether to incarcerate. After the implementation of sentencing guidelines, the effects of charge reductions on sentence length increased slightly to -6.012 (p E .001), but the difference between the pre- and postguidelines coefficient is small and far from statistically significant. In other words, for all practical purposes the relationship between charge reductions and sentence length did not change as a result of sentencing guidelines.

Again, the reader should note the effects of bail status on sentence length. The direct effects of bail status on sentence length were 5.209 (p = .062) before guidelines and 3.718 (p = .056) after guidelines. In other words, those who were released pending adjudication, if subsequently sentenced to incarceration, received sentences that were on the average 5.209 months shorter before guidelines and 3.718 months shorter after guidelines than did similarly situated offenders who were not released. Note also that pretrial release was associated with charge reductions; the indirect effects of bail status on sentence length through charge reductions (not shown separately on Table 4-1) were 2.170 before guidelines and 2.447 after guidelines. In other words, those who are detained pending adjudication tend to receive less substantial charge reductions and consequently receive longer sentences. This lends some support to the hypothesis that one's bail status affects one's bargaining strength, but a more sophisticated analysis is needed before a definitive statement can be made.

Our path analyses corroborate the results of our interviews. Charge reductions are an important part of the sentencing process in Denver, but sentencing guidelines had no detectable effect on the sentence-value of a charge reduction. Charge reductions led no more predictably to sentence concessions after guidelines than they did before. This is consistent with our earlier finding that Denver judges were not substantially influenced by the guidelines.

The predicate for a shift in discretion from the judge to the lawyers failed to materialize. This does not mean, of course, that the hypothesis of shifting discretion is wrong. It is simply that a necessary precondition was not met. The idea that restrictions on judicial sentencing discretion will cause the attorneys to usurp the sentencing function through the mechanism of charge bargaining remains eminently plausible.

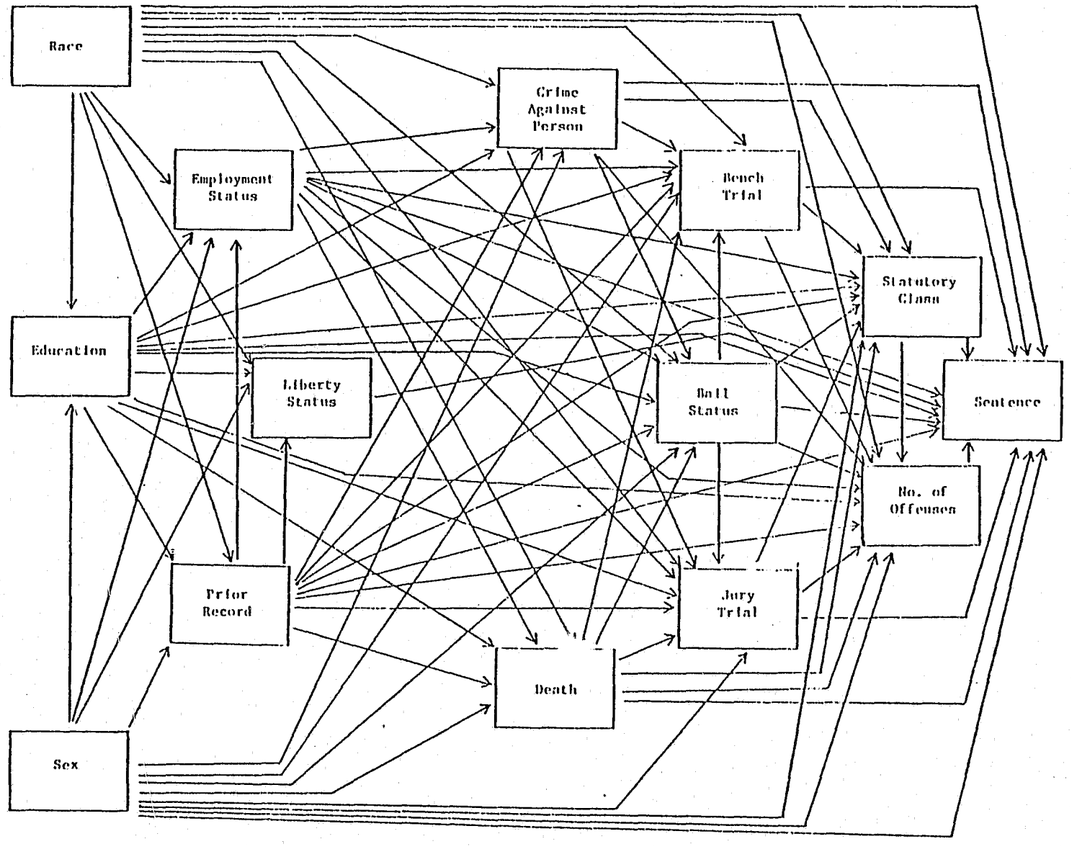
Philadelphia: the effects of method of conviction on sentence. Our descriptive analysis of plea bargaining in the Philadelphia Court of Common Pleas suggests that inter-judge disparities are an intrinsic part of the process by which cases are disposed of.[118] To the extent that sentencing guidelines reduce differences between judges, they should destroy the incentive to waive jury trial rights. The result presumably would be an increase in the number of jury trials. It seems more likely, however, that the guidelines simply will fail to reduce inter-judge disparities because of the competing institutional need to induce guilty pleas.

Our Philadelphia case data permit us to test whether sentencing guidelines reduced the incentive to waive trial rights. Figure 4-3 depicts a causal model of the effects of method of conviction on the decision whether to incarcerate.[119] Several aspects of this model should be noted. First, it includes considerably more variables, and is therefore more complex, than the Denver models. This is partly due to

118. See pp. 171-172.

119. The following variables are measured as dichotomies or dummies: race (0 = nonblack; 1 = black); sex (0 = female; 1 = male); employment status (0 = unemployed; 1 = employed); liberty status at time of sentencing (0 = no restrictions; 1 = on bail or probation from another case); crime against person (0 = not crime against person; 1 = crime against person); death (0 = no victim killed; 1 = victim killed); bail status (0 = released pending adjudication; 1 = not released); bench trial (0 = not bench trial; 1 = bench trial); jury trial (0 = not jury trial; 1 = jury trial); and sentence (0 = not incarcerated; 1 = incarcerated). Education is measured as an interval level variable according to the number of years of education successfully completed. Prior criminal record is measured by a nine-point scale based on prior arrests, convictions, and incarcerations. The statutory class of the offense at conviction is measured as if it were interval by assigning numbers to each statutory class (1 = Capital Crime, 2 = Class One Felony, 3 = Class Two Felony, . . . 7 = Class Three Misdemeanor). The number of offenses at conviction is measured as an interval level variable.

Figure 4
Causal Model of the Effects
of Method of Conviction on the
Decision Whether to Incarcerate



the fact that a greater number of variables seem to influence sentences in Philadelphia than in Denver. Second, the reader should note that two of the three methods of conviction (bench trial and jury trial but not guilty plea) are included in the model. For technical reasons, where a categorical variable such as method of conviction is dummy-coded in a multiple regression equation, one must omit a variable corresponding to one of the categories.[120] In this case, we have omitted the dummy variable for guilty plea. Consequently, we can interpret the coefficient for the jury trial variable as the difference in probability of incarceration between defendants who plead guilty and those who go to jury trial.[121] Similarly, the coefficient for the bench trial variable represents the difference between those who plead guilty and those who go to bench trial. Finally, the reader should note that, like the other multi-equation models used in this study, the model depicted in Figure 4-3 is hierarchical. Since we shall assume that the error terms are independent of each other, the model is recursive and permits ordinary least squares solutions.[122]

The results of the path analysis of the effects of method of conviction on the decision whether to incarcerate are presented in Table 4-4. (See also Appendix B, Tables D-5 and D-6.) Although there are several interesting aspects of this analysis,[123] we shall focus on the relationships of primary concern. The direct effects of bench trial on type of sentence were -0.006 before guidelines and 0.023 after guidelines. Both coefficients are small and are not statistically significant. In other words, when all other variables in the model are held constant, offenders who were tried and convicted by the court were just as likely to be imprisoned as were those who pled guilty, both before and after guidelines.

The direct effects of jury trial on the decision whether to incarcerate were somewhat greater than those for bench trial, but again did not reach statistical significance. Before guidelines, the direct effects of jury trial on type of sentence were 0.121, but the significance level was only 0.122. In other words, since we presumably have unbiased estimates, our best guess is that offenders who were convicted by juries were 12.1 percent more likely to be imprisoned before guidelines than were similarly situated offenders who were convicted by guilty plea. Nevertheless, we cannot confidently say that the true effect is greater than zero; the coefficient estimate had a 12.2-percent probability of occurrence merely as a result of sampling error.

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120. See, e.g., F. Kerlinger and E. Pedhazur, Multiple Regression in Behavioral Research 107 (1973); E. Hanushek and J. Jackson, supra note 89, at 104; J. Cohen, Multiple Regression as a General Data-Analytic System, 70 Psych. Bull. 426, 428 (1968).

121. See note 89 supra.

122. See E. Hanushek and J. Jackson, supra note 89, at 229.

123. Note, for example, that bail status has significant effects on the decision whether to incarcerate. See p. 186. Note also that the racial effects reported in Chapter 3 appear in these analyses as well, despite differences in model specification. See pp. 122-127.

Table 4-4
Path Analysis of the Effects of Method of Conviction on the Decision Whether to Incarcerate
Unstandardized Coefficients
Philadelphia

	BEFORE GUIDELINES			AFTER GUIDELINES		
Variable	Direct Effects	Indirect Effects	Total Effects	Direct Effects	Indirect Effects	Total Effects
Liberty Status	0.024	--	0.024	0.043	--	0.043
No. of Offenses	0.060	--	0.060	0.047	--	0.047
Statutory Class	-0.008	-0.011	-0.019	-0.019	-0.012	-0.031
Jury Trial	0.121	0.045	0.166	0.056	0.048	0.104
Bench Trial	-0.006	0.027	0.021	0.023	0.009	0.032
Bail Status	0.120	0.004	0.124	0.148	0.007	0.155
Death of Victim	0.281	0.059	0.340	0.314	0.036	0.350
Crime Against Person	0.170	0.098	0.268	0.061	0.123	0.184
Employment Status	0.022	0.017	0.039	-0.026	0.059	0.033
Prior Record	0.046	0.020	0.066	0.047	0.034	0.081
Highest Grade	-0.005	-0.020	-0.025	-0.017	-0.012	-0.029
Sex	0.194	0.113	0.307	0.097	0.108	0.205
Race	0.071	0.093	0.164	0.123	0.113	0.236

Before	After
The variance in sentence explained by variables in model = 0.3624	The variance in sentence explained by variables in model = 0.3949
F = 41.01 n = 952 Probability = 0.0001	F = 22.54 n = 463 Probability = 0.0001

After guidelines, the direct effects of jury trial on type of sentence were even smaller. In our postguidelines sample, offenders convicted by juries were only 5.6 percent more likely to be incarcerated than were similarly situated offenders who were convicted by guilty plea. Again, this coefficient does not reach statistical significance ($p = .659$). Since neither the preguidelines coefficient nor the postguidelines coefficient are reliably greater than zero, the difference between them cannot be significant. Method of conviction appears to have had very little if any effect on the decision whether to incarcerate either before or after the introduction of sentencing guidelines.

Some simple statistics suggest an explanation of the nonsignificance of the coefficients for jury trial. In our entire Philadelphia case sample (pre- and postguidelines cases together), only 3.2 percent of the cases went to jury trial. In other words, the jury trial variable is highly skewed. Since it has very little variance to share with the sentence variable, it is unlikely that its coefficient, which signifies

the direct relationship between jury trial and sentence, could be large enough to reach statistical significance. The most revealing statistic, however, is that of the 61 persons in our total sample who were convicted by juries, only 6, or 9.8 percent, were sentenced to probation. Conversely, 90.2 percent of those who were convicted by juries were sentenced to imprisonment. Our interviews suggested that jury trials are rarely even considered by defendants who would likely receive probation if they pled guilty. Apparently jury trials occur only in serious cases where imprisonment is a foregone conclusion and the only issue is length of confinement.

The effects of method of conviction on length of incarceration are much different. A causal model of the sentence length decision[124] is depicted in Figure 4-4. It differs from the causal model of the decision whether to incarcerate in that the offender's educational achievement, employment status, and liberty status at the time of the offense have been omitted, while a new variable, injury,[125] has been added. The omission of variables was necessitated by the fact that the number of imprisonment cases, to which this analysis is limited, is considerably smaller than the total number of cases.[126] Preliminary analysis indicated that the omitted variables are unrelated to sentence length. The injury variable was added because preliminary analysis showed that, although it does not seem to affect the decision whether to incarcerate, it does affect sentence length.

The results of path analyses performed separately on the pre- and postguidelines samples are summarized in Table 4-5. (See also Appendix B, Tables D-7 and D-8.) The direct effects of bench trial on sentence length in the preguidelines sample were -8.902. In other words, offenders who were convicted at bench trial received prison terms that were, on the average, 8.902 months shorter than the prison terms given to offenders who pled guilty but who were otherwise similarly situated with respect to the variables in the model. This coefficient, however, is not statistically significant ($p = .314$). In the postguidelines sample, the direct effects of bench trial on sentence length were -13.243, indicating that when all other variables in the model are held constant, offenders convicted by bench trial receive prison terms that are, on the average, 13.243 months shorter than those given in guilty plea cases. This coefficient, too, is nonsignificant ($p = .250$). The difference between the pre- and postguidelines coefficient is, of course, also nonsignificant. Thus, offenders convicted by bench trials do not appear to have received longer sentences than did those who pled guilty, either before or after guidelines. If anything, they received shorter sentences. This is consistent with our earlier observation that there is little pressure to plead guilty rather than go to bench trial.[127]

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124. Sentence length is measured in months according to the minimum term imposed by the judge. See Chapter 3, note 48.

125. The injury variable is coded as a dummy (0 = no physical injury to any victim; 1 = physical injury to a victim).

126. The lower the ratio of cases to variables, the more difficult it is to make statistical inferences.

127. See note 72 and accompanying text supra.

Figure 4-4
Causal Model of the Effects
of Method of Conviction on
Sentence Length

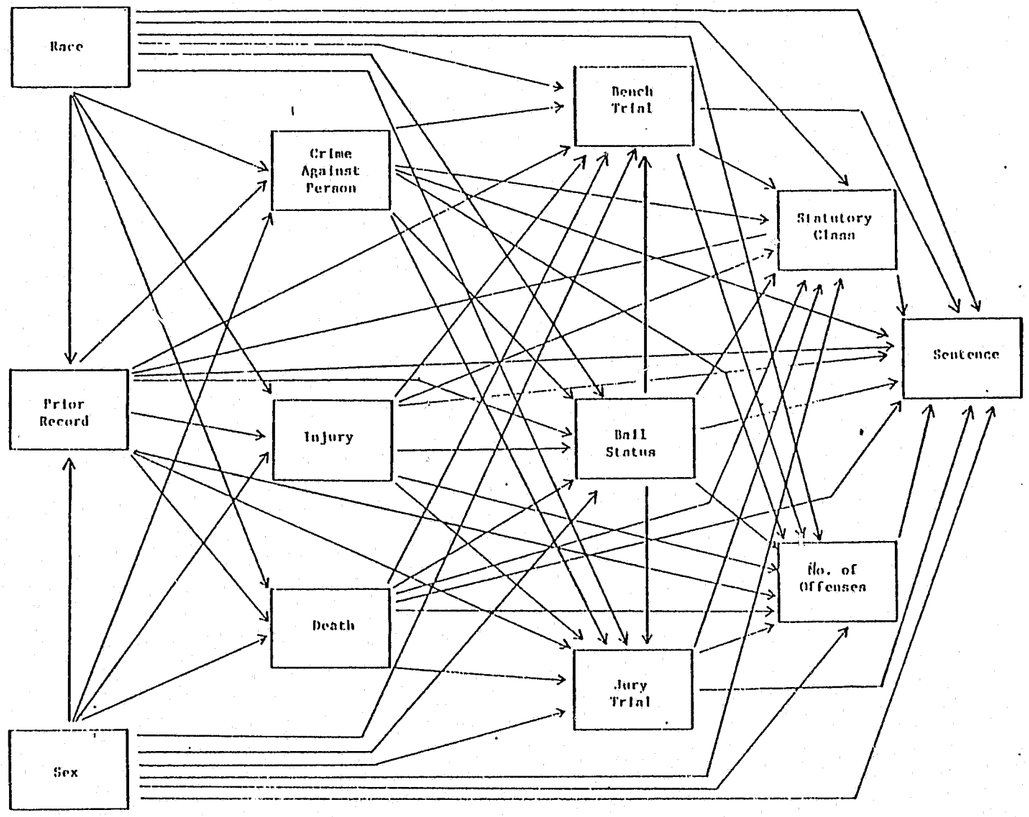


Table 4-5
Path Analysis of the Effects of Method of Conviction
on Length of Incarceration
Unstandardized Coefficients
Philadelphia

	BEFORE GUIDELINES			AFTER GUIDELINES		
Variable	Direct Effects	Indirect Effects	Total Effects	Direct Effects	Indirect Effects	Total Effects
Number of Convictions	5.475	--	5.475	4.429	--	4.429
Statutory Class	-10.020	-1.358	-11.378	-9.109	-1.125	-10.234
Jury Trial	106.615	2.998	109.613	138.295	5.142	143.437
Bench Trial	-8.902	0.922	-7.980	-13.243	-0.121	-13.364
Bail Status	-0.855	0.809	-0.046	4.924	3.842	8.766
Death of Victim	112.810	24.979	137.789	206.109	25.854	231.963
Injury	4.420	1.919	6.339	5.917	-4.212	1.705
Crime Against Person	-7.875	15.903	8.028	-15.022	20.399	5.377
Prior Record	0.617	-2.404	-1.787	-2.095	-0.493	-2.588
Sex	19.559	-7.670	11.889	25.793	-15.662	10.131
Race	1.414	2.700	4.114	6.793	8.189	14.982

Before	After
The variance in sentence explained by variables in the model = 0.2500	The variance in sentence explained by variables in the model = 0.3446
F = 15.03	F = 12.00
n = 508	n = 263
Probability = 0.0001	Probability = 0.0001

The effects of jury trial are another story entirely. Before guidelines, the direct effects of jury trial on sentence length were 106.615. This means that, when all other variables in the model are held constant, defendants who were convicted by juries received prison terms that were, on the average, 106.615 months (or approximately 8.8 years) longer than those given to defendants who pled guilty. This coefficient is significant at p E .001. In other words, if the true population coefficient were zero, an estimate as high as 106.615 would be obtained in fewer than one out of one thousand procedures such as this one. Alternatively, the 95-percent confidence interval lies between 73.362 and 139.868. This means that in the long run 95 percent of the intervals obtained by this procedure will include the true parameter.128

After guidelines, the direct effects of jury trial on sentence length were 138.295, meaning that offenders convicted by juries received prison

128. See H. Blalock, Social Statistics 208-211 (1979); P. Hoel, Introduction to Mathematical Statistics 200-202 (1971).

terms that were, on the average, 138.295 months (or approximately 11.5 years) longer than those given to offenders who pled guilty but otherwise were similarly situated with respect to the other variables in the model. This coefficient also is significant at p $\leq$ .001. The 95-percent confidence interval lies between 81.555 and 195.035. The difference between the pre- and postguidelines coefficients, however, is nonsignificant (t = 0.944).

From the foregoing analyses, it appears that convicted defendants pay a heavy penalty for demanding a jury trial. Among those who are sentenced to imprisonment, defendants convicted by juries received sentences that were roughly ten years longer than similarly situated defendants who pled guilty. Moreover, fewer than ten percent of those who are convicted by juries receive probation. These results are consistent with the descriptive analysis reported above. Further, it appears that sentencing guidelines did not reduce differential sentencing based on method of conviction. (If anything, the differences between guilty plea and jury trial cases increased from the pre- to postguidelines sample.) This, too, is consistent with what we learned during our interviews. Sentencing guidelines did not affect the plea bargaining process in any significant way.

Implications for Future Attempts to Structure Sentencing Discretion

Our original purpose in studying the plea bargaining processes in Denver, Philadelphia, and Chicago was to investigate the impact of sentencing guidelines on the exercise of discretion by prosecution and defense. As a result of our study, however, we are struck by the implications of the plea bargaining processes in those three cities for future attempts to structure judicial sentencing discretion.

The experience with sentencing guidelines in Denver, Philadelphia, and Chicago argues strongly for taking a broader view of sentencing reform. It seems clear that sentencing guidelines cannot fulfill their purposes unless they are developed and implemented with due consideration of the larger system of discretionary powers that influence judicial sentencing decisions. To ignore the fact that the majority of criminal cases are settled by negotiation is foolish and ultimately fatal. The courts' need to induce guilty pleas must be taken into account in any successful attempt to reform sentencing.

The interdependence of judges, prosecutors, and defense attorneys implies that voluntary sentencing guidelines, which lack the force and effect of law, will not influence the decision process without the support of these three principal actors. Yet there is little reason to expect or even to desire the increased confluence of interests that would engender support among judges, prosecutors, and defense attorneys. Their roles are differentiated by legal tradition and presumably by current values as well. Indeed, much of the recent dissatisfaction with the functioning of our criminal courts is attributable to an excess of common interests among the judge, prosecutor, and defense attorney. The formation of close-knit work groups encourages lawyers to compromise the

parties' interests for reasons that may be unrelated to the merits of a case, and prevents the judge from assuming a position of independence and neutral detachment. Voluntary sentencing guidelines do not work, and the conditions under which they might work would be unacceptable. Accordingly, the idea of voluntary sentencing guidelines should be abandoned, if indeed it was ever taken seriously.

One alternative to voluntary sentencing guidelines is, of course, guidelines that have the imprimatur of law. These could take the form of regulations promulgated by an administrative sentencing commission,[129] judicial rules,[130] or conceivably legislation.[131] Such guidelines could retain the flexibility needed to accommodate the endless variety of factual circumstances by allowing departures from the guidelines for good and sufficient reasons. They could have all of the properties for which sentencing guidelines have been touted over legislatively fixed sentences. Judicial sentencing discretion could be structured, with many of the consequent benefits urged by Professor Davis.[132] Indeed, sentencing guidelines could fulfill the promise of a common law of sentencing.[133]

Merely giving sentencing guidelines the force and effect of law will not, however, obviate the difficulties caused by the interdependence of judges, prosecutors, and defense attorneys. The need to induce guilty pleas will continue to create pressure to charge-bargain. As long as the prosecutor can negotiate reduced charges, the judge's role in sentencing will be diminished. Although some commentators, especially prosecutors,[134] have argued that increased prosecutorial discretion is

129. See M. Tonry, The Sentencing Commission in Sentencing Reform, 7 Hofstra L. Rev. 315 (1979); J. Coffee, The Repressed Issues of Sentencing: Accountability, Predictability, and Equality in the Era of the Sentencing Commission, 66 Geo. L. J. 975 (1978); R. Singer, In Favor of "Presumptive Sentences" Set by a Sentencing Commission, 1978 Crime and Delinquency 401; M. Zalman, A Commission Model of Sentencing, 53 Notre Dame Law. 266 (1977); P. O'Donnell, M. Churgin, and D. Curtis, Toward a Just and Effective Sentencing System (1977); M. Frankel, Criminal Sentences: Law Without Order 118-123 (1973).

130. The courts of New Jersey have implemented statewide sentencing guidelines with the approval of the New Jersey Supreme Court, but the guidelines have not been promulgated as court rules. In many states there would appear to be no reason why sentencing guidelines could not be adopted in other states under supreme court rulemaking authority. But see the works cited in note 129 supra with respect to the advantages of the administrative model.

131. The obvious disadvantages of legislatively enacted "guidelines" are the cumbersomeness of the legislative process and, somewhat paradoxically, the ease with which presumptive sentences could change with the fluctuations of political opinion.

132. See K. Davis, Discretionary Justice: A Preliminary Inquiry (1977).

133. See N. Morris, Toward Principled Sentencing, 37 Maryland L. Rev. 267 (1977).

134. See A. Alschuler, supra note 105, at note 70.

desirable, most would agree that the sentencing decision should belong to the judge rather than to the designated representative of a party in interest. With some exceptions, judges are more experienced and objective than prosecutors. Our system is designed to permit the prosecutor to act as an adversary. In the absence of fundamental changes in the adversarial nature of criminal proceedings, it would be anomalous to consciously delegate sentencing authority to one of the adversaries.

If sentencing discretion is to be structured without further distortion of the criminal process, method of conviction must be explicitly included as a factor determining sentences. This idea is not new. Two of the researchers who pioneered the empirical approach to sentencing guidelines have suggested that, if plea bargaining is to continue, "each guideline category should contain a sentencing range for conviction by plea as well as a sentencing range for conviction by trial."[135] In the context of legislatively fixed sentences, Professor Alschuler has argued the necessity of legislative specification of rewards for guilty pleas.[136] As Alschuler notes, however, such a legislative provision would be more susceptible to constitutional attacks than the present system is.[137]

A brief reconsideration of the plea bargaining processes in our three study sites helps to sharpen the issues. No doubt the most striking aspect of our investigation of plea bargaining is the Philadelphia Court's unusual system for disposing of cases. To the ordinary observer, the Philadelphia system seems obviously unfair. Indeed, the decision of the Philadelphia judges' committee not to include method of conviction in the guidelines despite its empirical importance can be seen as a tacit admission that the current practices, if they could be proved, would be held to violate the Constitution.[138]

It is important to note what is not at issue here. In Philadelphia the large disparity exists between sentences imposed after jury trials and those imposed after either bench trials or guilty pleas; it is not, as is often assumed, between guilty pleas and trials. This is consistent with the findings of Tiffany, Avichai, and Peters in their study of

135. P. Hoffman and L. DeGostin, An Argument for Self-Imposed Explicit Judicial Sentencing Standards, 3 J. Crim. Just. 195, 201 (1975).

136. A. Alschuler, supra note 105, at 575, conditions his argument on the necessity of inducing pleas to spare the expenses of trials, which neither he nor we concede. "I am not at all persuaded that our society is too impoverished to give its criminal defendants their day in court." Id. at 565.

137. Id. at note 72. Developments in case law since the publication of that article make a successful attack seem less probable. See Corbitt v. New Jersey, 439 U.S. 212 (1978).

138. See p. 173.

federal sentencing practices.[139] As they point out, several theories have been advanced in justification of differential sentencing on the basis of method of conviction.[140] The most popular argument is that by pleading guilty the offender has shown contrition, which is the first step toward his or her successful rehabilitation.[141] This argument, however, obviously does not apply to differences between jury trials and bench trials.

Another popular theory is that defendants tend to perjure themselves during jury trials, and that perjury is both a cause of and a legally sufficient basis for more severe sentences, even in the absence of a perjury conviction.[142] There is no doubt that defendants frequently perjure themselves at trial, but one wonders whether there exists a difference in the incidence, extent, and blatancy of perjury between jury and bench trials sufficient to cause, let alone justify, large differences in sentences. As Tiffany, Avichai, and Peters put it,

> Whatever one may think of the reasons now commonly advanced as justification of differences in sentencing between those who plead guilty and those convicted following trial, no correctionally relevant criterion comes to mind that would serve to justify more severe sentences following jury conviction than sentences following trial court convictions.[143]

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139. L. Tiffany, Y. Avichai, and G. Peters, A Statistical Analysis of Sentencing in Federal Courts: Defendants Sentenced After Trial, 1967-68, 4 J Legal Studies 369, 379-387 (1975). See also R. Dawson, Sentencing: The Decision as to Type, Length, and Conditions of Sentence 187-188 (1969) (finding that Detroit prosecutors believe judges are more lenient with defendants convicted by bench trials than with those convicted by jury trials; judges, however, denied this).

140. Id.

141. See, e.g., A.B.A. Project on Standards for the Administration of Criminal Justice, Standards Relating to Pleas of Guilty §1.8(a)(ii) and comments at pp. 41-45 (Approved Draft, 1968) [hereinafter cited as A.B.A. Standards]; Brady v. United States, 397 U.S. 742, 753 (1970). This argument is paradoxical because, to the extent that sentence concessions are necessary to induce guilty pleas, it is the concessions, and not contrition, that motivate defendants to plead guilty. If defendants were motivated by feelings of contrition, sentence concessions would be unnecessary. See North Carolina v. Alford, 400 U.S. 25 (1970) (defendant's conviction by guilty plea upheld despite his protestations of innocence).

142. See L. Tiffany et al., supra note 139, at 383. When asked about the relative incidence of perjury at jury and bench trials, one Philadelphia defense attorney observed that perjury is most common during guilty plea colloquies, when the defendant is required to deny that he or she has been made promises with respect to the eventual sentence.

143. Id. at 385.

The only adequate explanation for differential sentencing in the Philadelphia court, as well as the federal courts, is economy.[144] The fact of the matter is that most courts do not have the resources to accommodate significant increases in trials.[145] The constitutional question is whether economy justifies the observed differences between sentences following jury trial convictions and those following other methods of conviction. We shall return to this question shortly.

We focus on Philadelphia not because it is fundamentally different from other courts but because the essential facts are so conspicuous in Philadelphia. The point is that the Philadelphia Court of Common Pleas is not unusual in any important respect. Although statistical evidence may be lacking, it is clear that other courts sentence differentially according to the method of conviction. Some Chicago judges make their practices quite explicit.[146] The underlying reality is the same in Denver as well, even though most Denver judges do not participate in plea bargaining and some might prefer not to sentence differentially. For many Denver defendants, sentence concessions require charge reductions, which can only be obtained by a plea agreement with the prosecutor.[147] In one form or another, the problem exists in courts all over the country. Why should something that seems unacceptable in one form be widely accepted in others? In some ways the Philadelphia system offers

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144. See id. at 386. See also R. Dawson, supra note 139, at 182; D. Rotenberg, The Progress of Plea Bargaining: The A.B.A. Standards and Beyond, 8 Conn. L. Rev. 44, 84 (1975) ("The real impetus behind the wider adoption of plea bargaining in the United States today is clearly economic.")

The supposedly higher probability of conviction is sometimes offered as a justification for differential sentencing based on method of conviction. See Scott v. United States, 419 F. 2d 264 (D.C. Cir. 1969); A.B.A. Standards 1.8(a)(i) and Comment. But see L. Tiffany et al., supra note 139, at 386 (the probability of conviction does not differ according to the type of trial in the federal courts). Whether or not the probability of conviction is higher in bench trials than in jury trials, and we are willing to suppose that it may be so in the state courts, our interviews convince us that the dominant subjective motive of judges and prosecutors is economic. Indeed, the probability-of-conviction argument can, in part, be reduced to a matter of economics.

145. There is a dearth of jury trial facilities in many urban courts around the country. Recently, for example, the District of Columbia courts threatened to discontinue jury trials unless additional funding was forthcoming. While we were interviewing in Denver, several judges and lawyers complained that, because of a shortage of funds, the Court was unable to buy air conditioners. The result, they said, is that on hot summer days jurors become uncomfortable and drowsy. Both judges and lawyers indicated some reluctance to conduct jury trials during July and August. Finally, the accommodations for jurors are notoriously bad in almost all courts. Consequently, there is pressure on courts to discourage jury trials in order to spare the discomfort of prospective jurors, who in some states are part of judges' electoral constituency.

146. See pp.176-177.

147. See p. 160.

greater protection to defendants than do other, less visible systems of inducing waivers.[148] As far as the Constitution is concerned, all should stand or fall together.

The U. S. Supreme Court has decided a series of cases that, with one notable exception, appear to uphold differential sentencing according to method of conviction.[149] In our view, and those of other commentators, the opinions in most of those cases have been plagued by specious issues and unconvincing arguments.[150] In an apparent effort to avoid the ultimate issue, the Court has distorted the law of waiver of rights and reached patently unfair results. The inclusion of method of conviction as a factor in sentencing guidelines would force the Court to face the issue squarely.[151]

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148. Consider, for example, the plight of the defendant in People v. Lambrecht, supra note 88. In practice, Philadelphia defendants who shop for lenient judges are unlikely to fare so poorly.

149. See Brady v. United States, 397 U.S. 742 (1970); McMann v. Richardson, 397 U.S. 759 (1970); Parker v. North Carolina, 397 U.S. 790 (1970); North Carolina v. Alford, 400 U.S. 25 (1970); Ludwig v. Massachusetts, 427 U.S. 618 (1976); Bordenkircher v. Hayes, 434 U.S. 357 (1978); Corbitt v. New Jersey, 439 U.S. 212 (1979). But see United States v. Jackson, 390 U.S. 570 (1968).

150. See A. Alschuler, The Supreme Court, the Defense Attorney, and the Guilty Plea, 47 U. Colo. L. Rev. 1 (1975); M. Tigar, The Supreme Court, 1969 Term--Foreword: Waiver of Constitutional Rights: Disquiet in the Citadel, 84 Harv. L. Rev. 1 (1970); G. Dix, Waiver in Criminal Procedure: A Brief for More Careful Analysis, 55 Tex. L. Rev. 193 (1977).

151. It can be argued that the Court completely resolved the issue in Bordenkircher v. Hayes, 434 U.S. 357 (1978). Hayes was charged with uttering a forged instrument in the amount of $88.30, an offense punishable by a prison term of two to ten years. During plea negotiations, the prosecutor threatened to obtain an additional indictment under a habitual criminal statute, which carried a penalty of mandatory life imprisonment, if Hayes did not plead guilty to the original charge. Hayes protested his innocence and opted for a jury trial. The prosecutor carried out his threat, the jury convicted Hayes of both uttering and being a habitual criminal, and Hayes was sentenced to life imprisonment.

The U.S. Supreme Court, in a five-to-four decision, affirmed Hayes's conviction, holding that in the "give and take" of plea bargaining it is permissible for the prosecutor to exercise his or her discretion for the express purpose of inducing guilty pleas, so long as the defendant is free to accept or reject the prosecutor's offer. Although this ruling would seem to resolve the issue of including method of conviction as a determinative factor in sentencing guidelines, it is possible that the Court would distinguish Bordenkircher v. Hayes on the grounds that the prosecutor in that case sought a guilty plea rather than a mere jury waiver, relying on the traditional rationales for encouraging guilty pleas. See p. 199. The Court also might attribute its holding to the judiciary's long history of deference to prosecutorial charging decisions, which is inapplicable to the issue raised here. Less compelling distinctions have been made. Despite our doubts, we shall assume that the issue has not yet been resolved.

The principal obstacles to a clear ruling on the constitutionality of differential sentencing have been the difficulty of proving the practice and the Supreme Court's preoccupation with the finality of guilty pleas. In the absence of statistical evidence, which is costly to obtain and inevitably subject to various interpretations, the defendant is at a loss to prove differential sentencing unless the trial judge is so careless as to make an admission on the record.[152] Even when the difficulties of proof are surmounted, the Court's extreme concern for the finality of guilty pleas has obscured the fundamental issue. The Court is simply unwilling to disturb pleas of guilty except perhaps where defense counsel's advice was so egregious that it fails the Court's lax standards of competence.[153] The inclusion of method of conviction as a determinative factor in sentencing guidelines promulgated by a legislatively authorized administrative body, or under a state supreme court's constitutional rulemaking authority, would dispense with these impediments by permitting defendants to attack the facial validity of the guidelines.

Moreover, the very process by which guidelines are created should completely disable the already-lame argument, which the Supreme Court has accepted in the past, that the differential is a reward for a plea of guilty rather than punishment for the exercise of constitutional rights.[154] It should become clear that the determination of an exact sentence for a particular kind of case involves an arbitrary decision. There are no known, absolute standards for the severity of sentences. Where the guidelines are the product of empirical analysis, the need to take averages among disparate cases should make apparent the arbitrary nature of the choice. Where, on the other hand, guidelines are the product of deliberate policy choices, it will be evident that the only nonarbitrary decisions will involve relative values. In many instances it will be easy to decide on principle that one kind of case deserves more severe or more lenient treatment than another, but no principles--only standardless choices on which there presumably will be consensus--will govern the assignment of absolute sentence values to individual sentence categories. In the absence of known, absolute, correctionally appropriate standards for sentencing in particular cases, it is mere verbal legerdemain to say that a sentence differential is a reward for a waiver of rights rather than a penalty for their exercise.

Finally, assuming that the guidelines were to differentiate between jury trials and bench trials, as well they should if economy is at issue, we would at last dispense with the contrition and perjury arguments that have obscured the real reason for differential sentencing. Perhaps then the Supreme Court would fully resolve the issue: Can the courts continue to sentence differentially according to method of conviction, consistent with the constitutional privilege against compulsory self-incrimination,

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152. Note that in Bordenkircher v. Hayes the prosecutor admitted on the record his motivation for threatening the habitual criminal charge.

153. See A. Alschuler, supra note 150, at 5-6.

154. See Brady v. United States, 397 U.S. 742 (1970); Corbitt v. New Jersey, 439 U.S. 212 (1979). See also A.B.A. Standards 51-52.

the right to a jury trial, the right to confront adverse witnesses, the traditional values of due process, and the very concept of rights?

Having enumerated some of the constitutional values, we hasten to add that it does not seem likely that the Supreme Court would strike down differential sentencing. To the contrary, the Court has approved the inherently coercive practice of plea bargaining.[155] Furthermore, the Court has ruled that state legislatures may authorize more lenient sentences for convictions obtained by guilty plea than for those following jury trials, so long as the difference is not too great.[156] For all the Supreme Court has said, the difference can be literally anything short of the choice between life and death.[157]

A constitutional decision about the propriety of including method of conviction as a determinative factor in sentencing guidelines would necessitate hard choices. There is no way around the fact that the exercise of rights is costly to society; rights cost money. Jury trials are particularly expensive. If we are to abandon the inducement of guilty pleas and jury waivers by differential sentencing, society must be willing to provide the resources necessary to afford jury trials to all defendants who freely choose them. It simply will not do to condemn trial courts' differential sentencing practices while refusing to allocate to them the funds necessary to provide more

155. See Brady v. United States, 397 U.S. 742 (1970); Santobello v. New York, 404 U.S. 257 (1971); Bordenkircher v. Hayes, 434 U.S. 357 (1978).

156. Corbitt v. New Jersey, 439 U.S. 570 (1968).

157. The only case of differential sentencing in which the defendant prevailed in the Supreme Court is United States v. Jackson, 390 U.S. 570 (1968), where the Federal Kidnapping Act was held invalid because it authorized the death penalty only in jury trial cases. In Corbitt, however, the Court upheld a New Jersey statute that mandated life imprisonment for those convicted by juries of first-degree murder but authorized shorter sentences as well for those convicted by plea of non vult. Justice White, writing for the Court, relied heavily on the fact that Corbitt did not face the possibility of the death penalty if he went to jury trial:

> The principal difference is that the pressures to forgo trial and plea to the charge in this case are not what they were in Jackson. First, the death penalty, which is "unique in its severity and irrevocability," . . . is not involved here. Although we need not agree with the New Jersey court that the Jackson rationale is limited to those cases where a plea avoids any possibility of the death penalty being imposed, it is a material fact that under the New Jersey law the maximum penalty for murder is life imprisonment, not death.

439 U.S. at 217.

jury trials. The trial courts are in an untenable position, forced by financial circumstances to deny rights guaranteed by the law they are responsible for administering. So far they have responded obligingly by sweeping the problem under a carpet of individualized discretionary decisions. We believe the time has come to pull back the carpet and face the mess.

To be sure, a decision to permit differential sentencing would not be without cost. The cost, however, would be imposed upon criminal defendants, and ultimately upon the concept of rights and the rule of law. We would be telling criminal defendants in no uncertain terms that although the Constitution guarantees their trial rights, they may suffer additional punishment if they choose to exercise them. Such a decision is unlikely to engender the respect for law on which a free society must depend. But the situation is hardly different now. It is perhaps less clear, but no less felt by those who have experienced criminal justice in our courts. Indeed, the present system is worse because it is also dishonest. For all of their insensitivity to societal norms, the sanctity of law, and standards of human decency, many offenders recognize hypocrisy when they see it.[158] The least they are entitled to is forthrightness; our lack of it undermines the integrity of our legal system and ultimately our social structure.

The inclusion of method of conviction in sentencing guidelines would have salutary consequences in addition to the virtue of honesty. To begin with, it would constitute a truth-in-advertising act for criminal defendants. Due process would, of course, require that sentencing guidelines having the force and effect of law be disclosed to both prosecution and defense. If defendants were permitted to withdraw their guilty pleas whenever their sentences exceed the guidelines, their waivers would at least be knowing if not wholly voluntary. The present system is a grab bag of real and illusory bargains. Where additional punishment can be imposed for the exercise of rights, those rights are worth no more than the concessions for which they can be exchanged. Illusory bargains can reduce to nothing the value of rights. The inclusion of method of conviction in sentencing guidelines would eliminate much phony bargaining.

As two of the guidelines researchers have pointed out, if it is determined necessary and legally appropriate to induce a particular proportion of guilty pleas in order to spare the expense of too many trials, the incorporation of method of conviction into the guidelines can

158. It is difficult to observe a guilty plea colloquy such as those in Denver, Chicago, or Philadelphia without being struck by the hypocrisy of requiring the defendant to stand up in court and deny that promises have been made concerning the sentence to be imposed. Defendants are not oblivious to the fact that judges, prosecutors, and defense attorneys require them to lie on the record in order for their guilty pleas to be accepted. It is at once amusing and sad to see the embarrassment that results when a defendant naively replies honestly to the judge's routine inquiry about promised sentence concessions. We note that such denials are no longer required in jurisdictions that follow the model of Rule 11 of the Federal Rules of Criminal Procedure.

help to achieve the desired result by the least amount of coercion necessary.[159] The guidelines could be continually revised on the basis of empirical evidence of the relationship between the size of the sentence differential and the proportion of cases resulting in jury trials. The principle of the least drastic alternatives could prevail.[160]

Once charge reductions and other concessions from the prosecutor are no longer needed to induce guilty pleas, prosecutorial plea bargaining can be prohibited.[161] The charges of which an offender is convicted could constitute an accurate description of the crimes of which he is guilty. The prosecutor's "hammer" can be taken away, along with some of the worst excesses of the present plea bargaining regime. The judge can assume his or her proper role as head of the trial court system and doer of justice.

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159. P. Hoffman and L. DeGostin, supra note 135, at 104.

160. See R. Singer, Sending Men to Prison: Constitutional Aspects of the Burden of Proof and the Doctrine of the Least Drastic Alternative as Applied to Sentencing Determinations, 58 Cornell L. Rev. 51 (1972).

161. Professor Alschuler has been the most forceful advocate of a ban on prosecutorial plea bargaining. See A. Alschuler, supra note 104, at 563-577; A. Alschuler, The Defense Attorney's Role in Plea Bargaining, 84 Yale L. J. 1179 (1975). See also National Advisory Commission on Criminal Justice Standards and Goals, Courts, Standard 3.1 (1973); R. Parnas and R. Atkins, Abolishing Plea Bargaining: A Proposal, 14 Crim. L. Bull. 101 (1978).

APPENDIX A

A COST ANALYSIS OF DEVELOPMENT AND IMPLEMENTATION OF SENTENCING GUIDELINES

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INTRODUCTION

The debate over a proposed reform usually takes place on several levels including the desirability of the goals and objectives it is expected to achieve; the probability of the change having the hoped for effects; and, inevitably when resources are scarce, the cost of developing and implementing the program.[1] The desirability of the objectives is beyond the scope of an empirical inquiry. But the effectiveness and cost implications of a program are issues which are subject to objective analysis.[2]

This paper addresses the issue of the cost of developing and implementing sentencing guidelines in a court. It is intended as a companion to the larger study carried out by the National Center for State Courts which assesses the effectiveness of this reform in reducing disparity. The experience of one of the five sites which have adopted sentencing guidelines--the Philadelphia Court of Common Pleas--is used to estimate the kind, volume, and distribution of resources required to develop and implement sentencing guidelines. Such an analysis is designed to expand the knowledge available in the debate over sentencing reforms to include the resource implications of guidelines as well as their effectiveness and the desirability of the outcomes.

An Economic Approach to Sentencing Guidelines

From an economic perspective, sentencing guidelines represent an attempt to increase the efficiency of the production process. That is, it is designed to enhance the quality of the outcome (uniformity among sentences) by altering the distribution of critical resources (information about sentencing practices) while minimizing the cost of the inputs. In these terms desirability of the reform depends upon whether this production process achieves the desired outcomes at less cost than an alternative (e.g., mandatory sentences or current practice);[3] or, whether the ratio of the value of the output is greater than that of the inputs.

This study is an attempt to place a cost on the input side of the equation. Because of the limitations on data availability, the outputs are not translated into dollar value terms but rather are left to the definition contained in the effectiveness study of the NCSC. It is important, however, for the reader to understand the relationship of the cost analysis which follows to the larger economic framework of which it is a part.

Economics and criminal justice. The conceptual model underlying economic theory is composed of three general elements: input, process, and output. Analyzing the relationship between these components may occur at an organizational (micro economics), national (macro economics) or social (welfare economics) level. The behavioral assumptions used to analyze input-process-output relationships are that decision-makers, whether represented by an individual, organization or society, are rational and maximizing entities.

Input refers to the labor, facilities, knowledge and time required to create an output or benefit. The cost of these inputs is defined broadly as the value of what is foregone by choosing one alternative rather than another. What specifically is foregone may be intangible (personal direction) or tangible (shorter sentences); subjective (sense of security) or objective (victimization). Therefore, the concept of value by encompassing whatever is useful or satisfies a need includes more than what can be expressed simply in dollar terms.[4]

The process component refers to those activities which transform inputs into outputs. For example, the judicial process converts criminal acts into sentences. The concept of opportunity cost is used to include in an analysis the fact that there is more than one way (method or process) for achieving the same end, goal or output. Simply put, opportunity cost is the value of what is foregone to pursue a particular course of action. The purpose of sentencing guidelines is to improve the quality of output (reduced disparity) resulting from judicial decisions, but there are alternative techniques, such as appellate review, flat time and mandatory sentencing, available to achieve the same goal . Economic analysis is intended to show which particular alternative will result in the largest positive difference between inputs and outputs. In the case of guidelines, an economic analysis ideally would estimate the relative costs of other methods for reducing disparity and relate these to expected changes in disparity occasioned by each alternative.

Several other concepts from economic theory are important for understanding the research described in the following sections. The first is the difference between consumption and investment in capital. Investment occurs when resources are used to meet future needs; the use of resources to meet current needs is classified as consumption. It should be emphasized that investment is not restricted to buildings, equipment and other tangible assets. For example, expenditures on health care and education are considered an investment in human capital since they increase a future productivity.[5] Research may be considered an investment as it is intended to increase our knowledge base and to evaluate the technology (methods) used by organizations to accomplish their goals.[6] Examples include evaluations of pretrial diversion, halfway houses, police patrol, and case management approaches.

Guidelines development can be conceived of as an investment in knowledge capital by the courts. They consolidate the knowledge regarding judicial outcomes, and this new knowledge becomes part of the process which transforms criminal acts into punishments. The resource and financial costs of the research to develop sentencing guidelines are an investment in capital which will be used over several future accounting periods. Updating guidelines is a cost of maintaining and lengthening (presumably) the useful life of this capital. Resources (judge, clerk, programmer, computer charges) used to complete the grid and monitor its application are consumption items. While this economic

description is an obvious over-simplification of the judicial process, it clarifies the focus of this study which is to estimate the resources invested in changing the technology used to make sentencing decisions.

The economic concept of externalities refers to the situation where an organization's activities are dependent upon the inputs or outputs of another.[7] The effects may be positive (i.e., enhance the organization's activities) or negative (i.e., increase its costs). This concept is especially relevant in analyzing criminal justice because of the formal distribution of responsibility for various process stages between judicial, legislative and executive branches. Theoretically, guidelines may affect (positively or negatively) the prosecutor's office, the corrections agency, the clerk and the defense attorneys.

The economic analysis of public programs applies the concepts and theoretical assumptions described above by techniques called cost, comparative cost, cost-effectiveness and cost-benefit. The techniques are differentiated by their inclusiveness in terms of the general input-process-output model and their treatment of the individual components. Estimating input values is common to cost, comparative cost, cost-effectiveness and cost-benefit. Comparative analysis simply relates these values for two or more processes. Cost-effectiveness introduces the output component (for example, reduced disparity) and its measurement (for example, reduced standard deviation in sentence length weighted by severity). If a dollar value is attached to the output and related in some way to the dollar value of inputs, a cost-benefit analysis has been performed.

This study is limited to a cost analysis of inputs to the investment and consumption activities of the Philadelphia Court of Common Pleas which are associated with one alternative for reducing sentencing disparity. The costs which will be addressed are further limited to the dollar value of reallocated or additional resources which could be quantified within the project's constraints. The research, therefore, does not examine non-pecuniary costs; relate inputs to reductions in disparity; or assess the effects external to the court. Given an agreed upon goal or output, determining input costs is the first step in any of the economic analysis techniques described earlier. The results of even the least complex technique--cost analysis--is an important addition to the information used in choosing among alternative judicial policies.

Cost Analysis

The issue of the cost of sentencing guidelines is not a simple one. It contains a series of specific cost-oriented policy questions. These more specific questions include: what are the aggregate resources needed to develop and implement sentencing guidelines; how much of these resources already exist in-house and can be reallocated, or how much of these aggregate resource-requirements entail additional financial expenditures; what are annual operating costs; and can every jurisdiction afford the costs of developing and implementing sentencing guidelines?

Our first answer to this series of questions is that, like beauty to the beholder, cost is relative to the socio-economic circumstances of the questioner. The amount of resources required to develop and implement sentencing guidelines depends on the size of the sentencing jurisdiction; and the extent of financial costs depends upon the availability of in-house planning resources and the level of prior commitments of those resources.

A Discussion of Costs

The overall concept of cost used in this report is opportunity cost. This is simply the value of activities foregone when resources are deployed for development and implementation of sentencing guidelines.

Development costs include all costs which are incurred in the development of sentencing guidelines and are generally classified as investments. Post-implementation costs occur after guidelines are put in place and include the operating costs associated with grid completion, monitoring, and updating. Updating is a cost of maintaining the capital investment represented by the guidelines themselves.

The development or investment costs of sentencing guidelines can be further categorized by their origin: some are covered by reallocating resources from existing activities; and others are additional financial outlays or increases to the court's budget. Lastly, several standard basic cost categories will be used in the analysis: personnel costs, consisting of the time of the judges, analysts, administrative personnel, coders and part-time assistants; and non-personnel costs, consisting of travel costs, computer costs, and indirect costs under which all other non-personnel costs are combined.

To illustrate these cost distinctions, let us assume that the development of sentencing guidelines requires participation of two computer programmers. One new programmer is hired; but the other is already part of the staff and is able to work on the sentencing guidelines project in addition to performing her other tasks. Participation of both programmers in guidelines development generates resource costs, and the salary and benefits of the new employee constitute an additional financial cost. The reallocated portion of opportunity costs are the salary and benefits of the previously employed programmer that can be attributed to the sentencing guidelines development effort, since the time she spends working on the guidelines is at the expense of some other in-house functions she could have carried out.

Site-Selection

The five local jurisdictions where sentencing guidelines have been implemented are Phoenix, Arizona; Denver, Colorado; Chicago, Illinois; Newark, New Jersey; and Philadelphia, Pennsylvania. As originally

conceived, the cost analysis was to assess the resource implications of the effects of sentencing guidelines--for example, shifts in time requested for trial; or the relative use of probation or prison. However, such an approach required a site with extensive experience with the guidelines to ensure that the outcome effects could be identified. Preliminary analysis revealed that implementation of the guidelines had not progressed at any of the sites to a point which would permit an assessment of resource implications. Therefore, in consultation with the National Center for State Courts, the thrust of the cost analysis was shifted from an assessment of process and outputs, to the costs of development and implementation, and Philadelphia was selected as the most appropriate site for study.

Philadelphia was chosen as the site for empirical inquiry for several reasons. First, the Court of Common Pleas developed their guidelines in-house rather than contracting it out. Therefore, the cost data were specific to the site and readily available from the participants. Second, implementation was just underway as this project began. This facilitated recapturing the events which led up to the implementation phase. Third, there were no serious deviations between the guidelines as developed in Philadelphia and the original model, which increases the confidence in the general application of the conclusions to other sites.

The major limitation on Philadelphia as a study site is its size. As will be described, the Court of Common Pleas has a large in-house research staff. This simplified many of the resource questions which are likely to be much more troubling to other jurisdictions. However, the concept of opportunity costs facilitated capturing the full range of resources which may be required so that the results of the analysis can be adjusted to the circumstances of other courts.

Development Costs

Let us now examine the resource costs, direct and indirect, generated during the development of sentencing guidelines in Philadelphia. The Philadelphia sentencing guideline development effort can be divided by similarity in activities performed into seven development phases. Figure 1 describes the activities performed in each phase and displays the development timeframe as well. The first three phases can be considered preliminary stages in the process. Most of the effort was devoted to becoming familiar with the general concept of guidelines, evaluating its presumed benefits, and preparing a plan of action once the decision was made to proceed. The last four phases describe the sequence of activities required to formally develop the guidelines. Most of the efforts correspond to traditional research requirements: data collection, analysis, revisions of the framework, and re-analysis of the data.

These description of the activities of personnel are critical to the cost analysis. The dollar value of personnel resources was drawn from the time devoted to the development by court staff and judges. Where possible, the time of each individual was used. Where individual

schedules could not be determined, an estimate was used. For example, resource requirements for the meetings with judges to review proposed guidelines categories was calculated using the estimated length of time for each meeting and the number of judges in attendance. The cost of that time is based on the salary schedules for the relevant positions in the Court of Common Pleas. (See Appendix I for a description of the methodology and calculations used.)

Table 1 summarizes the cost of the resources required to develop sentencing guidelines in Philadelphia Court of Common Pleas. As might be expected, the principal cost category is personnel. Approximately 70% of the total resources required for the development effort are contained in staff time. Within that category, the largest item is for the research skills required to carry out the actual study. The full-time and part-time analysts, coders, and research assistants account for 83% of the total personnel costs. These results suggest that although judges may be critical to the quality of the final guidelines, the development process does not place a heavy demand on their time.

The non-personnel costs are dominated by the administrative costs associated with carrying out a large research effort. These include such things as reproduction costs, office supplies and rental of equipment. In Philadelphia computer time was leased from a time-share company. Despite the large data manipulations required in the guidelines development effort, these costs constitute a very small proportion of the total.

FIGURE 1

DESCRIPTION OF DEVELOPMENT EFFORT BY PHASE

Development Phase	Functional Components	Elapsed Time
I	Court progress analyst attends seminar on court sentencing guidelines and subsequently visits the Criminal Justice Resource Center (CJRC). A Common Pleas Court judge also visits, another two confer with the Presiding Judge and Court Administrator.	4 Months
II	A CJRC representative meets with Common Pleas Court operation staff and judges; more visits to CJRC office are made; decision to develop sentencing guidelines made.	3 Months
III	Project Tasks: Data analysis, modeling, testing, guideline grid design defined, coding manual developed and coders hired; judges advisory board formed, meets monthly, list of regression variables agreed upon by judges.	4 Months
IV	Data coded, cleaned, and analyzed.	8 Months
V	Modeling	4 Months
VI	Preliminary courtroom testing, validation of original (1974) data sample by coding, cleaning, and analysis of new (1977) sample.	8 Months
VII	Court-wide testing, fixed guideline grid developed, approved, implementation date set for March 5, 1979.	7 Months

TABLE 1

TOTAL RESOURCE COSTS OF GUIDELINES DEVELOPMENT

Development Phase	Personnel				Non-Personnel				Total
	Judges	Operations Personnel[1]	Temporary Staff[2]	Subtotal	Travel	Computer	Indirect[3]	Subtotal	
I	$ 349	$ 247	$ -0-	$ 596	$ 472	$ -0-	$ 209	$ 681	$ 1,277
II	763	913	-0-	1,676	611	-0-	464	1,075	2,751
III	1,509	3,870	432	5,811	873	-0-	1,884	2,757	8,568
IV	2,572	13,025	1,906	17,503	1,905	1,200	5,459	8,564	26,067
V	872	9,129	-0-	10,001	-0-	960	3,500	4,460	14,461
VI	3,123	17,537	-0-	20,660	-0-	800	7,483	8,293	28,953
VII	3,318	14,007	-0-	17,325	-0-	500	6,020	6,520	23,845
Total	$12,506	$58,728	$2,338	$73,572	$3,861	$3,460	$25,019	$32,340	$105,922

1. Administrators, Researchers, Program Analysts
2. Coders, Research Assistants
3. Rental, Telephone, Printing, Postal, Office Supplies, and Insurance.

As one would expect, resource requirements escalated dramatically when the project shifted from planning in Phase III to data collection in Phase IV. It was at this point in the process that new staff were added including a second program analyst, and temporary coders and research assistants. This accelerated level of resource utilization continued through the end of the project. Travel costs ran counter to this pattern because a major component of the preliminary activity was consultation with staff of the Criminal Justice Resource Center in Albany, New York, and observation of sites with on-going guidelines programs in Chicago and Phoenix.

Relative to the resources available in-house, resource requirements in the sentencing guidelines development effort were not very sizable. The total time spent by judges amounts to less than one-quarter of one percent of the total working hours of Common Pleas Judges over the course of one year (and the development effort was spread out over two and one-half years). Court administrative research staff time devoted to guidelines as a proportion of total hours is also low, amounting to one percent of the annual hours. The dollar value of the total resources used over the two and one-half years of development amounts to less than one-quarter of one percent.

The financial burden was even less if a distinction is drawn between the resources which were drawn from within the court's existing pool and those which had to be added to the budget. Table 2 summarizes the cost of the reallocated resources and those which required additional expenditures. Most of the cost was absorbed in the Philadelphia Court of Common Pleas by reallocating existing resources. Less than $10,000 of additional expenditures were required to develop the guidelines. Because of the availability of research staff, the only additional personnel costs required were those associated with the part-time services of coders, and of a research assistant. Additional non-personnel costs are accounted for by two items: travel, and leased computer time.

The financial burden of the guidelines development effort, therefore, was relatively small in Philadelphia when compared to total court expenditures. If the focus is narrowed further to center only on the additional funds required, the significance of the costs becomes even lower. However, it should be kept in mind that this does not imply that other courts will find the problem of resources easily solved. The availability of a skilled research staff gave the Philadelphia court an advantage which may not be shared by other jurisdictions.

Post-Implementation Costs of Sentencing Guidelines in Philadelphia

As the sentencing guidelines were not implemented until March 5, 1979, they have not been in operation for a sufficient time to provide a firm basis for a long-run cost analysis. Nevertheless, enough data exists to develop average internal operating cost estimates for 1979.

Operation of sentencing guidelines consists of two activities: completion of the guideline grid forms by the judge or law clerk (prior

TABLE 2

REALLOCATED RESOURCES AND FINANCIAL COSTS OF GUIDELINES DEVELOPMENT

Development Phase	Personnel			Non-Personnel			Total
	Reallocated	Financial	Subtotal	Reallocated	Financial	Subtotal	
I	596	-0-	596	209	472	681	1,27
II	1,672	-0-	1,672	464	611	1,075	2,75
III	5,379	432	5,811	1,884	873	2,757	8,56
IV	15,597	1,906	17,503	5,459	3,105	8,564	26,06
V	10,001	-0-	10,001	3,500	960	4,460	14,46
VI	20,660	-0-	20,660	7,483	800	8,283	28,95
VII	17,325	-0-	17,325	6,020	500	6,520	23,84
Total	$71,234	$2,338	$73,572	$25,019	$7,321	$32,340	$105,91

to sentencing) and subsequent monitoring and evaluation by a court program analyst. As part of the monitoring activity, completed guideline grids are screened for errors; coded, keypunched, and entered into the computer; and frequency analyses, and analyses of predictiveness computed monthly.

Table 3 presents monthly and total 1979 operating costs by operating function. As Table 3 illustrates, guidelines grid completion costs account for less than five percent of total operating costs. Almost all operating costs are attributed to the full-time court programs analyst and the continuing monthly meeting of the Judge's Advisory Committee. Total 1979 operating costs are approximately thirty percent of total development resource costs, and 1979 guideline grid completion costs are slightly more than one percent of total development resource costs. As Table 3 further illustrates, financial costs amount to less than two percent of total operating costs. Out-of-pocket costs for the first ten months of operation total less than $500.00. In operation, as well as in development, therefore, Philadelphia achieved great success in minimizing out-of-pocket costs.

In addition to the two operating activities, two other types of capital maintenance activities with cost implications can be distinguished: data analysis in which post-implementation sentencing decisions are compared to pre-implementation decisions and differences analyzed; and continuing development in which guidelines are developed for special types of crime. As of December 31, 1979, these two activities were still in the planning stage and, therefore, it was not possible to assess the costs. Current plans call for coding a sample of 500 cases drawn from the early post-implementation period. Plans also call for development of sentencing guidelines for white-collar crime.

SUMMARY AND CONCLUSION

The purpose of this study has been two-fold: to estimate the cost of developing and implementing sentencing guidelines; and to illustrate the utility of economic analysis in informing policy decisions faced by judicial policymakers, even when data is severely limited. Since only one site was available for analysis, the estimate of cost must be approached with caution. However, several conclusions can be drawn which are likely to apply to most jurisdictions.

The pre-eminant conclusions emerging from this analysis is that the financial costs of sentencing guidelines development and implementation need not be high. In Philadelphia, total resource costs of developing sentencing guidelines were just over $100,000, spread out over two and one half years. Financial costs, however, amounted to less than ten percent or $10,000, which is approximately $4,000 per year. Moreover, financial costs for the first ten months following implementation amounted to less than $500.00.

In Philadelphia Court of Common Pleas, it is true, is a large institution with an annual operating budget in excess of $32 million (and

TABLE 3

SENTENCING GUIDELINES OPERATING COSTS

	REALLOCATED COSTS	FINANCIAL COSTS
Completion Costs:		
Guideline Grid Completion:	93¢ per completion by judge 41¢ per completion by clerk	$ .15 .15
Total Monthly Completion Costs:	$ 134.00	30.00
Total Completion Costs, 1979	$ 1,318.00	292.50
Monitoring Costs:		
Time Cost	$ 2,102.21	--
Computer Cost	21.00	21.00
Indirect Cost	735.77	--
Total Monthly Monitoring Cost	$ 2,858.98	21.00
Total Monitoring Costs, 1979	$28,112.00	204.75
Total Monthly Operating Costs	$ 2,993.00	51.00
Total Operating Cost, 1979	$31,105.00	$497.25

this excludes judges' salaries which are in excess of $3 million per year). Smaller jurisdictions will likely have greater difficulty in freeing existing in-house resources for on-going activities simply because they have less resources. Nevertheless, fifty percent of the costs in developing and implementing sentencing guidelines in Philadelphia were attributable to the functions carried out by two court programs analysts. Any jurisdictions with in-house planners, or analysts, can reduce out-of-pocket costs substantially by having in-house perform as many of the necessary development and implementation functions as possible, given other functions that need to be performed.

The foregoing analysis makes clear that the question of cost is not a trivial one, even when limited to the input side of the equation. Cost-effective and cost-benefit analyses have much more theoretical appeal. However, a more limited cost analysis may be more useful for public decision-makers. It is unusual if there is sufficient time or comparable data to permit a cost-effectiveness study of a program, and cost-benefit raises conceptual problems for public programs because of the difficulty of identifying the universe of effects which should be included in the list of outcomes.[6] These problems are magnified for judicial analysis, as attempts to apply economic analysis to an understanding of the courts are in their infancy. At this stage of our knowledge, the first order of business must be to lay the foundation for more sophisticated theoretical inquiries, as this study has attempted to do by focusing on cost analyses of judicial functions.

ATTACHMENT A

CALCULATION OF DEVELOPMENT COSTS

Several cost items could be taken directly from the expenditure records of the courts. Included in this category were travel expenses, equipment purchases, and contracting for services such as lease of computer time and hiring part-time coders.

Personnel costs were calculated using a combination of staff salaries, time devoted directly to the project, and an estimation of related administration and management costs.

Salaries and Staff Time

Developing staff costs required a more elaborate strategy since most staff time devoted to guidelines development was incidental to other responsibilities. Eight categories of employees were identified as being involved at some point in the process: President Judge, Court Administrator, Judges, Deputy Court Administrator, Court Program Analysts, Budget Officer, Chief Probation Officer, and Law Clerk. Their 1979 annual salary was converted to an hourly wage using the following formula:

Annual Salary	Working Weeks (52 Week - 3 Weeks Average Vacation)	Working Days Per Week (5)	Hours Per Day (8)

The 1979 annual salary was used, even though the project began in 1976, in order to ensure that the costs are presented in constant dollar terms.

The time personnel devoted to the project during the course of guidelines development is based on estimates by the key actors. Where possible the estimates were tied to specific events to increase their accuracy, for example, attendance at a sentencing guidelines seminar, or monthly review sessions by judges of preliminary guidelines categories. In other cases, the estimates were structural around individuals, for example, one program analyst began working full-time on guidelines development during the second year of the project.

Indirect Costs

Time devoted directly to the project does not capture all of the personnel resources involved. To estimate the administrative and management costs incurred as a result of project activities, an indirect rate was calculated as follows:

Indirect administration and management costs

= 1979 estimated administrative and management costs. Common Pleas Courts allocated among service divisions, and personnel (all other personnel) including judges

$$= \frac{\$\ 3,825,198}{33,237,057} = .12$$

Plus

All other indirect costs

= 1979 estimated indirect costs for Common Pleas Courts allocated divided by estimated 1979 personnel costs

$$= \frac{\$\ 7,836,556}{33,452,255} = .23$$

Therefore (total) indirect costs = .35 x personnel (time) costs.

COST CALCULATION

Following is an example of how these figures were employed to calculate the cost of development. The period covered is January 1, 1978 to August 31, 1978. At this point a preliminary set of guidelines had been developed and were being tested in the courtrooms.

ATTACHMENT B

COMPILATION OF OPERATING COSTS

Completing the sentencing guidelines grid takes, on the average, one and one-half minutes. Applying loaded--that is, with indirect cost included--average costs of $31.00 per hour for judges and $10.75 per hour for clerks, and including a materials cost of .15/completion; total resource cost per guidelines completed amount to .93 per completion by judge and $.41 per completion by clerk. For 1979, completions per month averaged 200; therefore, assuming that judges and clerks fill out equal numbers of guidelines, as the data indicate, guideline completions cost an average $134 per month. Using this same average of 200 cases per month, total guidelines completion costs for 1979 amounted to $1,318. Monitoring costs per month averaged as follows:

Time cost, c.p.a.	$1,714.92	(5% time; prior to 8/79 was 33-1/3% time)
Time cost, c.p.a.$_2$	85.09	
Time cost, c.p.a.$_3$ (coding)	170.18	(10% time)
Time cost, Advisory	45.92	(2 Hours)
Time cost, JAC	86.10	(One meeting Every 2 months)
Time cost, total	2,102.21	
Indirect cost	735.77	
Computer cost	21.00	
Total Resource Cost Per Month	$2,858.98	

NOTES

1 The feasibility study of sentencing guidelines by Wilkins, et. al., addresses the first two of these issues. See Leslie T. Wilkins, Jack M. Kress, Don M. Gottfriedson, Joseph C. Calpin and Arthur M. Gelman, Sentencing Guidelines: Structuring Judicial Discretion, Final Report of Feasibility Study, (Albany, N.Y.: Criminal Justice Research Center, 1976). For a description of the same policy issues raised in another judicial reform area see Larry Berks or Susan Carbon, Court Unification: History, Politics and Implementation, (Washington: Government Printing Office, 1978).

2 E. S. Quade, Analysis for Public Decisions, (New York: American Elsevier, 1975).

3 For a discussion of the alternatives see Wilkins, op. cit., 5-12, and David Neubauer America's Courts and the Criminal Justice System, (North Scituate, Mass.: Duxbury Press, 1979), 392-419.

4 J. Morley English, Cost-Effectiveness: The Economic Evaluation of Engineered System (John Wiley: New York, 1968), p. 4.

5 Gary Becker, Human Capital: A Theoretical and Empirical Analysis with Special References to Education, 2nd Edition, (Columbia University Press: New York, 1975), pp. 9-10.

6 Edwin Mansfield, Technological Change, (W. W. Norton: New York, 1971), p. 17ff.

7 J. deV. Graaff, Theoretical Welfare Economics, (Cambridge University Press: London, 1967), p. 18.

8 Quade, op. cit., 103 ff.

APPENDIX B

TABLES

Table B-1
Demonstration of Separate Regression
Models for Individual Judges
Philadelphia

Predictor Variable	A	B	C	D	E	F	AGGREGATE
	0.038	0.037	0.005	0.006	0.032	0.084	0.047
NO. OF OFFENSES	0.144	0.062	0.013	0.011	0.083	0.160	0.112
	0.053	0.360	0.905	0.888	0.354	0.021	0.001
	0.116	0.092	0.179	0.052	0.062	0.436	0.146
CRIME AGAINST PERSON	0.114	0.101	0.184	0.055	0.063	0.430	0.149
	0.159	0.158	0.100	0.493	0.478	0.001	0.001
	0.463	--	--	--	--	--	0.424
INJURY	0.084	--	--	--	--	--	0.033
	0.254	--	--	--	--	--	0.294
PRIOR FAILURES	0.162	0.146	0.195	0.092	0.028	0.012	0.101
TO APPEAR	0.154	0.141	0.176	0.090	0.025	0.011	0.092
	0.071	0.061	0.123	0.334	0.805	0.879	0.009
	0.036	0.042	0.029	0.049	0.069	0.063	0.051
PRIOR RECORD	0.161	0.185	0.133	0.221	0.349	0.290	0.231
	0.068	0.013	0.234	0.015	0.001	0.001	0.001
	0.134	0.164	0.064	0.199	0.122	0.069	0.126
BAIL STATUS	0.389	0.436	0.183	0.554	0.328	0.167	0.338
	0.001	0.001	0.086	0.001	0.001	0.024	0.001
	0.051	0.141	0.188	0.234	0.051	0.096	0.117
RACE	0.047	0.133	0.175	0.220	0.050	0.085	0.107
	0.526	0.051	0.111	0.006	0.586	0.194	0.001
R^2	0.409	0.396	0.171	0.501	0.349	0.486	0.337
NUMBER OF CASES	124	146	84	93	96	137	676
F	11.47	15.19	2.65	14.39	7.93	20.51	48.39
PROBABILITY	0.001	0.001	0.022	0.001	0.001	0.001	0.001

Table C-1
Compliance on the Decision Whether to Incarcerate
According to Computed Guideline Sentences
Controlling for Date (in Quarters'
Denver

Quarter	Percent Compliance	(n)	Three-Quarter Moving Average
1	70.0	(50)	--
2	76.0	(75)	72.7
3	71.7	(106)	70.1
4	66.5	(167)	70.1
5	73.3	(131)	68.6
6	66.0	(103)	71.7
7*	74.0	(169)	70.2
8	68.8	(138)	72.0
9	72.6	(157)	70.9
10	71.2	(139)	70.7
11	68.7	(179)	70.4
12	71.4	(171)	70.6
13	71.9	(153)	71.7
14	71.7	(138)	71.4
15	70.5	(132)	70.4
16	68.0	(75)	--

*Sentencing guidelines implemented during first month of Quarter 7 (November 1976).

Table C-2
Compliance on the Decision Whether to Incarcerate
By Time in Quarters
Philadelphia

Quarter	Percent Compliance	(n)	Percent Lenient Deviations	(n)	Percent Severe Deviations	(n)
1	70.5	(44)	16.7	(18)	38.5	(26)
2	55.3	(72)	45.5	(33)	25.6	(39)
3	67.0	(88)	41.3	(46)	23.8	(42)
4	67.9	(84)	29.7	(37)	34.0	(47)
5	72.6	(95)	34.1	(41)	22.2	(54)
6	61.5	(96)	46.2	(52)	29.5	(44)
7	66.2	(74)	28.9	(38)	38.9	(36)
8	76.6	(64)	37.0	(27)	13.5	(37)
9	64.6	(48)	36.8	(19)	34.5	(29)
10	61.1	(54)	35.7	(28)	42.3	(26)
11	69.0	(42)	35.3	(17)	28.0	(25)
12	61.3	(80)	54.8	(31)	28.6	(49)
13	68.4	(79)	28.6	(35)	34.1	(44)
14*	72.9	(236)	34.9	(106)	20.8	(130)
15	74.6	(193)	25.3	(95)	25.5	(98)

*Sentencing guidelines implemented at the beginning of Quarter 14.

Table C-3
Compliance on Minimum Length of Incarceration
By Time in Quarters
Philadelphia

Quarter	Percent Compliance	Percent Lenient Deviation	Percent Severe Deviations	(n)
1	6.7	80.0	13.3	(15)
2	22.2	66.7	11.1	(18)
3	38.5	50.0	11.5	(26)
4	24.0	64.0	12.0	(25)
5	16.7	70.8	12.5	(24)
6	25.9	63.0	11.1	(27)
7	34.6	57.7	7.7	(26)
8	13.3	73.3	13.3	(15)
9	16.7	50.0	33.3	(12)
10	17.7	52.9	29.4	(17)
11	45.5	36.4	18.2	(11)
12	46.7	46.7	6.7	(15)
13	48.0	28.0	24.0	(25)
14*	28.8	56.1	15.2	(66)
15	45.3	46.9	7.8	(64)
TOTAL	31.1	55.2	13.7	(386)

*Sentencing guidelines implemented

Table C-4
Path Analysis of the Effects of
Race and Sex on the Decision
Whether to Incarcerate
Denver

BEFORE

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Education	Race	-0.144	-0.647	0.158	4.109	0.001
Education	Sex	-0.004	-0.031	0.274	0.114	0.909
Prior Record	Race	0.148	0.499	0.112	4.433	0.001
Prior Record	Sex	0.119	0.695	0.193	3.595	0.001
Prior Record	Education	-0.287	-0.215	0.025	8.590	0.001
Liberty Status	Race	0.018	0.018	0.034	0.525	0.600
Liberty Status	Sex	-0.068	-0.119	0.059	2.022	0.044
Liberty Status	Education	0.051	0.011	0.008	1.449	0.148
Liberty Status	Prior Record	0.342	0.102	0.011	9.557	0.001
Bail Status	Race	-0.069	-0.062	0.032	1.982	0.048
Bail Status	Sex	0.087	0.137	0.054	2.539	0.011
Bail Status	Education	-0.083	-0.017	0.007	2.317	0.021
Bail Status	Prior Record	0.153	0.041	0.010	4.007	0.001
Bail Status	Liberty Status	0.164	0.149	0.032	4.588	0.001
Statutory Class	Race	0.026	0.091	0.121	0.750	0.453
Statutory Class	Sex	-0.011	-0.068	0.208	0.325	0.745
Statutory Class	Education	0.012	0.009	0.028	0.330	0.742
Statutory Class	Prior Record	0.062	0.063	0.040	1.572	0.116
Statutory Class	Liberty Status	0.016	0.054	0.126	0.426	0.670
Statutory Class	Bail Status	0.229	0.863	0.136	6.363	0.001
Employment	Race	-0.067	-0.069	0.035	1.969	0.049
Employment	Sex	0.089	0.161	0.061	2.649	0.008
Employment	Education	0.068	0.016	0.008	1.944	0.052
Employment	Prior Record	-0.055	-0.017	0.012	1.448	0.148
Employment	Liberty Status	-0.029	-0.030	0.037	0.830	0.407
Employment	Bail Status	-0.287	-0.328	0.041	8.085	0.001
Employment	Statutory Class	-0.056	-0.017	0.010	1.626	0.104

Table C-4 (Continued)

BEFORE

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Sentence	Race	0.052	0.053	0.031	1.702	0.089
Sentence	Sex	0.079	0.141	0.053	2.629	0.009
Sentence	Education	-0.051	-0.012	0.007	1.621	0.106
Sentence	Prior Record	0.237	0.072	0.010	6.992	0.001
Sentence	Liberty Status	0.183	0.186	0.032	5.771	0.001
Sentence	Bail Status	0.146	0.164	0.037	4.426	0.001
Sentence	Statutory Class	0.192	0.057	0.009	6.265	0.001
Sentence	Employment	-0.102	-0.101	0.031	3.226	0.001
		AFTER				
Education	Race	-0.164	-0.740	0.108	6.857	0.001
Education	Sex	0.019	0.145	0.179	0.811	0.418
Prior Record	Race	0.101	0.357	0.082	4.356	0.001
Prior Record	Sex	0.154	0.904	0.134	6.725	0.001
Prior Record	Education	-0.271	-0.213	0.018	11.673	0.001
Liberty Status	Race	0.048	0.049	0.024	2.054	0.040
Liberty Status	Sex	-0.001	-0.002	0.039	0.051	0.959
Liberty Status	Education	0.006	0.001	0.005	0.228	0.820
Liberty Status	Prior Record	0.320	0.092	0.007	13.127	0.001
Bail Status	Race	-0.057	-0.052	0.021	2.424	0.016
Bail Status	Sex	0.106	0.160	0.035	4.546	0.001
Bail Status	Education	-0.074	-0.015	0.005	3.062	0.002
Bail Status	Prior Record	0.146	0.038	0.007	5.719	0.001
Bail Status	Liberty Status	0.198	0.178	0.022	8.133	0.001
Statutory Class	Race	-0.001	-0.003	0.080	0.035	0.972
Statutory Class	Sex	0.047	0.256	0.132	1.942	0.052
Statutory Class	Education	0.010	0.008	0.018	0.412	0.680
Statutory Class	Prior Record	-0.018	-0.017	0.025	0.676	0.499
Statutory Class	Liberty Status	0.016	0.053	0.083	0.641	0.522
Statutory Class	Bail Status	0.240	0.874	0.091	9.638	0.001

Table C-4 (Continued)

AFTER

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Employment	Race	-0.015	-0.015	0.023	0.653	0.514
Employment	Sex	0.073	0.126	0.039	3.261	0.001
Employment	Education	0.071	0.016	0.005	3.058	0.002
Employment	Prior Record	-0.115	-0.034	0.007	4.662	0.001
Employment	Liberty Status	-0.095	-0.098	0.024	4.019	0.001
Employment	Bail Status	-0.335	-0.383	0.027	14.059	0.001
Employment	Statutory Class	-0.011	-0.004	0.007	0.507	0.613
Sentence	Race	0.035	0.035	0.021	1.678	0.094
Sentence	Sex	0.101	0.170	0.035	4.854	0.001
Sentence	Education	-0.037	-0.008	0.005	1.699	0.090
Sentence	Prior Record	0.268	0.077	0.007	11.609	0.001
Sentence	Liberty Status	0.132	0.133	0.022	5.993	0.001
Sentence	Bail Status	0.117	0.130	0.026	4.989	0.001
Sentence	Statutory Class	0.191	0.059	0.007	9.085	0.001
Sentence	Employment	-0.128	-0.124	0.022	5.644	0.001

*Standard error of the unstandardized path coefficient.

Table C-5
Path Analysis of the Effects of
Race and Sex on the Decision
Whether to Incarcerate
Standardized Coefficients
Denver

BEFORE

Variable	Direct Effects	Indirect Effects	Total Effects
Employment Status	-0.102	--	-0.102
Statutory Class	0.192	0.006	0.198
Bail Status	0.146	0.074	0.220
Liberty Status	0.183	0.042	0.225
Prior Record	0.237	0.129	0.366
Education	-0.051	-0.117	-0.168
Sex	0.079	0.038	0.117
Race	0.052	0.079	0.131

AFTER

Employment Status	-0.128	--	-0.128
Statutory Class	0.191	0.001	0.192
Bail Status	0.117	0.089	0.206
Liberty Status	0.132	0.056	0.188
Prior Record	0.268	0.102	0.370
Education	-0.037	-0.121	-0.158
Sex	0.101	0.082	0.183
Race	0.035	0.062	0.097

CONTINUED

3 OF 5

Table C-6
Path Analysis of the Effects of Race and Sex on Sentence Length
Denver

BEFORE

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Education	Race	-0.094	-0.410	0.193	2.118	0.035
Education	Sex	-0.018	-0.150	0.366	0.411	0.681
Prior Record	Race	0.125	0.431	0.148	2.909	0.003
Prior Record	Sex	0.078	0.506	0.279	1.812	0.071
Prior Record	Education	-0.247	-0.196	0.034	5.748	0.001
Liberty Status	Race	0.063	0.068	0.047	1.445	0.149
Liberty Status	Sex	-0.138	-0.282	0.088	3.206	0.001
Liberty Status	Education	0.067	0.017	0.011	1.508	0.132
Liberty Status	Prior Record	0.262	0.082	0.014	5.829	0.001
Harm	Race	0.005	0.009	0.080	0.107	0.915
Harm	Sex	0.089	0.301	0.150	2.005	0.046
Harm	Education	-0.029	-0.012	0.019	0.638	0.524
Harm	Prior Record	-0.100	-0.052	0.026	2.119	0.035
Harm	Liberty Status	-0.158	-0.262	0.076	3.463	0.001
Bail Status	Race	-0.078	-0.082	0.046	1.782	0.075
Bail Status	Sex	0.070	0.138	0.087	1.591	0.112
Bail Status	Education	-0.076	-0.018	0.011	1.698	0.090
Bail Status	Prior Record	0.123	0.037	0.014	2.639	0.009
Bail Status	Liberty Status	0.148	0.143	0.044	3.253	0.001
Bail Status	Harm	0.184	0.108	0.026	4.165	0.001
Statutory Class	Race	0.002	0.006	0.145	0.040	0.968
Statutory Class	Sex	-0.010	-0.061	0.275	0.221	0.825
Statutory Class	Education	0.073	0.055	0.034	1.614	0.107

Table C-6 (continued)

BEFORE

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Statutory Class	Prior Record	-0.007	-0.006	0.045	0.140	0.889
Statutory Class	Liberty Status	0.012	0.037	0.141	0.261	0.795
Statutory Class	Harm	0.138	0.253	0.083	3.052	0.002
Statutory Class	Bail Status	0.200	0.626	0.142	4.416	0.001
Sentence Length	Race	-0.038	-2.584	2.890	0.894	0.372
Sentence Length	Sex	0.015	1.912	5.479	0.349	0.727
Sentence Length	Education	-0.034	-0.526	0.683	0.770	0.442
Sentence Length	Prior Record	-0.024	-0.467	0.900	0.519	0.604
Sentence Length	Liberty Status	0.000	0.026	2.803	0.009	0.993
Sentence Length	Harm	0.166	6.270	1.668	3.758	0.000
Sentence Length	Bail Status	0.075	4.818	2.881	1.673	0.095
Sentence Length	Statutory Class	0.256	5.276	0.896	5.891	0.000

AFTER

Education	Race	-0.089	-0.384	0.129	2.970	0.003
Education	Sex	0.023	0.202	0.261	0.772	0.440
Prior Record	Race	0.056	0.194	0.101	1.915	0.056
Prior Record	Sex	0.104	0.728	0.204	3.573	0.001
Prior Record	Education	-0.232	-0.187	0.024	7.938	0.001
Liberty Status	Race	0.052	0.055	0.031	1.754	0.080
Liberty Status	Sex	-0.049	-0.107	0.064	1.676	0.094
Liberty Status	Education	-0.022	0.005	0.008	0.722	0.470
Liberty Status	Prior Record	0.255	0.079	0.009	8.424	0.001
Harm	Race	0.105	0.206	0.059	3.510	0.001

Table C-6 (continued)

AFTER

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Harm	Sex	0.068	0.270	0.118	2.283	0.023
Harm	Education	-0.068	-0.031	0.014	2.217	0.027
Harm	Prior Record	-0.144	-0.082	0.018	4.545	0.001
Harm	Liberty Status	-0.072	-0.133	0.056	2.364	0.018
Bail Status	Race	-0.110	-0.114	0.030	3.797	0.001
Bail Status	Sex	0.089	0.187	0.060	3.092	0.002
Bail Status	Education	-0.070	-0.017	0.007	2.376	0.018
Bail Status	Prior Record	0.089	0.027	0.009	2.872	0.004
Bail Status	Liberty Status	0.190	0.184	0.029	6.415	0.001
Bail Status	Harm	0.215	0.113	0.015	7.363	0.001
Statutory Class	Race	-0.034	-0.109	0.095	1.142	0.254
Statutory Class	Sex	0.074	0.485	0.192	2.524	0.012
Statutory Class	Education	0.006	0.005	0.023	0.210	0.834
Statutory Class	Prior Record	-0.099	-0.093	0.029	3.171	0.002
Statutory Class	Liberty Status	0.025	0.076	0.092	0.827	0.409
Statutory Class	Harm	0.151	0.249	0.050	4.982	0.001
Statutory Class	Bail Status	0.195	0.611	0.096	6.370	0.001
Sentence Length	Race	0.019	1.342	2.006	0.669	0.504
Sentence Length	Sex	-0.041	-6.073	4.045	1.501	0.134
Sentence Length	Education	0.051	0.863	0.475	1.815	0.070
Sentence Length	Prior Record	0.141	2.962	0.620	4.778	0.001
Sentence Length	Liberty Status	0.003	0.236	1.941	0.121	0.903
Sentence Length	Harm	0.193	7.127	1.060	6.723	0.001
Sentence Length	Bail Status	0.064	4.512	2.051	2.200	0.028
Sentence Length	Statutory Class	0.337	7.537	0.636	11.849	0.001

*Standard error of the unstandardized path coefficient

Table C-7
Path Analysis of the Effects of
Race and Sex on Sentence Length
Standardized Path Coefficients
Denver

BEFORE

Variable	Direct Effects	Indirect Effects	Total Effects
Statutory Class	0.256	--	0.256
Bail Status	0.075	0.051	0.126
Harm/Jeopardy	0.166	0.058	0.224
Liberty Status	0.000	-0.013	-0.013
Prior Record	-0.024	-0.012	-0.036
Education	-0.034	0.011	-0.023
Sex	0.015	0.025	0.040
Race	-0.038	-0.012	-0.050

AFTER

Statutory Class	0.337	--	0.337
Bail Status	0.064	0.066	0.130
Harm/Jeopardy	0.193	0.079	0.272
Liberty Status	0.003	0.013	0.016
Prior Record	0.141	-0.056	0.085
Education	0.051	-0.045	0.006
Sex	-0.041	0.063	0.022
Race	0.019	0.009	0.028

Table C-8
Path Analysis of the Effects of
Race and Sex on the Decision Whether to Incarcerate
Philadelphia

Before

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Highest Grade	Race	0.104	0.460	0.143	3.218	0.001
Highest Grade	Sex	-0.025	-0.182	0.236	0.771	0.441
Prior Record	Race	0.079	0.404	0.166	2.432	0.015
Prior Record	Sex	0.154	1.307	0.273	4.793	0.001
Prior Record	Highest Grade	-0.041	-0.048	0.038	1.267	0.205
Failures to Appear	Race	-0.010	-0.011	0.031	0.349	0.727
Failures to Appear	Sex	-0.008	-0.013	0.051	0.264	0.792
Failures to Appear	Highest Grade	-0.042	-0.010	0.007	1.405	0.160
Failures to to Appear	Prior Record	0.407	0.081	0.006	13.459	0.001
Liberty Status	Race	-0.070	-0.159	0.062	2.580	0.010
Liberty Status	Sex	0.049	0.183	0.102	1.791	0.074
Liberty Status	Highest Grade	0.009	0.004	0.014	0.319	0.750
Liberty Status	Prior Record	0.506	0.223	0.013	16.879	0.001
Liberty Status	Failures to Appear	0.093	0.205	0.065	3.139	0.002
Employment Status	Race	0.042	0.045	0.035	1.288	0.198
Employment Status	Sex	-0.104	-0.183	0.058	3.184	0.002
Employment Status	Highest Grade	-0.055	-0.013	0.008	1.693	0.091
Employment Status	Prior Record	0.110	0.023	0.007	3.367	0.001
Crime Against Person	Race	0.145	0.166	0.037	4.518	0.001
Crime Against Person	Sex	-0.086	-0.162	0.061	2.659	0.008

Table C-8 (continued)

Before

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Crime Against Person	Highest Grade	-0.100	-0.026	0.008	3.106	0.002
Crime Against Person	Prior Record	-0.110	-0.024	0.007	3.383	0.001
Crime Against Person	Employment Status	-0.058	-0.062	0.034	1.795	0.073
Death	Race	0.044	0.021	0.015	1.348	0.178
Death	Sex	-0.027	-0.021	0.025	0.834	0.405
Death	Highest Grade	-0.081	-0.009	0.003	2.468	0.014
Death	Prior Record	-0.081	-0.007	0.003	2.447	0.015
Death	Employment Status	-0.031	-0.014	0.015	0.948	0.344
Bail Status	Race	0.090	0.278	0.093	3.007	0.003
Bail Status	Sex	0.112	0.569	0.153	3.725	0.001
Bail Status	Highest Grade	-0.052	-0.037	0.021	1.750	0.081
Bail Status	Prior Record	0.207	0.124	0.020	6.344	0.001
Bail Status	Failures to Appear	0.217	0.649	0.098	6.609	0.001
Bail Status	Employment Status	0.081	0.233	0.086	2.690	0.007
Bail Status	Crime Against Person	0.174	0.468	0.083	5.629	0.001
Bail Status	Death	0.072	0.474	0.199	2.381	0.017
Jury Trial	Race	0.025	0.010	0.013	0.778	0.437
Jury Trial	Sex	0.048	0.032	0.021	1.494	0.135
Jury Trial	Highest Grade	0.003	0.000	0.003	0.099	0.921
Jury Trial	Prior Record	0.152	0.012	0.003	4.285	0.001
Jury Trial	Failures to Appear	-0.109	-0.043	0.014	3.064	0.002

Table C-8 (continued)

Before

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Jury Trial	Employment Status	-0.017	-0.006	0.012	0.530	0.596
Jury Trial	Crime Against Person	0.084	0.029	0.012	2.511	0.012
Jury Trial	Death	0.215	0.185	0.028	6.704	0.001
Jury Trial	Bail Status	0.026	0.003	0.012	0.530	0.596
No. of Offenses	Race	-0.059	-0.177	0.098	1.813	0.070
No. of Offenses	Sex	-0.024	-0.117	0.162	0.723	0.470
No. of Offenses	Highest Grade	0.012	0.008	0.022	0.374	0.708
No. of Offenses	Prior Record	-0.009	-0.005	0.021	0.240	0.810
No. of Offenses	Failures to Appear	-0.017	-0.050	0.106	0.468	0.640
No. of Offenses	Employment Status	-0.006	-0.016	0.091	0.173	0.863
No. of Offenses	Crime Against Person	0.229	0.602	0.089	6.760	0.001
No. of Offenses	Death	-0.063	-0.407	0.214	1.899	0.058
No. of Offenses	Bail Status	0.045	0.044	0.034	1.275	0.203
No. of Offenses	Jury Trial	0.071	0.532	0.248	2.141	0.033
Sentence	Race	0.062	0.070	0.031	2.276	0.023
Sentence	Sex	0.106	0.200	0.051	3.924	0.001
Sentence	Highest Grade	-0.020	-0.005	0.007	0.755	0.451
Sentence	Prior Record	0.196	0.043	0.007	5.812	0.001
Sentence	Failures to Appear	0.042	0.047	0.033	1.410	0.159
Sentence	Liberty Status	0.042	0.021	0.016	1.325	0.186
Sentence	Employment Status	0.018	0.019	0.029	0.663	0.508
Sentence	Crime Against Person	0.180	0.179	0.029	6.250	0.001

Table C-8 (continued)

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Before						
Sentence	Death	0.116	0.283	0.068	4.198	0.001
Sentence	Bail Status	0.318	0.118	0.011	10.848	0.001
Sentence	Jury Trial	0.048	0.135	0.078	1.727	0.084
Sentence	No. of Offenses	0.158	0.060	0.010	5.843	0.001
After						
Highest Grade	Race	-0.032	-0.148	0.214	0.694	0.488
Highest Grade	Sex	-0.100	-0.839	0.390	2.151	0.032
Prior Record	Race	0.045	0.221	0.231	0.954	0.340
Prior Record	Sex	0.046	0.417	0.424	0.984	0.326
Prior Record	Highest Grade	-0.011	-0.012	0.051	0.236	0.814
Failures to Appear	Race	0.015	0.016	0.046	0.348	0.728
Failures to Appear	Sex	0.043	0.084	0.084	0.992	0.322
Failures to Appear	Highest Grade	0.002	0.000	0.010	0.037	0.970
Failures to Appear	Prior Record	0.417	0.091	0.009	9.755	0.001
Liberty Status	Race	-0.038	-0.088	0.091	0.965	0.335
Liberty Status	Sex	0.038	0.164	0.168	0.977	0.329
Liberty Status	Highest Grade	-0.027	-0.014	0.020	0.697	0.486
Liberty Status	Prior Record	0.554	0.263	0.020	12.861	0.001
Liberty Status	Failures to Appear	0.004	0.010	0.094	0.103	0.918
Employment Status	Race	0.133	0.148	0.052	2.866	0.004
Employment Status	Sex	-0.046	-0.094	0.095	0.988	0.324

Table C-8 (continued)

Criterion	Predictor	After: Standardized Path Coefficient	After: Unstandardized Path Coefficient	After: Standard* Error	t	Probability
Employment Status	Highest Grade	-0.076	-0.018	0.011	1.628	0.104
Employment Status	Prior Record	0.043	0.010	0.010	0.931	0.352
Crime Against Person	Race	0.193	0.230	0.056	4.133	0.001
Crime Against Person	Sex	0.000	0.000	0.101	0.002	0.998
Crime Against Person	Highest Grade	-0.012	-0.003	0.012	0.261	0.794
Crime Against Person	Prior Record	-0.020	-0.023	0.011	2.021	0.044
Crime Against Person	Failures to Appear	-0.020	-0.022	0.050	0.429	0.668
Death	Race	0.072	0.028	0.018	1.519	0.130
Death	Sex	-0.026	-0.018	0.033	0.556	0.578
Death	Highest Grade	-0.041	-0.003	0.004	0.864	0.388
Death	Prior Record	-0.065	-0.005	0.004	1.396	0.164
Death	Employment Status	-0.109	-0.038	0.016	2.308	0.022
Bail Status	Race	0.087	0.277	0.142	1.943	0.053
Bail Status	Sex	0.058	0.333	0.254	1.312	0.190
Bail Status	Highest Grade	-0.053	-0.037	0.030	1.204	0.229
Bail Status	Prior Record	0.225	0.145	0.031	4.675	0.001
Bail Status	Failures to Appear	0.117	0.346	0.143	2.429	0.016
Bail Status	Employment Status	0.139	0.397	0.127	3.131	0.002
Bail Status	Crime Against Person	0.166	0.441	0.120	3.679	0.001
Bail Status	Death	0.029	0.239	0.365	0.655	0.513
Jury Trial	Race	-0.029	-0.010	0.017	0.619	0.536

Table C-8 (continued)

After

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Jury Trial	Sex	0.035	0.022	0.030	0.750	0.454
Jury Trial	Highest Grade	0.055	0.004	0.004	1.189	0.235
Jury Trial	Prior Record	0.105	0.007	0.004	2.016	0.044
Jury Trial	Failures to Appear	-0.085	-0.028	0.017	1.655	0.099
Jury Trial	Employment Status	-0.028	-0.009	0.015	0.589	0.557
Jury Trial	Crime Against Person	0.064	0.019	0.014	1.332	0.184
Jury Trial	Death	0.157	0.144	0.043	3.360	0.001
Jury Trial	Bail Status	0.129	0.014	0.006	2.580	0.010
No. of Offenses	Race	-0.050	-0.176	0.163	1.079	0.281
No. of Offenses	Sex	-0.085	-0.548	0.290	1.886	0.060
No. of Offenses	Highest Grade	-0.033	-0.026	0.035	0.737	0.462
No. of Offenses	Prior Record	-0.052	-0.037	0.036	1.019	0.309
No. of Offenses	Failures to Appear	0.049	0.161	0.164	0.982	0.327
No. of Offenses	Employment Status	0.097	0.307	0.146	2.096	0.037
No. of Offenses	Crime Against Person	0.271	0.801	0.139	5.758	0.001
No. of Offenses	Death	-0.048	-0.437	0.422	1.037	0.301
No. of Offenses	Bail Status	0.085	0.095	0.054	1.741	0.082
No. of Offenses	Jury Trial	0.089	0.887	0.462	1.920	0.056
Sentence	Race	0.098	0.118	0.046	2.561	0.011
Sentence	Sex	0.049	0.108	0.082	1.314	0.189
Sentence	Highest Grade	-0.065	-0.017	0.010	1.756	0.080
Sentence	Prior Record	0.166	0.040	0.012	3.415	0.001
Sentence	Failures to Appear	0.091	0.102	0.046	2.203	0.028

Table C-8 (continued)

After

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Sentence	Liberty Status	0.091	0.047	0.023	2.027	0.043
Sentence	Employment Status	-0.020	-0.022	0.041	0.520	0.603
Sentence	Crime Against Person	0.077	0.078	0.041	1.919	0.056
Sentence	Death	0.100	0.312	0.119	2.628	0.009
Sentence	Bail Status	0.385	0.146	0.015	9.461	0.001
Sentence	Jury Trial	0.021	0.072	0.131	0.550	0.583
Sentence	No. of Offenses	0.147	0.050	0.013	3.751	0.001

*Standard error of the unstandardized path coefficient.

Table C-9
Path Analysis of the Effects of
Race and Sex on the Decision Whether to Incarcerate
Standardized Path Coefficients
Philadelphia

BEFORE

Variable	Direct Effects	Indirect Effects	Total Effects
No. of Offenses	0.158	--	0.158
Jury Trial	0.048	0.011	0.059
Bail Status	0.318	0.009	0.327
Death	0.116	0.027	0.143
Crime Against Person	0.180	0.098	0.278
Employment Status	0.018	0.008	0.026
Liberty Status	0.042	--	0.042
Failure to Appear	0.042	0.066	0.108
Prior Record	0.196	0.101	0.297
Highest Grade	-0.020	-0.073	-0.093
Sex	0.106	0.054	0.160
Race	0.062	0.078	0.140

AFTER

No. of Offenses	0.147	--	0.147
Jury Trial	0.021	0.013	0.034
Bail Status	0.385	0.016	0.401
Death	0.100	0.010	0.110
Crime Against Person	0.077	0.109	0.186
Employment Status	-0.020	0.053	0.033
Liberty Status	0.091	--	0.091
Failure to Appear	0.091	0.051	0.142
Prior Record	0.166	0.172	0.338
Highest Grade	-0.065	-0.040	-0.105
Sex	0.049	0.043	0.092
Race	0.098	0.092	0.190

Table C-10
Path Analysis of the Effects of
Race and Sex on Length of Incarceration
Philadelphia

Before

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Highest Grade	Race	0.127	0.622	0.236	2.641	0.009
Highest Grade	Sex	0.032	0.344	0.519	0.663	0.508
Prior Record	Race	0.022	0.140	0.304	0.459	0.646
Prior Record	Sex	0.064	0.880	0.664	1.325	0.186
Prior Record	Highest Grade	0.008	0.010	0.062	0.164	0.870
Employment Status	Race	0.092	0.106	0.056	1.887	0.060
Employment Status	Sex	-0.061	-0.156	0.123	1.268	0.206
Employment Status	Highest Grade	-0.013	-0.003	0.011	0.257	0.798
Employment Status	Prior Record	0.040	0.007	0.009	0.823	0.411
Crime Against Person	Race	0.069	0.084	0.057	1.461	0.145
Crime Against Person	Sex	0.021	0.056	0.125	0.450	0.653
Crime Against Person	Highest Grade	-0.006	-0.001	0.012	0.124	0.902
Crime Against Person	Prior Record	-0.272	-0.053	0.009	5.804	0.001
Injury	Race	0.021	0.026	0.059	0.435	0.664
Injury	Sex	-0.012	-0.033	0.128	0.259	0.796
Injury	Highest Grade	-0.037	-0.009	0.012	0.787	0.432
Injury	Prior Record	-0.221	-0.044	0.009	4.695	0.001
Injury	Employment Status	-0.151	-0.163	0.051	3.211	0.001
Death	Race	0.000	0.000	0.033	0.010	0.992
Death	Sex	-0.086	-0.129	0.072	1.793	0.074
Death	Highest Grade	-0.045	0.006	0.007	0.929	0.353
Death	Prior Record	-0.173	-0.019	0.005	3.615	0.001

Table C-10 (continued)

Before

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Death	Employment Status	-0.068	-0.040	0.028	1.417	0.157
Jury Trial	Race	0.076	0.047	0.029	1.645	0.101
Jury Trial	Sex	0.061	0.083	0.063	1.317	0.189
Jury Trial	Highest Grade	0.017	0.002	0.006	0.378	0.706
Jury Trial	Prior Record	0.136	0.014	0.005	2.842	0.005
Jury Trial	Employment Status	-0.054	-0.029	0.025	1.152	0.250
Jury Trial	Crime Against Person	0.061	0.031	0.027	1.144	0.254
Jury Trial	Injury	0.135	0.068	0.028	2.448	0.015
Jury Trial	Death	0.252	0.230	0.045	5.091	0.001
Statutory Class	Race	0.054	0.158	0.137	1.152	0.250
Statutory Class	Sex	0.003	0.017	0.301	0.058	0.954
Statutory Class	Highest Grade	0.019	0.012	0.028	0.412	0.681
Statutory Class	Prior Record	-0.030	-0.014	0.023	0.611	0.541
Statutory Class	Crime Against Person	-0.345	-0.829	0.130	6.372	0.001
Statutory Class	Injury	0.187	0.441	0.133	3.309	0.001
Statutory Class	Death	-0.070	-0.301	0.223	1.350	0.178
Statutory Class	Jury Trial	0.000	0.000	0.234	0.001	0.999
Sentence	Race	-0.016	-3.920	10.673	0.367	0.714
Sentence	Sex	0.034	18.173	23.256	0.781	0.435
Sentence	Highest Grade	0.049	2.457	2.158	1.139	0.256
Sentence	Prior Record	0.004	0.164	1.789	0.092	0.927
Sentence	Employment Status	-0.009	-1.873	9.277	0.202	0.840
Sentence	Person	-0.029	-5.811	10.527	0.552	0.581
Sentence	Injury	0.018	3.605	10.451	0.345	0.730
Sentence	Death	0.305	108.984	17.236	6.323	0.001

Table C-10 (continued)

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Before						
Sentence	Jury Trial	0.260	101.635	18.076	5.623	0.001
Sentence	Statutory Class	-0.133	-11.130	3.773	2.950	0.003
After						
Highest Grade	Race	0.097	0.538	0.382	1.408	0.161
Highest Grade	Sex	0.050	0.535	0.735	0.727	0.468
Prior Record	Race	-0.068	-0.466	0.474	0.983	0.327
Prior Record	Sex	0.086	1.128	0.908	1.242	0.216
Prior Record	Highest Grade	0.079	0.097	0.085	1.140	0.256
Employment Status	Race	0.055	0.072	0.090	0.799	0.425
Employment Status	Sex	-0.065	-0.162	0.172	0.943	0.347
Employment Status	Highest Grade	-0.096	-0.022	0.016	1.385	0.168
Employment Status	Prior Record	0.141	0.027	0.013	2.041	0.043
Crime Against Person	Race	0.150	0.207	0.094	2.208	0.028
Crime Against Person	Sex	-0.014	-0.037	0.180	0.203	0.839
Crime Against Person	Highest Grade	-0.048	-0.012	0.017	0.701	0.484
Crime Against Person	Prior Record	-0.173	-0.035	0.014	2.548	0.012
Injury	Race	0.153	0.217	0.096	2.268	0.024
Injury	Sex	-0.114	-0.312	0.184	1.697	0.091
Injury	Highest Grade	-0.043	-0.011	0.017	0.629	0.530
Injury	Prior Record	-0.206	-0.043	0.014	3.033	0.003
Injury	Employment Status	0.005	0.005	0.074	0.069	0.945
Death	Race	0.036	0.022	0.042	0.535	0.594

Table C-10 (continued)

After

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Death	Sex	-0.088	-0.105	0.081	1.295	0.197
Death	Highest Grade	-0.034	-0.004	0.008	0.495	0.621
Death	Prior Record	-0.090	-0.008	0.006	1.306	0.193
Death	Employment Status	-0.210	-0.100	0.033	3.075	0.002
Jury Trial	Race	0.001	0.001	0.041	0.013	0.990
Jury Trial	Sex	0.043	0.048	0.078	0.619	0.537
Jury Trial	Highest Grade	-0.025	-0.003	0.007	0.361	0.719
Jury Trial	Prior Record	0.165	0.014	0.006	2.338	0.020
Jury Trial	Employment Status	-0.127	-0.057	0.032	1.799	0.074
Jury Trial	Crime Against Person	0.069	0.030	0.034	0.876	0.382
Jury Trial	Injury	0.086	0.036	0.034	1.048	0.296
Jury Trial	Death	0.136	0.129	0.069	1.885	0.061
Statutory Class	Race	0.110	0.344	0.201	1.718	0.087
Statutory Class	Sex	-0.071	-0.424	0.383	1.109	0.269
Statutory Class	Highest Grade	-0.079	-0.045	0.036	1.250	0.213
Statutory Class	Prior Record	-0.006	-0.003	0.030	0.085	0.932
Statutory Class	Crime Against Person	-0.498	-1.131	0.167	6.782	0.001
Statutory Class	Injury	0.190	0.417	0.168	2.486	0.014
Statutory Class	Death	-0.033	-0.169	0.335	0.505	0.614
Statutory Class	Jury Trial	-0.056	-0.298	0.344	0.866	0.388
Sentence	Race	0.034	11.444	18.747	0.611	0.542
Sentence	Sex	0.046	30.285	35.678	0.849	0.397
Sentence	Highest Grade	0.028	1.697	3.332	0.509	0.611
Sentence	Prior Record	-0.059	-2.942	2.805	1.049	0.296
Sentence	Employment Status	0.054	14.112	14.745	0.957	0.340

Table C-10 (continued)

After

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Sentence	Crime Against Person	-0.058	-14.193	17.119	0.829	0.408
Sentence	Injury	0.013	3.004	15.799	0.190	0.849
Sentence	Death	0.498	273.270	31.727	8.613	0.001
Sentence	Jury Trial	0.338	195.330	32.169	6.072	0.001
Sentence	Statutory Class	0.098	10.632	6.543	1.625	0.106

*Standard error of unstandardized path coefficient.

Table C-11
Path Analysis of the Effects of
Race and Sex on Length of Incarceration
Standardized Path Coefficients
Philadelphia

BEFORE

Variable	Direct Effects	Indirect Effects	Total Effects
Statutory Class	-0.133	--	-0.133
Jury Trial	0.260	0.000	0.260
Death	0.305	0.075	0.380
Injury	0.018	0.010	0.028
Crime Against Person	-0.029	0.062	0.033
Employment Status	0.009	-0.044	-0.053
Prior Record	0.004	-0.037	-0.033
Highest Grade	0.049	-0.016	0.033
Sex	0.034	-0.014	0.020
Race	-0.016	0.014	-0.002

AFTER

Statutory Class	-0.098	--	-0.098
Jury Trial	0.338	0.006	0.344
Death	0.498	0.050	0.548
Injury	0.013	0.011	0.024
Crime Against Person	-0.058	0.073	0.015
Employment Status	0.054	-0.159	-0.105
Prior Record	-0.059	-0.014	-0.073
Highest Grade	-0.028	-0.018	-0.010
Sex	0.046	-0.027	0.019
Race	0.034	0.015	0.049

Table C-12
Unadjusted Quarterly Incarceration Rates
Denver

Quarter	Incarceration Rate	(n)	Three-Quarter Moving Average
1	65.0	(100)	--
2	63.1	(160)	62.5
3	61.0	(218)	64.0
4	66.7	(300)	64.8
5	65.9	(232)	65.1
6	61.8	(191)	66.9
7*	70.9	(295)	66.8
8	65.7	(254)	67.9
9	66.5	(260)	66.0
10	65.8	(231)	66.0
11	65.8	(281)	66.9
12	57.0	(258)	62.1
13	63.1	(244)	60.3
14	61.1	(211)	61.1
15	58.8	(216)	58.3
16	52.5	(120)	--

*Sentencing guidelines implemented.

Table C-13
Regression of Type of Sentence
on Best Predictors
Preguidelines Cases
Denver

Variable	b	Beta	Standard Error of b	F	Probability
Statutory Class	0.473	0.159	0.009	25.195	0.001
Prior Adult Sentence	0.767	0.177	0.017	21.146	0.001
Juvenile Record	0.399	0.104	0.013	9.583	0.002
Revocations	0.613	0.094	0.023	7.066	0.008
Liberty Status	0.177	0.173	0.034	27.857	0.001
Job Status	-0.821	-0.084	0.033	6.247	0.013
Age	-0.545	-0.088	0.002	6.213	0.013
Jeopardy	0.800	0.133	0.019	17.626	0.001
Retained Counsel	-0.111	-0.094	0.039	8.144	0.004
Private Appointed Counsel	0.772	0.045	0.052	2.190	0.139
Sex	0.124	0.073	0.053	5.503	0.019
Bail Status	0.138	0.123	0.039	12.509	0.001
Intercept	0.728				

n = 760
R^2 = 0.324
Adjusted R^2 = 0.313
F = 29.839
Probability = 0.0001

Table C-14
Regression of Type of Sentence
on Best Predictors
Postguidelines Cases
Denver

Variable	b	Beta	Standard Error of b	F	Probability
Statutory Class	0.542	0.177	0.007	67.008	0.001
Prior Adult Sentence	0.798	0.194	0.011	52.391	0.001
Juvenile Record	0.370	0.100	0.008	19.203	0.001
Revocations	0.495	0.082	0.015	11.140	0.001
Liberty Status	0.116	0.116	0.023	25.271	0.001
Job Status	-0.112	-0.115	0.023	23.940	0.001
Age	0.158	-0.028	0.001	1.374	0.242
Jeopardy	0.496	0.089	0.012	17.263	0.001
Retained Counsel	-0.939	-0.085	0.025	13.576	0.001
Sex	0.165	0.098	0.035	21.851	0.001
Bail Status	0.117	0.106	0.027	18.828	0.001
Appointed Counsel	-0.655	-0.004	0.039	0.028	0.867
Intercept	-0.569				

n = 1630
R^2 = 0.313
Adjusted R^2 = 0.308
F = 61.290
Probability = 0.0001

Table C-15
Regression of Type of Sentence
on Best Predictors
Pre- and Postguidelines Cases
Denver

Variable	b	Beta	Standard Error of b	F	Probability
Statutory Class	0.522	0.172	0.005	93.277	0.001
Prior Adult Sentence	0.789	0.190	0.009	74.284	0.001
Juvenile Record	0.378	0.101	0.007	28.708	0.001
Revocations	0.520	0.084	0.012	17.440	0.001
Liberty Status	0.134	0.133	0.019	49.838	0.001
Job Status	-0.102	-0.104	0.019	29.797	0.001
Age	-0.258	-0.045	0.001	5.080	0.024
Jeopardy	0.570	0.100	0.010	31.804	0.001
Retained Counsel	-0.984	-0.087	0.021	21.593	0.001
Appointed Counsel	0.234	0.013	0.031	0.562	0.454
Sex	0.149	0.089	0.029	26.065	0.001
Bail Status	0.124	0.112	0.022	31.351	0.001
Intercept	-0.184				

n = 2390
R^2 = 0.313
Adjusted R^2 = 0.309
F = 90.146
Probability = 0.0001

Table C-16
Unadjusted Mean Quarterly Sentence Lengths
Denver

Quarter	Mean Sentence Length (Months)	(n)	Three-Quarter Moving Average
1	11.7	(58)	---
2	11.7	(97)	13.4
3	15.3	(134)	17.8
4	22.4	(201)	20.7
5	23.2	(160)	20.6
6	14.0	(119)	18.6
7*	17.8	(207)	17.6
8	20.0	(171)	20.5
9	24.3	(170)	23.1
10	25.1	(153)	25.3
11	26.3	(182)	24.0
12	20.2	(153)	21.4
13	17.1	(162)	19.6
14	21.9	(138)	19.5
15	19.8	(134)	20.0
16	16.4	(66)	--

*Sentencing guidelines implemented.

Table C-17
Regression of Length of Incarceration
on Best Predictors
Preguidelines Cases
Denver

Variable	b	Beta	Standard Error of b	F	Probability
Statutory Class	5.438	0.273	1.368	15.802	0.001
Charge Reduction	2.139	0.095	1.515	1.993	0.159
Jury Trial	17.296	0.164	4.352	15.792	0.001
Number Counts Convicted of	8.948	0.152	2.362	14.354	0.001
Age	0.208	0.052	0.170	1.503	0.221
Prior Adult Sentence	-1.809	-0.068	1.162	2.422	0.120
Liberty Status	1.385	0.023	2.385	0.337	0.562
Jeopardy	5.143	0.139	1.523	11.401	0.001
Bail Status	5.165	0.083	2.432	4.509	0.034
Intercept	-31.710				

n = 595
R^2 = 0.205
Adjusted R^2 = 0.192
F = 16.709
Probability = 0.0001

Table C-18
Regression of Length of Incarceration
on Best Predictors
Postguidelines Cases
Denver

Variable	b	Beta	Standard Error of b	F	Probability
Statutory Class	10.471	0.470	1.013	106.838	0.001
Charge Reduction	5.800	0.222	1.160	24.997	0.001
Jury Trial	15.192	0.140	2.984	25.929	0.001
Number Counts Convicted of	2.956	0.047	1.658	3.180	0.075
Age	-0.166	-0.004	0.112	0.022	0.882
Prior Adult Sentence	2.828	0.097	0.822	11.829	0.001
Liberty Status	1.633	0.024	1.836	0.791	0.374
Jeopardy	4.608	0.125	1.038	19.715	0.001
Bail Status	4.058	0.058	1.870	4.711	0.030
Intercept	-59.615				

n = 1267
R^2 = 0.222
Adjusted R^2 = 0.216
F = 39.779
Probability = 0.0001

Table C-19
Regression of Length of Incarceration
on Best Predictors
Pre- and Postguidelines Cases
Denver

Variable	b	Beta	Standard Error of b	F	Probability
Statutory Class	8.875	0.412	0.818	117.758	0.001
Charge Reduction	4.615	0.184	0.926	24.818	0.001
Jury Trial	16.049	0.149	2.467	42.318	0.001
Number Counts Convicted of	4.470	0.072	1.359	10.825	0.001
Age	0.463	0.012	0.093	0.247	0.619
Prior Adult Sentence	1.481	0.052	0.671	4.866	0.028
Liberty Status	1.681	0.026	1.463	1.320	0.251
Jeopardy	4.895	0.133	0.856	32.678	0.001
Bail Status	4.303	0.064	1.492	8.321	0.004
Intercept	-50.403				

n = 1862
R^2 = 0.208
Adjusted R^2 = 0.204
F = 53.984
Probability = 0.0001

Table C-20
Unadjusted Quarterly Incarceration Rates
Philadelphia

Quarter	Incarceration Rate	(n)	Three-Quarter Moving Average
1	54.7	(53)	—
2	41.1	(95)	44.8
3	43.4	(113)	43.7
4	46.1	(115)	45.0
5	45.5	(121)	46.0
6	46.6	(131)	48.6
7	54.9	(102)	46.8
8	38.5	(96)	47.0
9	47.0	(66)	46.6
10	57.0	(72)	51.2
11	49.3	(67)	47.7
12	40.4	(104)	46.9
13	52.0	(100)	46.1
14*	46.2	(364)	48.9
15	51.4	(296)	

*Sentencing guidelines implemented.

Table C-21
Type of Sentence Regressed on
Best Predictors
Preguidelines Cases
Philadelphia

Variable	b	Beta	Standard Error of b	F	Probability
Number of Offenses	0.057	0.150	0.010	33.639	0.001
Crime Against Person	0.170	0.170	0.027	38.776	0.001
Death	0.279	0.117	0.063	19.658	0.001
Jury Trial	0.180	0.064	0.074	5.952	0.015
Failure to Appear	0.056	0.050	0.032	3.034	0.082
Sex	0.186	0.098	0.049	14.610	0.001
Prior Record	0.051	0.232	0.006	64.320	0.001
Bail Status	0.115	0.316	0.010	131.852	0.001
Intercept	-0.228				

n = 1054
R^2 = 0.342
Adjusted R^2 = 0.338
F = 67.96
Probability = 0.0001

Table C-22
Type of Sentence Regressed on
Best Predictors
Postguidelines Cases
Philadelphia

Variable	b	Beta	Standard Error of b	F	Probability
Number of Offenses	0.047	0.137	0.012	14.217	0.001
Crime Against Person	0.114	0.113	0.037	9.395	0.002
Death	0.287	0.091	0.112	6.641	0.010
Jury Trial	0.009	0.003	0.116	0.005	0.941
Failure to Appear	0.116	0.106	0.042	7.636	0.006
Sex	0.167	0.073	0.079	4.470	0.035
Prior Record	0.050	0.212	0.009	29.318	0.001
Bail Status	0.144	0.384	0.014	105.563	0.001
Intercept	-0.212				

n = 541
R^2 = 0.376
Adjusted R^2 = 0.368
F = 40.10
Probability = 0.0001

Table C-23
Type of Sentence Regressed on
Best Predictors
Pre- and Postguidelines Cases
Philadelphia

Variable	b	Beta	Standard Error of b	F	Probability
No. of Convictions	0.054	0.147	0.008	48.852	0.001
Crime Against Person	0.151	0.151	0.022	47.507	0.001
Death	0.283	0.110	0.055	26.975	0.001
Jury Trial	0.138	0.047	0.062	4.929	0.027
Failure to Appear	0.076	0.068	0.025	8.889	0.003
Sex	0.180	0.089	0.041	18.937	0.001
Prior Record	0.051	0.225	0.005	94.001	0.001
Bail Status	0.124	0.337	0.008	231.630	0.001
Intercept	-0.221				

n = 1595
R2 = 0.350
Adjusted R^2 = 0.347
F = 106.83
Probability = 0.001

Table C-24
Unadjusted Mean Quarterly
Sentence Lengths
Philadelphia

Quarter	Incarceration Rate	(n)	Three-Quarter Moving Average
1	43.3	(28)	--
2	7.9	(36)	19.2
3	13.4	(46)	10.9
4	10.7	(52)	10.9
5	8.8	(52)	10.2
6	11.0	(59)	17.4
7	32.5	(55)	19.0
8	10.9	(33)	29.5
9	44.5	(30)	32.2
10	40.8	(39)	33.6
11	11.0	(27)	19.7
12	11.5	(43)	27.3
13	51.6	(53)	29.7
14*	28.2	(162)	26.8
15	16.3	(132)	

*Sentencing guidelines implemented.

Table C-25
Length of Incarceration Regressed on
Best Predictors
Preguidelines Cases
Philadelphia

Variable	b	Beta	Standard Error of b	F	Probability
Crime Against Person	-4.553	-0.023	8.940	0.259	0.611
Injury	4.100	0.021	8.885	0.213	0.645
Death	106.291	0.293	15.071	49.742	0.001
Jury Trial	112.511	0.284	15.706	51.316	0.001
Statutory Class	-10.657	-0.129	3.299	10.432	0.001
Intercept	36.621				

n = 541
R^2 = 0.242
Adjusted R^2 = 0.236
F = 34.08
Probability = 0.0001

Table C-26
Length of Incarceration Regressed on
Best Predictors
Postguidelines Cases
Philadelphia

Variable	b	Beta	Standard Error of b	F	Probability
Crime Against Person	-7.375	-0.035	13.604	.294	0.588
Injury	2.811	0.013	12.808	.048	0.826
Death	186.230	0.399	25.342	54.003	0.001
Jury Trial	122.415	0.253	25.374	23.275	0.001
Statutory Class	-8.918	-0.099	5.088	3.072	0.081
Intercept	34.807				

n = 285
R^2 = 0.293
Adjusted R^2 = 0.283
F = 23.10
Probability = 0.0001

Table C-27
Length of Incarceration Regressed on
Best Predictors
Pre- and Postguidelines Cases
Philadelphia

Variable	b	Beta	Standard Error of b	F	Probability
Crime Against Person	-5.501	-0.027	7.483	0.541	0.462
Injury	3.496	0.017	7.316	0.228	0.633
Death	127.187	0.324	12.988	95.897	0.001
Jury Trial	115.019	0.272	13.404	73.630	0.001
Statutory Class	-10.088	-0.118	2.772	13.245	0.001
Intercept	36.143				

n = 826
R^2 = 0.252
Adjusted R^2 = 0.248
F = 55.14
Probability = 0.0001

Table D-1
Path Analysis of the Effects of Charge Reductions on the Decision Whether to Incarcerate
Denver

BEFORE

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard Error*	t	Probability
Liberty Status	Prior record	0.315	0.093	0.009	9.823	0.001
Bail Status	Prior record	0.182	0.049	0.009	5.381	0.001
Bail Status	Liberty status	0.142	0.130	0.031	4.182	0.001
Bail Status	Statutory class	0.174	0.086	0.016	5.425	0.001
Charge Reduction	Prior record	-0.078	-0.068	0.031	2.200	0.028
Charge Reduction	Liberty status	-0.042	-1.125	0.104	1.198	0.231
Charge Reduction	Statutory class	0.080	0.127	0.053	2.370	0.018
Charge Reduction	Bail status	-0.153	-0.489	0.112	4.366	0.001
Sentence	Prior record	0.265	0.078	0.009	8.572	0.001
Sentence	Liberty status	0.182	0.182	0.031	5.912	0.001
Sentence	Statutory class	0.128	0.069	0.016	4.336	0.001
Sentence	Bail status	0.187	0.204	0.033	6.090	0.001
Sentence	Charge reduction	-0.151	-0.052	0.010	5.151	0.001

AFTER

Liberty Status	Prior record	0.334	0.096	0.006	15.330	0.001
Bail Status	Prior record	0.185	0.048	0.006	8.046	0.001
Bail Status	Liberty status	0.194	0.177	0.021	8.462	0.001
Bail Status	Statutory class	0.172	0.081	0.010	7.936	0.001
Charge Reduction	Prior record	-0.025	-0.020	0.020	1.005	0.315
Charge Reduction	Liberty status	-0.032	-0.093	0.070	1.326	0.185
Charge Reduction	Statutory class	0.138	0.205	0.034	6.011	0.001
Charge Reduction	Bail status	-0.171	-0.538	0.076	7.070	0.001
Sentence	Prior record	0.317	0.090	0.006	14.944	0.001
Sentence	Liberty status	0.136	0.135	0.021	6.398	0.001
Sentence	Statutory class	0.150	0.077	0.010	7.463	0.001
Sentence	Bail status	0.169	0.185	0.023	7.943	0.001
Sentence	Charge reduction	-0.143	-0.050	0.007	7.102	0.001

*Standard error of the unstandardized path coefficient

Table D-2
Path Analysis of the Effects of Charge Reductions on the Decision Whether to Incarcerate
Standardized Coefficients
Denver

Variable	Direct effects	Indirect effects	Total effects
BEFORE			
Charge Reduction	-0.151	0	-0.151
Bail Status	0.187	0.023	0.210
Statutory Class	0.128	0.025	0.153
Liberty Status	0.182	0.036	0.218
Prior Record	0.265	0.119	0.384
AFTER			
Charge Reduction	-0.143	0	-0.143
Bail Status	0.169	0.024	0.193
Statutory Class	0.150	0.013	0.163
Liberty Status	0.136	0.042	0.178
Prior Record	0.317	0.099	0.416

Table D-3

Path Analysis of Effects of Charge Reductions on Length of Incarceration

Denver

BEFORE

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard Error*	t	Probability
Liberty Status	Prior record	0.230	0.071	0.006	11.216	0.001
Statutory Class	Harm	0.372	0.410	0.044	9.284	0.001
Bail Status	Prior record	0.161	0.049	0.013	3.780	0.001
Bail Status	Liberty status	0.108	0.105	0.042	2.483	0.013
Bail Status	Harm	0.120	0.070	0.027	2.632	0.009
Bail Status	Statutory class	0.151	0.079	0.024	3.356	0.001
Charge Reduction	Prior record	0.004	0.003	0.037	0.090	0.929
Charge Reduction	Liberty status	0.002	0.005	0.120	0.040	0.968
Charge Reduction	Harm	0.058	0.092	0.075	1.223	0.222
Charge Reduction	Statutory class	0.116	0.168	0.068	2.485	0.013
Charge Reduction	Bail status	-0.154	-0.423	0.122	3.464	0.001
Sentence Length	Prior record	-0.001	-0.020	0.835	0.025	0.981
Sentence Length	Liberty status	0.007	0.464	2.706	0.172	0.864
Sentence Length	Harm	0.163	6.262	1.702	3.678	0.001
Sentence Length	Statutory class	0.224	7.825	1.533	5.104	0.001
Sentence Length	Bail status	0.079	5.209	2.785	1.870	0.062
Sentence Length	Charge reduction	-0.213	-5.129	0.977	5.250	0.001

AFTER

Liberty Status	Prior record	0.261	0.081	0.009	9.278	0.001
Statutory Class	Harm	0.423	0.430	0.027	16.023	0.001
Bail Status	Prior record	0.130	0.039	0.009	4.523	0.001
Bail Status	Liberty status	0.182	0.176	0.028	6.340	0.001
Bail Status	Harm	0.148	0.079	0.016	4.827	0.001
Bail Status	Statutory class	0.139	0.073	0.016	4.564	0.001

Table D-3 Cont'd.

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard Error*	t	Probability
		AFTER				
Charge Reduction	Prior record	0.056	0.045	0.024	1.859	0.063
Charge Reduction	Liberty status	-0.031	-0.081	0.078	1.032	0.302
Charge Reduction	Harm	0.056	0.079	0.045	1.747	0.081
Charge Reduction	Statutory class	0.095	0.131	0.044	2.964	0.003
Charge Reduction	Bail status	-0.153	-0.407	0.080	5.059	0.001
Sentence Length	Prior record	0.139	2.954	0.578	5.113	0.001
Sentence Length	Liberty status	0.006	0.432	1.868	0.232	0.817
Sentence Length	Harm	0.151	5.656	1.086	5.209	0.001
Sentence Length	Statutory class	0.306	11.248	1.063	10.582	0.001
Sentence Length	Bail status	0.053	3.718	1.944	1.913	0.056
Sentence Length	Charge reduction	-0.227	-6.012	0.697	8.623	0.001

*Standard error of the unstandardized path coefficient

Table D-4
Path Analysis of the Effects of
Charge Reduction on
Length of Incarceration
Standardized Coefficients
Denver

Variable	Direct effects	Indirect effects	Total effects
BEFORE			
Charge Reduction	-0.213	0	-0.213
Bail Status	0.079	0.033	0.112
Statutory Class	0.224	-0.008	0.216
Harm/Jeopardy	0.163	0.081	0.244
Liberty Status	0.007	0.012	0.019
Prior Record	-0.001	0.022	0.021
AFTER			
Charge Reduction	-0.227	0	-0.227
Bail Status	0.053	0.035	0.088
Statutory Class	0.306	-0.010	0.296
Harm/Jeopardy	0.151	0.151	0.302
Liberty Status	0.006	0.023	0.029
Prior Record	0.139	0.006	0.145

Table D-5
Path Analysis of the Effects of Method of Conviction
on the Decision Whether to Incarcerate
Standardized Path Coefficients
Philadelphia

Variable	BEFORE Direct Effects	BEFORE Indirect Effects	BEFORE Total Effects	AFTER Direct Effects	AFTER Indirect Effects	AFTER Total Effects
Liberty Status	0.047	0	0.047	0.084	0	0.084
Number of Offenses	0.158	0	0.158	0.141	0	0.141
Statutory Class	-0.020	-0.027	-0.047	-0.048	-0.030	-0.078
Jury Trial	0.043	0.016	0.059	0.017	0.015	0.032
Bench Trial	-0.006	0.025	0.019	0.023	0.009	0.032
Bail Status	0.325	0.010	0.335	0.394	0.018	0.412
Death	0.116	0.024	0.140	0.100	0.012	0.112
Crime Against Person	0.171	0.098	0.269	0.061	0.123	0.184
Employment Status	0.021	0.017	0.038	-0.024	0.053	0.029
Prior Record	0.207	0.087	0.294	0.194	0.133	0.327
Highest Grade	-0.019	-0.071	-0.090	-0.064	-0.042	-0.106
Sex	0.103	0.057	0.160	0.044	0.047	0.091
Race	0.062	0.081	0.143	0.103	0.093	0.196

Table D-6
Path Analysis of the Effects of Method of Conviction
on the Decision Whether to Incarcerate
Philadelphia

BEFORE

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Highest Grade	Race	0.103	0.455	0.143	3.194	0.001
Highest Grade	Sex	-0.025	-0.186	0.235	0.789	0.430
Prior Record	Race	0.076	0.393	0.166	2.368	0.018
Prior Record	Sex	0.153	1.298	0.272	4.771	0.001
Prior Record	Highest Grade	-0.038	-0.045	0.038	1.188	0.235
Employment Status	Race	0.042	0.045	0.035	1.281	0.200
Employment Status	Sex	-0.104	-0.183	0.058	3.185	0.002
Employment Status	Highest Grade	-0.052	-0.013	0.008	1.611	0.108
Employment Status	Prior Record	0.110	0.023	0.007	3.392	0.001
Crime Against Person	Race	0.148	0.170	0.037	4.634	0.001
Crime Against Person	Sex	-0.084	-0.158	0.061	2.598	0.010
Crime Against Person	Highest Grade	-0.100	-0.026	0.008	3.142	0.002
Crime Against Person	Prior Record	-0.112	-0.025	0.007	3.469	0.001
Crime Against Person	Employment Status	-0.057	-0.061	0.034	1.777	0.076
Death	Race	0.046	0.022	0.015	1.412	0.158
Death	Sex	-0.027	-0.021	0.026	0.819	0.413
Death	Highest Grade	-0.081	-0.009	0.003	2.491	0.013
Death	Prior Record	-0.079	-0.007	0.003	2.405	0.016
Death	Employment Status	-0.038	-0.017	0.014	1.161	0.246
Bail Status	Race	0.092	0.286	0.095	3.018	0.003
Bail Status	Sex	0.112	0.571	0.156	3.659	0.001
Bail Status	Highest Grade	-0.062	-0.044	0.021	2.043	0.041
Bail Status	Prior Record	0.287	0.172	0.019	9.313	0.001
Bail Status	Employment Status	0.109	0.313	0.088	3.577	0.001
Bail Status	Crime Against Person	0.151	0.408	0.084	4.847	0.001
Bail Status	Death	0.062	0.407	0.201	2.025	0.043
Bench Trial	Race	0.063	0.068	0.036	1.901	0.058
Bench Trial	Sex	0.006	0.011	0.059	0.186	0.853
Bench Trial	Highest Grade	-0.009	-0.002	0.008	0.265	0.791
Bench Trial	Prior Record	0.066	0.014	0.007	1.917	0.056

Table D-6 (Continued)

BEFORE

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Bench Trial	Employment Status	-0.003	-0.003	0.033	-0.086	0.931
Bench Trial	Crime Against Person	0.059	0.056	0.032	1.742	0.082
Bench Trial	Death	0.044	0.101	0.076	1.335	0.182
Bench Trial	Bail Status	-0.123	-0.043	0.012	3.497	0.001
Jury Trial	Race	0.028	0.011	0.013	0.881	0.379
Jury Trial	Sex	0.052	0.035	0.021	1.615	0.107
Jury Trial	Highest Grade	0.007	0.001	0.003	0.213	0.832
Jury Trial	Prior Record	0.118	0.009	0.003	3.528	0.001
Jury Trial	Employment Status	-0.032	-0.012	0.012	1.013	0.311
Jury Trial	Crime Against Person	0.098	0.035	0.012	3.002	0.003
Jury Trial	Death	0.240	0.208	0.028	7.544	0.001
Jury Trial	Bail Status	0.001	0.000	0.004	0.024	0.981
Statutory Class	Race	0.021	0.064	0.094	0.674	0.500
Statutory Class	Sex	-0.111	-0.544	0.156	3.495	0.001
Statutory Class	Highest Grade	0.043	0.029	0.021	1.378	0.168
Statutory Class	Prior Record	-0.022	-0.013	0.019	0.664	0.507
Statutory Class	Employment Status	-0.104	-0.287	0.087	3.296	0.001
Statutory Class	Crime Against Person	-0.228	-0.589	0.085	6.954	0.001
Statutory Class	Death	-0.016	-0.102	0.206	0.495	0.621
Statutory Class	Bail Status	-0.112	-0.107	0.032	3.306	0.001
Statutory Class	Bench Trial	0.080	0.218	0.087	2.519	0.012
Statutory Class	Jury Trial	0.021	0.153	0.238	0.644	0.520
Number of Offenses	Race	-0.064	-0.194	0.095	2.029	0.043
Number of Offenses	Sex	-0.043	-0.212	0.159	1.339	0.181
Number of Offenses	Highest Grade	0.021	0.014	0.021	0.661	0.509
Number of Offenses	Prior Record	-0.037	-0.022	0.020	1.103	0.270
Number of Offenses	Employment Status	-0.021	-0.060	0.089	0.679	0.497
Number of Offenses	Crime Against Person	0.181	0.478	0.088	5.434	0.001
Number of Offenses	Death	-0.079	-0.509	0.208	2.443	0.015
Number of Offenses	Bail Status	0.043	0.042	0.033	1.284	0.199

Table D-6 (Continued)

BEFORE

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Number of Offenses	Bench Trial	0.182	0.509	0.088	5.781	0.001
Number of Offenses	Jury Trial	0.107	0.792	0.241	3.279	0.001
Number of Offenses	Statutory Class	-0.172	-0.176	0.033	5.334	0.001
Liberty Status	Race	-0.069	-0.157	0.062	2.531	0.012
Liberty Status	Sex	0.049	0.185	0.103	1.794	0.073
Liberty Status	Highest Grade	0.006	0.003	0.014	0.230	0.818
Liberty Status	Prior Record	0.541	0.239	0.012	19.670	0.001
Sentence	Race	0.062	0.071	0.031	2.298	0.022
Sentence	Sex	0.103	0.194	0.051	3.790	0.001
Sentence	Highest Grade	-0.019	-0.005	0.007	0.733	0.464
Sentence	Prior Record	0.207	0.046	0.007	6.382	0.001
Sentence	Employment Status	0.021	0.022	0.029	0.784	0.433
Sentence	Crime Against Person	0.171	0.170	0.029	5.913	0.001
Sentence	Death	0.116	0.281	0.067	4.176	0.001
Sentence	Bail Status	0.325	0.120	0.011	11.226	0.001
Sentence	Bench Trial	-0.006	-0.006	0.029	0.221	0.825
Sentence	Jury Trial	0.043	0.121	0.078	1.550	0.122
Sentence	Statutory Class	-0.020	-0.008	0.011	0.715	0.475
Sentence	No. of Offenses	0.158	0.060	0.011	5.686	0.001
Sentence	Liberty Status	0.047	0.024	0.016	1.492	0.136

AFTER

Highest Grade	Race	-0.040	-0.183	0.212	0.866	0.387
Highest Grade	Sex	-0.098	-0.828	0.390	2.123	0.034
Prior Record	Race	0.047	0.234	0.229	1.019	0.309
Prior Record	Sex	0.046	0.421	0.423	0.993	0.321
Prior Record	Highest Grade	-0.013	-0.014	0.050	0.281	0.779
Employment Status	Race	0.133	0.147	0.051	2.869	0.004
Employment Status	Sex	-0.046	-0.094	0.095	0.988	0.324
Employment Status	Highest Grade	-0.069	-0.017	0.011	1.485	0.138
Employment Status	Prior Record	0.043	0.010	0.010	0.939	0.348
Crime Against Person	Race	0.200	0.239	0.055	4.335	0.001
Crime Against Person	Sex	-0.001	-0.002	0.101	0.025	0.980

Table D-6 (continued)

AFTER

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Crime Against Person	Highest Grade	-0.015	-0.004	0.012	0.336	0.737
Crime Against Person	Prior Record	-0.097	-0.024	0.011	2.125	0.034
Crime Against Person	Employment Status	-0.027	-0.029	0.050	0.578	0.564
Death	Race	0.071	0.027	0.018	1.518	0.130
Death	Sex	-0.027	-0.019	0.033	0.569	0.570
Death	Highest Grade	-0.040	-0.003	0.004	0.858	0.392
Death	Prior Record	-0.065	-0.005	0.004	1.410	0.159
Death	Employment Status	-0.107	-0.037	0.016	2.284	0.023
Bail Status	Race	0.081	0.257	0.144	1.792	0.074
Bail Status	Sex	0.065	0.384	0.258	1.488	0.138
Bail Status	Highest Grade	-0.046	-0.032	0.031	1.050	0.295
Bail Status	Prior Record	0.268	0.174	0.029	6.094	0.001
Bail Status	Employment Status	0.128	0.367	0.128	2.879	0.004
Bail Status	Crime Against Person	0.160	0.427	0.120	3.551	0.001
Bail Status	Death	0.029	0.241	0.371	0.651	0.515
Bench Trial	Race	0.017	0.021	0.057	0.361	0.718
Bench Trial	Sex	0.002	0.005	0.102	0.049	0.961
Bench Trial	Highest Grade	-0.095	-0.025	0.012	2.058	0.040
Bench Trial	Prior Record	0.115	0.028	0.012	2.397	0.017
Bench Trial	Employment Status	0.045	0.049	0.051	0.955	0.340
Bench Trial	Crime Against Person	0.165	0.166	0.048	3.441	0.001
Bench Trial	Death	0.019	0.060	0.146	0.411	0.681
Bench Trial	Bail Status	-0.144	-0.054	0.019	2.931	0.004
Jury Trial	Race	-0.022	-0.008	0.017	0.473	0.637
Jury Trial	Sex	0.029	0.019	0.031	0.628	0.530
Jury Trial	Highest Grade	0.039	0.003	0.004	0.845	0.398
Jury Trial	Prior Record	0.073	0.005	0.004	1.534	0.126
Jury Trial	Employment Status	-0.052	-0.017	0.015	1.104	0.270
Jury Trial	Crime Against Person	0.081	0.025	0.015	1.702	0.090
Jury Trial	Death	0.141	0.135	0.044	3.040	0.003
Jury Trial	Bail Status	0.131	0.015	0.006	2.670	0.008
Statutory Class	Race	-0.022	-0.066	0.137	0.480	0.631
Statutory Class	Sex	-0.145	-0.807	0.246	3.280	0.001
Statutory Class	Highest Grade	0.021	0.014	0.029	0.468	0.640
Statutory Class	Prior Record	-0.051	-0.031	0.029	1.099	0.272

Table D-6 (continued)

AFTER

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Statutory Class	Employment Status	-0.143	-0.392	0.122	3.191	0.002
Statutory Class	Crime Against Person	-0.279	-0.709	0.118	5.990	0.001
Statutory Class	Death	-0.044	-0.347	0.357	0.972	0.331
Statutory Class	Bail Status	-0.030	-0.029	0.045	0.632	0.528
Statutory Class	Bench Trial	0.149	0.377	0.115	3.287	0,001
Statutory Class	Jury Trial	0.016	0.137	0.379	0.361	0.718
No. of Offenses	Race	-0.051	-0.183	0.159	1.145	0.253
No. of Offenses	Sex	-0.111	-0.731	0.289	2.527	0.012
No. of Offenses	Highest Grade	-0.016	-0.013	0.034	0.378	0.706
No. of Offenses	Prior Record	-0.070	-0.051	0.033	1.540	0.124
No. of Offenses	Employment Status	0.052	0.168	0.144	1.165	0.245
No. of Offenses	Crime Against Person	0.188	0.564	0.143	3.953	0.001
No. of Offenses	Death	-0.064	-0.604	0.415	1.456	0.146
No. of Offenses	Bail Status	0.121	0.136	0.053	2.580	0.010
No. of Offenses	Bench Trial	0.149	0.443	0.135	3.282	0.001
No. of Offenses	Jury Trial	0.112	1.098	0.440	2.496	0.013
No. of Offenses	Statutory Class	-0.209	-0.247	0.055	4.524	0.001
Liberty Status	Race	-0.032	-0.075	0.090	0.831	0.407
Liberty Status	Sex	0.037	0.160	0.167	0.957	0.339
Liberty Status	Highest Grade	-0.031	-0.016	0.020	0.800	0.424
Liberty Status	Prior Record	0.562	0.268	0.018	14.562	0.001
Sentence	Race	0.103	0.123	0.046	2.694	0.007
Sentence	Sex	0.044	0.097	0.084	1.156	0.250
Sentence	Highest Grade	-0.064	-0.017	0.010	1.726	0.085
Sentence	Prior Record	0.194	0.047	0.011	4.216	0.001
Sentence	Employment Status	-0.024	-0.026	0.041	0.627	0.531
Sentence	Crime Against Person	0.061	0.061	0.042	1.465	0.144
Sentence	Death	0.100	0.314	0.119	2.634	0.009
Sentence	Bail Status	0.394	0.148	0.015	9.715	0.001
Sentence	Bench Trial	0.023	0.023	0.039	0.598	0.550
Sentence	Jury Trial	0.017	0.056	0.127	0.442	0.659
Sentence	Statutory Class	-0.048	-0.019	0.016	1.177	0.240
Sentence	No. of Offenses	0.141	0.047	0.014	3.498	0.001
Sentence	Liberty Status	0.084	0.043	0.023	1.871	0.062

*Standard error of unstandardized path coefficient.

Table D-7
Path Analysis of the Effects of Method of Conviction
on Length of Incarceration
Standardized Path Coefficients
Philadelphia

	BEFORE			AFTER		
Variable	Direct Effects	Indirect Effects	Total Effects	Direct Effects	Indirect Effects	Total Effects
Number of convictions	0.086	0	0.086	0.075	0	0.075
Statutory Class	-0.115	-0.016	-0.131	-0.091	-0.011	-0.102
Jury Trial	0.268	0.008	0.276	0.264	0.010	0.274
Bench Trial	-0.042	0.005	-0.037	-0.063	0.000	-0.063
Bail Status	-0.012	0.011	-0.001	0.064	0.051	0.115
Death	0.310	0.069	0.379	0.426	0.054	0.480
Injury	0.022	0.010	0.032	0.027	-0.019	0.008
Crime Against Person	-0.038	0.077	0.039	-0.068	0.093	0.025
Prior Record	0.016	0.083	0.099	-0.048	-0.013	-0.061
Sex	0.034	-0.009	0.025	0.040	-0.023	0.017
Race	0.006	0.015	0.021	0.023	0.028	0.051

Table D-8
Path Analysis of Effects of Method of Conviction
on Length of Incarceration
Philadelphia

BEFORE

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Prior Record	Race	0.030	0.188	0.280	0.280	0.503
Prior Record	Sex	0.035	0.510	0.653	0.781	0.435
Crime Against Person	Race	0.054	0.064	0.051	1.263	0.207
Crime Against Person	Sex	0.035	0.097	0.118	0.818	0.414
Crime Against Person	Prior Record	-0.307	-0.058	0.008	7.232	0.001
Injury	Race	0.035	0.044	0.054	0.814	0.416
Injury	Sex	-0.017	-0.050	0.125	0.402	0.688
Injury	Prior Record	-0.230	-0.045	0.009	5.312	0.001
Death	Race	0.001	0.000	0.030	0.014	0.989
Death	Sex	-0.072	-0.114	0.069	1.649	0.100
Death	Prior Record	-0.189	-0.020	0.005	4.334	0.001
Bail Status	Race	0.090	0.317	0.153	2.068	0.039
Bail Status	Sex	0.126	1.038	0.358	2.900	0.004
Bail Status	Prior Record	0.179	0.100	0.026	3.878	0.001
Bail Status	Crime Against Person	0.040	0.117	0.148	0.786	0.432
Bail Status	Injury	0.059	0.169	0.147	1.148	0.252
Bail Status	Death	-0.007	-0.038	0.243	0.156	0.876
Bench Trial	Race	0.067	0.079	0.051	1.536	0.125
Bench Trial	Sex	0.028	0.077	0.121	0.635	0.526
Bench Trial	Prior Record	0.090	0.017	0.009	1.920	0.055
Bench Trial	Crime Against Person	-0.057	-0.056	0.050	1.127	0.260
Bench Trial	Injury	0.138	0.131	0.049	2.675	0.008
Bench Trial	Death	-0.006	-0.011	0.081	0.137	0.891
Bench Trial	Bail Status	-0.236	-0.079	0.015	5.312	0.001
Jury Trial	Race	0.085	0.053	0.026	2.005	0.046
Jury Trial	Sex	0.070	0.101	0.062	1.637	0.102
Jury Trial	Prior Record	0.125	0.012	0.004	2.763	0.006
Jury Trial	Crime Against Person	0.066	0.034	0.025	1.352	0.177
Jury Trial	Injury	0.120	0.060	0.025	2.396	0.017
Jury Trial	Death	0.252	0.230	0.042	5.533	0.001
Jury Trial	Bail Status	-0.053	-0.009	0.008	1.230	0.219
Statutory Class	Race	0.048	0.137	0.121	1.132	0.258

Table D-8 (continued)

BEFORE

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Statutory Class	Sex	0.013	0.088	0.282	0.312	0.755
Statutory Class	Prior Record	-0.030	-0.014	0.021	0.653	0.514
Statutory Class	Crime Against Person	-0.343	-0.819	0.116	7.049	0.001
Statutory Class	Injury	0.191	0.441	0.117	3.782	0.001
Statutory Class	Death	-0.089	-0.373	0.195	1.912	0.056
Statutory Class	Bail Status	-0.141	-0.114	0.036	3.166	0.002
Statutory Class	Bench Trial	0.072	0.174	0.107	1.620	0.106
Statutory Class	Jury Trial	-0.015	-0.071	0.209	0.338	0.736
Number of Convictions	Race	-0.064	-0.249	0.167	1.490	0.137
Number of Convictions	Sex	-0.018	-0.168	0.390	0.430	0.667
Number of Convictions	Prior Record	-0.068	-0.042	0.029	1.458	0.146
Number of Convictions	Crime Against Person	0.135	0.441	0.168	2.620	0.009
Number of Convictions	Injury	0.082	0.258	0.163	1.581	0.115
Number of Convictions	Death	-0.146	-0.834	0.271	3.083	0.002
Number of Convictions	Bail Status	-0.021	-0.024	0.050	0.472	0.638
Number of Convictions	Bench Trial	0.161	0.530	0.149	3.562	0.001
Number of Convictions	Jury Trial	0.064	0.400	0.289	1.383	0.167
Number of Convictions	Statutory Class	-0.181	-0.248	0.062	4.002	0.001
Sentence	Race	0.006	1.414	9.821	0.144	0.886
Sentence	Sex	0.034	19.559	22.864	0.856	0.393
Sentence	Prior Record	0.016	0.617	1.682	0.367	0.714
Sentence	Crime Against Person	-0.038	-7.875	9.921	0.794	0.428
Sentence	Injury	0.022	4.420	9.593	0.461	0.645
Sentence	Death	0.310	112.810	15.995	7.053	0.001
Sentence	Bail Status	-0.012	-0.855	2.945	0.290	0.772
Sentence	Bench Trial	-0.042	-8.902	8.825	1.009	0.314
Sentence	Jury Trial	0.268	106.615	16.966	6.284	0.001
Sentence	Statutory Class	-0.115	-10.020	3.684	2.720	0.007
Sentence	No. of Convictions	0.086	5.475	2.627	2.084	0.038

Table D-8 (continued)

AFTER

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Prior Record	Race	-0.012	-0.079	0.406	0.194	0.847
Prior Record	Sex	0.085	1.263	0.915	1.381	0.168
Crime Against Person	Race	0.109	0.143	0.080	1.795	0.074
Crime Against Person	Sex	-0.011	-0.032	0.180	0.179	0.858
Crime Against Person	Prior Record	-0.180	-0.036	0.012	2.948	0.004
Injury	Race	0.076	0.101	0.081	1.256	0.210
Injury	Sex	-0.105	-0.315	0.182	1.729	0.085
Injury	Prior Record	-0.194	-0.039	0.012	3.203	0.002
Death	Race	0.053	0.032	0.037	0.859	0.391
Death	Sex	-0.067	-0.091	0.083	1.085	0.279
Death	Prior Record	-0.085	-0.008	0.006	1.366	0.173
Bail Status	Race	0.089	0.336	0.231	1.456	0.147
Bail Status	Sex	0.081	0.689	0.522	1.320	0.188
Bail Status	Prior Record	0.168	0.096	0.036	2.688	0.008
Bail Status	Crime Against Person	0.145	0.419	0.203	2.069	0.040
Bail Status	Injury	-0.050	-0.143	0.206	-0.694	0.489
Bail Status	Death	0.019	0.117	0.402	0.292	0.771
Bench Trial	Race	0.044	0.061	0.085	0.720	0.472
Bench Trial	Sex	-0.048	-0.150	0.192	0.784	0.434
Bench Trial	Prior Record	0.119	0.025	0.013	1.884	0.061
Bench Trial	Crime Against Person	0.052	0.055	0.075	0.734	0.464
Bench Trial	Injury	0.169	0.175	0.075	2.314	0.022
Bench Trial	Death	-0.040	-0.093	0.147	0.635	0.526
Bench Trial	Bail Status	-0.127	-0.046	0.023	2.022	0.044
Jury Trial	Race	0.002	0.001	0.034	0.039	0.969
Jury Trial	Sex	0.035	0.043	0.076	0.570	0.570
Jury Trial	Prior Record	0.090	0.008	0.005	1.431	0.154
Jury Trial	Crime Against Person	0.056	0.024	0.030	0.804	0.422
Jury Trial	Injury	0.051	0.021	0.030	0.706	0.481
Jury Trial	Death	0.200	0.185	0.058	3.179	0.002
Jury Trial	Bail Status	0.131	0.019	0.009	2.122	0.035
Statutory Class	Race	0.055	0.158	0.166	0.955	0.341
Statutory Class	Sex	-0.058	-0.379	0.375	1.010	0.313
Statutory Class	Prior Record	-0.021	-0.009	0.026	0.349	0.727
Statutory Class	Crime Against Person	-0.477	-1.053	0.146	7.197	0.001

Table D-8 (continued)

AFTER

Criterion	Predictor	Standardized Path Coefficient	Unstandardized Path Coefficient	Standard* Error	t	Probability
Statutory Class	Injury	0.209	0.453	0.149	3.034	0.003
Statutory Class	Death	-0.051	-0.247	0.293	0.845	0.399
Statutory Class	Bail Status	-0.002	-0.001	0.045	0.028	0.978
Statutory Class	Bench Trial	0.088	0.185	0.126	1.471	0.143
Statutory Class	Jury Trial	-0.018	-0.096	0.318	0.303	0.763
Number of Convictions	Race	-0.048	-0.234	0.284	0.823	0.412
Number of Convictions	Sex	-0.178	-1.957	0.643	3.045	0.003
Number of Convictions	Prior Record	-0.037	-0.028	0.045	0.609	0.543
Number of Convictions	Crime Against Person	0.197	0.732	0.275	2.666	0.008
Number of Convictions	Injury	0.062	0.227	0.260	0.874	0.383
Number of Convictions	Death	-0.152	-1.237	0.501	2.469	0.014
Number of Convictions	Bail Status	0.086	0.111	0.077	1.435	0.153
Number of Convictions	Bench Trial	0.113	0.400	0.216	1.853	0.065
Number of Convictions	Jury Trial	0.106	0.939	0.545	1.724	0.086
Number of Convictions	Statutory Class	-0.151	-0.254	0.108	2.364	0.019
Sentence	Race	0.023	6.793	15.028	0.452	0.652
Sentence	Sex	0.040	25.793	34.574	0.746	0.456
Sentence	Prior Record	-0.048	-2.095	2.386	0.878	0.381
Sentence	Crime Against Person	-0.068	-15.022	14.719	1.021	0.308
Sentence	Injury	0.027	5.917	13.755	0.430	0.668
Sentence	Death	0.426	206.109	26.797	7.692	0.001
Sentence	Bail Status	0.064	4.924	4.107	1.199	0.232
Sentence	Bench Trial	-0.063	-13.243	11.495	1.152	0.250
Sentence	Jury Trial	0.264	138.295	28.949	4.777	0.001
Sentence	Statutory Class	-0.091	-9.109	5.746	1.585	0.114
Sentence	No. of Convictions	0.075	4.429	3.328	1.331	0.184

*Standard error of unstandardized path coefficient.

APPENDIX C

INTERVIEW RESULTS

There are obvious limitations to what archival court data can tell us about why sentencing guidelines had the limited effects discussed in Chapter 3 and 4. To fully understand the impact of sentencing guidelines, it is useful to consider the attitudes and objectives of the intended consumers of this reform. To this end, the impressions, expectations, and reactions of dozens of significant justice personnel who were variously involved in the creation, implementation, and/or administration of guidelines were systematically collected and analyzed. This appendix summarizes the results of those interviews.

The Interview/Survey Method

The interview/survey component of this project was intended broadly to gauge various impressions and reactions to sentencing guidelines of persons in significant justice system roles in several sites where guidelines had been implemented. Interview subjects were purposively identified according to occupation in the criminal justice system, site, and role in the development and/or use of guidelines in the respective sites. Specifically, the impressions of both advocates and opponents of the reform were solicited. Beyond this number, additional interviews, especially with prosecutors and public defenders, were conducted when opportune.

Interview subjects were identified as follows. Project staff first interviewed key personnel in each site about how guidelines were administered and about how guidelines had come to be introduced in the site. In Chicago, for example, it was learned that judges calculate the guideline sentence only after the sentencing hearing; further, the sheets upon which guidelines are calculated are not made available to defense attorneys. As a result, it was decided that interviews in Chicago focus on the judges and that interviews with defense attorneys could safely be abbreviated. In Newark, we learned that use of countywide guidelines was limited and that, soon after their implementation, the state Supreme Court introduced an independent set of statewide guidelines. Problems of recall and confusion about which guidelines were at issue convinced us that extensive interviewing would not be fruitful.

This strategy ultimately yielded 170 interviews in four sites. Of these, 132 conformed to a standard interview schedule; they were coded and provide the basis of the tabulations which follow. The remaining 38 interviews involved particular respondents for whom the standard interview format was not appropriate (e.g., offenders) or toward whom much more exploratory kinds of questions were directed (e.g., court administrators). The respondents are indicated, by site and role, in Table 1.

Substance of the Interview/Survey

The interview design was constructed to include assessments in several areas: the history of guidelines' development in the jurisdiction, initial reactions of the respondent and others to the

Table 1: Number and Type of Interviews Conducted

	SITE				
Respondent	Chicago	Denver	Philadelphia	Newark	Total
Judges	14	10	22 (2)*	2 (2)	48 (4)
Prosecutors	7 (7)	15 (1)	25	0	47 (8)
Defense Attorneys	8 (8)	21 (1)	29	0	58 (9)
Court Administrators	1 (1)	2 (2)	1 (1)	0	4 (4)
Probation Staff	0	2 (2)	2 (2)	0	4 (4)
Corrections Officers	0	0	1 (1)	0	1 (1)
Offenders	0	0	8 (8)	0	8 (8)
Total	30 (16)	50 (6)	88 (14)	2 (2)	170 (38)

*Number in parentheses indicates the subset of interviews in a cell which were conducted but not coded.

concept, format, and procedures of guidelines, perceptions of the impact of guidelines on several aspects of sentencing (plea bargaining, sentence type and length, and sentence disparity), overall "satisfaction" with the guidelines, philosophical attitude toward sentencing, and information needed for sentencing. The latter two areas were covered in a self-administered questionnaire which was given to each respondent at the conclusion of the structured interview, to be completed and returned by mail. The return rate of the surveys was 78% (103/132).

Reliability

Five similar questions were asked in both the interview and the survey questionnaire. In the interviews, three- or two-choice responses were used for questions, whereas in the questionnaire, 5-point scales were used. A crosstabulation of the responses to the common questions provides an estimate of the reliability of the interview/survey method. Gamma provides a proportionate-reduction-in-error reliability statistic for this type of ordered pairs of observations. The results are presented in Table 55.

The reliability for two variables--guidelines sentence severity and comfort with guidelines--approached .90. Four of the five variables

achieved reasonable reliability, with gammas approaching .70 or above. Only one variable showed questionable reliability: the amount of influence guidelines "should" have. Nonetheless, the comparison of respondents' answers to repeated questions demonstrates that the reliability of the data collection method tended to be high.

Missing Data

Because of the interactive character of an interview, it is frequently true, even in a structured interview, either that questions were not asked of respondents or answers were not given. Sometimes, questions were not asked because of the dynamics of the interview. More frequently the respondent's answer to one question rendered subsequent questions inappropriate. Missing data of both types varies by question, from a low of about 9% to a high of nearly two-thirds. For survey items the minimum missing value rate is 22% for all respondents (the complement of the return rate).

A Caveat

It is important to recall, in perusing the tabulations which follow, that the data were not generated from a random sample of respondents; nor is the distribution of data proportionate across either the role of the respondent or site. Consequently, interpretation of aggregated data must issue with the utmost caution. Bivariate and multivariate statistics based on the assumption of random sampling are, of course, wholly inappropriate, and, therefore, are not computed. Even descriptive statistics, particularly where based on aggregations across either role or site, must be used with care.

Notwithstanding these limitations, however, these data are rich with insights about how significant justice actors view guidelines reform. While the information presented in this appendix does not claim to be strictly representative of the opinions of all those from whom reactions were sought, the information was collected from a significant portion of those persons whose opinions it is meant to describe. These findings, in other words, are the product of detailed and extensive interviews with well over 100 criminal justice officials in key roles as consumers of guidelines reform in a variety of courts across the country. As such, they merit careful attention.

Results

The results of the interview/survey are presented in the following tables. Mindful of our own admonitions, we have refrained from the presentation of descriptive statistics and analyses. For easy reference, a listing of table titles is provided as part of the Table of Contents of this report.

TABLE 2: When did you first become aware of the idea of guidelines for sentencing?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Before the preliminary discussions about guidelines here	2	4	0	6	0	2	0	2	2	4	0	6	4	10	0	14
During preliminary discussions about guidelines	0	8	0	8	0	1	0	1	0	1	0	1	0	10	0	10
While guidelines were being developed	3	3	0	6	0	2	0	2	1	3	0	4	4	8	0	12
While guidelines were being tested	0	1	0	1	0	2	0	2	0	5	0	5	0	8	0	8
When guidelines were first implemented	0	0	0	0	2	12	0	14	3	14	0	17	5	26	0	31
After guidelines were first implemented	3	0	0	3	8	1	0	9	5	0	0	5	16	1	0	17
Not until contacted for this interview	0	0	0	0	1	0	0	1	1	0	0	1	2	0	0	2
Not asked/missing	2	4	14	20	3	5	0	8	8	2	0	10	13	11	14	38
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 3: When did you first become aware of the guidelines project here in [city]?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
During preliminary discussions of guidelines	2	6	0	8	0	0	0	0	0	0	0	0	2	6	0	8
While guidelines were developed here	3	10	0	13	0	3	0	3	1	2	0	1	4	15	0	19
While guidelines were being tested here	0	2	0	2	0	3	0	3	0	8	0	2	0	13	0	13
When guidelines were first implemented	0	0	0	0	2	15	0	17	3	17	0	11	5	32	0	37
After guidelines had been implemented	5	0	0	5	11	1	0	12	9	0	0	26	25	1	0	26
Not until contacted for this interview	0	0	0	0	1	0	0	1	2	0	0	2	3	0	0	3
Not asked/missing	0	2	14	16	0	3	0	3	5	2	0	7	5	7	14	26
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 3: When did you first become aware of the guidelines project here in [city]?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
During preliminary discussions of guidelines	2	6	0	8	0	0	0	0	0	0	0	0	2	6	0	8
While guidelines were developed here	3	10	0	13	0	3	0	3	1	2	0	1	4	15	0	19
While guidelines were being tested here	0	2	0	2	0	3	0	3	0	8	0	2	0	13	0	13
When guidelines were first implemented	0	0	0	0	2	15	0	17	3	17	0	11	5	32	0	37
After guidelines had been implemented	5	0	0	5	11	1	0	12	9	0	0	26	25	1	0	26
Not until contacted for this interview	0	0	0	0	1	0	0	1	2	0	0	2	3	0	0	3
Not asked/missing	0	2	14	16	0	3	0	3	5	2	0	7	5	7	14	26
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 4: How did you become awa. of the actual guidelines project?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Not aware until this interview	0	0	0	0	1	0	0	1	1	0	0	1	2	0	0	2
Involved in preliminary discussions with judges	3	4	0	7	0	0	0	0	0	0	0	0	3	4	0	7
Participated in or observed development	1	8	0	9	0	0	0	0	0	2	0	2	1	10	0	11
Received memorandum requesting input	0	5	0	5	0	1	0	1	0	0	0	3	0	6	0	6
Staff meeting in prosecutor's/public defender's office	0	1	0	1	1	9	0	10	0	3	0	18	1	13	0	14
Formal announcement of implementation	1	0	0	1	0	10	0	10	0	18	0	1	1	28	0	29
Judge referred to them in court	0	0	0	0	7	0	0	7	1	1	0	1	8	1	0	9
Judge referred to them out of court	0	0	0	0	2	0	0	2	0	0	0	0	2	0	0	2
Observed documents in case file	0	0	0	0	0	1	0	1	8	0	0	8	8	1	0	9
When I came to the criminal bench	5	0	0	5	3	0	0	3	0	0	0	0	8	0	0	8
Not asked/missing	0	2	14	16	0	4	0	4	10	5	0	15	10	11	14	35
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	60	14	132

TABLE 5: What arguments were advanced in favor of sentencing guidelines?[1]

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
They will reduce disparity	4	17	10	31	4	17	0	21	9	19	0	28	17	53	10	80
They will aid in judge socialization	2	1	3	6	0	0	0	0	0	1	0	1	2	2	3	7
They will increase information for sentencing	0	3	0	3	0	4	0	4	0	6	0	6	0	13	0	13
They will improve the public understanding of sentencing	0	4	0	4	0	1	0	1	0	1	0	1	0	6	0	6
They will help avoid sentencing reform	1	5	0	6	0	1	0	1	0	3	0	3	1	9	0	10
They will help us to know if there is disparity	0	0	4	4	0	0	0	0	0	0	0	0	0	0	4	4
Number of persons responding[2]	(6)	(18)	(13)	(37)	(4)	(20)	0	(24)	(9)	(24)	0	(33)	(19)	(62)	(13)	(94)

[1]Respondents may give more than one response.
[2]Cell values in this row are respondents; other cell values are responses. Number of respondents may exceed the sum of responses, because "unique" responses (i.e., those given by no more than one person in response either to this or to comparable questions) are not included elsewhere in table.

TABLE 6: Was there any opposition to sentencing guidelines?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	2	11	5	18	1	7	0	8	10	4	0	14	13	22	5	40
Yes	4	7	8	19	1	13	0	14	4	21	0	25	9	41	8	58
Don't know	2	0	0	2	11	3	0	14	3	1	0	4	16	4	0	20
Not asked/missing	2	2	1	5	1	2	0	3	3	3	0	6	6	7	1	14
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 7: Who opposed guidelines?[1]

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Judges	4	5	6	15	0	1	0	1	1	6	0	7	5	12	6	23
Prosecutors	0	0	0	0	1	8	0	9	0	3	0	3	1	11	0	12
Defense Attorneys	0	3	0	3	0	6	0	6	3	18	0	21	3	27	0	30
Don't know	2	1	0	3	6	2	0	8	0	0	0	0	8	3	0	11
Not asked/missing	4	13	8	25	7	11	0	18	16	9	0	25	27	33	8	68
Number of persons responding[2]	6	7	6	19	7	14	0	22	4	20	0	24	17	41	6	64

[1]Respondent may give more than one response.
[2]Cell values in this row indicate respondents; other cell values are responses.
Number of respondents may exceed the sum of responses, because "unique" responses (i.e., those given by no more than one person in response either to this or to comparable questions) are not included elsewhere in table.

TABLE 8: What arguments were advanced in opposition to sentencing guidelines?[1]

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
They would inappropriately limit judicial discretion	3	4	2	9	1	3	0	4	1	7	0	8	5	14	2	21
They would preclude individualized sentencing	1	3	0	4	1	4	0	5	5	16	0	21	7	23	0	30
They would institutionalize past sentencing practice	1	0	0	1	0	5	0	5	0	1	0	1	1	6	0	7
They would create undue pressure on judges to conform	0	0	0	0	0	1	0	1	0	3	0	3	0	4	0	4
They will limit the kinds of information considered by the judge	0	1	0	1	0	1	0	1	0	1	0	1	0	3	0	3
They discriminate against racial minorities or the poor	0	1	0	1	0	0	0	0	0	0	0	0	0	1	0	1
They create too much paperwork	1	0	4	5	0	0	0	0	0	0	0	0	1	0	4	5
They will be ineffective	0	1	0	1	0	4	0	4	0	1	0	1	0	6	0	6
They will be too severe	0	1	0	1	0	1	0	1	1	5	0	6	1	7	0	8
They will be too lenient	0	1	0	1	0	6	0	6	0	0	0	0	0	7	0	7
Number of persons responding[2]	(4)	(11)	(10)	(25)	(2)	(20)	0	(22)	(10)	(25)	0	(35)	(16)	(56)	(10)	(82)

[1]Respondent may give more than one response.
[2]Cell values in this row are respondents; other cell values are responses.
Number of respondents may exceed the sum of responses, because "unique" responses (i.e., those given by no more than one person in response either to this or to comparable questions) are not included elsewhere in table.

TABLE 9: Are guidelines generally supported by your colleagues now?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	0	2	5	7	2	7	0	10	5	17	0	22	7	26	5	38
Yes	7	9	3	19	1	10	0	11	3	5	0	8	11	24	3	38
Neutral	0	1	0	1	3	2	0	5	2	0	0	2	5	3	0	8
Depended on the person	0	0	0	0	0	2	0	2	1	0	0	1	1	2	0	3
Don't know	2	4	1	7	8	2	0	10	7	0	0	7	17	6	1	24
Not asked/missing	1	4	5	10	0	2	0	2	2	7	0	9	3	13	5	21
TOTAL	10	20	14	44	14	25	0	40	20	29	0	49	44	74	14	132

CONTINUED

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TABLE 10: Were you generally in favor of (or opposed to) sentencing guidelines when they were first implemented?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Opposed	2	3	3	8	3	8	0	11	5	24	0	29	10	35	3	48
Neutral	2	4	3	9	4	8	0	12	4	4	0	8	10	16	3	29
Favored	5	11	4	20	3	9	0	12	8	0	0	8	16	20	4	40
Don't know	0	0	0	0	3	0	0	3	0	0	0	0	3	0	0	3
Not asked/missing	1	2	4	7	1	0	0	1	3	1	0	4	5	3	4	12
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 11: Are you generally in favor of (or opposed to) guidelines now?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Opposed	0	3	1	4	2	14	0	16	7	23	0	30	9	40	1	50
Neutral	3	5	3	11	8	4	0	12	2	5	0	7	13	14	3	30
Favor	7	9	5	21	3	7	0	10	8	0	0	8	18	16	5	39
Don't know	0	0	0	0	1	0	0	1	0	0	0	0	1	0	0	1
Not asked/missing	0	3	5	8	0	0	0	0	3	1	0	4	3	4	5	12
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 12: Why do you favor sentencing guidelines?[1]

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
They help reduce disparity	2	1	0	3	3	3	0	6	7	0	0	7	12	4	0	16
They help in judge socialization	1	0	0	1	0	0	0	0	0	0	0	0	1	0	0	1
They provide judges with additional information	0	3	0	3	0	2	0	2	0	1	0	1	0	6	0	6
They help avoid sentencing reform	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Number of persons responding[2]	(5)	(9)	(0)	(14)	(3)	(10)	(0)	(13)	(7)	(1)	(0)	(8)	(15)	(20)	(0)	(35)

[1]Respondents can give more than one response.
[2]Cell values in this row are respondents; other cell values are responses.
Number of respondents may exceed the sum of responses, because "unique" responses (i.e., those given by no more than one person in response either to this or to comparable questions) are not included elsewhere in table.

TABLE 13: Why do you oppose sentencing guidelines?[1]

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
They inappropriately limit discretion	1	0	0	1	1	0	0	1	1	3	0	4	3	3	0	6
They do not allow for needed flexibility	1	2	0	3	2	0	0	2	5	9	0	14	8	11	0	19
They don't work	1	2	0	3	0	5	0	7	0	2	0	2	1	9	0	10
They are too harsh	0	1	0	1	0	0	0	0	1	8	0	9	1	9	0	10
They are too lenient	0	0	0	0	1	5	0	6	1	1	0	2	2	6	0	8
Number of persons responding[2]	(1)	(3)	(0)	(4)	(3)	(8)	(0)	(11)	(5)	(22)	(0)	(27)	(4)	(33)	(0)	(42)

[1]Respondents may give more than one response.
[2]Cell values in this row are respondents; other cell values are responses.
Number of respondents may exceed the sum of responses, because "unique" responses (i.e., those given by no more than one person in response either to this or to comparable questions) are not included elsewhere in table.

TABLE 14: Do you find guidelines relatively easy or difficult to use?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Easy	3	15	0	18	0	19	0	19	0	20	0	20	3	54	0	57
Difficult	0	2	0	2	0	3	0	3	0	2	0	2	0	7	0	7
Don't know	0	0	0	0	1	0	0	1	0	0	0	0	1	0	0	1
Not asked/missing	7	3	14	24	13	3	0	16	10	7	0	17	30	13	14	67
TOTAL	10	20	14	44	14	25	0	39	10	29	0	39	34	74	14	132

TABLE 15: Are errors in the use of guidelines of such magnitude and frequency that they constitute a major problem?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	6	12	0	18	0	14	0	14	8	19	0	27	14	45	0	59
Yes, computational errors	0	1	0	1	0	0	0	0	0	0	0	0	0	1	0	1
Yes, factual errors	0	0	0	0	0	6	0	6	0	6	0	6	0	12	0	12
Yes, both kinds	0	2	0	2	0	1	0	1	0	2	0	2	0	5	0	5
Don't know	1	0	0	1	12	1	0	13	7	0	0	7	20	1	0	21
Not asked/missing	3	5	14	22	2	3	0	5	5	2	0	7	10	10	14	34
TOTAL	10	20	14	44	14	24	0	38	20	29	0	49	44	73	14	131

TABLE 16: Do the guidelines include all the factors that ought to be included?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	3	9	4	16	7	16	0	23	10	27	0	37	20	52	4	76
Yes	6	8	4	18	5	8	0	13	6	1	0	7	17	17	4	38
Not asked/missing	1	3	6	10	2	1	0	3	4	1	0	5	7	5	6	18
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 17: What factors ought to be added to the guidelines?[1]

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
(Generally) offense variable	1	3	1	5	3	5	0	8	2	1	0	3	6	9	1	16
(Generally) offender variable	1	7	3	11	5	11	0	16	7	20	0	27	13	38	3	54
[specific offender variables]																
Juvenile record	1	0	0	1	0	10	0	10	0	0	0	0	1	10	0	11
Time since last conviction	1	0	0	1	1	0	0	1	2	3	0	5	4	3	0	7
Employment	0	1	3	4	0	1	0	1	1	0	0	1	1	2	3	6
Social stability	0	0	2	2	0	0	0	0	3	5	0	8	3	5	2	10
Number of persons responding[2]	(3)	(9)	(3)	(15)	(7)	(16)	(0)	(23)	(13)	(28)	(0)	(41)	(23)	(53)	(3)	(79)

[1]A respondent may give more than one response.
[2]Cell values in this row are respondents; other cell values are responses.

TABLE 18: In your opinion, do the guidelines include any factors that should not be included?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	8	13	6	27	11	8	0	19	9	14	0	23	28	35	6	69
Yes	1	3	1	5	2	16	0	18	6	14	0	20	9	33	1	43
Not asked/missing	1	4	7	12	1	1	0	2	5	1	0	6	7	6	7	20
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 19: What factors should be deleted?[1]

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
(Generally) offense variable	0	0	0	0	1	8	0	9	2	5	0	7	3	13	0	16
(Generally) offender variable	1	3	1	5	1	12	0	13	3	10	0	13	5	25	1	31
[specific offender variables]																
Juvenile convictions	1	0	0	1	1	0	0	1	4	1	0	5	6	1	0	7
Social stability	0	2	0	2	0	10	0	10	0	7	0	7	0	19	0	19
Number of persons responding[2]	(1)	(4)	(1)	(6)	(2)	(15)	(0)	(17)	(7)	(14)	(0)	(21)	(10)	(33)	(1)	(44)

[1] A respondent may give more than one response.
[2] Cell values in this row are respondents; other cell values are responses.

TABLE 20: In your opinion, are the sentences that are specified by the guidelines generally too severe, too lenient, or about right?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Too severe	1	0	0	1	1	2	0	3	7	13	0	20	9	15	0	24
About right	6	9	6	21	6	2	0	8	4	6	0	10	16	17	6	39
Too lenient	1	6	1	8	3	20	0	23	0	3	0	3	4	29	1	34
Depends on the situation	0	0	0	0	0	0	0	0	0	5	0	5	0	5	0	5
Don't know	0	1	0	1	4	1	0	5	7	1	0	8	11	3	0	14
Not asked/missing	2	4	7	13	0	0	0	0	2	1	0	3	4	5	7	16
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 21: Do you think that, as a result of guidelines, convicted defendants are more likely, less likely or equally likely to be incarcerated than they were before guidelines?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
More likely	1	8	1	10	0	7	0	7	4	19	0	23	5	34	1	40
Equally likely	5	7	6	18	5	15	0	20	8	6	0	14	18	28	6	52
Less likely	2	1	0	3	5	1	0	6	1	1	0	2	8	3	0	11
Don't know	2	1	0	3	4	2	0	6	5	2	0	7	11	5	0	16
Not asked/missing	0	3	7	10	0	0	0	0	2	1	0	3	2	4	7	13
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 22: As a result of guidelines, do you think that, on the average, defendants who are incarcerated receive longer terms, shorter terms, or the same terms as they did before guidelines?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Longer	1	6	1	8	0	6	0	6	1	14	0	15	2	26	1	29
Same	2	4	4	10	4	16	0	20	6	11	0	17	12	31	4	47
Shorter	2	1	0	3	7	0	0	7	8	2	0	10	17	3	0	20
Depends on the situation	0	1	0	1	0	1	0	1	0	0	0	0	0	2	0	2
Don't know	5	3	2	10	3	2	0	5	4	1	0	5	12	6	2	20
Not asked/missing	0	5	7	12	0	0	0	0	1	1	0	2	1	6	7	14
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 23: Overall, have sentences become more severe or more lenient as a result of guidelines?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
More severe	1	6	1	8	0	6	0	6	2	17	0	19	3	29	1	33
Same	4	6	4	14	5	15	0	20	6	9	0	15	15	30	4	49
More lenient	2	0	0	2	5	1	0	6	5	1	0	6	12	2	0	14
Depends on the situation	0	0	0	0	0	1	0	1	0	0	0	0	0	1	0	1
Don't know	3	3	1	7	3	2	0	5	5	0	0	5	11	5	1	17
Not asked/missing	0	5	8	13	1	0	0	1	2	2	0	4	3	7	8	18
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 24: How much influence do you think guidelines have on judges, generally, in this court?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No influence	0	1	0	1	0	2	0	2	0	2	0	2	0	5	0	5
Little influence	2	3	1	6	4	12	0	16	11	10	0	21	17	25	1	43
Substantial influence	1	4	0	5	4	5	0	9	4	7	0	11	9	16	0	25
Depends on the judge	1	1	0	2	2	4	0	6	0	8	0	8	3	13	0	16
Don't know	3	6	1	10	2	1	0	3	4	1	0	5	9	8	1	18
Not asked/missing	3	5	2	20	2	1	0	3	1	1	0	2	6	7	12	25
TOTAL	10	20	4	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 25: In your opinion, how much influence should sentencing guidelines have on judges' sentencing decisions?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No influence	1	2	0	3	1	7	0	8	4	11	0	15	6	20	0	26
Little influence	3	6	4	13	8	8	0	16	5	13	0	18	16	27	4	47
Substantial influence	4	8	1	13	2	9	0	11	8	1	0	9	14	18	1	33
Depends on the judge	0	0	0	0	0	0	0	0	0	1	0	1	0	1	0	1
Don't know	0	0	0	0	3	0	0	3	1	2	0	3	4	2	0	6
Not asked/missing	2	4	9	15	0	1	0	1	2	1	0	3	4	6	9	19
TOTAL	10	20	14	42	14	25	0	39	20	29	0	49	44	73	14	131

TABLE 26: Some people have suggested that sentencing guidelines that are derived from data on past cases will perpetuate past sentencing practices, some of which may have been undesirable. Do you think this is a problem?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	7	9	1	17	2	4	0	6	7	6	0	13	16	19	1	36
Yes	2	4	0	6	3	20	0	23	6	18	0	24	11	42	0	53
Don't know	0	1	0	1	3	0	0	3	5	3	0	8	8	4	0	12
Not asked/missing	1	6	13	20	6	1	0	7	2	2	0	4	9	9	13	31
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 27: "Disparity" is used by some people to refer to sentence decisions that are made partly on the basis of race or some other impermissible criterion. Using this definition of disparity, do you think that there was sentence disparity here in [city] before sentencing guidelines?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	5	4	4	13	5	8	0	13	5	1	0	6	15	13	4	32
Yes	4	8	3	15	7	16	0	23	11	27	0	38	22	51	3	76
Don't know	1	3	0	4	2	1	0	3	3	0	0	3	5	4	0	10
Not asked/missing	0	5	7	12	0	0	0	0	1	1	0	2	2	6	7	14
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 28: Do you think that (invidious) disparity has been eliminated, reduced, not affected or increased as a result of guidelines?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Eliminated	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Reduced	1	7	0	8	3	7	0	10	3	5	0	8	7	19	0	26
Not affected	2	5	1	8	4	9	0	13	11	19	0	30	17	33	1	51
Increased	0	0	0	0	1	2	0	3	0	2	0	2	1	4	0	5
Don't know	2	1	0	3	5	0	0	5	3	1	0	4	10	2	0	12
Not asked/missing	5	7	13	25	1	7	0	8	3	2	0	5	9	16	13	38
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 29: "Disparity" is also sometimes used to mean unpredictability or inconsistency in sentences: in other words, like cases treated differently. Do you think that this kind of disparity existed before sentencing guidelines?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	1	2	2	5	1	0	0	1	0	0	0	0	2	2	2	6
Yes	8	13	5	26	11	23	0	34	16	28	0	44	35	64	5	104
Don't know	1	1	0	2	2	0	0	2	3	0	0	3	6	1	0	7
Not asked/missing	0	4	7	11	0	2	0	2	1	1	0	2	1	7	7	15
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 30: Do you think disparity (inconsistency) ought to be reduced?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	1	2	3	6	1	1	0	2	1	9	0	10	3	12	3	18
Yes	8	11	2	21	13	22	0	35	16	17	0	33	37	50	2	89
Don't know/depends	0	1	0	1	0	0	0	0	1	1	0	2	1	2	0	3
Not asked/missing	1	6	9	16	0	2	0	2	2	2	0	4	3	10	9	22
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 31: Do you think that sentencing guidelines have reduced, not affected, or increased unpredictability or apparently unexplainable inconsistency in sentencing?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Reduced	6	10	2	18	6	7	0	13	9	13	0	22	21	30	2	53
Not affected	2	4	1	7	3	13	0	16	4	13	0	17	9	30	1	40
Increased	0	0	0	0	1	2	0	3	1	0	0	1	2	2	0	4
Don't know/depends	1	0	2	3	4	2	0	6	5	2	0	7	10	4	2	16
Not asked/missing	1	6	9	16	0	1	0	1	1	1	0	2	2	8	9	19
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 32: Do you think that, before sentencing guidelines, there was too much, too little, or the right amount of judicial discretion in sentencing?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Too much	3	3	0	6	3	17	0	20	6	5	0	11	12	25	0	37
Right amount	5	11	0	16	10	5	0	15	8	19	0	27	23	35	0	58
Too little	0	0	0	0	0	0	0	0	1	1	0	2	1	1	0	2
Don't know/depends	1	0	0	1	0	1	0	1	4	2	0	6	5	3	0	8
Not asked/missing	1	6	14	21	1	2	0	3	1	2	0	3	3	10	14	27
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 33: Have sentencing guidelines increased, decreased or not affected the amount of discretion in sentencing?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Increased	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Not affected	6	10	0	16	12	15	0	27	12	4	0	16	30	29	0	59
Decreased	1	4	0	5	0	6	0	6	6	0	0	6	7	10	0	17
Don't know/depends	1	1	0	2	1	0	0	1	1	1	0	2	3	2	0	5
Not asked/missing	2	5	14	21	1	4	0	5	1	24	0	25	4	33	14	51
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 34: In your opinion, have sentencing guidelines resulted in fairer sentences?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	1	1	4	6	2	11	0	13	6	2	0	8	9	14	4	27
Yes	6	12	3	21	5	8	0	13	9	0	0	9	20	20	3	43
Don't know	2	3	1	6	6	2	0	8	4	2	0	6	12	7	1	20
Not asked/missing	1	4	6	11	1	4	0	5	1	25	0	26	3	33	6	42
TOTAL	10	20	14	44	14	25	0	39	20	27	0	49	44	74	14	132

TABLE 35: Does charge bargaining occur in the cases you handle?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	0	4	0	4	0	4	0	4	0	7	0	7	0	15	0	15
Yes	10	11	3	24	14	19	0	33	19	21	0	40	43	51	3	97
Not asked/missing	0	5	11	16	0	2	0	2	1	1	0	2	1	8	11	20
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 36: Does sentence bargaining occur in the cases that you handle?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	4	3	0	7	10	7	0	17	2	12	0	14	16	22	0	38
Yes	5	13	3	21	4	16	0	20	17	16	0	33	26	45	3	74
Not asked/missing	1	4	11	16	0	2	0	2	1	1	0	2	2	7	11	20
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 37: Which form of plea bargaining is more predominant?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Charge bargaining	8	6	0	14	14	13	0	27	17	16	0	33	39	35	0	74
Sentence bargaining	0	5	5	10	0	9	0	9	1	7	0	8	1	21	5	27
Neither	0	2	0	2	0	3	0	3	1	4	0	5	1	9	0	10
Don't know	1	2	0	3	0	0	0	0	0	0	0	0	1	2	0	3
Not asked/missing	1	5	9	15	0	0	0	0	1	2	0	3	2	7	9	18
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 38: As a result of guidelines, have prosecutors' recommendations become more influential or less influential?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
More	0	2	0	2	0	8	0	8	1	8	0	9	1	18	0	19
Same	5	8	0	13	8	9	0	17	11	14	0	25	24	31	0	55
Less	1	1	0	2	1	4	0	5	3	1	0	4	5	6	0	11
Depends on the judge	0	0	0	0	1	1	0	2	0	0	0	0	1	1	0	2
Don't know	1	2	0	3	3	2	0	5	4	3	0	7	8	7	0	15
Not asked/missing	3	7	14	24	1	1	0	2	1	3	0	4	5	11	14	30
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 39: During plea negotiations, are you generally aware of the sentence that is specified by the guidelines?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	8	3	0	11	13	7	0	20	18	11	0	29	39	21	0	60
Yes	0	0	0	0	0	11	0	11	1	14	0	15	1	25	0	26
Depends on the case	0	1	0	1	0	0	0	0	0	0	0	0	0	1	0	1
Don't know	0	1	0	1	0	0	0	0	0	0	0	0	0	1	0	1
Not asked/missing	2	15	14	31	1	7	0	8	1	4	0	5	4	26	14	44
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 40: During plea negotiations, are prosecutors generally aware of the guideline sentence?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	8	3	3	14	13	17	0	30	10	11	0	21	31	31	3	65
Yes	0	5	0	5	0	4	0	4	4	13	0	17	4	22	0	26
Depends on the case	0	1	0	1	0	0	0	0	0	0	0	0	0	1	0	1
Don't know	1	5	0	6	1	2	0	3	4	0	0	4	6	7	0	13
Not asked/missing	1	6	11	18	0	2	0	2	2	5	0	7	3	13	11	27
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 41: During plea negotiations, are defense attorneys generally aware of the guideline sentence?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	8	2	3	13	11	13	0	24	14	10	0	24	33	25	3	61
Yes	0	6	0	6	1	8	0	9	1	13	0	14	2	27	0	29
Depends on the case	0	1	0	1	0	0	0	0	0	0	0	0	0	1	0	1
Don't know	1	5	0	6	2	2	0	4	1	3	0	4	4	10	0	14
Not asked/missing	1	6	11	18	0	2	0	2	4	3	0	7	5	11	11	27
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 42: During plea negotiations, are defendants generally aware of the guideline sentence?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	8	6	3	17	11	16	0	27	19	3	0	22	38	25	3	66
Yes	0	2	0	2	1	1	0	2	0	6	0	6	1	9	0	10
Depends on the case	0	1	0	1	0	0	0	0	0	3	0	3	0	4	0	4
Don't know	1	4	0	5	2	3	0	5	0	2	0	2	3	9	0	12
Not asked/missing	1	7	11	19	0	5	0	5	1	15	0	16	2	27	11	40
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	15	132

TABLE 43: Do you think that, with sentencing guidelines, the prosecution and defense are better able to predict the sentence that the judge will impose?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Better	0	1	0	1	0	10	0	10	5	7	0	12	5	18	0	23
Same	4	2	0	6	12	14	0	26	13	15	0	28	29	31	0	60
Worse	0	0	0	0	0	0	0	0	0	3	0	3	0	3	0	3
Depends on the case	0	0	0	0	0	0	0	0	0	1	0	1	0	1	0	1
Don't know	1	1	0	2	2	0	0	2	0	2	0	2	3	3	0	6
Not asked/missing	5	16	14	35	0	1	0	1	2	1	0	3	7	18	14	39
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 44: As far as you know, are the guidelines ever discussed during plea negotiations?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phi	Chi	Total
No	4	4	1	9	13	14	0	27	19	14	0	33	36	32	1	69
Yes	0	1	0	1	0	10	0	10	0	12	0	12	0	23	0	23
Don't know	5	6	0	11	1	1	0	2	0	2	0	2	6	9	0	15
Not asked/missing	1	9	13	23	0	0	0	0	1	1	0	2	2	10	13	25
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 45: Do you think that sentencing guidelines have affected the outcomes of plea negotiations, either in favor of the prosecution or the defense?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Prosecution	0	0	0	0	0	1	0	1	2	9	0	11	2	10	0	12
Not affected	4	6	2	12	11	15	0	26	17	13	0	30	32	34	2	68
Defense	0	1	1	2	2	8	0	10	0	2	0	2	2	11	1	14
Don't know/Depends	5	6	0	11	1	1	0	2	0	3	0	3	6	10	0	16
Not asked/missing	1	7	11	19	0	0	0	0	1	2	0	3	2	9	11	22
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 46: Do you think that judges in this court generally impose lesser sentences on defendants who plead guilty than on those who are found guilty after trial?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	3	2	0	5	8	1	0	9	9	1	0	10	20	4	0	24
Yes	2	10	0	12	1	22	0	23	8	26	0	34	11	58	0	69
Depends on the judge	0	0	0	0	0	0	0	0	2	0	0	2	2	0	0	2
Don't know	4	4	0	8	2	0	0	2	0	0	0	0	6	4	0	10
Not asked/missing	1	4	14	19	3	2	0	5	1	2	0	3	5	8	14	27
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 47: Has the existence of sentencing guidelines had any effect on this practice (of lesser penalties for guilty pleas)?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	3	7	1	11	8	12	0	20	10	11	0	21	21	30	1	52
Yes	1	2	0	3	1	4	0	5	4	9	0	13	6	15	0	21
Don't know	3	3	0	6	2	5	0	7	1	6	0	7	6	14	0	20
Not asked/missing	3	8	13	24	3	4	0	7	5	3	0	8	11	15	13	39
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 48: Do you think that, as a result of guidelines, defendants are more, less, or equally likely to go to trial than they were without guidelines?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
More	0	1	0	1	0	2	0	2	0	7	0	7	0	10	0	10
Equally	4	9	1	14	9	18	0	27	15	16	0	31	28	43	1	72
Less	0	2	2	4	0	0	0	0	3	0	0	3	3	2	2	7
Depends on the case	0	0	0	0	0	1	0	1	0	1	0	1	0	2	0	2
Don't know	4	2	0	6	3	1	0	4	0	4	0	4	7	7	0	14
Not asked/missing	2	6	11	19	2	3	0	5	2	1	0	3	6	10	11	27
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 49: Do you think that sentencing guidelines have, in effect, shifted some of the control over the sentencing decision from the judge to the attorneys?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
No	7	9	2	18	13	18	0	31	17	19	0	36	37	46	2	85
Yes	0	3	0	3	0	5	0	5	1	6	0	7	1	14	0	15
Don't know	2	1	0	3	1	0	0	1	0	1	0	1	3	2	0	5
Not asked/missing	1	7	12	20	0	2	0	2	2	3	0	5	3	12	12	27
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 50: How has the existence of sentencing guidelines affected the prosecutor's initial charging decision; do they tend to charge either more serious or less serious offenses as a result of guidelines?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
More serious	0	0	0	0	0	0	0	0	0	2	0	2	0	2	0	2
Same	8	13	0	21	14	22	0	36	15	25	0	40	37	60	0	97
Less serious	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Don't know	1	0	0	1	0	0	0	0	2	0	0	2	3	0	0	3
Not asked/missing	1	7	14	22	0	3	0	3	3	2	0	5	4	12	14	30
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

TABLE 51: In your opinion, has the use of sentencing guidelines improved, detracted from, or not affected the quality of justice here?

	JUDGE				PROSECUTOR				DEFENDER				TOTAL			
	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total	Den	Phl	Chi	Total
Improved	7	10	2	19	6	7	0	13	6	2	0	8	19	19	2	40
Not affected	2	3	0	5	2	13	0	15	10	12	0	22	14	28	0	42
Detracted	0	0	0	0	1	2	0	3	1	13	0	14	2	15	0	17
Don't know	0	2	1	3	2	1	0	3	1	1	0	2	3	4	1	8
Not asked/missing	1	5	11	17	3	2	0	5	2	1	0	3	6	8	11	25
TOTAL	10	20	14	44	14	25	0	39	20	29	0	49	44	74	14	132

Table 52: Please rate the importance of the following factors in sentencing decisions, using a 5-point scale in which "1" is very unimportant and "5" is very important (n=104).
[Table entries indicate the percent of respondents who rated the item within each of the three categories. Each row totals 100%.]

Type of Information	Importance[1]		
	Unimportant	Neutral	Important
Whether there was a person as a victim	--	2.9	97.2
Personal injury to a victim	--	2.9	97.1
Use, threat, or possession of a weapon	--	3.8	96.2
Type of crime	--	4.8	95.2
Prior adult felony convictions	--	5.7	94.3
Prior adult incarcerations	4.8	5.8	89.4
Prior parole/probation revocations	6.7	11.4	81.9
Type of weapon used	6.7	18.1	75.3
Offender's age	3.8	21.0	75.2
Offender's liberty status at offense	13.4	22.9	63.8
Whether offender has a job	13.4	32.4	54.3
Sale of drugs	11.7	34.3	53.9
Offender's habitual use of drugs	12.5	35.6	51.9
Offender's habitual use of alcohol	17.1	42.9	40.0
Offender's sex	29.5	32.4	38.1
Offender's family ties	28.6	35.2	36.2
Property Damage or loss	22.1	43.3	34.6
Prior juvenile convictions	33.3	33.3	33.4
Offender's educational achievement	24.8	41.9	33.3
Offender's occupation	31.1	35.9	33.0
Prior adult misdemeanor convictions	26.7	41.9	31.4
Offender's ties to the community	39.0	29.5	31.4
Offender's alcohol intoxification at offense	30.5	40.0	29.5
Offender's use/possession of drugs at offense	30.5	41.9	27.6

[1]"Important" includes scale values 4 and 5; "unimportant" includes values 1 and 2; "neutral" includes value 3. The scale break-offs are also used in Tables 54 and 55.

Table 53: Please rate the following sources of information in terms of their importance to your sentencing decisions (n=104).
[Table entries indicate the percent of respondents who rated the item within each of the three categories. Each row totals 100%.]

Source of Information	Importance[1]		
	Unimportant	Neutral	Important
Presentence report	2.9	7.6	89.5
Psychiatric report	8.7	25.0	66.3
Observation of defendant in court	25.3	28.2	46.6
Sentencing guidelines	29.4	24.5	46.1
Probation officer's rec'n	24.3	34.0	41.8
Testimony of witness (other than defendant)	22.4	36.9	40.8
Statements made by defendant in court	28.8	38.5	32.7
Defense attorney's recommendation	29.8	50.0	20.2
Prosecutor's recommendation	32.0	48.5	19.4

[1]"Important" includes scale values 4 and 5; "unimportant" includes values 1 and 2; "neutral" includes value 3. The scale break-offs are also used in Tables 54 and 55.

Table 54: Please rate each [objective] in terms of its overall importance to you in making sentencing decisions (n=104).
[Table entries indicate the percent of respondents who rated the objective within each of the three categories. Each row totals 100%.]

Objective	Importance[1]		
	Unimportant	Neutral	Important
Special deterrence	6.8	16.5	76.7
Isolation	13.6	15.5	70.9
Desert	24.3	23.3	52.4
Rehabilitation	25.2	27.2	47.6
General deterrence	35.9	34.0	30.1

[1]"Important" includes scale values 4 and 5; "unimportant" includes values 1 and 2; "neutral" includes value 3. The scale break-offs are also used in Tables 54 and 55.

Table 55: Interview-Questionnaire Reliability

Item	Number of cases	Interview values	Questionnaire scale	Gamma
Guidelines' Sentence Severity	78[1]	1 = too severe 2 = about right 3 = too lenient	1 = too lenient 2 = 3 = 4 = 5 = too severe	-.84
Influence guidelines <u>should</u> have	84[1]	1 = no influence 2 = some influence 3 = substantial influence	1 = not influential 2 = 3 = 4 = 5 = influential	.47
Influence guidelines have on <u>your</u> decision	26[2]	1 = no influence 2 = some influence 3 = substantial influence	1 = not influential 2 = 3 = 4 = 5 = influential	.68
Have guidelines made sentences more fair	53[1]	0 = No 1 = Yes	1 = less fair 2 = 3 = 4 = 5 = more fair	.78
Have guidelines made you more comfortable	21[2]	0 = No 1 = Yes	1 = less comfortable 2 = 3 = 4 = 5 = more comfortable	.86

[1]Maximum possible n = 103.
[2]The question was asked of judges only; maximum n = 44.

APPENDIX D

VARIABLES IN DENVER AND PHILADELPHIA
CASE RECORD DATA

Denver Codebook Variable List

V101	Card Number
V102	Disposition of Case
V103	Date of Disposition
V109	Judge
V111	Coder ID Number
V113	Docket Number
V119	Defendant Number
V120	# Co-Defendants Charged in District Court
V121	Defendant's Last Name
V133	Defendant's First Name
V141	Defendant's Middle Name
V149	Type of Defense Attorney
V150	Date Complaint Filed in County Court
V156	Most Serious Charge at Complaint in County Court
V163	Class of Most Serious Charge at Complaint in County Court
V164	2nd Most Serious Charge at Complaint in County Court
V171	Class of 2nd Most Serious Charge at Complaint in County Court
V201	Card Number
V202	Total Number of Counts at Complaint in County Court
V203	Was Offender Charged Under Habitual Criminal Statute?
V204	Was Offender Charged Under Sex Offender Statute?
V205	Was Offender Charged Under Violent Offense Statute?
V206	Date of Filing of Information in District Court
V212	Most Serious Charge at Filing of Information in District Court
V219	Class of Most Serious Charge at Filing of Information in District Court
V220	Total Number of Counts at Filing of Information in District Court
V221	Type of Sentence
V223	Was a Special Treatment Program Ordered?
V224	What Kind of Program was Ordered?
V225	Basis of Adjudication
V226	Most Serious Criminal Charge at Conviction
V233	Class of Most Serious Charge at Conviction
V234	Total Number of Counts at Conviction
V235	Was Offender Convicted of Special Habitual Criminal Charge OR Mandatory Felony Conviction in Past 5 Years
V236	Was Offender Convicted of Special Sex Offender Charge?
V237	Was Offender Convicted of Special Violent Offense Charge?
V238	Number of Counts Added after Case Filed in District Court
V239	Minimum Sentence of Incarceration
V242	Maximum Sentence of Incarceration
V245	Length of Probation
V247	Number of Days Credited to Sentence for Time in Jail
V250	Date of Sentencing
V256	Any Appeal Filed by the Defendant?
V257	Was a Motion to Reduce Sentence Filed?
V258	Did Motion to Reduce Sentence Refer to Sentencing Guidelines?
V259	Was the Motion to Reduce Sentence Granted?
V260	Did Judge Have Defendant Diagnostically Evaluated?
V261	Was There an Application for a Particular Kind of Sentence?

V262 Was a PSI Report Filed?
V263 Defendant's Permanent Address
V264 Marital Status
V265 Age of Defendant at Disposition
V301 Card Number
V302 Defendant's Date of Birth
V308 Sex of Defendant
V309 Race/Ethnicity of Defendant
V310 Days Spent in Presentence Confinement
V313 Number of Persons Defendant Responsible For
V314 Is Defendant Clearly Supporting at Least One of Those Whom He Is Responsible
V315 Threat of Harm/Injury as Part of Actual Offense
V316 Possession/Type of Weapon as Part of Actual Offense
V317 Physical Harm Inflicted as Result of Actual Offense
V318 Did Defendant Sell Drugs as Part of Actual Offense
V319 Restitution Being Made/Offered by Defendant?
V320 Juvenile Record/Contact with Juvenile Authorities
V321 # of Defendant's Prior Juvenile "Convictions"
V322 Prior Incarceration of Defendant When He Was A Juvenile?
V323 Date of Defendant's First Arrest (Juvenile or Adult)
V329 # of Defendant's Prior Adult Misdemeanor Arrests
V330 # of Defendant's Prior Adult Misdemeanor Convictions
V331 # of Defendant's Prior Adult Felony Arrests
V332 # of Defendant's Prior Adult Felony Convictions
V333 Longest Prior Adult Post-Conviction Incarceration
V335 # of Prior Adult Probation Revocations
V336 Prior Adult Parole Revocations
V337 Highest Grade Education Completed
V339 Highest Degree Obtained
V340 Defendant's Job Status at Time of Sentencing
V341 Defendant's Monthly Income at Time of Offense
V344 Length of Defendant's Employment at Most Recent Job
V346 Offender's Job Classification
V347 Current Alcohol Use
V348 Current Drug Use
V349 Probation Recommendation on PSI
V350 Offender Under Influence of Drugs/Alcohol at Time of Offense
V351 Type of Victim
V352 Was a Person put in Jeopardy/Injury
V353 Sex of Jeopardized Victim
V354 Residential Status of Offender
V355 Offender Liberty Status During Court Proceedings
V356 Offender Relationship to Justice System at Time of Offense
V357 # Prior Adult Incarcerations (any length)
V358 # Prior Adult Incarcerations (more than 1 month)
V359 Most Serious Prior Adult Sentence
V401 Card Number
V402 Most Serious Conviction Offense--gl
V409 Intra-Class Rank of Most Serious Offense--gl

V410 Seriousness Modifier--gl
V411 Injury--gl
V412 Weapon--gl
V413 Drugs--gl
V414 Victim Modifier--gl
V416 Current Legal Status--gl
V417 Prior Juvenile Convictions--gl
V418 Prior Adult Misdemeanor Convictions--gl
V419 Prior Adult Felony Convictions--gl
V420 Prior Adult Probation/Parole Revocations--gl
V421 Prior Adult Incarcerations--gl
V422 Offense Class of Most Serious Conviction Offense--gl
V424 Offense Score--gl
V425 Offender Score--gl
V427 Was Guideline Indicated for Conviction Offense--gl
V428 Does Definite or Mandatory Sentence Apply--gl
V429 Guideline Sentence: Type--gl
V430 Which Alternative Sentence Indicated on Worksheet--gl
V431 Guideline Sentence: Low Minimum--gl
V434 Guideline Sentence: High Minimum--gl
V437 Guideline Sentence: Low Maximum--gl
V440 Guideline Sentence: High Maximum--gl
V443 First Reason--gl
V446 Second Reason--gl
V449 Number of Reasons for Deviating from Guidelines

V101 Card Number
V102 Coder Number
V104 Case Filing Number
V112 Defendant's Date of Birth
V118 Sex of Defendant
V119 Ethnicity of Defendant
V120 Date of Preliminary Hearing
V126 Date of Arraignment
V132 Date of Conviction
V138 Date of Sentencing
V144 Most Serious Offense Charged at Arrest
V148 Second Most Serious Offense Charged at Arrest
V152 Third Most Serious Offense Charged at Arrest
V156 Total Number of Offenses Charged at Arrest
V158 Total Number of Offenses Charged at Complaint
V160 Total Number of Offenses Bound Over at Pre. Hearing in Municipal Court
V162 Final Disposition of Most Serious Offfense Charged at Arrest
V164 Lesser-Included Offense to Which Most Serious Offense at Arrest was Reduced
V168 Final Disposition Second Most Serious Offense Charged at Arrest
V201 Card Number
V202 Lesser-Included Offense to Which Second Most Serious Offense at Arrest was Reduced
V206 Final Disposition of Third Most Serious Offense Charged at Arrest
V208 Lesser-Included Offense to Which Third Most Serious Offense at Arrest was Reduced
V212 Most Serious Offense Charged at Arraignment
V216 Second Most Serious Offense Charged at Arraignment
V220 Third Most Serious Offense Charged at Arraignment
V224 Total Number of Offenses Charged at Arraignment
V226 Final Disposition of Most Serious Offense Charged at Arraignment
V228 Lesser-Included Offense to Which Most Serious Offense at Arraignment was Reduced
V232 Final Disposition of Second Most Serious Offense Charged at Arraignment
V234 Lesser-Included Offense to Which Second Most Serious Offense at Arraignment was Reduced
V238 Final Disposition of Third Most Serious Offense Charged at Arraignment
V240 Lesser-Included Offense to Which Third Most Serious Offense at Arraignment was Reduced
V244 Most Serious Offense for Which Defendant was Convicted
V248 Second Most Serious Offense for Which Defendant was Convicted
V252 Third Most Serious Offense for Which Defendant was Convicted
V256 Total Number of Offenses for Which Defendant was Convicted
V258 Basis of Adjudication
V259 Type of Sentence Imposed for Most Serious Charge at Conviction
V261 Length of Probation for Most Serious Offense at Conviction
V264 Minimum Sentence of Incarceration for Most Serious Offense at Conviction

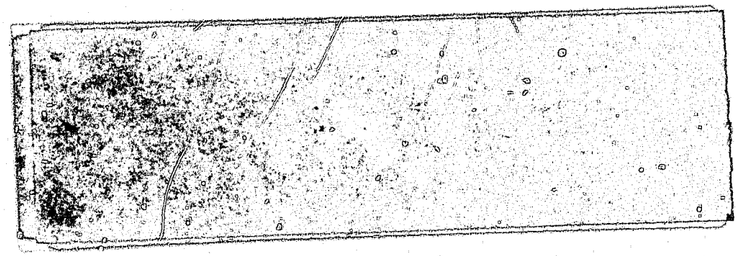
V267 Maximum Sentence of Incarceration for Most Serious Offense at Conviction
V270 Was Sentence for Most Serious Crime at Conviction Imposed CS or CC with Sentences for Other Offenses?
V301 Card Number
V302 Type of Sentence for Second Most Serious Charge at Conviction
V304 Probation Term for Second Most Serious Offense at Conviction
V307 Minimum Sentence of Incarceration Imposed for Second Most Serious Offense at Conviction
V310 Maximum Sentence of Incarceration for Second Most Serious Offense at Conviction
V313 Was Sentence for Second Most Serious Crime at Conviction Imposed CS or CC with Sentences Imposed for Other Offenses?
V315 Type of Sentence Imposed for Third Most Serious Charge at Conviction
V317 Probation Term Imposed for Third Most Serious Offense at Conviction
V320 Minimum Sentence of Incarceration for Third Most Serious Offense at Conviction
V323 Maximum Sentence of Incarceration for Third Most Serious Offense at Conviction
V326 Was Sentence for Third Most Serious Crime at Conviction Imposed CS or CC with Sentences Imposed for Other Offenses?
V328 # of Days Credited in Jail Awaiting Adjudication and Sentencing
V331 Offender Liberty Status Pending Disposition
V332 Was the Most Severe Sentence Imposed for One of the Three Most Serious Offenses at Conviction?
V333 Most Severe Type of Sentence for Any Single Offense for Conviction
V335 Total Probation Finally Imposed for All Offenses at Conviction
V338 Total Minimum Term of Incarceration Imposed for All Offenses at Conviction
V341 Total Maximum Term of Incarceration Imposed for All Offenses at Conviction
V344 Did Final Sentence Involve CC or CS Terms?
V345 Amount of Fine Imposed
V346 Amount of Restitution Imposed
V347 Did Defendant Act w/Others in Commission of Most Serious Offense at Conviction
V348 Sentencing Judge
V351 Defendant's Address
V352 Residential Status of Defendant
V353 Marital Status
V354 Employment Status at Time of Offense
V355 Defendant's Job Classification, Whether Employed or Not
V356 Length of Employment at Most Recent Job
V358 Monthly Gross Legal Income at Time of Offense
V361 Highest Education Grade Completed
V363 Any Prior "Failures to Appear"
V364 Was Defendant Recommended for ROR?
V365 # of Distinct Criminal Transactions Represented by Charges for Which Defendant was Arraigned

V366 Classification of Target of Most Serious Offense Charged at Arraignment
V367 Actual Threat of Harm/Injury as Part of Any Offense Charged at Arraignment
V368 Was a Person Put in Jeopardy of Personal Injury at the Commission of any of the Offenses Charged at Arraignment?
V369 Type of Weapon Possessed or Used by Defendant in ANY of the Offenses Charged at Arraignment
V370 Harm or Injury to Victim(s) of ANY Offenses Charged at Arraignment
V401 Card Number
V402 Value of Property Lost as Result of ALL Offenses Charged at Arraignment
V407 Number of Jeopardized Victims for All Offenses Charged at Arraignment
V408 Sex of Jeopardized Victim(s) of Offenses Charged at Arraignment
V409 Ethnicity of Victim(s) Jeopardized by Offenses Charged at Arraignment
V410 Age (in Years) of Victim(s) Jeopardized by Offenses Charged at Arraignment
V411 Sale of Drugs by Defendants in ANY of the Offenses Charged at Arraignment
V412 Defendant's Behavior at Arrest
V413 Was Pre-Sentence Investigation Ordered/Requested
V414 Was Psychiatric Report/Investigation Ordered/Requested
V415 Was a Petition for Reconsideration of Sentence Filed by the Defendant?
V416 Disposition of Petition for Reconsideration of Sentence
V417 Any Appeal From the Court of Common Pleas Filed by the Defendant?
V418 Type of Defense Counsel

APPENDIX E

EXAMPLES OF SENTENCING GUIDELINES

CONTINUED



Denver Guidelines Computation Sheet and
Grids Indicating Guideline Sentence

OFFENDER _____ DOCKET NUMBER _____

JUDGE _____ DATE _____

Offense(s) Convicted Of: _____

_____

_____

OFFENSE CLASS (MOST SERIOUS OFFENSE)

OFFENSE SCORE

A. Intra-Class Rank _____ +

B. Seriousness Modifier _____ +

0 = No injury 0 = No weapon 0 = No sale of drugs
1 = Injury 1 = Weapon 1 = Sale of drugs
2 = Death

C. Victim Modifier (Crime Against Person) _____ =

0 = Unknown victim
-1 = Known victim

OFFENDER SCORE

A. Current Legal Status _____ +

0 = Not on probation/parole, escape
1 = On probation/parole, escape

B. Prior Juvenile Convictions _____ +

0 = No convictions
1 = 1-3 convictions
2 = 4 or more convictions

C. Prior Adult Misdemeanor Convictions _____ +

0 = No convictions
1 = 1-3 convictions
2 = 4 or more convictions

D. Prior Adult Felony Convictions _____ +

0 = No convictions
1 = 1 conviction
3 = 2 or more convictions

E. Prior Adult Probation/Parole Revocations _____ +

0 = None
2 = One or more revocations

F. Prior Adult Incarcerations (Over 30 Days) _____ =

0 = None
1 = 1 incarceration
3 = 2 or more incarcerations

GUIDELINE SENTENCE _____

ACTUAL SENTENCE _____

REASONS (If actual sentence does not fall within guideline range):

OFFENSE CLASS

OFFENSE SCORE

OFFENDER SCORE

FELONY 3

Offender Score

Offense Score	0-1	2	3	4	5-8	9-10	11-13
4-5	5-7 yrs. minimum 8-10 yrs. maximum	7-9 yrs. minimum 12-15 yrs. maximum	10-12 yrs. minimum 15-20 yrs. maximum	12-15 yrs. minimum 15-20 yrs. maximum	12-15 yrs. minimum 15-20 yrs. maximum	17-22 yrs. minimum 35-40 yrs. maximum	17-22 yrs. minimum 35-40 yrs. maximum
3	OUT	7-9 yrs. minimum 12-15 yrs. maximum	7-9 yrs. minimum 12-15 yrs. maximum	7-9 yrs. minimum 12-15 yrs. maximum	8-10 yrs. minimum 15-20 yrs. maximum	17-22 yrs. minimum 35-40 yrs. maximum	17-22 yrs. minimum 35-40 yrs. maximum
2	OUT	5-7 yrs. minimum 12-15 yrs. maximum	5-7 yrs. minimum 12-15 yrs. maximum	5-7 yrs. minimum 12-15 yrs. maximum	8-10 yrs. minimum 12-15 yrs. maximum	17-22 yrs. minimum 35-40 yrs. maximum	17-22 yrs. minimum 35-40 yrs. maximum
1	OUT	OUT	OUT	5-7 yrs. minimum 12-15 yrs. maximum	5-7 yrs. minimum 12-15 yrs. maximum	8-10 yrs. minimum 12-15 yrs. maximum	8-10 yrs. minimum 15-20 yrs. maximum

FELONY 4

Offense Score	Offender Score: 0-1	2	3	4-5	6-7	8	9-10	11-13
4-6	Indeterm. minimum 8-10 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum
3	OUT*	Indeterm. minimum 3-5 yrs. maximum	Indeterm. minimum 3-5 yrs. maximum	Indeterm. minimum 3-5 yrs. maximum	Indeterm. minimum 5-7 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum
2	OUT	OUT*	Indeterm. minimum 3-5 yrs. maximum	Indeterm. minimum 3-5 yrs. maximum	Indeterm. minimum 5-7 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum
1	OUT	OUT	OUT*	Indeterm. minimum 3-5 yrs. maximum	Indeterm. minimum 3-5 yrs. maximum	Indeterm. minimum 3-5 yrs. maximum	Indeterm. minimum 5-7 yrs. maximum	Indeterm. minimum 8-10 yrs. maximum

*Potential candidate for work project or community corrections.

FELONY 5

Offender Score

Offense Score	0-1	2	3	4-5	6-8	9-13
4-5	Indeterm. minimum 4-5 yrs. maximum	Indeterm. minimum 4-5 yrs. maximum	Indeterm. minimum 4-5 yrs. maximum	Indeterm. minimum 4-5 yrs. maximum	Indeterm. minimum 4-5 yrs. maximum	Indeterm. minimum 4-5 yrs. maximum
3	OUT	OUT*	Indeterm. minimum 4-5 yrs. maximum	Indeterm. minimum 4-5 yrs. maximum	Indeterm. minimum 4-5 yrs. maximum	Indeterm. minimum 4-5 yrs. maximum
2	OUT	OUT	OUT	Indeterm. minimum 4-5 yrs. maximum	Indeterm. minimum 4-5 yrs. maximum	Indeterm. minimum 4-5 yrs. maximum
1	OUT	OUT	OUT	Indeterm.* minimum 3-4 yrs. maximum	Indeterm. minimum 3-4 yrs. maximum	Indeterm. minimum 4-5 yrs. maximum

* Potential candidate for work project, split sentence, or community corrections.

MISDEMEANOR 1

Offender Score

Offense Score	0-1	2-3	4-5	6-7	8-11	12-13
4-5	20-24 mo.	20-24 mo.	20-24 mo.	20-24 mo.	20-24 mo.	20-24 mo.
3	OUT*	9-12 mo.	14-18 mo.	20-24 mo.	20-24 mo.	20-24 mo.
2	OUT.	OUT	14-18 mo.	14-18 mo.	14-18 mo.	20-24 mo.
1	OUT	OUT	OUT	9-12 mo.	14-18 mo.	14-18 mo.

* Work project or Community Correction Center.

MISDEMEANOR 2

Offender Score

Offense Score	0	1	2	3-4	5-7	8-13
4-5	9-12 mo.	9-12 mo.	9-12 mo.	9-12 mo.	9-12 mo.	9-12 mo.
3	OUT*	9-12 mo.	9-12 mo.	9-12 mo.	9-12 mo.	9-12 mo.
2	OUT	OUT	OUT	4-7 mo.	5-8 mo.	9-12 mo.
1	OUT	OUT	OUT	OUT	3-5 mo.	7-10 mo.

* Work project or Community Correction Center.

MISDEMEANOR 3

Offense Score	Offender Score 0-1	2-3	4-5	6-8	9-13
4-5	4-6 mo.	4-6 mo.	4-6 mo.	4-6 mo.	4-6 mo.
3	OUT	2-4 mo.	2-4 mo.	2-4 mo.	4-6 mo.
2	OUT	OUT	2-4 mo.	2-4 mo.	2-4 mo.
1	OUT	OUT	OUT	2-4 mo.	2-4 mo.

FELONY 4

Drug Offenses

Offense Score	Offender Score: 0	1	2-4	5-6	7-9	10-13
3	OUT	2-3 yr. minimum 5-7 yr. maximum	2-3 yr. minimum 5-7 yr. maximum	2-3 yr. minimum 5-7 yr. maximum	2-3 yr. minimum 5-7 yr. maximum	2-3 yr. minimum 5-7 yr. maximum
2	OUT	OUT	2-3 yr.* minimum 5-7 yr. maximum	2-3 yr. minimum 5-7 yr. maximum	2-3 yr. minimum 5-7 yr. maximum	2-3 yr. minimum 5-7 yr. maximum
1	OUT	OUT	OUT	OUT	2-3 yr. minimum 2½-4 yr. maximum	2-3 yr. minimum 2½-4 yr. maximum

*Potential candidate for work project or community corrections.

FELONY 5

Drug Offenses

Offender Score

Offense Score	1	2	3-4	5-8	9-10	11-13
3	OUT	OUT	1½-2 yrs. minimum maximum	2-3 yrs. minimum 4-5 yrs. maximum	3-4 yrs. minimum 4-5 yrs. maximum	3-4 yrs. minimum 4-5 yrs. maximum
1-2	OUT	OUT	OUT	1+1½ yrs. minimum 2½-4 yrs. maximum	1½-2 yrs. minimum 3-4 yrs. maximum	2-2½ yrs. minimum 4-5 yrs. maximum

Philadelphia Guidelines Computation Sheets and
Grids Indicating Guideline Sentence

SENTENCING – OFFENSE AGAINST A PERSON	CITY OF PHILADELPHIA COURT OF COMMON PLEAS
OFFENDER	JUDGE
CASE NO.	OFFENSE(S) CONVICTED
DATE OF SENTENCING	
TYPE OF PROGRAM ☐ FELONY JURY ☐ FELONY NON-JURY ☐ HOMICIDE	DISPOSITION TYPE ☐ CONVICTION/TRIAL ☐ NEGOTIATED PLEA ☐ OPEN PLEA

OFFENSE SCORE

A. SERIOUSNESS RANKING
- 9 = MURDER
- 8 = FELONY I AGAINST A PERSON
- 6 = FELONY II AGAINST A PERSON
- 4 = FELONY III AGAINST A PERSON
- \+ 2 = MISDEMEANOR AGAINST A PERSON

B. VICTIM INJURY
- 0 = NO INJURY
- 1 = INJURY, NON-PERMANENT
- 2 = PERMANENT INJURY AND RAPE
- \+ 3 = DEATH

C. WEAPON USAGE
- 0 = NO WEAPON USED
- \+ 1 = WEAPON USED

D. PROPERTY INVOLVED
- 0 = PROPERTY LOSS $500 OR LESS
- \+ 1 = PROPERTY LOSS OVER $500

E. NUMBER OF VICTIMS
- 0 = ONE VICTIM
- \+ 1 = MORE THAN ONE VICTIM

☐ OFFENSE SCORE

OFFENDER SCORE

A. PRIOR ADULT INCARCERATIONS
- 0 = NONE
- 1 = ONE
- 2 = TWO
- \+ 3 = THREE OR MORE

B. RELATIONSHIP TO CRIMINAL JUSTICE SYSTEM AT ARREST
- 0 = NONE OR PENDING CASES
- \+ 1 = COURT OR CRIMINAL JUSTICE SUPERVISION

C. PRIOR ADULT CONVICTIONS
- 0 = NONE
- 1 = ONE
- 2 = TWO
- \+ 3 = THREE OR MORE

D. PRIOR FELONY PERSON CONVICTIONS
- 0 = NONE
- \+ 1 = ONE OR MORE

E. SOCIAL STABILITY
- 0 = UNEMPLOYED/DID NOT COMPLETE HIGH SCHOOL
- – 1 = EMPLOYED OR COMPLETED HIGH SCHOOL

☐ · OFFENDER SCORE

GUIDELINE SENTENCE
ACTUAL SENTENCE
REASONS (IF ACTUAL SENTENCE DIFFERS FROM GUIDELINE SENTENCE)

OFFENDER SCORE

OFFENSE SCORE	-1	0	1	2	3	4	5	6	7	8
12+	1 - 5 yrs 6 -10 yrs	4 - 6 yrs 10 - 20yrs	4 - 6 yrs 10 - 20yrs	4 - 6 yrs 10 - 20yrs	5 - 7 yrs 10 - 20 yrs	5 - 7 yrs 10 - 20 yrs	5 - 7 yrs 10 - 20 yrs	6 - 8 yrs 12 -20 yrs	6 - 8 yrs 12 -20 yrs	8 - 10 yrs 16 - 20 yrs
11	6 -11½mos 21mo - 4yrs	18mo - 3½yrs 3 - 7 yrs	2 - 4 yrs 4 - 8 yrs	2 - 4 yrs 4 - 8 yrs	2 - 4 yrs 4 - 8 yrs	2 - 4 yrs 4 - 8 yrs	3 - 5 yrs 6 - 10 yrs	6 -8 yrs 12-16 yrs	6 -8 yrs 12 -16 yrs	6 - 8 yrs 12 - 16 yrs
10	6 -11½mos 23mo - 4yrs	18mo - 3½yrs 3 - 7 yrs	2 - 4 yrs 4 - 8 yrs	2 - 4 yrs 4 - 8 yrs	2 - 4 yrs 4 - 8 yrs	2 - 4 yrs 4 - 8 yrs	3 - 5 yrs 6 -10 yrs	6 - 8 yrs 12 -16 yrs	6 - 8 yrs 12 -16 yrs	6 - 8 yrs 12 - 16 yrs
9	OUT	6 - 11½mos 18mo- 2½yrs	1½ - 3 yrs 4 - 6 yrs	1½ - 3 yrs 4 - 6 yrs	1½ - 3 yrs 4 - 6 yrs	1½ - 3 yrs 4 - 6 yrs	2 - 4 yrs 4 - 8 yrs	2 - 4 yrs 4 - 8 yrs	2 - 4 yrs 4 - 8 yrs	3 - 6 yrs 6 - 12 yrs
8	OUT	OUT	OUT	1 - 6 mos 18mo -2½yrs	6 - 11½mos 18mo - 2½yrs	6 - 11½mos 18mo - 2½yrs	6 - 11½mos 18mo -2½yrs	1 - 2½yrs 23mo-5yrs	1 - 2½yrs 23mo-5 yrs	1½- 2½ yrs 23mo-5 yrs
6-7	OUT	OUT	OUT	OUT	OUT	TIME IN	TIME IN	TIME IN	6 - 12 mos 1½- 2½ yrs	6 - 12 mos 1½- 2½ yrs
4-5	OUT	OUT	OUT	OUT	OUT	TIME IN	TIME IN	TIME IN	TIME IN	1 - 6 mos 1½ - 2½ yrs

FELONY AGAINST A PERSON

OFFENDER SCORE

OFFENSE SCORE	-1	0	1	2	3	4	5	6	7	8
5	OUT	OUT	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25 mos	3 - 6 mos 21 - 25 mos
4	OUT	OUT	OUT	OUT	OUT	OUT	OUT	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25 mos	3 - 6 mos 21 - 25 mos
3	OUT	OUT	OUT	OUT	OUT	OUT	OUT	OUT	3 - 6 mos 21 - 25 mos	3 - 6 mos 21 - 25 mos
2	OUT	OUT	OUT	OUT	OUT	OUT	OUT	OUT	3 - 6 mos 21 - 25 mos	3 - 6 mos 21 - 25 mos
1	OUT	OUT	OUT	OUT	OUT	OUT	OUT	OUT	2 - 4 mos 21 - 25 mos	3 - 6 mos 21 - 25 mos

MISDEMEANORS

SENTENCING – OFFENSE NOT AGAINST A PERSON	CITY OF PHILADELPHIA COURT OF COMMON PLEAS
OFFENDER	JUDGE
CASE NO.	OFFENSE(S) CONVICTED
DATE OF SENTENCING	
TYPE OF PROGRAM ☐ FELONY JURY ☐ FELONY NONJURY ☐ HOMICIDE	DISPOSITION TYPE ☐ CONVICTION/TRIAL ☐ NEGOTIATED PLEA ☐ OPEN PLEA

OFFENSE SCORE

A. SERIOUSNESS RANKING
- 7 = FELONY I NOT AGAINST A PERSON
- 5 = FELONY II NOT AGAINST A PERSON
- 3 = FELONY III
- \+ 1 = MISDEMEANOR NOT AGAINST A PERSON

___

B. VICTIM INJURY
- 0 = NO INJURY
- 1 = INJURY, NON-PERMANENT
- 2 = PERMANENT INJURY AND RAPE
- \+ 3 = DEATH

___

C. WEAPON USAGE
- 0 = NO WEAPON USED
- \+ 1 = WEAPON USED

___

D. PROPERTY INVOLVED
- 0 = PROPERTY LOSS $500 OR LESS
- \+ 1 = PROPERTY LOSS OVER $500

___

E. VICTIM CLASSIFICATION
- 0 = LARGE STORE, ORGANIZATION, INSTITUTION
- \+ 1 = PRIVATE CITIZEN, SMALL STORE

___

☐ OFFENSE SCORE

OFFENDER SCORE

A. PRIOR ADULT INCARCERATIONS
- 0 = NONE
- 1 = ONE
- 2 = TWO
- \+ 3 = THREE OR MORE

___

B. RELATIONSHIP TO CRIMINAL JUSTICE SYSTEM AT ARREST
- 0 = NONE OR PENDING CASES
- \+ 1 = COURT OR CRIMINAL JUSTICE SUPERVISION

___

C. PRIOR ADULT CONVICTIONS
- 0 = NONE
- 1 = ONE
- 2 = TWO
- \+ 3 = THREE OR MORE

___

D. PRIOR FELONY PERSON CONVICTIONS
- 0 = NONE
- \+ 1 = ONE OR MORE

___

E. SOCIAL STABILITY
- 0 = UNEMPLOYED/DID NOT COMPLETE HIGH SCHOOL
- – 1 = EMPLOYED OR COMPLETED HIGH SCHOOL

___

☐ OFFENDER SCORE

GUIDELINE SENTENCE
ACTUAL SENTENCE
REASONS (IF ACTUAL SENTENCE DIFFERS FROM GUIDELINE SENTENCE)

OFFENDER SCORE

OFFENSE SCORE	-1	0	1	2	3	4	5	6	7	8
9-13	9 - 12 mos 1½ - 2½ yrs	1 - 1½ yrs 2 - 3 yrs	1 - 2 yrs 3 - 4 yrs	2 - 4 yrs 6 - 8 yrs	3 - 5 yrs 9 - 11 yrs	3 - 5 yrs 9 - 11 yrs	3 - 5 yrs 9 - 11 yrs	3 - 5 yrs 9 - 11yrs	3 - 5 yrs 9 - 11yrs	3 - 5 yrs 9 - 11 yrs
7-8	OUT	OUT	OUT	TIME IN	9 - 12 mos 1½ - 2½ yrs	9 - 12 mos 1½ - 2½ yrs	9 - 12 mos 1½ - 2½ yrs	9 - 12mos 1½ - 2½yrs	1 - 2 yrs 3 - 4 yrs	1 - 2 yrs 3 - 4 yrs
5-6	OUT	OUT	OUT	OUT	TIME IN	3 - 6 mos 1½ - 2½ yrs	3 - 6 mos 1½ - 2½ yrs	3 - 6 mos 1½ - 2½yrs	3 - 6 mos 1½ - 2½yrs	3 - 6 mos 1½- 2½ yrs
3-4	OUT	OUT	OUT	OUT	OUT	OUT	2 - 4 mos 1½ - 2½ yrs	2 - 4 mos 1½ - 2½yrs	3 - 6 mos 1½ - 2½yrs	3 - 6 mos 1½ - 2½ yrs

FELONIES NOT AGAINST A PERSON

OFFENDER SCORE

SERIOUS FELONIES	-1	0	1	2	3	4	5	6	7	8
5	OUT	OUT	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25 mos	3 - 6 mos 21 - 25 mos
4	OUT	OUT	OUT	OUT	OUT	OUT	OUT	3 - 6 mos 21 - 25mos	3 - 6 mos 21 - 25 mos	3 - 6 mos 21 - 25 mos
3	OUT	OUT	OUT	OUT	OUT	OUT	OUT	OUT	3 - 6 mos 21 - 25 mos	3 - 6 mos 21 - 25 mos
2	OUT	OUT	OUT	OUT	OUT	OUT	OUT	OUT	3 - 6 mos 21 - 25 mos	3 - 6 mos 21 - 25 mos
1	OUT	OUT	OUT	OUT	OUT	OUT	OUT	OUT	2 - 4 mos 21 - 25 mos	3 - 6 mos 21 - 25 mos

MISDEMEANORS

END