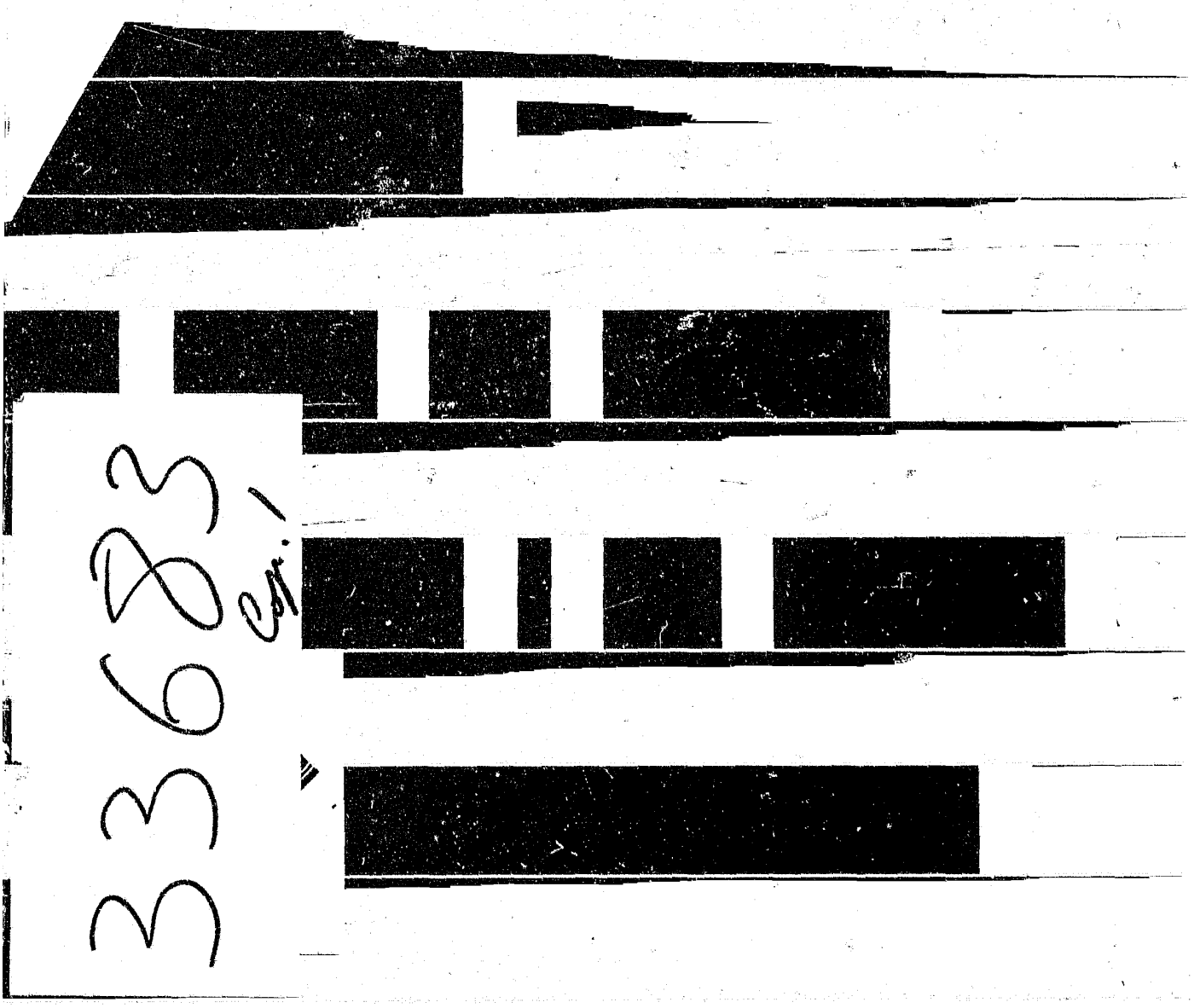
Federal Criminal Sentencing

Perspectives of Analysis and a Design for Research



U.S. DEPARTMENT OF JUSTICE Law Enforcement Assistance Administration
National Criminal Justice Information and Statistics Service
UTILIZATION OF CRIMINAL JUSTICE STATISTICS ANALYTIC REPORT 16

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Utilization of Criminal Justice Statistics Project

ANALYTIC REPORT 16

FEDERAL CRIMINAL SENTENCING: Perspectives of Analysis and a Design for Research

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THE UTILIZATION OF CRIMINAL JUSTICE STATISTICS Project was funded initially in 1972 by the National Criminal Justice Information and Statistics Service of the Law Enforcement Assistance Administration. One primary aim of the project is the production of annual editions of the Sourcebook of Criminal Justice Statistics, a compilation of available nationwide criminal justice statistical data. A second aim has been and continues to be an examination of the utility that a variety of criminal justice statistical data bases have for addressing questions of practical and theoretical interest in the field.

One product of that examination is a series of analytic reports, of which this volume is one. These reports, written by research staff members of the Utilization of Criminal Justice Statistics Project, all have a common theme: the discussion of a central criminal justice topic using an exemplary or innovative criminal justice data base. Each report in the series not only discusses substantive findings in regard to particular issues, but also considers the qualities and limitations of the data, as well as techniques and problems of analysis, in relation to the substantive findings.

At a time when criminal justice statistics development is extensive, and often expensive, these analytic reports focus attention on one often overlooked function of criminal justice statistics—the analysis of current issues and questions based on available data. In fact, the utilization issue is perhaps as important as any in the area of criminal justice statistics. It often happens that data are collected—usually at great expense—without subsequent efforts to utilize such data to address the pressing problems that confront criminal justice. This series of Analytic Reports explores the problems and prospects inherent in the application of various sources of criminal justice statistical data to issues of interest and concern to agency personnel, planners, researchers, and the public alike.

MICHAEL J. HINDELANG
Project Director

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PREFACE

THE TASK AND PROBLEMS of "doing justice" are perhaps nowhere more clearly visible than at the imposition of criminal sentence. Yet surprisingly few have attempted comprehensive analysis of the process by which sentence is imposed or systematic study of factors that seem to influence the decision. This series of four analytic reports on criminal sentencing is a response to the relative inattention sentencing has won from empiricists.

Starting from the assumption that a comprehensive assessment and understanding of experience is a necessary prelude to productive reform, the series carefully inventories that experience. The first report provides examples of sentencing research, explores the contributions and limitations of that research, and proposes a design aimed at overcoming many of the limitations of earlier inquiries. The second and third reports use this design to provide both bivariate and multivariate analysis of sentencing for eight individual Federal offenses; analysis includes the national level, five Federal circuits, and six Federal district courts. The fourth report takes a novel look at consistency in Federal sentencing patterns across courts and over time, by measuring how well sentence outcome can be predicted for individual cases by the use of model (regression) equations derived from the analysis of sentencing patterns of *different* courts, and at *different* times than those for which predictions are being made.

On the basis of these analyses, it appears that a useful and equitable decision model can be developed—in part, from experience—and applied to the sentencing decision.

These analytic reports are based on analyses completed in 1975, which are more fully presented in a document entitled *Criminal Sentencing: An Empirical Analysis of Variations in Sentencing Imposed in Federal District Courts*. This source document is available on loan from the Law Enforcement Assistance Administration Library, U.S. Department of Justice, Washington, D.C. 20531.

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THE PROCESS OF criminal sentencing, like so many other aspects of the criminal justice system, is ripe for reform. A necessary prelude to productive reform is a thorough assessment of the object of reform. This report is the first of a series of four analytic reports that assesses criminal sentencing in order to isolate those facets of the process that are in need of reform. It examines extant criminal sentencing studies, reports both their contributions and their defects, and suggests a research design that will overcome the limitations found in earlier studies.

Several limitations were found in earlier studies. First, many focused on a single offense or on similar offenses. Second, a large number failed to explore systematically beyond the zero- or first-order correlation the significance of the relationship between a particular offender characteristic (e.g., sex, race, prior record) and sentence. A third constraint was the relatively small scope of the data bases used by earlier studies, a scope limited in number of cases, in regional analysis, and in time span. The fourth limitation was earlier studies' failure to differentiate between the type of sentence (i.e., incarceration or some other sentence) and length of incarceration sentence.

The research design proposed by this report incorporates the following features in order to overcome the limitations outlined above. First the proposed study will examine *several* crimes significantly *different* in type: a violent offense (robbery), property offenses (auto theft, larceny, and theft), white-collar offenses (counterfeiting, embezzlement), drug offenses, and Selective Service offenses. Second, it will incorporate a number of controls sufficient for an examination of correlations beyond the zero- and first-order level. Third, its data base, a Federal data base, includes a relatively large number of cases, can accommodate regional analysis, and includes more than 1 year (fiscal years 1964 and 1971). Finally, whereas most studies have viewed sentencing as involving a single decision, this study will distinguish between the judge's decision about the length of incarceration and the vital determination of whether to incarcerate the offender at all.

FEDERAL CRIMINAL SENTENCING: Perspectives of Analysis and a Design for Research

Introduction

THE PROCESS OF criminal sentencing has endured criticism from all quarters. Judges have been charged with racism, arbitrariness, and caprice, sentences being said to turn on little more than whim, hunch, or prejudice. Yet, few such charges are derived from observation or rigorous analysis; critics and apologists, alike, argue on equally tenuous grounds.

There are two aims of this report. One objective is to survey the types of sentencing research that has been attempted, with special focus on the problems, issues, and limitations that flow from various perspectives underlying sentencing research. This report will also introduce a design aimed at overcoming many of the constraints noted in earlier research.

Why Study Sentencing?

There can be little doubt that, however justifiable, the imposition of criminal sentence is one of the most substantial intrusions the State can effect upon individual liberty. Judges are given nearly unparalleled discretion over the lives of millions who come before them each year. Bounded only by the broadest of statutory constraints, the sentencing judge is generally empowered to exact penalties ranging from little more than verbal reprimand to life imprisonment or even death. Although not all offenders face such a range of sentences, sentences actually imposed for each offense category tend to cover the statutorily allowed range. That discretion of such formidable consequence should be authorized—indeed, that it should be exercised so variably—should provide not only a justification but a compulsion to examine the manner of its use.

How We Can Use the Products of Research

Systematic research can teach much about many of the little understood aspects of criminal sentencing. First, such analysis can create a better understanding of how various factors describing the offense, the offender, and the manner of adjudication relate to sentence outcome. It can identify the factors that appear most strongly related to sentence and ascertain the extent to which variations in criminal sentences can be explained in terms of those particular criteria.

Once the basic kinds of patterns that characterize criminal sentencing are revealed, some of the subtle ways in which these patterns may vary can be explored. Are patterns the same for all types of crime? That is, is the information that appears to influence sentence outcome the same, irrespective of the specific type of offense in question, the particular court in which sentence is imposed, the year in question? Does there appear to be any noticeable difference in patterns that characterize the "in-out" decision (the judge's determination of whether an offender will be sentenced to prison or probation) and the "how long" decision (the determination of the maximum length of incarceration)? Certainly this list does not exhaust the unanswered questions about criminal sentencing. These and others demand careful empirical analysis.

Such an inquiry has critical theoretical, legal, practical, and administrative implications. First, that so many questions like the above remain

unanswered is testimony to the embryonic state of sentencing decision theory. By rigorously exploring these questions our understanding of the nature of sentencing can be increased—what concerns and objectives appear to motivate the sentence decision, whether and how the most important criteria may vary according to the offense involved and the type of decision being made, and how the decision patterns vary across judicial districts and over time.

Second, the conspicuous variability in sentences along the dimensions discussed has invoked charges of "disparity," "arbitrariness," "caprice," and the like. It must be understood from the outset that variation is often appropriate, particularly if it is founded on differences like offense severity. However, when it endures in the absence of apparent justification, or when the bases of discrepancies in sentences are not carefully articulated, then the whole enterprise becomes the object of attack, even though compelling reasons may have occasioned observed discrepancies. Research can suggest the respective roles of each of a variety of objective criteria in the sentencing decision. Once the bases of variations (e.g., demographic characteristics, offense, prior criminal record) are known, then evaluation of sentencing can become informed, and as such, concrete and constructive. Furthermore, where criticisms prove unfounded, they can be exposed.

The imposition of criminal sentence has remained essentially free from review, and judges are virtually exempt from control. However, as more is learned about the sentencing decision, the entire process will become ripe for scrutiny, and a number of specific legal issues will begin to crystallize. Legal criticism of sentencing has traditionally been couched in the language of equal protection and in terms of the propriety and relevance of sentencing criteria. Given data about defendants and their respective sentences, it is a straightforward matter of analysis to determine whether comparable information tends to yield comparable sentences (the question of equal protection) and to identify those factors most strongly related to sentence outcome. Once those factors have been identified and their respective impact determined, their propriety and relevance are issues that must be resolved elsewhere and by other than empirical means. In the interim, the answers provided by empirical analysis can inform the kinds of ethical, legal, and philosophical judgments that underlie any serious assessment of criminal sentencing and that must inevitably precede the reformation of that enterprise.

Perspectives of Analysis

Before looking at some of the specific findings of sentencing research, a brief diversion into the nature of the sentencing process will help to put the present and previous studies into perspective. The decision process is popularly viewed as consisting of three elements—input, conversion, and output. The information brought to bear on the "decision" constitutes the fundamental "input" source. The social, political, economic, and psychological milieu of the decision-set comprise the conversion phase, wherein the information is thought to generate certain conclusions or decisions, which are called "outputs." Applying this frame of reference to sentencing (see Figure 1), the output is the sentence decision, and the inputs are information about the offender (e.g., demographic characteristics, prior criminal record), the offense of conviction, and the circumstances surrounding the conviction (e.g., whether the offender was convicted by guilty plea or by trial, whether the offender was represented by appointed or by retained counsel).

The role and significance of the conversion process to the translation of input ("fact") to output (decision) is the focus of a long-standing debate among students of legal decisionmaking.[1] On the one hand, many scholars view the conversion process as a "black box," the contents and processes of which are either unknowable or of little significance to outcome. In fact, most studies concerned with the impact of specific variables upon sentence outcome have assumed this perspective.

On the other hand, students of another school[2] contend that there is nothing automatic or self-evident in the translation of input to output. Rather, they hold that the background, circumstances, preconceptions, and beliefs of the decisionmaker bear very strongly upon the so-called "factual" inputs to

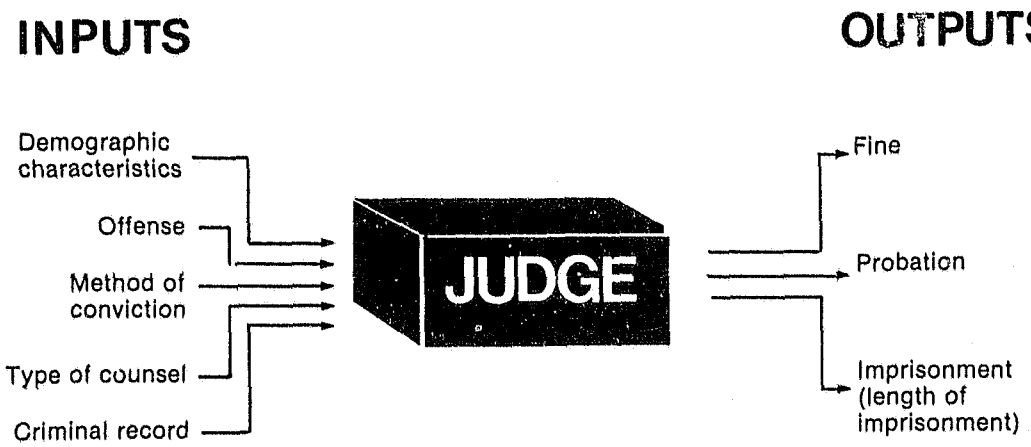
[1] S. Goldman and T. Jahnige, **The Federal Courts as a Political System** (New York: Harper and Row) 1971, pp. 198-199. See also S. Peltason, **Federal Courts in the Political Process** (New York: Random House) 1965; and G. Schubert, **Judicial Policy-Making: The Political Role of the Courts** (Chicago: Scott, Foresman and Company) 1965, pp. v-vi.

[2] See J. Frank, **Law and the Modern Mind** for a good discussion of the "fact skeptic" school of jurisprudence. Says Frank:

> The process of conducting a trial and the *"facts"* that emerge are in large part attributable to the judge—his likes, dislikes, what he had for breakfast, his relations with his wife and family, his anxieties, hopes, aspirations, religious, social, economic, and political beliefs.

In J. Howard, **Crisis in the Courts** (New York: David McKay Co.) 1968, p. 151.

FIGURE 1 Conventional "black box" decision model



the decision, that the decision is not simply the independent product of a particular combination of objective facts. In short, the former perspective views the converstion step simply as a "conduit" through which inputs become outputs; the latter views the conversion as the all-important point at which a number of judge-related factors, by being brought to bear upon the "facts," significantly affect the ultimate decision. In order to truly understand sentencing, say proponents of the latter view, one cannot ignore those factors (e.g., judicial attitudes) that are brought to bear on the so-called "facts" of each individual case.

The comparative utility or validity of the two approaches is not at issue here; both commend themselves. In fact, the question of merit is an empirical one. If a substantial portion of the variation in sentence can be accounted for in terms of conventional "input" factors, then criticism of the "black box" perspective because it ignores the particulars of the "conversion box" is perhaps inappropriate.[3]

Jurimetrics—the study of judicial decisionmaking—has been enriched by recent strides in statistical analysis and computer technology. Because of the complex interrelationships among myriad factors and the massive numbers of individual cases to be analyzed, comprehensive analysis of the sentencing decision was impractical. Until very recently, most empirical accounts of judical decisionmaking were limited to anecdotal recounts of select cases. At best, the empirical focus on variations in criminal sentences extends back no more than 5 decades, to a time when questions of possible inequities in sentencing were just beginning to be systematically researched.[4]

Since that time, rigorous empirical studies of sentence variations have rarely appeared in legal and sociological annals, being far outnumbered by accounts of the *presumed* inequities of sentencing. It is not clear whether the absence of systematic empirical analysis was a function of a lack of appreciation for the statistical approach, the overwhelming nature of analysis of dozens of variables and thousands of cases, or the modest and unsensational nature of the findings of early studies. As is usually

[3]The apparent preference of statisticians for the "black box" perspective is possibly an unconscious product of expedience. Inasmuch as formal judicial records are more likely to include information about the offense, the offender, and the process by which he was convicted (i.e., traditional "input" variables) than about the social, political, and penological perspective of the judge (i.e., conversion elements), one might reasonably expect research to prefer—albeit tacitly—the former model.

It is appropriate to note that the research proposed here synthesizes the two approaches. The analytical perspective is principally input-output. However, a number of factors that might be viewed as "inputs" can, in fact, be properly thought of as "conversion" elements, e.g., workload of the court and judge, general dispositional features of the court, and the like. Moreover, analysis of possible interaction among "input" and "conversion" elements can speak to the variable impact that certain "facts" might have when acting conjointly with different conversion "sets," an issue of concern to proponents of the latter model.

[4]Thorsten Sellin introduced the topic of judicial discrimination to research in a 1928 work, "The Negro Criminal: A Statistical Note," 140 **The Annals of the American Academy of Political and Social Science** 52 (1928).

the case, each factor likely had some effect on the slow rate of development of the empirical perspective.

Even as empirical studies emerged, however, they exhibited a variety of significant limitations. Few suffered all of the shortcomings that will be discussed, but none has been free of them all. Irrespective of minor differences in focus, design, and methodology, most sentencing studies have been characterized by a number of common dimensions. For the most part, they have generally approached the sentencing question as an input-output construct, treating a few specific offender-related variables as inputs and examining their single or combined impact on the output—usually defined as length of prison sentence. Most inquiries have also been ex post facto analyses of institutionally collected data, employing few, if any, levels of control, and using measures of statistical significance in the absence of appropriate measures of association.[5]

The limited findings generated by some studies are noteworthy in suggesting legislative and judicial sources of variation and explaining the sentencing decision in piecemeal fashion, but few have significantly increased our understanding of sentencing by measuring the singular and joint effects of specific factors or by assessing the degree to which their significance may vary from court to court, across region, and over time. Even the works of those who met the analytical challenge display infirmities or limitations of design and/or analysis that rendered their findings equivocal.

It is important, however, that previous research has defined some of the relevant parameters of the sentencing process, identified a number of factors critical to the decision, and, to a limited extent, suggested some tenable hypotheses about the possible spurious effects of a number of variables. The value of these studies derives, therefore, from an ability to recognize and learn from their shortcomings and to build upon their strengths.

The Study of Sentencing to Date

Johnson was one of the first to attempt to systematically explain sentence variance in terms of observable criteria.[6] His investigation of homicides in North Carolina, Georgia and Virginia from 1930 to 1940 "verified" his hypothesis that sentence severity was a function first of the race of the victim and second of the race of the offender, that is, blacks who assailed whites were accorded the harshest treatment (in terms of sentence length); this was followed by white offenders against white victims, black offenders against black victims, and white offenders against black victims. Thus, for homicide in part of the South, Johnson concluded that blacks were systematically discriminated against in sentences imposed during the fourth decade of this century.

Garfinkel amplified Johnson's study by distinguishing two degrees of homicide and including manslaughter in an analysis of North Carolina data for the same years.[7] Besides increasing the sample size by 25 percent over Johnson's, Garfinkel considered a number of dependent variables besides sentence severity as indices of systematic discrimination. These included the percentage of offenders in each of the four combinations (i.e., black offender-white victim, white offender-white victim, black offender-black victim, white offender-black victim) who were indicted, charged, and convicted and the percentage for whom charges were reduced. In his assessment of sentence severity, Garfinkel also tabulated the respective percentages of each of the four offender types who were sentenced to death or life imprisonment as opposed to other, less severe penalties for first degree murder. The sentencing

[5]The failure of many studies to distinguish *statistical* from substantive significance has drawn severe criticism from many quarters. One student summarizes the distinction thus:

> A relationship is considered statistically significant where we have established, subject to an accepted risk of error, that *there is* a relationship between two variables. Separate from the issue of whether a relationship exists is the question of *how strong* the relationship is. The strength of a relationship is indicated by a measure of association. Tests of significance are inappropriate for this purpose because they are markedly influenced by the size of the sample involved. For example, when the sample size is large, as is usually the case in studies of sentencing, it is generally quite easy to establish statistical significance for even a very small relationship. Within the context of large samples, then, one says very little by indicating that a relationship is "statistically significant."

J. Hagan, "Extra-Legal Attributes and Criminal Sentencing: An Assessment of a Sociological Viewpoint," **Law and Society** 357, 361 (Spring 1972). For additional discussion of the issue of statistical significance, see Selvin, "A Critique of Tests of Significance in Survey Research," 22 **American Sociological Review** 519 (1957); H. Blalock, Jr., **Social Statistics** (2d ed.) (New York: McGraw-Hill) 1972; and Labovitz, "The Nonutility of Significance Tests: The Significance of Tests of Significance Reconsidered," 13 **Pacific Sociological Review** 141, 143 (1969).

[6]Johnson, "The Negro and Crime," 271 **Annals** 93 (1941).

[7]Garfinkel, "Research on Inter- and Intra-racial Homicides," 27 **Social Forces** 369 (1949).

pattern was clear, inasmuch as the order of decreasing severity among the four offender-victim groupings was the same as that yielded by Johnson's study. Garfinkel's results, apparently confirming Johnson's findings of sentencing discrimination as a dual function of the races of both offender and victim, appear in Table 1. However, it cannot be overemphasized that both Johnson and Garfinkel failed to investigate the existence of legally relevant antecedent or intervening variables that may have accounted for the variation ascribed to racial differences.

In their study of 662 homicides in urban Cleveland between 1947 and 1954, Bensing and Schroeder demonstrated the methodological frailty of designs that fail to control for important outcome-related factors.[8] Like Garfinkel, they discovered a statistically significant difference between the proportion of black offenders with white victims and white offenders with black victims who were "convicted (of homicide) as charged": 46 percent versus 0 percent, respectively. But where Garfinkel had suggested that the difference might be a function of racial prejudice, Bensing and Schroeder attributed the apparent distinction to the impact of an important legally relevant intervening variable—whether the defendant had been charged with "felony murder"[9] as opposed to some *other* form of first degree murder. Because the legal definition of felony murder makes it more easily proven than other forms of first degree murder, an offender charged with felony murder is more likely to be "convicted as charged" than is one who was originally charged with a nonfelony murder in the first degree.

The significance of the legal distinction lies in Bensing and Schroeder's finding that significantly more black offenders with white victims than white offenders with black victims were originally charged with felony murder. With a greater probability of having been charged with felony murder, black offenders would be expected to exhibit a higher "convicted as charged" ratio than white offenders. Thus, it is certainly reasonable that the apparent correlation between race and being convicted as charged results from the intervening correlations between race and being charged with felony murder and, simultaneously, between being charged with felony murder and being convicted as charged. The latter correlation, of course, reflects a legally relevant basis for the differential outcome.[10] Although Bensing and Schroeder were not addressing the possibility of *sentencing* discrimination, per se, their findings present compelling implications for studies like those of Johnson and Garfinkel that fail to examine the existence of legally relevant intervening factors that may correlate significantly with both race and outcome.

TABLE 1 Offenders convicted of first degree murder, by sentence and race of offender and victim

[Percent]

	Sentences of life imprisonment or death	Sentences other than life imprisonment or death
Black offender—white victim	54	46
White offender—white victim	19	81
Black offender—black victim	4	96
White offender—black victim	0	100

Source: Garfinkel, "Research on Inter- and Intra-racial Homicides," 27 **Social Forces** 369 (1949).

In a 1958 study of sentencing for murder, rape, and burglary in Texas, Bullock appeared to confirm the findings of Johnson and Garfinkel, but only by a dubious post hoc interpretation of his findings.[11] Defining a "short" sentence as imprisonment for 10 or fewer years and a "long" sentence as imprisonment for more than 10 years, Bullock found that for murder, whites were significantly ($p < .01$) more likely than blacks to get a "long" sentence. For burglary, however, he found the inverse, blacks being more likely than whites ($p < .001$) to get a long sentence.

In an effort to "explain" this apparent inconsistency, Bullock made some assumptions about the

[8] R. Bensing and O. Schroeder, **Homicide in an Urban Community** (Springfield, Ill.: Charles C. Thomas Co.) 1960.

[9] Felony murder involves a homicide—whether originally intended or not—in the course of or as a consequence of any felony. In contrast, most other instances of first degree murder require that the homicide occur with "malice aforethought" or be otherwise premeditated.

[10] Of course, there always remains the possibility of low-visibility police or prosecutorial prejudice during charging. Unfortunately, when the focus is on judicial discrimination, instances of equally important overt and covert discrimination that may exist at other points along the procedural spectrum are not and cannot always be addressed.

[11] Bullock, "Significance of the Racial Factor in the Length of Prison Sentence," 22 **J. Crim. L. C. and P. S.** 411 (1961).

nature of the victim-offender relationship for the crimes of murder and burglary—assumptions necessitated by the fact that Bullock had no specific victim-related data. In the first place, murder was assumed to be basically intra-racial,[12] meaning that murders were most likely either black offenders with black victims or white offenders with white victims. Given these pairs, white offenders would be expected to receive the harsher sentences—just as Bullock had found—because, according to the findings of Johnson and Garfinkel, offenders with white victims (regardless of the offenders' own race) received longer sentences than offenders with black victims. However, burglary was assumed to be basically intra-racial for white offenders (making them white offenders with white victims) and interracial for black offenders (making them black offenders with white victims). Once again, against Johnson's and Garfinkel's earlier findings that if the race of the victim is held constant, black offenders would receive longer sentences than white offenders, Bullock's findings make sense.

It cannot be overemphasized that Bullock did not really contradict the findings of Bensing and Schroeder; nor, in fact, did he actually verify the findings of his predecessors. Rather, he accepted and invoked their conclusions to reconcile what was otherwise a troublesome inconsistency in his own findings. Since his data did not include information about the actual case-by-case victim-offender relationships for the offenses studied, his inferences about discriminatory sentencing transcend the bounds of his data.

Commendably, however, Bullock did introduce two significant dimensions to the analysis of factors relevant to differential sentencing patterns—the impact of geography and urbanization. Holding race and offense constant, he found that in east Texas, convicted offenders were sentenced to longer terms than in west Texas ($p < .001$), and that "large-city" counties in Texas sentenced defendants to longer terms than did "small-city" counties ($p < .01$).[13]

Seldom have researchers in this area attempted to replicate studies over time. A rare exception is a study conducted by Vines and Jacob focusing on 4,000 cases disposed of in 1954, 1958, and 1960 in the Orleans Parish Court in Louisiana.[14] The study examined the impact of race on both dismissal rate and the likelihood of being sentenced to prison for a year or more. The 1954 data indicated little difference in the court's treatment of blacks and whites in terms of the two measures used. Dismissal rates for the two groups differed by only 1.6 percent points in 1954; similarly, the respective proportions of black and white offenders sentenced to a year or more in prison differed by only 2.8 percent points.

The next few years, however, brought evidence of increasingly harsh treatment of black offenders. By 1958, the discrepancy in dismissal rates for the two groups increased to 13.8 percent points ($p < .05$)[15] with black offenders having lower dismissal rates than whites. In 1960, the difference in the proportion of offenders being sentenced to a year or more in prison grew to 13.5 percent points ($p < .05$) with blacks being more likely than whites to receive the longer term.

If black offenders were guilty of more serious offenses than whites, the differences in treatment might, of course have been appropriate. However, the authors suggest that these patterns persisted for most of the offenses studied, although at that level of analysis, they provided no measures of statistical significance or of association. Nevertheless, the study did formally address the dynamics of sentencing patterns, attempting to assess empirically the degree to which certain sentencing patterns change over time.

Where statistical measures of the impact of various factors on sentencing are adequate, other limitations of design seem to remain, rendering the findings similarly tenuous. In a now classic study of race and the death penalty for rape cases in 11 Southern States, researchers found that knowing the inter- or intra-racial nature of rape cases significantly and strongly enhanced their ability to predict a life or death sentence for the defendant. Interracial cases received a disproportionately high rate of death sentences.[16] Although the authors state that controlling for prior record failed to eliminate the statistical significance of the association between race and type of sentence, it is not made clear what effects these controls had on the magnitude of the measure of association, nor what effects additional relevant controls might have had. Consequently, the

[12] Ibid., p. 16, note 21.

[13] A "large-city" county was defined as a county with at least one city of 50,000 population or more. The remainder were designated "small-city" counties.

[14] Jacob and Vines, "Studies in Judicial Politics," 8 **Tulane Studies in Political Science** 77 (1963).

[15] The .05 level of significance was used in the aggregate analysis of all defendants. However, in the offense-specific analysis, no tests of statistical significance were computed.

[16] M. Wolfang and M. Riedl, "Race, Judicial Discretion, and the Death Penalty," 407 **Annals of the American Academy of Political and Social Sciences** 119 (1973).

impact of race on sentencing remained an unsettled issue.[17]

A study by Nagel systematically compared Federal and State sentencing patterns for grand larceny and assault.[18] Using data from 194 "representatively chosen" counties in all 50 States and Federal data from all Federal district courts, Nagel dichotomized offenders along the following dimensions: social class, sex, race, age, education, and the degree of urbanization and industrialization of the region served by the sentencing court. He tabulated the respective proportions of each type of offender: those who received a preliminary hearing, were awarded bail, had a lawyer, experienced more than a 2-month delay pending disposition, received a jury trial, were convicted, were sentenced to prison, and were sentenced to more than a year in prison. The design of the study must be commended for including a variety of dimensions in the analysis of differential sentencing and, particularly, for distinguishing State from Federal sentencing. However, its simple tabular presentation coupled with the lack of more sophisticated types of measurement[19] seriously limits its utility in fostering any conclusions about judicial decisionmaking.

One of the more recent studies seeking to account for variations in sentencing lengths was that of Chiricos, Waldo, and Marston,[20] who examined data on 2,583 offenders received by the Florida Division of Corrections from 1969 to 1970. The focal population consisted of those offenders who had been convicted of any of the 10 crimes accounting for the greatest number of prison admissions in that year.[21]

Prior record, age, socioeconomic status, and urbanization of the court's jurisdiction were each dichotomized and, in turn, held constant, while the relationships between race and sentence length were examined. Prior to controlling these factors, blacks were found to have received longer sentences for crimes of violence; whites were found to have received longer (but not significantly longer) sentences for three property crimes and escape. However, as controls were introduced, these patterns changed substantially. For example, controlling for number of prior sentences, the study found that among offenders with one or no prior sentence, blacks received longer sentences than whites for every offense except auto theft. The only statistically significant difference, however, was for armed robbery, for which blacks received substantially longer sentences than whites with comparable records (154.8 months versus 85.7 months, respectively). Conversely, for offenders with two or more prior sentences whites received longer sentences than blacks for 7 of 10 offenses (significant only for grand larceny and narcotics). Blacks with more than one prior sentence received longer sentences than whites with comparable records for three crimes—rape, unarmed robbery, and aggravated assault; the difference was statistically significant only for the first two.

When age was dichotomized, it was found that for offenders younger than 21, blacks received longer sentences than whites for six of nine offenses; the difference was statistically significant for only armed and unarmed robbery. Young whites convicted of narcotics offenses received significantly longer sentences than young blacks. In the 21 years or older group, blacks received significantly longer sentences for rape and whites received significantly longer sentences for larceny and escape.

For low socioeconomic status (SES) offenders, blacks received significantly longer sentences than whites for all four violent crimes. Whites were more likely to receive a longer sentence than blacks for one property crime—grand larceny. For high SES, however, the correlation between race and sentence almost disappears. The only significant differences related to longer sentences for high-SES whites convicted of forgery or auto theft.

Finally, the urban/rural nature of the sentencing jurisdiction appears to have been significantly related to sentence. In urban counties, blacks received longer sentences than whites for assault and armed and unarmed robbery. For no offense did white offenders receive a significantly longer mean sentence

[17]Hagan, op. cit., provides an excellent review and partial statistical reconstruction of 20 notable research projects on sentencing. In his summary of the impact of race on sentencing, he states that studies focusing on noncapital cases showed no relationship between race and sentence. Three of five studies on sentencing of capital cases (four of the five were done in the South) found that race played a significant role in sentencing; the only study reviewed that was conducted outside of the South found no relationship between race and sentence at the third level of control.

[18]Nagel, "Disparities in Criminal Procedure," 14 **UCLA L. Rev.** 1272 (1967).

[19]Beyond the frequent notation of percentage differences unaccompanied by T-test or any comparable evaluative measure, the most sophisticated statistics used included four tables of zero-order correlations between the several "offender" and "process" variables, controlling offense and type of jurisdiction. Otherwise, the morass of figures cited were simple percentage distributions of offender types into alternative outcome dichotomies.

[20]Chiricos, Waldo, and Marston, "Race, Crime and Sentence Length" (Paper presented at the American Sociological Association Conference), New Orleans, 1972.

[21]The crimes included rape, aggravated assault, armed and unarmed robbery, grand larceny, forgery, auto theft, burglary, narcotics, and escape.

than black offenders. In rural counties, blacks received significantly longer sentences than did whites for rape; the inverse was true for auto theft.

If the profound influence of introducing even one control on the relationship between race and sentence length can be taken as indicative of the influence of even more controls, it would be interesting to test the strength of these correlations by introducing additional controls. Unfortunately, the authors of this study reported no higher levels of analyses than those reported here. Moreover, although findings of statistical significance are instructive with respect to whether a genuine relationship exists, they are of little help in suggesting the *strength* of that relationship. Unfortunately, this study reported no measures of association.

Green's 1961 study of 333 defendants sentenced by the criminal court of Philadelphia in 1956-57 addresses the racial issue more rigorously than any of its antecedents and offers compelling evidence of the critical significance of second- and third-level controls.[22] Like so many others, Green found that at the zero-order level of analysis, blacks were significantly ($p < .01$) more likely than whites to receive a severe sentence. However, when offense (burglary, robbery, and auto theft) and the number of previous convictions were held constant, differences in the treatment of black and white offenders vanished.

His subsequent analysis of the same data in 1964[23] generated some fascinating hypotheses about the relation of race to a number of legally relevant sentencing factors, suggesting that the alleged correlation between race and sentence may be only spurious. In this regard, Green claims that

> . . . patterns of criminal behavior constituting a given offense differ intrinsically not only between the races, but within each race according to the race of the victim . . . *[S]uch differences are legally sufficient to account for the apparent racial differentiation in sentencing.*[24] (Emphasis added)

In essence, Green is saying that robbery involving a black offender with a white victim, for example, embodies an entirely different set of behavior(s) and is, therefore, a "different" crime (for the purpose of sentencing) than a robbery in which both offender and victim are black; moreover, those offenses are *behaviorally* distinct from robberies involving a white offender-black victim or a white offender-white victim. Specifically, his analysis indicated that 61 percent of black robbery offenders whose victims were white used a weapon in the course of the crime (versus 13 percent of black offenders with black victims); that 73 percent of the former (versus 38 percent of the latter) had more than one indictment against them; and that 33 percent versus 18 percent of the two groups, respectively, had a prior conviction for either robbery or some other felony against the person.

Those three criteria—use of a weapon, multiple offense, and prior record—are all independently related to sentence outcome, and appropriately so. Consequently, the discrimination in the sentencing of black robbery defendants on the basis of the race of their victims, Green concludes, is not racial at all, but is founded on criteria that are of compelling legal relevance to the sentencing decision. The existence of significant intervening and antecedent variables is the ever-present possibility in social science research and one to which students of sentencing must remain acutely sensitive.

Only very recently have sentencing studies begun to use mass data processing and sophisticated methods of analysis. Three studies, in particular, merit attention in this review of the new methodology.

One of the most extensive and comprehensive studies of sentencing discretion addresses the rather narrow question of jury sentencing in capital cases.[25] The study scrutinized some 178 items of information about each of 238 California cases disposed of from 1958 to 1966 in which the defendant faced a possible death penalty at sentencing.[26] Although the study focused on a particular dimension of sentencing—that is, the death penalty—that bears little substantive significance to the research proposed here, its design, and the inferences it makes about the decision process merit attention. Using a combination of regression analysis, factor analysis, cluster analysis, and partial correlations, the authors were able to identify and assess the significance and relative priority of dozens of variables associated with dichotomous life and death sentence alternatives for capital crimes.

The single variable that most accurately predicted the jury decision was whether the defendant's prior criminal record was introduced to the jury.

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[22] E. Green, **Judicial Attitudes Toward Sentencing** (London: MacMillan and Co., Ltd.) 1961.

[23] Green, "Inter- and Intra-Racial Crime Relative to Sentencing," 55 **J. Crim. L. C. and P. S.** 348 (1964).

[24] Ibid., 349-350.

[25] "Standardless Sentencing," 21 **Stanford L. Rev.** 1297 (1969).

[26] The death penalty was invoked in 103 of the cases.

Second in importance was the socioeconomic status of the defendant. In this regard, a blue-collar background was demonstrably injurious and a white-collar background advantageous to the defendants convicted of capital crimes. When the effects of *all* other variables, including job stability and employment, were controlled, the socioeconomic factor dropped slightly below the threshold of statistical significance ($.05 < p < .1$). Other characteristics bearing a strong positive association with a death sentence included having poor job stability, having sole responsibility for killing at least one victim, having actively resisted arrest, having committed rape or kidnaping in conjunction with murder, *not* having been under the influence of alcohol at the time of the crime, having unsuccessfully invoked an insanity defense, and having a co-defendant testify for the prosecution. It is interesting to note that when these kinds of factors were held constant, race and type of counsel were found to bear *no* significant linear relationship to the sentencing decision.

Another study of note is a recently completed analysis of sentencing in the Philadelphia Criminal Courts.[27] The study analyzed the sentences imposed on 8,119 defendants convicted in the Philadelphia Court of Quarter Sessions for calendar year 1964. Twenty-seven crime groups and 24 independent variables were used in predicting sentence severity, which was coded as an interval-level continuum from 1 to 14, ranging from suspended sentence to death, respectively.[28]

A regression equation was produced for each offense. Then comparisions, in terms of total variation accounted for and variables of major importance in explaining variation, were made across offenses. Generally, for all offenses considered, pretrial status of the defendant ("detained" versus "out on bail" or "released on own recognizance") seemed to be the most consistently important variable in accounting for sentence variation, because it appeared as a salient factor in the regression solutions of 25 of the 27 offenses studied. Record of prior arrests and convictions for *similar* crimes ranked second; record of prior arrests and convictions for *other* crimes ranked third. If *total* prior record is considered, it becomes the most important predictor. Other variables that proved significantly related to outcome in a majority of the solutions included time from indictment to trial and the number of indictments.[29] Significant for a half dozen offenses were the length of time on the bench, political party, and age of the sentencing judge, and the age of the offender. Engle found that the method of conviction (plea of guilty versus trial) had little independent effect on sentence severity in Philadelphia.[30]

Of course, all the caveats of regression analysis must be recalled in this assessment of Engle's findings, particularly the insensitivity of the method to other than linear relationships, the nature of the stepwise solution, and the tendency of the regression solution to capitalize on chance variation, especially when small samples are involved. Because of the algorithm of the stepwise solution, it is possible for the effect of a "significant" variable (X_2) to be effectively masked if it is highly correlated with any other predictor variable (X_1) that manifests only a slightly stronger relationship with the dependent variable (Y) than it (X_2) does, itself. In such a case, the criterion variable exhibiting the stronger zero-order

[27] C. Engle, **Criminal Justice in the City: A Study of Sentence Severity and Variation in the Philadelphia Criminal Court System** (Unpublished Ph.D. Dissertation: Temple University) 1971.

[28] The validity of the scale, as the author concedes, is questionable, since one would be hard-pressed to defend the implied proposition that death is only 14 times as "severe" as a suspended sentence and twice as severe as imprisonment for 1 1/2 to 2 1/2 years (assigned a weight of 7). Similarly tenuous is the implied equivalence of the increment in severity from a fine (2) to probation (3) with the increment from a sentence of life imprisonment (13) with the prospect of parole to a sentence of death (14). It is difficult to estimate the effects of this kind of coded scale on the "substantive findings."

On the other hand, measurement of severity of imprisonment might be illusory on any scale. One might even make the case that imprisonment in months is not a truly valid interval measure of severity of sentence: that a sentence of 1 year is much more than twice as severe as a sentence of 6 months; or that a sentence of 20 years is much more than double the severity of a 10 year sentence. Moreover, at a subtler level, the variable formulae for good-time computation exacerbate an already complex methodological issue. Thus, the utility of an unvalidated scale that incorporates suspended sentences, monetary fines, probation, death, and variously defined intervals of imprisonment (some categories reflect 3-month-long terms and some define terms of indefinite length, e.g., over 15 years) is problematic.

[29] As long as additional predictors were "statistically significant," they were included in Engle's discussion. Consequently, many of the variables deemed "significant" bear little substantive import, as they account for little more than 1 or 2 percent of the variance in the dependent variable. By the same token, the reported multiple R's include every variable which entered the stepwise solutions at a statistically significant level, even though many of these predictors accounted for less than 1 percent of the variance. As a result, most of the multiple R's which were reported are inflated, as they strongly capitalize on chance variation.

[30] Ibid., pp. 244-245. Specifically, a plea of guilty was negatively correlated with sentence severity for drug, sex, and auto offenses and *serious* crimes against person or property. Engle also explored the impact of type of trial (jury versus court), finding that a jury conviction is generally associated with a more severe sentence.

correlation (X_1) with the dependent variable will appear significant in the solution; the second variable (X_2) will appear to have no effect, because it will emerge much later, if at all, in the solution. Consequently, if there are substantial intercorrelations among predictor variables, the substantive interpretation of regression output can be difficult.

A unique contribution of Engle's work of special relevance to this study is the computation of the multiple R^2 for each of the offenses analyzed. In short, this statistic reflects the amount of variance in the dependent variable, sentence severity, that can be accounted for in terms of variance in all of the independent variables that were introduced in the analysis. As one might expect, the amount of variance in sentence severity that can be explained by various predictor variables varies widely by offense. In Engle's study, for example, the proportion of explained variance in sentence severity ranged from 19 percent (gambling) to 28 percent (motor vehicle code violations), to 52 percent (possession of drugs), to 82 percent (sale of drugs),[31] with criminal record being consistently among the best predictors. An interesting feature of Engle's findings is that although the proportion of explained variance fluctuates considerably by offense, the predictor variables that best account for that variance are generally consistent across offense.

A recent addition to sentencing literature is the work of Hogarth in Ontario, Canada.[32] Others[33] have addressed the issue of judicial attitudes as instrumental in judicial decisionmaking, but none have done so more extensively than Hogarth. From 100 items of information—on the age, race, occupation, marital status, and criminal record of the defendant; the offense charged; the manner by which the case was disposed of; the presentence report; the attitudes, penal philosophies, and job-related perceptions of the judges—derived from 71 full-time magistrates and 2,500 cases involving six offenses, the study sought to identify those factors most relevant to the sentence outcome. Using multiple regression techniques, Hogarth concluded that the sentencing behavior of Ontario judges was attributable in large part to the various attitudes, perceptions, predispositions, and "cognitive complexity" of the magistrates concerned. The impact of conventionally defined inputs like the age, race, sex, prior record of the defendant, was relegated to secondary status.[34]

Hogarth has been among the most vocal critics of sentencing research that ignores so-called "conversion" elements, like judicial attitudes and philosophies. Although this study will not marshal the variety of sophisticated measures of judicial attitudes that Hogarth employed, its findings do provide grounds for seriously questioning Hogarth's skepticism about the predictive utility of traditional "input" factors.

Because sentence variation is probably a product of both judicial idiosyncrasy and variation deriving from regionally-based subcultural norms and because the two are likely to be related,[35] it has been particularly difficult to attribute variation in sentence exclusively to either source. Nevertheless, a few studies have attempted to address the question. Hood's study of English magistrates' courts in the fifties, for example, found that the imprisonment policies of magistrates related to *both* the social characteristics of the area and the social class of the bench. Middle-class magistrates in relatively small and "stable" communities (in contrast to their counterparts in other types of communities), for instance, were found to sentence working-class offenders rather severely relative to middle-class offenders. The findings generally suggest that judicial attitudes and predispositions may interact with both the social environment of the court and with factors relating specifically to the cases before it.

The comparability of data across Federal district courts makes the Federal judicial system an ideal focus for the study of geographical variation. A few researchers have used the Federal data in just that fashion. However, a serious methodological problem attends many studies that assume this perspective, a problem arising from the use of the *district* rather than the *individual defendant* as the

[31]Ibid., pp. 87-88.

[32]J. Hogarth, **Sentencing as a Human Process** (Toronto: University of Toronto Press) 1971. For an excellent series of critiques of Hogarth's study, see "International Review Symposium—**Sentencing as a Human Process** by John Hogarth," 10 **Osgood Hall L. J.** 233 (Aug. 1972).

[33]McGuire and Holtzoff, "The Problems of Sentencing in the Criminal Law" 413 **B.U.L. Rev.** 426 (1940); Gaudet, "The Sentencing Behavior of the Judge" in Brandom and Katash, eds., **Encyclopedia of Criminology** 449 (1949); R. Hood, **Sentencing in Magistrates' Courts: A Study in Variance of Policy** (1962); Shoham, "The Procedure and Sentencing Powers of the Criminal Courts in Israel," in **Crime and Social Deviation** (1966); E. Green, **Judicial Attitudes**, op. cit.

[34]J. Hogarth, op. cit., p. 163.

[35]See Hogarth's discussion, op. cit., p. 163. "...some magistrates may be responding, both in their sentencing behavior and in their attitudes, to public opinion in the community in which they are situated."

unit of analysis. In studies that suffer this so-called ecological fallacy, independent variables—arrest and conviction records, offense, method of conviction, time interval for disposition of the case, and type of counsel—are generally measured in terms of the *percentage of defendants* in the specified categories of these types of variables for *each court*. Then the analysis often proceeds under the assumption that a correlation between, say, having a high proportion of defendants with prior records and exhibiting a high average sentence length, truly signifies a correlation between an *individual's* having a prior record and receiving a long sentence. The assumption is, of course, unwarranted. Although the value of the statistical analysis is severely limited by the ecological problem, ecological studies can still point to broad variation in sentencing patterns and suggest some possible bases of that variation.[36] Definitive statements about the propriety of those variations, however, cannot be made on the basis of the information generated therein. For that reason, it is critical that the focus be on the individual, whenever possible and not some aggregate of individuals as the unit of analysis, particularly if inferences about *individual* outcomes are planned.

A recent study by Green involves a new and unique approach to the question of sentencing variation.[37] Green sought to determine the extent to which a judge's sentencing decision in any given case might be influenced by his decision in the immediately preceding case. The study focused on sentences imposed by 21 judges in 1,437 consecutive convictions in a 17-month period from 1956 to 1957 in the Philadelphia Court of Quarter Sessions.[38] In an earlier study[39] Green found that the three variables most strongly related to sentence were (1) offense, (2) number of indictments against the accused, and (3) number of prior felony convictions. On the basis of these criteria, Green grouped all the cases to be studied into categories so that all cases being compared were "legally equivalent" in terms of the three factors just mentioned. Subsequently, he tested the null hypotheses that (1) sentences imposed for legally equivalent cases do not differ significantly from the mean for the respective groups and that (2) if they do differ, then the difference is unrelated to the severity of the sentence imposed in the immediately preceding case.[40]

Both hypotheses were rejected because Green found that sentences imposed for legally equivalent cases did vary significantly and that, more importantly, they appeared to be affected by both the relative and absolute severity of the sentences imposed in the respective antecedent cases. Green concluded:

> Lacking an explicit internal standard, the relative severity of the sentence . . . [the judge] imposes in a particular case requires the force of an external anchor. It [any sentence] becomes a precedent, as it were, for the judgement of the gravity of the next case.[41]

In short, even though a case might be wholly unrelated (consecutive cases involving co-defendants convicted of the same crime(s) were excluded from the analysis) and unlike the case following it, it appears to affect the sentence the judge imposes in the later case, just as it appears to have been influenced by the judge's decision in the immediately preceding case.

According to Green, any decisionmaker is forced to seek some set of standards by which to guide or regulate his or her decisions. When a concrete external or internal standard is absent, the judge is compelled (perhaps unconsciously) to invent one. As Green found,

> . . . the judge is thrust into undue reliance on his most recent experience in sentencing; the greater the resemblance between the stimulus case and the preceding case, the more powerful the anchoring effect of the preceding case.[42]

Green's work is at least as important to the nature of the sentencing decision as it is to the understanding of factors that influence sentence outcome. One of the objectives of this series of reports on criminal sentencing is to explore the need for (and the plausibility of) hard criteria to guide the imposition of sentence. Green's finding is certainly compelling evidence of this need.

[36]A good example of the ecological problem can be found in Harries and Lura, "The Geography of Justice: Sentencing Variation in U.S. Judicial Districts," 57 **Judicature** 392 (April 1974).

[37]Green, "The Effect of Stimulus Arrangements on Normative Judgement in the Award of Penal Sanctions," 31 **Sociometry** 125 (June 1968).

[38]A total of 381 cases were excluded: 218 were the first case of the day; 163 involved consecutive trials of co-defendants charged with similar or identical offenses.

[39]E. Green, **Judicial Attitudes**, op. cit.

[40]The severity of preceding cases was measured in both absolute and relative terms. The absolute measure was a trichotomy based on whether the defendant received (1) a nonprison sentence, (2) a prison sentence of less than 12 months, or (3) a prison sentence of 12 months or more. For the relative measure, each case was compared to the median sentence imposed for all legally equivalent cases, and was classified as severe or lenient, depending on whether the sentence imposed in that case was above or below the median sentence for equivalent cases.

[41]Green, "The Effect of Stimulus Arrangements," op. cit., p. 136.

[42]Ibid.

Although strict "laboratory" experiments in the area of sentence decisionmaking are not unheard of, they are rare. What the laboratory design sacrifices in the "reality of the decision setting" by requiring judges to sentence fictitious defendants, it gains in the control of factors and circumstances describing the offense, the offender, the method of disposition, and so on. The design thus enables one to examine differential judicial treatment of the same cases rather than cases *presumed* similar in a number of important respects. Therefore, the kinds of variation exhibited within this truly "experimental" design are more clearly attributable to judicial sources rather than to external criteria relating to the offense, offender, and other legally relevant considerations. In all ex post facto studies of sentencing, it is impossible to determine the precise impact of factors not included in the analysis. Because of these residual unmeasured factors, the claims of sentence disparity by researchers who have investigated only roughly comparable cases remain open to question.

A recent example of the laboratory design is the Second Circuit Study, in which the 50 Federal district court judges of the Second Circuit—including the districts of Connecticut, New York North, New York East, New York South, New York West, and Vermont—were asked to sentence 20 "individuals" based on information contained in actual presentence reports of cases selected as representative of sentencing in these courts.[43] The result was unequivocal: when presented with precisely the same set of "objective" criteria about an offender, the offense, the method of conviction, and the like, judges invoked highly "disparate" criminal sanctions. A case involving extortionate credit transactions and related income tax violations, for example, drew an incredible range of sentences—from 3 years of imprisonment to 20 years of imprisonment with a $65,000 fine.[44] Similarly, a case involving conviction for bank robbery received sentences ranging from 5 years of imprisonment to 18 years of imprisonment with a $5,000 fine.

Certainly not all the cases exhibited this magnitude of variation; nor did judges cluster around the extremes in the range of selected sentences. However, the pattern was clear. A substantial lack of consensus about the appropriate criminal sanction in each particular case was clearly the norm, rather than the exception.[45]

The second phase of the experiment was designed to explore the sentencing impact of a number of factors presumed relevant to sentence outcome—for example, whether the presentence report carried a recommendation for probation, whether or not the offender was a narcotic addict, whether she or he was convicted by trial or plea of guilty, the nature and extent of prior criminal record, and the offender's socioeconomic status. Judges were divided into two groups, *A* and *B*; members of each group were handed slightly different versions—*a* and *b*—of the same 10 case studies, the two versions of each case being distinguished along one of the dimensions just mentioned. As one would expect, the distinctions did affect the length of sentences imposed, that is, the median sentence imposed by group *A* (with version *a* of the presentence report) differed from that of group *B* (with version *b*). However, the researchers noted that despite the difference in *severity* of sentence, the respective distinctions in the versions generally failed to produce any notable difference in the magnitude of the *variation* in sentences imposed by group *A* versus group *B*.

Apart from its substantive findings, one of the most encouraging aspects of the Second Circuit Study is that it was a judicial self-study, designed and administered by judicial officials and administrators. The formulation and execution of the design, as well as the candid conclusions of the analysis, perhaps suggest that the once fashionable palliatives of "individualization," "discretionary justice," "judicial independence," and the like, which have traditionally been offered in defense of broad variations of sentence, and which have insulated the decision process from scrutiny, may be going out of vogue.[46] Although it is unlikely that a

[43]See A. Partridge and W. Eldridge, **The Second Circuit Sentencing Study: A Report to the Judges of the Second Circuit** (Washington, D.C.: Federal Judicial Center), August 1974, p. 1.

[44]Ibid., pp. 4-7.

[45]Ibid., p. 9. Comparable studies have yielded similar results. Judges from East Michigan Federal District Court sentencing council participated in the same kind of experiment a decade ago. In that study, an extortion case drew a range of sentences from a 3-month commitment for study to a 10-year prison term. See Doyle, "A Sentencing Council in Operation" 25 **Fed. Prob.** 27 (September 1961).

The findings of the Boulder Pilot Institute on Sentencing were consistent with the above. See "Discussion at Pilot Institute on Sentencing," 26 **F.R.D.** 231 (1959).

[46]A number of authors have suggested that the noble-sounding principles relating to the need for "individualization of treatment" have long excused substantial variations in criminal sanction by camouflaging what is, in fact, little more than judicial arbitrariness or caprice. See M. Frankel, **Criminal Sentences: Law Without Order** (New York: Hill and Wang) 1972, pp. 9-10, 103, 111. Also, see Wilkins, "Directions for Corrections" (Paper presented before the American Philosophical Society), November, 1973.

single study of the dimension of the Second Circuit Study will revolutionize criminal sentencing, it is significant that a group of judges have responsibly submitted themselves to serious scrutiny without apology for or compromise of the results of that analysis. In this sense, the study could mark an important step in the judiciary's willingness to endure scrutiny and criticism in an area of decisionmaking that has traditionally been kept well beyond public view. This openness is essential to the fullest understanding of the sentencing decision and integral to any subsequent efforts to reform or standardize that process.

A Review of What Is and Is Not Known About Sentencing

If the preceding discussion strikes one as piecemeal, it is apropos, for it is characteristic of the strikingly noncumulative chronology of sentencing research. From the above discussion, a few points become particularly clear. Although many of the studies cited have made some unquestionably valuable contributions, the point remains that their contributions have been limited, that advances in the body of knowledge about sentencing have generally been both modest and tenuous. Because of a number of factors relating to the varying scope, design, and methodology of previous studies, it is difficult to reconcile incompatible findings or to compare results of incompatible designs. A brief review of the common constraints of sentencing research should clarify why knowledge about judicial decisionmaking is so limited, despite the attention it has received.

Perhaps the most pervasive and debilitating constraint of earlier studies is best characterized as conceptual myopia. Whatever the reason, nearly all studies have focused principally on a *single court*, a *single offense or type of offense* (usually violent offenses, like homicide or rape), single or few "independent" variables (usually race), and a *single point in time*. They have often relied exclusively on gross dichotomizations of variables (e.g., "long" versus "short" sentence length, "serious" versus "nonserious crimes); most have failed to introduce even the most modest kinds of controls, thus ignoring subtle but confounding possibilities of statistical interaction, spuriousness, and the like. Moreover, the analyses have been generally narrow in terms of the types of variables studied. Most have been concerned exclusively with sentence *length*, disregarding the equally important determination of whether a defendant will be imprisoned at all. Even where a design might be sensitive to these kinds of conceptual issues, there remain the "problems" of statistical analysis; for example, the frequent misuse of measures of statistical significance in the absence of critically important measures of association, and the ecological fallacy. Finally, because few have even posed the question, almost nothing is known about how sentencing patterns or practices may vary across region or change over time.

In short, because of the *micro* perspective of most inquiries, what is now "known" about sentence variation consists of a few possible inferences about the nature of sentencing practices with respect to homicide in North Carolina in the 1930's (e.g., Johnson and Garfinkel), or in urban Cleveland around 1950 (e.g., Bensing and Schroeder); with respect to murder, rape, and burglary in Texas in 1958 (e.g., Bullock); most offenses in New Orleans in the late fifties (e.g., Vines and Jacob); robbery, burglary, and theft in Philadelphia in 1956-57 (e.g., Green); 10 major offenses resulting in imprisonment in Florida in 1969 (e.g., Chiricos, et al.); capital offenses in California from 1958 to 1966 (e.g., the jury sentencing study), and so on.

Because most studies to date have been insufficient to sustain any but the most tentative hypotheses about the particular dimension of sentencing they address and have been too narrowly focused to address the sentencing decision from a broader perspective than a single court or jurisdiction at a particular time, it should not be surprising that so little is known about sentencing in general.

Little is still known, for example, about what items constitute major inputs into the determination of sentence; the relative priority of various factors in sentencing; how much variance can be explained by legally relevant compared with irrelevant factors; and the variability of sentencing policies and priorities over time and as functions of jurisdiction, caseload, and other processing features. Fortunately, the lack of understanding about sentencing is surmountable. Variations can be examined. They are susceptible to quantification and analysis by various statistical techniques by which relevant variables can be arranged in order of importance to the sentencing decision.

A New Perspective—A Proposal for Research

From the discussion of the limitations of previous research, it becomes evident that the threshold task of data collection constitutes an undertaking of

Herculean proportion. Ideally, for example, the data base should be sufficiently large to permit rigorous and detailed analysis of sentences. It should include a multiplicity of offenses. A variety of variables describing offender attributes and processing factors should also be available. In order to overcome the constraints imposed by examinations of a single jurisdiction at a single point in time, it is desirable to have comparable data available for several jurisdictions from distinctively different geographic areas; it is also necessary to have this information available over a period of several years.

The development or discovery of a data set that can satisfy these requirements *simultaneously* is problematic, and the difficulty is exacerbated by the jurisdictional and temporal requirements. State penal codes tend to differ substantially with respect to definitions of criminal offenses, statutory sentencing provisions, and criminal procedures for adjudication and sentencing. Comprehensive data collection systems tend to be rare and at best embryonic; where operational they are far from uniform in terms of the variables included and the coding of those variables. In addition, few have been in existence long enough to support analysis over any substantial period of time.

The Federal Judicial System

A variety of factors make the Federal criminal justice system an ideal focus for the study of criminal sentencing, because it is compatible with each of the methodological requirements discussed. A principal advantage of the Federal focus is the systematic method of data collection. Administrative officers of each Federal district court annually record and report the business of their respective jurisdictions to the Administrative Office of the U.S. Courts in Washington, D.C. Concerning criminal cases, a variety of data relating to the offense, the defendant, the judicial procedures by which the case was introduced and disposed of, and the sentence—if any—are all recorded for each defendant, each year. The product of this fastidious effort—coordinated by the Administrative Office—is a wealth of comparable data about defendants and sentences imposed by judges in nearly 100 different jurisdictions over the past 10 years for a variety of offenses that are essentially identical across both jurisdiction and time. Clearly, no other data collection effort relating to sentencing has yielded a body of information so rich and at the same time so encompassing as this.

Of course, because no current collection effort can claim to have included all relevant factors, it is only reasonable that this data base has "ignored" some factors about which information might be desirable. That limitation is not of undue concern, however, because those factors that are included strongly commend themselves to close analysis. Moreover, the temporal and geographical dimensions uniquely embodied in this data set permit a perspective of the sentencing decision that is largely unexplored. Certainly the merit of such an addition outweighs the shortcomings the data may otherwise be thought to suffer. Following is a brief exploration of the compatibility of the Federal data system with each of the four methodological concerns noted.

A Variety of Offenses

Because every case processed in a Federal court is coded according to the offense involved, and because the Federal penal code includes an abundance of offenses, the Federal data base poses no problems to the inclusion of several offenses in a study of judicial sentencing. Rather, the principal objection to Federal data is the alleged unrepresentativeness of Federal offenses vis-a-vis "conventional crime." The point is not without merit, but it is not altogether applicable to this design. Where it might seem troublesome, problems can be avoided by a judicious selection of the offenses to be studied.

Indeed, with respect to a number of offenses, the substantive provisions of the Federal law are much like those of most State jurisdictions. The principal differences between State and Federal codes generally involve the specific conditions surrounding the offense factors that are thought to appropriately tie the Federal government to the prosecution of the offense. Examples of these conditions include (1) committing an offense on Federal property (land reserves, national parks or waterways, military installations) or in a Federal building (e.g., a post office); (2) committing an offense against the Federal government or one of its agencies, offices, or agents (income tax evasion, robbery or theft from a post office, postal fraud, bribery of a Federal official, assaulting or killing a Federal officer, robbery of a federally-insured bank or savings institution); (3) preparing, perpetrating, or escaping from, an offense across a State boundary (e.g., interstate flight to avoid prosecution; crossing State lines to engage in illicit sexual activity; interstate transportation of a stolen motor vehicle, forged securities, stolen goods, etc.; crossing State lines to induce a riot); (4) committing an offense against an agent whose activity or business occasions the cross-

ing of State lines (larceny or theft from businesses engaged in interstate commerce).

The counterparts of these offenses—State tax evasion, bribery, robbery, theft, larceny, sex offenses, auto theft, receiving and transporting stolen goods, etc., exist in State law. The corpus delecti, or "body," of State vis-a-vis Federal offenses may be quite similar; the particular feature that elicits a Federal interest in its prosecution is often the only real distinction between them and, as indicated, often bears little relation to the "criminal" nature of the act itself. Of course, there are a few offenses over which the Federal government exercises exclusive domain, because of the unique or generalized nature of their harm or because no single State jurisdiction can claim an abiding interest in or capability of prosecuting offenders. Most of these involve Federal regulatory statutes, national boundary violations, infringements against national security, and the like. With the exception of Selective Service violations, included for other reasons, these kinds of offenses have been excluded from the analysis to be outlined here.

A Variety of Independent Variables

The comprehensive nature of the Federal data base also strongly commends it to analysis. Data relating to the offense, the defendant, and the method of conviction or disposition are routinely compiled within each district court. This information includes the specific offense for which the defendant was charged (and convicted); the age, race, sex, and prior criminal record of the defendant; whether the case was initiated by indictment, information, or waiver of indictment; the time elapsed from the filing of the case to its termination; the type of counsel representing the defendant; and the method of conviction (plea of guilty or trial by jury or court). In addition, the Administrative Office of the U.S. Courts annually reports on a variety of administrative factors characterizing the workload and operational efficiency of individual Federal district courts. Included in those reports are variables like the number of criminal cases disposed of per judge within each district, the median length of time elapsed for the disposition of criminal cases within each court, and the proportion of case dispositions effected by dismissal, conviction, etc., for individual jurisdictions. These factors can be easily coded and introduced into the analysis.

Inclusion of Several Jurisdictions and Regions

Regional analysis is a stranger to the sentencing inquiry. Although a few individuals have compared jurisdictions in terms of some aggregate measure of sentencing performance (e.g., mean sentence weight or length, percent sentenced to probation), any kind of refined attempt to contrast sentencing *patterns* of distinct regions or jurisdictions is virtually unknown.

This is one dimension for which the Federal data base is particularly well suited. Because the base is national in scope, data on *individuals* (not just districts) are available for any region of the country (see Figure 2). Focal jurisdictions can be selected on the basis of any number of relevant criteria—region, urbanization, caseload, or specific court attributes.

Moreover, as noted earlier, the exploration of regional variation generally implies a number of methodological difficulties—jurisdictional differences in statutory definitions of offenses and sentencing provisions, incomparability of variables and coding procedures, and related problems stemming from the lack of uniformity of criminal procedure from one jurisdiction to the other. The Federal perspective overcomes these problems by its built-in standardization. Naturally, the same penal code (hence, offense definitions), and code of criminal procedure operate for all the Federal districts.[47] Similarly, statistical coding and reporting activities are also uniform for each court.

Time Change

It has been suggested that much of the confusion about sentencing is a product of the incompatibility

[47]It is important to note not only that procedures are comparable across *Federal jurisdictions* but also that *Federal* and State criminal procedures are quite comparable, in themselves. That is, Federal law enforcement officials are bound by many of the same investigation, arrest, and interrogation constraints as are State and local authorities. Federal courts also operate with codes of criminal procedure that are essentially comparable to those of most States, including the stages of arraignment, indictment or information, trial by judge or jury, presentence investigation, and sentencing. For a discussion of the Federal criminal process, see Goldman and Jahnige, op. cit.

While the substantive prescriptions and proscriptions of Federal law are by no means identical to those of State codes, the essential point is that they are sufficiently comparable to sustain the kind of analysis proposed for this study and to make important inferences about the behavior of State judges on the basis of their counterparts in the Federal system.

FIGURE 2 The Federal judicial system

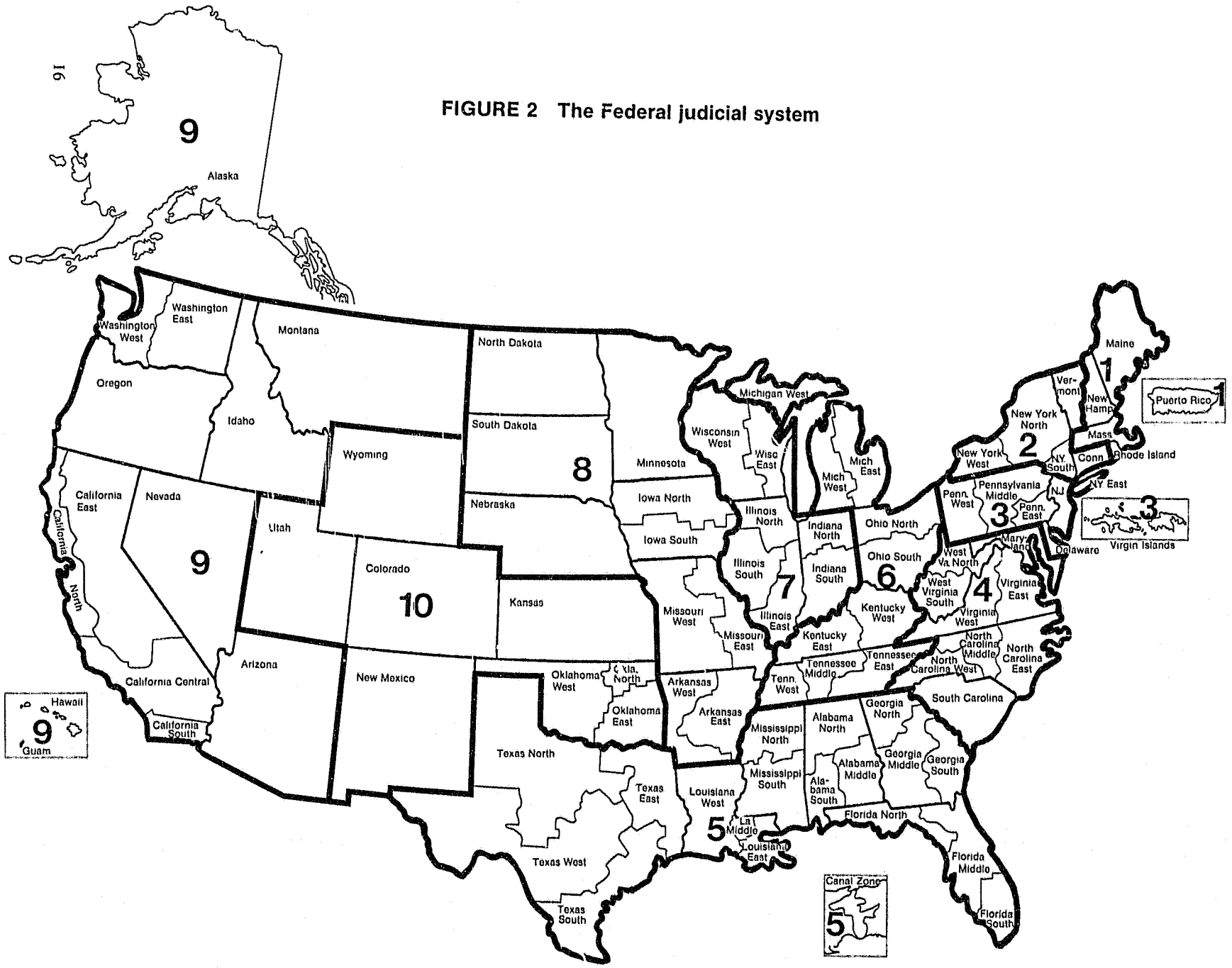


FIGURE 2 The Federal judicial system

FIRST CIRCUIT
- Maine
- Massachusetts
- New Hampshire
- Rhode Island
- Puerto Rico

SECOND CIRCUIT
- Connecticut
- New York North
- New York East
- New York South
- New York West
- Vermont

THIRD CIRCUIT
- Delaware
- New Jersey
- Pennsylvania East
- Pennsylvania Middle
- Pennsylvania West

FOURTH CIRCUIT
- Maryland
- North Carolina East
- North Carolina Middle
- North Carolina West
- South Carolina
- Virginia East
- Virginia West
- West Virginia North
- West Virginia South

FIFTH CIRCUIT
- Alabama North
- Alabama Middle
- Alabama South
- Florida North
- Florida Middle
- Florida South
- Georgia North
- Georgia Middle
- Georgia South
- Louisiana East
- Louisiana West
- Mississippi North
- Mississippi South
- Texas North
- Texas East
- Texas South
- Texas West

SIXTH CIRCUIT
- Kentucky East
- Kentucky West
- Michigan East
- Michigan West
- Ohio North
- Ohio South
- Tenessee East
- Tennessee Middle
- Tennessee West

SEVENTH CIRCUIT
- Illinois North
- Illinois East
- Illinois South
- Indiana North
- Indiana South
- Wisconsin East
- Wisconsin West

EIGHTH CIRCUIT
- Arkansas East
- Arkansas West
- Iowa North
- Iowa South
- Minnesota
- Missouri East
- Missouri West
- Nebraska
- North Dakota
- South Dakota

NINTH CIRCUIT
- Alaska
- Arizona
- California North
- California East
- California Central
- California South
- Hawaii
- Idaho
- Montana
- Nevada
- Oregon
- Washington East
- Washington West

TENTH CIRCUIT
- Colorado
- Kansas
- New Mexico
- Oklahoma North
- Oklahoma East
- Oklahoma West
- Utah
- Wyoming

[Numerals indicate the Courts of Appeals. The heavy lines represent the jurisdictional boundaries of each circuit. The thin lines represent State boundaries. The broken lines represent jurisdictional boundaries of District Courts in States having more than one district.]

of findings among previous studies. A number of analysts have suggested that the inconsistency might lie in regional or temporal variations in sentencing practices—which those studies did not attempt to address—rather than in any "error" in any of the studies, themselves.[48]

If the study of regional variation is a stranger to sentencing, then the study of temporal variation is altogether alien. Because systematic data collection is only a recent development, comprehensive systems dating back more than 5 years are rare. Again, the Federal focus is persuasive, inasmuch as crucial information on the processing of Federal defendants has been uniformly and systematically compiled by district courts for over a decade. Offense definitions and administrative procedures are identical; coding is uniform; finally, any procedural or substantive changes that might have occurred within one jurisdiction during the last 10 years would have occurred fairly uniformly within *all* of the Federal district courts, making the Federal perspective all the more compelling.

The Design

In this section, the parameters of the design that underlie forthcoming reports on criminal sentencing will be reviewed. Their principal objective will be the identification and analysis of several offender, offense, process, and court-related factors generally thought to have some bearing on the determination of criminal sentence. Specifically, their independent and combined contribution to the decisions about both *type* (prison versus probation) and *length* of sentence (prison sentences only) will be explored and evaluated. Additionally, the magnitude and direction of influence of each factor will be contrasted in terms of each type of sentencing decision. Controls will be routinely introduced in the analysis to examine the independent effects of a number of factors like race, factors that have long been suspected of having substantial impact on sentence outcome. In this regard, the analysis will also explore the range of effects of spurious and/or intervening variables as well as incidents of interaction that, in the final analysis, may serve to substantiate or obviate popular misgivings about the equity of sentencing.

In response to this issue, this study will address the variability of sentences as well as the variability in sentencing patterns across both jurisdiction and time. By controlling for these dimensions, closure may be approached on a variety of issues that remain unanswered simply because of the lack of comparability among previous designs. Throughout the analysis, the substantive theoretical focus will be the degree to which observed variations appear to be explainable and, where explainable, the extent to which variations can be explained in terms of factors generally considered legally relevant vis-a-vis those presumed legally irrelevant to the sentencing decision.

The investigation focuses on Federal offenders convicted and sentenced in U.S. District Courts during fiscal years 1964 and 1971.[49] The analysis was limited to eight felony offenses, selected on the basis of their substantive, theoretical, and numerical significance, as will be discussed shortly. These offenses will be examined from both an aggregate and an individual perspective, as the degree to which different sentencing patterns may appear to vary by offense or offense "type" is explored. Moreover, the analysis will focus on sentencing in a few specific jurisdictions, selected on the basis of location and sentencing workload.

The independent variables chosen for analysis measure a number of dimensions of presumed statistical relevance to sentence. They relate to the offense, the offender, and the process and setting that attended the conviction. The decision of whether to incarcerate and the determination of the length of imprisonment have both been included in the analysis as dependent variables, because it is quite possible that the so-called "sentencing decision" may, in fact, represent a truly bifurcated enterprise in which two separate and logically distinct determinations are made. Exploring the constellation of factors relevant to each may help to address the extent of this bifurcation, and thus to broaden understanding of sentencing behavior.

The principal methods of analysis to be used—stepwise multiple regression analysis and predictive attribute analysis—were chosen for their complementary features. These methods can process massive amounts of data and at the same time yield

[48]See, especially Hindelang, "Equality Under the Law," 60 **J. Crim. L. C. & P. S.** 306, at pp. 312-313 (1969). See also Hagan, op. cit.; and Chirlcos, et at., op. cit.

[49]This includes offenders whose cases were disposed of from July 1, 1963 to June 30, 1964, and from July 1, 1970 to June 30, 1971. The national analysis excludes cases disposed of in the District of Columbia, Guam, the Virgin Islands, Puerto Rico, and the Canal Zone.

results that present no major difficulties in interpretation. The dual approach is of particular value in allowing an exploration of specific kinds of subtle relationships among variables, that is, interaction and non-additivity, which cannot easily be detected by the regression approach alone.

Eight Focal Offenses

The decision to narrow the analysis to eight offenses was a product of several considerations. In the first place, Federal prosecutions in both 1964 and 1971 yielded more than 30,000 convictions. The analysis of such a number of records would be unwieldy as well as financially prohibitive. Moreover, that number included a variety of Federal regulatory violations of little substantive or theoretical relevance to this inquiry.[50] Conclusions about general *Federal* sentencing practices that were based on the disposition of these kinds of cases would be equivocal at best; they would likely be wholly untenable with respect to sentencing from a *State* perspective.

Criteria for Selection of Focal Offenses

Just as the aggregate analysis of all Federal offenses would pose serious methodological and inferential difficulties because of the likely differential treatment of major and minor offenses, so aggregation at *any* level must acknowledge the problems inherent in that approach. In order to be truly instructive, therefore, this analysis ought to regard each offense by itself. With this in mind, once minor offenses were removed from consideration, a number of specific criteria were invoked to guide the selection of eight focal offenses—bank robbery, bank embezzlement, larceny from businesses involved in interstate commerce, auto theft, counterfeiting, Marihuana Tax Act violations, narcotics violations, and Selective Service violations—from the several major crime categories that remained. Of course, all the selection criteria could not be applied in every case because they sometimes proved incompatible with each other. In such cases, the selection was based on a balancing of the criteria thought most relevant to the specific case being considered.

Of threshold significance to the selection of focal offenses was the number of cases available for analysis. If the number was inadequate to sustain analysis along the desired dimensions, particularly in terms of tabular and predictive attribute analysis, then the offense was summarily excluded from consideration. In addition to being a prerequisite to sustain analysis, the raw frequency of cases also suggests some measure of the administrative and practical significance of offense categories.[51]

Certainly a major consideration in the value of sentencing research is the generalizability of results. In order to assure the validity of inferences about sentencing in general made on the basis of the specific offenses studied, focal offenses were also selected, in part, on the basis of their comparability to offenses defined in State penal codes. Robbery, larceny, auto theft, embezzlement, and drug offenses comprise a major and substantial portion of the adjudicatory business of State as well as Federal jurisdictions.

A common and valid criticism of sentencing research is that a single offense *category* can include a multitude of behaviors, a condition sufficient in itself to warrant a broad range of criminal sanctions. Studies that aggregate altogether different offenses cannot begin to respond to this methodological challenge. Those that treat offenses separately have begun to move in the right direction. But any classification scheme remains deceptively simple. As researchers, we can attempt to construct or select categories so as to minimize variability around the critical dimension(s) being summarized by the classification scheme—in this case, the specific criminal behavior being penalized. Focal offenses were thus selected on the basis of the homogeneity of the behavior included in the respective offense groups. For example, *bank* robbery and *bank* embezzlement

[50] About 25 percent of Federal convictions each year are for violations of immigration, gambling, and Federal regulatory statutes.

[51] Of course, the rule was not rigid, since the largest single Federal offense category relates to immigration violations, an offense of limited practical and theoretical significance to this study. In short, the number of cases was a necessary but not sufficient basis for selection. For example, the eight focal offenses chosen annually account for between 25 percent and 40 percent of all convictions for major Federal offenses—i.e., other than immigration, gambling, and regulatory act violations. In 1971, the number of convictions for the focal offenses ranged from 727 (counterfeiting) to 2,027 (auto theft); the 1964 figures ranged from 200 (Selective Service) to 3,546 (auto theft).

A corollary consideration was the theoretical significance of the offense. As suggested, the consideration is partially, though not wholly, reflected by case frequency. There remain a few offenses that, despite their infrequency, are of compelling interest. A case in point is the Selective Service violator, whose treatment at the hand of the criminal justice system has invoked heated controversy over the last decade. The temporal and regional perspectives on sentencing that these data afford may be quite telling in terms of the social and political temper of the times.

rather than some ill-defined or unspecified category of robbery or embezzlement, in general, were selected. Similarly, where dozens of kinds of offenses would have been included in a "theft" category, the focus in this study was narrowed to larceny from interstate commerce. The balance of offenses were also selected with an eye to this issue.

Also considered in choosing offenses were specific dimensions relating to violence, social class, and moral offensiveness (victimless crimes) in an effort to shed some light on possible differential sentencing based on different types of offenses. Bank robbery, for example, suggests the element of force or violence, whereas bank embezzlement carries overtones of non-violent, middle- or upper-class white-collar crime. Robbery and larceny are both forms of outright theft of property; the two are distinguished from one another by the element of force. Narcotics and marihuana offenses are both ostensibly victimless crimes involving drugs, the two being distinguished from each other by the potentially harmful nature of the respective drugs involved. The focal offenses selected enable us to begin to evaluate the variability of sentencing patterns as a function not only of the specific offenses involved, but also of the general nature of the offenses—violent versus non-violent, conventional versus white-collar, *mala in se* (morally, intrinsically wrong, e.g., bank robbery) versus *mala prohibita* (not necessarily morally wrong, but legally proscribed for other reasons, e.g., Selective Service violations, drug offenses), and personal versus property.

As a final point, it should be noted that offenses were not chosen in anticipation of or with the expectation of discovering *particular* kinds of patterns or magnitudes of variance, except that the selection process was designed to include a *spectrum* of offenses that would exhibit the broadest possible set of sentencing patterns.

The Selection of Focal Jurisdictions

Just as the aggregation of offenses may conceal quite distinct sentencing patterns characteristic of different offenses, so the analysis of all jurisdictions at once may mask regionally distinct patterns. The analysis of specific jurisdictions is intended to uncover the latter. At the district level, six jurisdictions were chosen for this purpose, principally on the basis of the total number of offenders sentenced for the eight focal offenses, the heterogeneity of the caseload within the district with respect to those offenses, and region of the country. Six districts, representing four areas, were chosen: Eastern District of New York and Southern District of New York, from the Northeast; Southern District of Texas, from the South; Northern District of Illinois and Eastern District of Michigan, from the Midwest; and Central District of California, from the West.[52] Three of these districts—Eastern District of New York, Northern District of Illinois, and Eastern District of Michigan—employ sentencing councils or panels, a fact that will permit a comparison and assessment of the effects of that strategy with the effects of sentencing by a single judge. Thus, focusing on the patterns exhibited by all six districts will permit investigation into the extent to which different jurisdictions exhibit distinct sentencing patterns and discussion of some implications sentencing councils may have on the nature and extent of sentence variation.

Focal Circuits

Unfortunately, despite the careful selection of both offenses and districts, there were frequently an insufficient number of cases to sustain analysis at the district level. Consequently, the analysis at that level is often restricted to fewer than the eight focal offenses and the six focal districts. In order to ensure the robustness of the regional analysis, circuits were also used as "jurisdictions." For this purpose, data from the five circuits—the second, fifth, sixth, seventh, and ninth—that include the six focal districts, were also used. However, it should be made quite clear that the circuit is not properly thought of as a court of original jurisdiction; rather, it is a geographical aggregate of several specific district courts that exercise original jurisdiction in most criminal matters. Although the circuit *can* be viewed as a "court" for some purposes—for example, appellate cases, some special types of civil matters over which the circuit court exercises original jurisdiction—it is not a *single* judicial jurisdiction for the purpose of trying criminal cases as are the district courts. Analysis at the circuit level, therefore, represents a compromise necessitated by the frequently small criminal caseloads of even the largest Federal district courts. A concern in the regional analysis will be how accurately circuit-level patterns portray the practices of individual *district* courts contained therein, and, on the other hand, how much more accurately actual district patterns can be described if

[52]Central District of California was created by statute in 1966. Therefore, 1964 analysis focused on the Southern District of California, which included the area that later constituted the new Central District of California in 1966.

the data were disaggregated from the circuit to the district level.

Independent Variables

The impact on sentence outcome of a variety of offense, offender, and process-related factors is to be analyzed. Briefly, these variables include the age, race, sex, and prior criminal record of the offender, the conviction offense, the type of legal representation afforded, the method of conviction, and the time elapsed from filing to disposition of the case. Different portions of the analysis require different levels of measurement for the same variables; Appendix 1 contains details of the definition and coding of variables.

A number of jurisdiction-based variables for 1971 were also developed in an effort to explore the possibility that sentencing patterns are linked to various administrative or environmental features of the district or area in which a court is situated. Both raw and weighted measures were developed to reflect the absolute and relative workloads of each court; the size of the jurisdiction; the relative speed with which, and manner by which, each court disposes of the business before it; the relative "effectiveness" of each court in securing convictions (by plea and by trial); the differential conviction rates of defendants tried by court vis-a-vis jury in each district; and the relative efficiency of juror usage. Each of these factors was coded in interval fashion—most as percentages—and in dichotomous fashion. With respect to these district-related measures, the appropriate value was computed for each district, and each offender was assigned the respective values of the district. Then the individual scores were used in the analysis just like scores measuring more conventional "personal" attributes like race or age. Where it was necessary to dichotomize the variables, that is, for the predictive attribute analysis, the median value of the distribution of *district* (rather than individual) scores was used as the breaking point for each variable. Accordingly, each person was coded as having been sentenced by a "low" or "high" ranking district depending on the sentencing district's score in relation to the median score for all districts. Although the process did not ensure high and low categories of equal sizes for all the dimensions, it did not result in any seriously unbalanced dichotomies. An enumeration and description of each of the district-related factors is provided in Appendix 1, variables numbered 10 through 24.

Dependent Variables

An important distinction between this and other sentencing studies is the treatment of sentencing as a bifurcated or twofold decision, as this study focuses on both type and length of sentence. The object will be to compare the particular constellations of variables that prove relevant to each decision and to determine from these the extent to which sentencing might be more constructively and instructively viewed as two separate decisions rather than as one. For both the regression and predictive attribute analyses, sentence type is treated as a dichotomous nominal variable. For the regression analyses, length of imprisonment for those incarcerated is coded in months (as an interval-level variable). For the predictive attribute analyses, which require a dichotomous dependent variable, length of imprisonment for those incarcerated was dichotomized as 36 months and less versus more than 36 months. Although this is admittedly a somewhat arbitrary cutting point, it has the statistical advantage of allowing a large enough number of cases for analysis to fall on each side of the cutting point. Additionally, it seems reasonable to consider sentences over 3 years to be "long."

The Federal Criminal Code provides a variety of sentencing provisions, including deportation; suspended sentence; fine; probation; split sentence; mixed sentence; commitment for "study and observation"; a set of statutes exclusively applicable to young, youthful, or juvenile offenders; provisions for violations involving narcotics or controlled substances; special indeterminate statutes whereby, once the judicially determined maximum is set, either the judge or the parole board is allowed to set the minimum sentence; and "regular sentences" whereby the minimum sentence is set automatically at one-third of the judicially determined maximum. A complete outline of Federal sentencing provisions is included in Appendix 2.

Because special sentencing provisions can involve mandatory or radically different sentences,[53] only offenders sentenced under the most straightforward kinds of provisions have been included in this analysis. Specifically, this criterion limited the analysis to persons sentenced under: (1) "regular

[53]A study and observation commitment, for example, is routinely sentenced to the maximum allowable term for the commitment offense. Generally, that sentence is subsequently amended at the termination of the observation period.

adult" provisions (maximum set by judge within statutory limit, with the minimum set automatically at one-third of the imposed maximum); (2) adult indeterminate with judge setting minimum (maximum set by judge within statutory limit with minimum set by judge at less than one-third the maximum); (3) adult indeterminate with parole board setting minimum (maximum set by judge within statutory limit with minimum imposed by parole board at less than one-third the maximum).

1. Type of Sentence. As a result of the criteria above, type of criminal sentence was limited to suspended sentence, deportation,[54] fine, probation, and prison. For purposes of this analysis, the variable was dichotomized as prison and nonprison sentences. Conceptually, the distinction between prison and nonprison is certainly more marked than that separating any other dichotomy of these alternatives. Moreover, this break yields fairly equivalent frequencies of 5,100 (54.3 percent) and 4,284 (45.7 percent) for the prison and nonprison categories, respectively.

2. Maximum Length of Sentence of Imprisonment. One of the principal concerns of this study is the magnitude of discretion afforded the judge in the imposition of sentence. Judges are under virtually no restrictions in the determination of type of sentence. With respect to the length of sentence, they are generally constrained only by the statutorily prescribed maximum. Within that limit, each judge is free to exact a specific punishment on whatever basis and in whatever measure he or she deems "appropriate."

As an interval-level variable, sentence length was coded in months. Maxima ranged from zero[55] to 480, depending, of course, on the particular offense involved. Irrespective of offense, however, maxima tended to span the full range of legally allowable sentences. Where it was necessary to dichotomize sentence length, the break was made at 3 years or less versus more than 3 years, which placed about half of those convicted of the eight focal offenses in each category—2,644 (51.9 percent) and 2,452 (48.1 percent), respectively.

3. Sentence Weight. Because this analysis assumes so many analytic perspectives, for example, regional and temporal, it is useful to have a single summary sentence measure incorporating both dimensions of the "decision." Sentence weight is such a measure. The values assigned the different categories of penalty increase approximately in proportion to the severity of the corresponding penalties, as presented in Table 2.

The primary difficulty of scaling "sentence severity" into this sentence weight measure is the assignment of specific interval-level values to the spectrum of available sentences, especially where these sentences differ both quantitatively (length of sentence) and qualitatively (type of sentence). That is, if a suspended sentence (i.e., no imprisonment or probation) is assigned a value of "0" and one year of probation is value at "1," the obvious and difficult problem becomes that of deciding how to weight 2 years probation (2?); 4 years probation (4?); 1 year imprisonment (less than 4 or more than 4?); 10 years imprisonment (10 times the value of 1 year or more or less than 10 times the value of 1 year?); and so on. A second issue related to whether different sentences should be treated "independently" for weighting purposes or whether they might usefully be grouped (e.g., prison sentences from 6 years to 10 years) and assigned the same "weight."

The second issue was resolved by modeling the weighting scheme to be used in this study on that designed by the Administrative Office of the U.S. Courts (A.O.) to facilitate comparison of sentences of all kinds across jurisdiction and over time. The A.O. scheme groups sentences according to the categories presented in Table 2. A primary difference between the A.O. scheme and the one devised for this analysis is that the latter attempts to break some of the A.O.'s rather inclusive sentence categories into smaller categories. For example, the A.O. scheme used a single category with a weight of "50" for all prison sentences of 10 years to life; the scheme used in this study has four subdivisions (10 years to less than 15 years, 15 years to less than 20 years, 20 years to less than 45 years, and life). Additionally, the new scheme treats all sentences of imprisonment as more severe than any sentence of probation.

In attempting to resolve the first issue, the selection of appropriate "weights" for various sentence categories, several weighting schemes were investigated. In the simplest, an ordinal ranking of weights from 0 to 17 was applied to the sentence categories listed in Table 2. Other, more complex interval-level weighting schemes were also devised and

[54]Sentences of deportation are included by the Administrative Office of the U.S. Courts with sentences of probation without supervision, suspended sentences, imprisonment for 4 days or less, time served and fine *only*—which was subsequently remitted or suspended. However, they are infrequent and generally limited to violations of immigration laws.

[55]A sentence length of zero indicates that a person received a maximum sentence of at least 4 but no more than 14 days.

TABLE 2 Sentence weight index

Actual sentence	Weight assigned by the Administrative Office scheme[a]	Weight used in this study
Suspended sentence or fine only	0,1	0
Probation:		
Less than 3 years	1,2	1
3 years to less than 5 years	4	2
5 years or more	4	3
Prison:		
Less than 6 months	3	4
(Split sentence) prison 0 to 6 months and probation	4	5
6 months to less than 1 year	5	6
(Mixed sentence) prison 6 to less than 12 months and probation	Not applicable	7
1 year to less than 2 years	8	8
2 years to less than 3 years	10	10
3 years to less than 4 years	12	12
4 years to less than 5 years	14	14
5 years to less than 6 years	25	20
6 years to less than 10 years		30
10 years to less than 15 years	50	40
15 years to less than 20 years		50
20 years to less than 45 years		65
Life		80

[a]The Administrative Office's weighting scheme is reported in Hindelang, Dunn, Aumick, and Sutton, **Sourcebook of Criminal Justice Statistics-1974**, U.S. Law Enforcement Assistance Administration (Washington, D.C.: U.S. Government Printing Office) 1975.

tested, including the original scale used by the A.O. Regression analysis was performed on each weighting scheme, such that the same set of predictors was used to predict variations in outcome, as measured by each of the respective schemes. It is notable that among all the scaling models examined, the range in the level of variance (R^2) was less than 5 percent points. Thus, the precise calibration of sentence weights beyond a simple ordinal ranking appears almost inconsequential when fewer than two dozen categories of penalties are used. Nevertheless, the scheme ultimately selected for this analysis, and represented in Table 2, represents the model that yielded the highest level of explained variance of all models examined.

Analytical Design

As indicated earlier, the analysis will combine the advantages of two complementary approaches—stepwise multiple regression analysis and predictive attribute analysis. In this section, the general application, interpretive value, advantages, and limitations of each approach will be discussed.

Multiple Regression

Stepwise multiple regression is becoming increasingly popular in the analysis of simultaneous interrelationships among a multitude of variables and their single and combined "effects" on a specified dependent variable.[56] One of the most attractive features of the regression solution is that it expresses the relationship of a number of independent variables with a dependent variable by means of a single, relatively simple, mathematical equation. Because this particular investigation will focus on the relationship between the dependent variable and the same set of predictors (i.e., the sentence decision vis-a-vis a multitude of offense, offender, and environmental factors) from a variety of different perspectives—offense, jurisdiction, and time—the regression equation model should prove

[56]See F. Kerlinger and E. Pedhazur, **Multiple Regression in Behavioral Research** (New York: Holt, Rinehart and Winston, Inc.) 1973, pp. 3-9, for a basic discussion of the growing role of multiple regression in multivariate analysis involving large numbers of cases. See also the discussion in H. Blalock, Jr., op. cit., pp. 362-376. Both excerpts also include good discussions of the role of regression in "prediction" and the role of prediction in science. The aim of this investigation is description as much as prediction; the regression technique is equally well suited to both ends.

especially useful for comparative and summary purposes.

The initial step in the stepwise regression solution is to identify the factor that independently accounts for the greatest amount of variance in the dependent variable. In short, that is the variable displaying the greatest zero-order correlation with the dependent variable. Then, this factor is extracted from the solution, that is, "controlled," and the factor that then singularly accounts for the greatest residual or unexplained variation is identified. The successive repetition of this operation can isolate the independent impact of each independent variable, as well as the cumulative impact of all variables considered at each point. The process continues until additional operations fail to yield significant reductions in the residual variance, until all the variables have been used, or until all the variance in the dependent variable has been explained.[57]

Of course, the descriptive/predictive utility of the regression equation is directly proportional to the cumulative efficiency of the independent variables in accounting for the total variance in the dependent variable. If, for example, only 20 percent of the variance can be explained by all the independent variables included in the solution, then the predictive utility of the regression equation is less than if 75 percent of the variance can be explained. The former result might suggest that (1) the dependent variable is not strongly related to many of the independent variables introduced into the equation, (2) independent variables *other* than those considered in the analysis account for most of the variance in the dependent variables, (3) the relationship between the independent and dependent variables is not linear, or (4) variations in the dependent variable are random.

In this regard, two limitations of regression analysis, in the way it will be used in this study ought to be noted. First, multiple *linear* regression will be employed and, hence, the analysis is not sensitive to nonlinear relationships among variables. Second, the form of regression used for this analysis ignores statistical interactions among the variables.[58] Hence, there may yet be a significant relationship among independent and dependent variables, despite the failure of the regression solution to account for much of the variance in the dependent variable. Because interaction may be important in these data, and because the levels of measurement of many of the variables used may not meet the assumptions of the regression model, predictive attribute analysis—a technique which is sensitive to interaction and permits the use of categorical variables—will also be used.

Predictive Attribute Analysis

By use of a decision-tree, predictive attribute analysis (PAA), in much the same manner as regression analysis, "breaks out" the important variables accounting for variance in the dependent variable in order of their importance.[59] The first step identifies the variable explaining the greatest amount of variance in the dependent variable. Two categories divided along the dimension shown to be most significant are thus created. Then a similar operation is performed for *each* new category created by successive operations, until, by some convention stipulated by the researcher, additional breaks prove "insubstantial."

For example, to explain whether a convicted offender is sentenced to prison or not, the first step is to identify the variable that exhibits the strongest zero-order correlation[60] with the criterion variable,

[57]After the introduction of six or seven variables, the marginal predictive utility of additional dependent variables generally proved minimal, as they accounted for only chance variation; that is, after a point, correlations between the dependent variable and residual independent variables is likely a product of chance. For this analysis, results of the stepwise solution were disregarded after the *marginal* predictive contribution of additional independent variables dropped below 1 percent of the variance in the dependent variable—even though results continued to be statistically significant at the .01 level. This rule generally limited the descriptive/predictive models (regression equations) to between four and eight salient factors. Similarly, the reported multiple R's generally do not include any of the variables that entered the solution at late stages even though their effects remained statistically significant. In this manner, the typical tendency of regression to capitalize on chance variation was mitigated.

[58]"Interaction" suggests one of two circumstances. First, it may be that the observed effect of the combination of variables is more or less than their simple additive effect.

Second, it may be that the magnitude and/or direction of the relationship between an independent and dependent variable may be significantly different for different population subgroups, e.g., the relationship between race and sentence length may be different for younger and older offenders. Interaction is discussed in F. Kerlinger and E. Pedhazur, op. cit., pp. 245-259. In their discussion they suggest how multiple regression analysis can be modified to handle the possibility of interaction. The suggested procedure, however, is too cumbersome and expensive to be used with these data. See Kerlinger and Pedhazur for their discussion of interaction vectors.

[59]For a good discussion of the adaptation of PAA to social science, see Wilkins and MacNaughton-Smith, "New Prediction and Classification Methods in Criminology," **Journal of Research in Crime and Delinquency** 19 (1964).

[60]For this analysis, the measure of association used was Somers' *d*.

that is, sentence outcome. Assume, for example, that having been convicted of robbery (versus *any* of the other eight focal offenses) was more strongly associated with the prison/nonprison decision than was any of the other independent variables. In this case, the population would "break" into two groups, and the analysis would proceed independently for those convicted for robbery and those convicted for any of the other seven offenses. It is at the second and subsequent levels of analysis that the value of predictive attribute analysis becomes apparent. Clearly, the variable that accounts for the most variance in the dependent variable for one of the subgroups (for example, robbery offenders) need not also account for the most variance in the complementary subgroup (nonrobbery offenders) nor must the direction of the relationship between dependent and independent variables be the same for the two subgroups. At the second stage, for example, the factor that best "predicts" outcome for robbery offenders may be the sex of the offender, whereas for offenders convicted of an offense other than robbery, prior criminal record might be the best predictor of whether or not the offender is sentenced to prison. Proceeding in this fashion, predictive attribute analysis permits detection of interaction among variables. It is also useful in identifying multiple combinations of attributes that display varying strengths of association with sentence outcome.

The operation proceeds for each new group created by the sequential breaks described until (1) additional "breaks" of a category fail to yield dichotomies that differ significantly with respect to possession of the criterion variable; (2) the statistic measuring the association between the dependent and independent variables drops below a predetermined point below which a correlation can no longer be considered significant; or (3) one of the groups yielded by a split contains too few cases to sustain further meaningful breaks. The conventions established to guide the analysis herein were (1) breaking on the predictor variable exhibiting the largest correlation (Somers' *d*) with the dependent variable; (2) stopping a branch when the measure of association (Somers' *d*) between the dependent and all predictor variables is less than 0.10; (3) stopping a branch on that side of the dichotomy where fewer than 50 cases remained after a break; or (4) in any case, stopping a branch after six breaks had been effected if none of the above conditions had yet been met.

This technique "holds constant," in turn, the effect of each of those variables that exhibits a significant relationship to the dependent variable, as new variables are sequentially identified as "independently" contributing to the ability to "predict" sentence outcome.

It is clear that predictive attribute analysis "uses up" cases very quickly because successive breaks can decrease the number of cases in each new subgroup by as much as 50 percent. Thus, it is appropriate only for a large data set. For the significantly large number of cases available for the analysis proposed, predictive attribute analysis will be appropriate; by accommodating those limitations inherent in the multiple regression technique, it should prove especially valuable.

The analyses based on these data will culminate with the development of a number of predictive models describing the aggregate or collective sentencing patterns for the eight focal offenses (as well as for several specific offenses) exhibited by the 88 major Federal district courts (as well as by specific jurisdictions) for fiscal years 1964 and 1971.

Summary

It may be instructive to review the design based on the Federal perspective in light of the limitations of earlier studies and to point out some of the ways in which it is designed to answer their shortcomings. First, whereas former studies have tended to focus on single or comparable offenses, this design will incorporate the analysis of several offenses of significantly different character. Included is a characteristically violent offense (robbery), property offenses (auto theft, larceny, and theft), "white-collar" offenses (counterfeiting, embezzlement), drug offenses (narcotics, marihuana) and Selective Service offenses. With this spectrum, previous findings concerning sentencing patterns for specific types of offenses can be examined insofar as they may apply to sentencing for other categories of offense as well. By incorporating a variety of types of offense, the design permits the identification of manifold configurations of offender and process-related factors differentially ordered for the several offenses or types of offenses under analysis.

Second, whereas many studies have failed to explore systematically the significance of correlations beyond the zero- or first-order level that might have explained the relationship between various offender characteristics (usually race) and sentence, this design incorporates a substantial number of "controls" to facilitate the examination of the "independent effect" of each variable on sentencing. Information on prior criminal record and other offender at-

tributes, as well as on method of conviction, type of counsel, and other process-related factors, should augment the analysis by introducing higher levels of controls than most of the studies conducted to date.

Third, sufficiently large numbers of cases are available to sustain types of analyses previously found difficult. On a national level, the Federal data base includes nearly 7,000 and 10,000 convictions for fiscal years 1964 and 1971, respectively, for the eight focal offenses. Complex analyses at that level, therefore, will pose fewer difficulties than when smaller samples have been involved.

Fourth, because this study will employ data pertaining to *type* as well as to *length* of sentence, an essential dimension of the sentence decision that has received little attention in most empirical studies can be addressed. In this portion of the analysis, whether and the degree to which variables that are significant to the determination of the generic sanction (prison/nonprison) are also influential in the specification of the conditions (i.e., length) of imprisonment will be assessed.

Fifth, because the Federal data system is national in scope, comprehensive data are uniformly available from a variety of jurisdictions across the country, permitting an examination of regional variations in sentencing patterns. Conclusions about regional variations in sentencing—for example, the differential impact of race on sentences in the North and South—have been, at best, only tenuous inferences based on studies bearing little more in common than race as an "independent" variable, and using "controls," analytic techniques, sampling techniques, offenses, and dates that are not comparable with each other. This design can accommodate the exploration of the possible regional effects of race, as well as a variety of other factors, while holding constant other factors and dimensions (e.g., offense, time, and offender attributes) important to the sentence decision.

Sixth, the temporal parameter provides a dimension acknowledged only modestly in previous studies. Examination of patterns of variance over time may provide valuable insights into not only the nature and flexibility of sentencing patterns, but also some time-related factors correlated to changes in sentencing patterns. This kind of dynamic relationship of factors relevant to the sentencing decision is an area that remains altogether unexplored.

In sum, this design is calculated to reach closure on a number of those questions that remain unanswered and to test in a single comprehensive study the accuracy of a variety of yet equivocal inferences about the complex nature of judicial sentencing. Relying on data from fiscal years 1964 and 1971, the proposal can address the extent to which those patterns that seem to characterize contemporary sentencing were applicable a decade ago. That is, if race seems to be significant in 1971 did it also appear so in 1964? Has the impact of criminal record on sentence increased or decreased during this time span? Was method of conviction more, or less important to type and length of sentence in 1971 than it was in 1964? If its import has changed, is the direction and magnitude of the change consistent for all offenses studied and for all regions, or does it apply only to specific offenses or to specific jurisdictions? To what extent can sentences be predicted for specific offenses and specific jurisdictions on the basis of models derived from samples of offenders convicted in other jurisdictions and at other times? In short, to what extent ought current assumptions and conclusions about the nature of the "sentencing decision" be modified or qualified to accord with the variable dimensions—relating to offense, region, and time—of that decision? Such queries are illustrative of the kinds of questions that will be addressed in subsequent reports on the basis of the Federal judicial data set and the design introduced in this report.

APPENDIX 1 Independent Variables

The mnemonic terms in parentheses in the definitions below (e.g., ROB) have been used in some of the analytic reports in this series and in the source document from which these analytic reports derive.

1. Offense. Each of eight offenses was dummied and treated as an independent variable. This means that a variable was created for *each* offense and coded such that all persons convicted for that offense were assigned one value, e.g., 1, and all persons convicted for any of the other seven focal offenses were assigned another value, e.g., 0. These dummied variables included bank robbery (ROB), bank embezzlement (EMB), larceny from interstate commerce (LARC), counterfeiting (COUNT), auto theft (AUTO), Marihuana Tax Act (MARH), narcotics (NARC), and Selective Service violations (SS).

2. Age. The age of the offender at the time of sentencing was also reported. Where dichotomized in the analysis, age was broken so that about half the population would be in each category. The "young" category includes those under 30 years of age, the "old" includes everyone 30 years of age or older.

3. Race. Only about 1 percent of all offenders were reported to be neither white nor black. However, it was not known into which category—for practical or theoretical reasons—these individuals ought to be placed. Consequently, race was dichotomized as two variables: white/other than white and black/other than black.

4. Sex. Sex forms a natural male/female dichotomy and was so coded. Other than individual offenders—that is, corporations and firms—were excluded from the analysis, since they were quite rare.

5. Prior Criminal Record (REC). Criminal record forms a natural ordinal scale. Least serious is "no record of prior conviction." Next is a "prior conviction which resulted in a nonincarcerative sentence," for example, fine, probation, or suspended sentence. Third is a "prior conviction which resulted in an institutional commitment for a maximum of less than 1 year" (misdemeanor). Fourth is a "prior conviction and institutional commitment under juvenile delinquency procedures."[1] Fifth and most serious is a "prior conviction resulting in imprisonment for a maximum of more than 1 year" (felony). When dichotomized, prior record was broken into record of incarceration (for those having been convicted and previously institutionalized for *any* period of time) and no record of incarceration (for those having either no prior convictions at all, or a conviction that resulted in a nonincarcerative sentence).

6. Type of Counsel. Legal representation falls basically into one of three categories: 1) waived or no counsel (NOCNS); 2) assigned counsel, whether court-appointed or a public defender (ACNS); and 3) privately retained counsel (RCNS). A simple counsel/no counsel dichotomy would not permit exploration of the possibly differential impact on sentence of assigned versus private counsel. Therefore,

[1]One might dispute the relatively high rank of a juvenile record. But it must be realized that juveniles (under 18 years of age at the time of the offense) 1) are generally committed for only the more serious offenses and 2) are seldom institutionalized for their first conviction. For example, the Federal Bureau of Prisons' **Statistical Report, Fiscal Years 1971 and 1972**, Table B-15A, pp. 136-137, reports that most juveniles committed under the Federal Juvenile Delinquency Act (F.J.D.A.) had been convicted of auto theft (84 out of 280 juveniles, or 30 percent), drug offenses (30 out of 280, or 11 percent), or robbery (22 out of 280, or 8 percent). Moreover, an annual statistical report of the Administrative Office of the U.S. Courts, **Federal Offenders in U.S. District Courts, 1971**, reports that of the 261 youths who were received by prisons in 1971 as F.J.D.A. commitments and for whom information on prior record was reported, 189 (72 percent of the total number sentenced to prison) already had a prior criminal record (Table 20, p. 58).

Perhaps most salient to the severe scaling of juvenile record is that the Bureau of Prisons, op. cit., Table B-16A, pp. 142-143, reports that the mean maximum sentence length for a Federal juvenile delinquent committed in 1971 was relatively substantial. Nearly three-fourths (203 out of 280) were committed for the duration of their "minority"—that is, until they reached legal adulthood (age 21), an interval that averaged 39.6 months. The average sentence of those committed for less than their minority was 22.7 months. By comparison, the average maximum term for all sentenced offenders received by the Bureau of Prisons in 1971 was 34.6 months.

The point, in sum, is that a record of prior juvenile commitment can be fairly viewed as more serious than a record of incarceration for less than 1 year.

each of the three categories was dummied (dichotomized) according to the presence or absence of the type of representation: counsel/no counsel; assigned counsel/not assigned counsel (the latter referring to defendants with retained counsel or no counsel); and retained counsel/no retained counsel (the latter referring to defendants with assigned counsel or no counsel).

7. Method of Conviction. One may be convicted in one of several ways: by an original (unchanged) plea of guilty or nolo contendere; by a plea of guilty or nolo contendere after an original plea of not guilty; by a court or "bench" trial (judge sitting without a jury); or by a jury trial. Because pleas of nolo contendere are relatively rare and are essentially pleas of guilty, the two types of plea were not distinguished. As a result, four variables, each dummied in the fashion described above, were created: unchanged plea of guilty (UPLEA)/other than unchanged plea of guilty; changed plea of guilty (CPLEA)/other than changed plea of guilty; court trial (CTRIAL)/other than court trial; and jury trial (JTRIAL)/other than jury trial. Additionally, in order to explore the broader relationship of method of conviction to sentence, a fifth dichotomized variable, conviction by trial (TRIAL)/plea of guilty was created.

8. Interval (INT). The interval of time elapsed from the original filing of the case to its ultimate disposition by the court (sentencing) is recorded in months. Where it was necessary to dichotomize the time interval, the break was made so that the created categories were approximately equal in size—3 months or less/over 3 months.

9. Method of Case Initiation. Two variables were dummied to describe method of case initiation: case initiated by indictment (INDICT)/other than indictment, and defendant waived right to formal indictment hearing and consented to be charged by information (WAIVER)/other than waiver.

The following district-related factors were computed from 1971 data and were used only in the 1971 analysis.

10. Criminal Dispositions per Judgeship (CRDPJ). Criminal dispositions per judgeship refers to the number of criminal cases disposed of (including dismissals and acquittals)[2] in a district, divided by the number of judgeships authorized for that district in the same fiscal year (1971).[3]

11. Total Dispositions per Judgeship (TDPJ). Because much of the business of Federal courts relates to civil processes, one might argue that a truly representative measure of the judicial workload—inasmuch as one is exploring the relationship between criminal sentences and the caseload (or "business") of the court—ought to include civil as well as criminal cases. This variable measures the *total* dispositions per judgeship in the same fashion as criminal dispositions per judgeship measured the crime-related workload.[4] The number of total dispositions per judgeship ranged from 119 (Delaware) to 1,058 (Southern California).

12. Weighted Filings per Judgeship (WFPJ). This more sophisticated measure of judicial workload considers not only the number but also the difficulty of the kinds of cases being handled. The weighting scheme was developed by the Administrative Office of the U.S. Courts on the basis of the amount of time required for the disposition of different types of both civil and criminal cases.[5] Thus, two districts that rank the same on weighted filings can be considered to have comparable workloads, even though one may annually process hundreds more cases than the other. Across the 88 districts, the number of weighted filings per judge ranged from 98 (North Dakota) to 577 (Western Wisconsin) in fiscal year 1971.

13. Criminal Dispositions Standardized by Civilian Population (ZDISP). This weighted measure of court caseload standardizes the number of criminal cases disposed of in fiscal year 1971 by

[2]The number of criminal dispositions was derived directly from the data tapes used in the analysis. According to that record, 47,945 cases were disposed of by Federal courts in 1971. This number *excludes* 75 cases from the Southern District of New York, which were coded as "statistical dismissals"—cases that, in fact, had not yet actually been disposed of in 1971.

The number for all percentage figures subsequently based on the number of criminal dispositions per district was derived by subtracting from the total number of criminal dispositions: 1) all cases that were coded as "statistical dismissals," 2) all Narcotic Addiction Rehabilitation Act commitments [28 USC 2902(a), (b)], and 3) cases having no value recorded for method of conviction. There were few instances of any of the three cases.

[3]The number of authorized judgeships for each Federal district in 1971 is reported in Administrative Office of the U.S. Courts, **Management Statistics for U.S. Courts, 1971.** The actual value used here was computed by dividing the number of "vacant judgeship months" for each district by 12 and then subtracting this number from the reported number of authorized judgeships for the year. The correction, while yielding a more precise measure of the actual number of judges sitting in a jurisdiction, resulted in only minor adjustments of the original figure for "authorized judgeships."

[4]Since the data tapes used in this analysis have no information relating to noncriminal cases, these figures were obtained from the Administrative Office of the U.S. Courts **1972 Annual Report of the Director,** Table 20, pp. II-35, II-36.

[5]Data for this variable were obtained from **Management Statistics, 1971, op. cit.**

units of 100,000 civilian population.[6] In 1971, the districts ranged from 6 (Northern New York) to 214 (Southern California) criminal dispositions per 100,000 population.

14. Median Interval from Filing to Disposition of All Cases (MINT). This factor is a measure of the median time (in months) required for the disposition of *all* cases disposed of within the jurisdiction during fiscal year 1971.[7] Values ranged from .3 (Southern Texas) to 12.4 months (New Jersey).

With respect to the variables that follow, two points are important: first, for all rate figures that used total criminal dispositions as a base, all statistical dismissals, Narcotic Addiction Rehabilitation Act commitments, and cases with missing values were excluded from the base figures before the rates were calculated;[8] second, no rate was calculated if the base was less than 10.

15. Dismissal Rate (DSMRT). Dismissal rate is the percent of all criminal defendants who were disposed of by the dismissal of charges. Clearly, dismissal rates varied widely across the nation. In Southern Texas, for example, only 7 percent of all dispositions were by dismissal. In contrast, nearly half (47 percent) of those cases that were concluded in Nevada were dismissed.

16. Plea Rate (PRT). Plea rate refers to the proportion of criminal case dispositions in a district that were effected by a changed or an unchanged plea of guilty or nolo contendere. Plea rates ranged from a low of 37 percent in Nevada to a high of 90 percent in Southern Texas.

17. Trial Rate (TRT). Trial rate refers to the proportion of a district's total criminal case dispositions that were effected by a court or a jury trial. A high trial rate suggests that a district is expending considerable human and material resources on the adjudication process compared to districts that have high dismissal and/or plea rates. District values range from a low of 2 percent for Southern Texas to a high of 36 percent in Eastern Tennessee. Half the defendants processed in 1971 were disposed of in jurisdictions wherein fewer than 15 percent of all dispositions were by trial.

18. Jury Trial Rate (JRT) This factor (jury trials as a percentage of all trials) refers to the proportion of all trials that were heard before a judge and jury (vis-a-vis bench trials that are argued before a single judge without a jury). The distribution of court and jury trials varied considerably from one district to the next. In Middle-North Carolina, for example, only one in five trials (21 percent) in 1971 was heard by a jury. On the other hand, every one of Rhode Island's 22 Federal trials was presented to a jury. Across districts, "preference" was clearly for *jury* trials in 1971, despite their apparent "cost" to the defendant in terms of relatively severe sentences, a factor that will be explored in detail in reports in this series. In 1971, half the persons convicted in the 88 major Federal district courts were convicted in districts where nearly three-quarters of all trials were jury trials.

19. Conviction Rate (CVRT). A summary rate of convictions for each district was also calculated and assigned to each individual record. Any disposition other than a dismissal, an acquittal, a statistical dismissal, or a missing value was tabulated as a conviction. The lowest conviction rate of any district was 49 percent (Nevada). In sharp contrast, more than 9 in 10 (92 percent) of those persons whose cases were processed in Southern Texas were convicted. Half of all defendants disposed of in 1971 were processed in jurisdictions exhibiting conviction rates of better than 68 percent.

20. Plea Conviction Rate (PCRT). This variable reflects the number of pleas of guilty or nolo contendere expressed as a percentage of all convictions in a district. This rate is extremely high, ranging from a low of 63 percent (Eastern Tennessee) to a high of 98 percent (Southern Texas), emphasizing that the preponderance of convictions in every Federal court derive from the defendants' own admissions of guilt.

21. Trial Conviction Rate (TCRT). Trial conviction rate is a measure of trial "effectiveness," as it reflects the percent of all trials within each jurisdiction that resulted in convictions. Values ranged from 31 percent in Alaska to a staggering 100 percent in Hawaii. Most jurisdictions have a better than even record of trial victories; indeed, over half (which were responsible for disposing of about half of all Federal cases) exhibited trial conviction rates of around 75 percent in 1971!

[6]The 1970 census figures for Federal judicial districts is reported in **Reports of the Proceedings of the Judicial Conference of the United States**, March 15-16 and October 28-29, 1971, (Washington, D.C.: U.S. Government Printing Office), 1972, Table X-10, pp. 421-423.

[7]The values for this variable were taken from **Management Statistics, 1971**, op. cit.

[8]These exclusions were generally limited to no more than 2 or 3 percent of the respective district totals.

22. Court Trial Conviction Rate (CCRT). Court trial conviction rate measures the "effectiveness"—with respect to convictions—of nonjury trials, that is, those heard only by a judge without a jury. The proportion of victories in court trials ranged from 32 percent (New Jersey) to 100 percent (Hawaii and Kansas).

23. Jury Trial Conviction Rate (JCRT). The counterpart of court conviction rates for jury trials relates a district's conviction rate for all jury trials. Not unlike the range for court effectiveness, jury trial effectiveness ranged from 30 percent (Alaska) to 96 percent (Western Kentucky). On the whole, however, jury trials were much more "effective" than court trials.

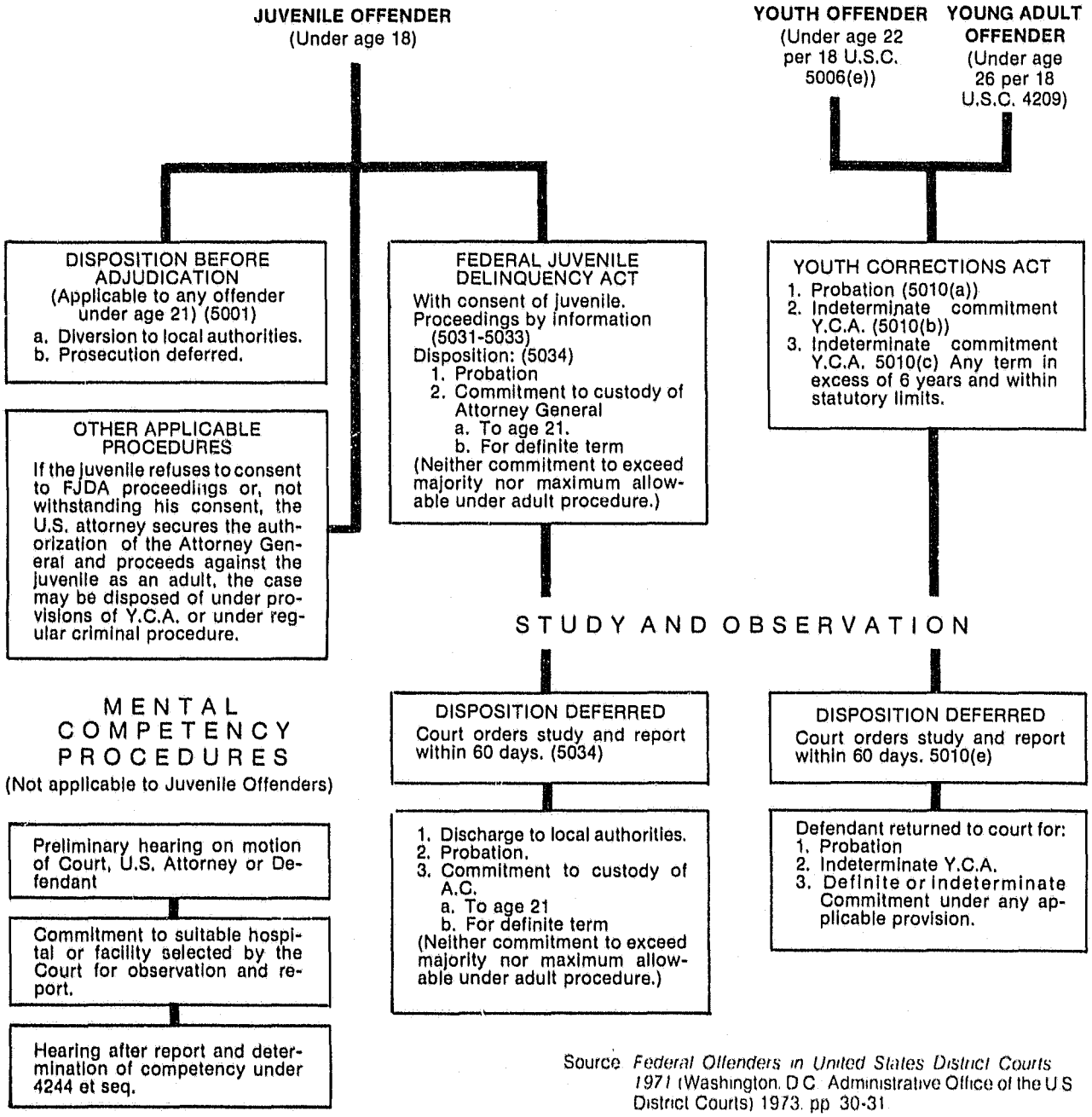
24. Juror Usage Index (JUI) A popular hypothesis used to account for the often cited relationship between a jury trial conviction and a severe sentence relates to the relative "cost" and tedium—in terms of human and material resources—of a jury trial versus the economy and expedience of a guilty plea. The Juror Usage Index provides a rather sophisticated measure of how the expense of jury trials may vary from district to district.[9]

The Index is a ratio of the number of jurors on hand and paid *per jury trial day* during the year. One "jury trial day" is counted for each day each trial is being held in the district. Thus, if there were five jury trials going on for 4 days, that would count as 20 jury trial days. If 400 jurors were compensated during this period, the index for the 4-day period would be 400 jurors paid divided by 20 jury trials days = 20 (actually, the JUI is tabulated for the entire year). In 1971, JUI ranged from an economical 15 jurors paid per jury trial day (Colorado, Wyoming, Western Michigan) to a high of 58 (Southern New York).

[9]The Index was developed by the Administrative Office and is defined and reported in **Management Statistics, 1971**, op. cit.

APPENDIX 2 Federal Criminal Sentencing Provisions

NOTE: This analysis applied only to persons sentenced under the "regular criminal procedure." The preponderance of Federal criminal cases are sentenced in accord with these provisions. In addition, this analysis excludes defendants sentenced to "study and observation."



Source *Federal Offenders in United States District Courts 1971* (Washington, D.C. Administrative Office of the U.S. District Courts) 1973, pp. 30-31.

ADULT OFFENDER
(any age)

REGULAR CRIMINAL PROCEDURE

1. Definite sentence within statutory limits with parole eligibility at 1/3. (4202)
   a. Probation. (3651)
   b. Commitment to prison or if misdemeanor to jail
   c. Split sentence — 6 months — jail plus probation
2. Indeterminate sentence
   a. Judge in sentence specifies a minimum term of parole eligibility less than 1/3 of maximum sentence he imposes. (4208(a)(1))
   b. Judge fixes a maximum term of imprisonment, specifying prisoner shall become eligible for parole at time Parole Board shall determine. (4208(a)(2))
3. Fine

OBSERVATION PROCEDURES

MAXIMUM SENTENCE ALLOWABLE BY LAW

Court orders study and report within 3 months. (4208(b))

Defendant must be returned to court for:
1. Probation.
2. Affirm or reduce original sentence.
3. Give definite or indeterminate commitment under any applicable provision (including Y.C.A.).

NARCOTICS PROCEDURE

1. Special parole terms of 2-6 year minimum built into sentence. (21 U.S.C. 841)
2. One-year probation without entry of conviction for first time possessors of controlled substances, with provisions for dismissal of proceedings if successful, and also expungement of record for those under 21 at time of offense.
3. Community supervision for addicts as condition of regular probation or parole. (18 U.S.C. 3651, 4203, as amended by P.L. 92-293)
4. More severe penalties for person engaged in a continuing criminal enterprise plus forfeiture of profits and property used. (21 U.S.C. 848)
5. Dangerous special drug offender sentencing procedures include harsher penalties after special sentencing hearing. (21 U.S.C. 849)
6. Certain offenders can be sentenced to civil commitment in lieu of prosecution under NARA. (28 U.S.C. 2901-6)
7. Drug maintenance programs available as part of either civil or criminal commitment programs, as part of supervised aftercare programs, or as part of community treatment program for probationers, parolees, or conditional releases. (P.L. 92-420)

DISPOSITION DEFERRED (NARA)

Court order examination and report within 30 days. (18 U.S.C. 4252)

1. If addict is likely to be rehabilitated, court may order him committed for indeterminate period not to exceed 10 years, or maximum sentence if shorter. (18 U.S.C. 4253)
2. Court may impose any other authorized sentence. (Ibid)
3. Provision for conditional release under supervision after 6 months treatment. (18 U.S.C. 4254-5)

ORGANIZED CRIME PROCEDURE

1. Besides penalty of fine and imprisonment, criminal forfeiture of property and business interests illegally derived. (18 U.S.C. 1963)
2. Increased sentence for dangerous special offenders after special sentencing hearing. (18 U.S.C. 3575)

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U.S. DEPARTMENT OF JUSTICE
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USER EVALUATION QUESTIONNAIRE

Federal Criminal Sentencing: Perspectives
of Analysis and a Design for Research
Analytic Report No. 16

Dear Reader:

The Criminal Justice Research Center and the Law Enforcement Assistance Administration are interested in your comments and suggestions about this report, produced under the Utilization of Criminal Justice Statistics project. We have provided this form for whatever opinions you wish to express about this report. Please cut out both of these pages, staple them together on one corner, and fold so that the Law Enforcement Assistance Administration address appears on the outside. After folding, use tape to seal closed. No postage stamp is necessary.

Thank you for your help.

1. For what purpose did you use this report?

2. For that purpose, the report– ☐ Met most of my needs ☐ Met some of my needs ☐ Met none of my needs

3. How will this report be useful to you?

- ☐ Data source
- ☐ Teaching material
- ☐ Reference for article or report
- ☐ General information
- ☐ Criminal justice program planning
- ☐ Other *(please specify)* _____
- ☐ Will not be useful to me *(please explain)* _____

4. Are there any other data sources you could suggest to address the topic of this report?

5. Would you like to see any other analyses of the data contained in this report?

CUT ALONG THIS LINE

6. Which parts of the report, if any, were difficult to understand or use? How could they be improved?

7. Can you point out specific parts of the text or table notes that are not clear or terms that need to be defined?

8. Can you point out any specific statistical techniques or terminology used in this report that you feel should be more adequately explained? How could these be better explained?

9. Are there ways this report could be improved that you have not mentioned?

10. Please suggest other topics you would like to see addressed in future analytic reports.

11. Please suggest any specific criminal justice data bases or sources of criminal justice data that could be explored in future analytic reports. (Please give as full a citation as possible.)

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