THE PERFORMANCE OF PROSECUTION, DEFENSE, AND COURT AGENCIES INVOLVED IN FELONY PROCEEDINGS

ANALYSIS AND DEMONSTRATION

PREPARED UNDER A GRANT FROM THE NATIONAL INSTITUTE OF LAW ENFORCEMENT AND CRIMINAL JUSTICE, LEAA, U.S. DEPARTMENT OF JUSTICE

SORREL WILDHORN, MARVIN LAVIN, ANTHONY PASCAL, SANDRA BERRY, STEPHEN KLEIN
WITH CONTRIBUTIONS FROM RAYMOND SINETAR, GERALD SUMNER, GERALD UELMEN

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INDICATORS OF JUSTICE: MEASURING THE PERFORMANCE OF PROSECUTION, DEFENSE, AND COURT AGENCIES INVOLVED IN FELONY PROCEEDINGS

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PREFACE

This report, the second in a series of two volumes, presents the results of an eighteen-month study of the use of statistical performance measures in the context of felony proceedings. The study, supported by a grant from the National Institute of Law Enforcement and Criminal Justice of the Law Enforcement Assistance Administration, U.S. Department of Justice, had two broad objectives:

- To identify, screen, and evaluate sets of statistical performance measures as indices of progress.
- To demonstrate the applicability of these performance measures in two selected (county) jurisdictions.

This report is a comprehensive and detailed description of all aspects of the work of this study. It provides a background discussion and literature review; professional views on performance measures; a discussion of a theoretical basis for selecting performance measures; a description of the data collection efforts; how the demonstration jurisdictions were selected; the application of selected performance measures in the two demonstration jurisdications; the role of criminal case auditing in performance measurement; the methods, procedures, and results of surveying lay participant attitudes; and the general findings and implications of the study. The report is directed primarily to the criminal justice research community—analysts, academicians, and survey researchers—but it should also be of interest to practitioners who wish to examine the details of the analysis supporting the findings of the first report.

The first report (R-1917-DOJ), *Indicators of Justice: Measuring the Performance of Prosecution, Defense, and Court Agencies Involved in Felony Proceedings: A Guide to Practitioners*, summarizes and synthesizes the approach, the methods used, and the overall findings of the study, and draws general implications for jurisdictions interested in applying the approach. It is intended as a guide to officials and practitioners in the criminal courts—judges, prosecutors, public defenders, private defense counsel, and court administrators. It should also be of interest to other criminal justice practitioners, such as police and probation officials, whose work brings them in contact with the courts.

EXECUTIVE SUMMARY

FOCUS AND PURPOSES

The primary focus of the study is on the selection, estimation, and analysis of performance measures as *statistical devices* that aid in the interpretation of data drawn from *court system operations* (i.e., from case files and other records in court, prosecution, and public defender agencies). Performance measures may be viewed either as (1) quantitative descriptors of *what* is being done in felony proceedings or (2) progress indices of *how well* these functions are being performed. (Examples of the former are the number and proportion of all felony filings that are disposed of by dismissal, plea of guilty, conviction at trial, and trial acquittal or dismissal; whereas an example of the latter is the proportion of felony trials exceeding the speedy trial standard.)

This study emphasizes the latter role of performance measures. Its emphasis is to be contrasted with, for example, the application of standards and goals as articulated in the series of volumes issued since 1968 by the American Bar Association on Standards for Criminal Justice,[1] or those that resulted in 1973 from the work of the National Advisory Commission on Criminal Justice Standards and Goals. Few of these hundreds of individual goals and standards relating to criminal proceedings are couched in quantitative terms or lend themselves to quantitative interpretation, the primary exception being those concerning the "speediness" of the proceeding.

The secondary focus of this study is on performance measures of the court system as viewed through the eyes of *lay participants* in the felony proceeding—victims, other witnesses, jurors, and defendants. That is, their attitudes toward the court system are performance measures of interest, which can be elicited through survey techniques. Moreover, with proper statistical analysis, their attitudes can be related to their individual experiences with, and treatment by, the court system.

The objectives of the study were:

- To identify, screen, and evaluate sets of performance measures (to be estimated from agency records and surveys of lay participants) as *indices of progress.*
- To demonstrate the applicability of these performance measures in two jurisdictions, Multnomah County, Oregon, and Dade County, Florida.

SCOPE

The scope of the study was confined to *adult felony proceedings;* thus, we considered neither misdemeanor nor civil proceedings. We addressed only the *primary* activities of the court system, excluding supporting activities and functions performed by court clerks, court reporters, bailiffs, or paralegals in the prosecutor's office or the public defender's office. Of the several potential roles and uses of

[1] See footnote 1, page 1, for a listing of these volumes.

statistical performance measures, two broad types of applications were made: *retrospective* comparisons of the full court system and of its component agencies (rather than of individual practitioners) *within* the jurisdiction at different times;[2] and *retrospective interjurisdictional* comparisons of court systems and component agencies at the same time.

The study focused on a limited set of persistently important issue areas (described below) and sought to select and apply sets of performance measures that would clarify them.

THE ISSUE AREAS AND THE POLICY INTERESTS OF PRACTITIONERS AND AGENCIES

We selected issue areas that involve major aspects of the performance of the court system. For some issue areas—such as delay, efficiency, evenhandedness, charging accuracy, and attitudes of lay participants—practitioners and observers all agree on the direction of improvement to be sought, even though they might not agree on a structure of goals (and their relative importance) for the felony proceeding. For other issue areas—such as the charging threshold, the effect of plea bargaining, and sentence variation—they can agree only that further clarification is desirable. Also, certain agencies and types of practitioners find particular issues to be of greater interest than others, either because of relevance to their own performance or because of concerns about current policy.

Charging Standards

Prosecutors' offices in most jurisdictions need objective evidence of the standards being implemented to discern whether they conform to policy and what the effects on the system are if policy changes. For example, if the charging threshold is lowered, how are court workload, delay, and plea bargaining affected? Measures of the operation of the charging threshold over time also can reveal trends in police performance as a by-product.

Charging Accuracy

Is the nature of the disposition of cases being unduly affected by the accuracy with which charges are filed against defendants? Is the court workload being magnified by the consequences of inaccurate charging? Such questions concern not only prosecutors, but also judges, defense counsel, and court administrators in assessing charging policy and practices in their jurisdiction.

Plea Bargaining

What are the nature and frequency of the practice of plea bargaining in the jurisdiction? Quantitative evidence available to practitioners is often scant on this question. And the public rarely has seen even the rudiments of an objective picture.

[2] Comparisons were made in two contexts: when no major change (i.e., routine monitoring) is introduced, and when a major policy change or innovation (procedural, legislative, administrative) is to be evaluated.

What is the court system gaining or losing from plea negotiation? How are delay and efficiency of resource use being affected? Is punishment significantly lighter than in the absence of such negotiation? All practitioners, and the public as well, have a vital stake in these questions, even though plea-bargaining policy is primarily a prosecutorial responsibility.

Sentence Variation

Judges and other practitioners want to know the degree of consistency in sentencing in their court system as compared with others, how sentencing practices change over time, and how they vary among judges within a court system. If, in addition, quantitative evidence were available to explain how much of the observed variation was accounted for by various legitimate and illegitimate factors, such information can help to reduce disparities or enhance the effectiveness of specific devices (e.g., sentencing panels or appellate review of sentences) aimed at reducing sentence disparity.

Evenhandedness

All practitioners and lay participants in the system, as well as the general public, are concerned that the courts be evenhanded in the delivery of justice, although the bench has the primary responsibility in ensuring that it occurs. If "illegitimate" factors (e.g., the defendant's ethnicity, pretrial custody status, or type of defense counsel) that should not significantly affect how cases are disposed or sentences imposed, have done so, then steps can be taken to guard against such occurrences in the future.

Delay

Although it is universally recognized that justice should be speedy, few jurisdictions have comprehensive objective evidence on the duration of their cases. They are even less prepared to isolate the effects of the separate factors (e.g., nature of offense, type of disposition, type of defense counsel, backlog problems) that may tend to delay individual cases. Although some factors that might cause delay are not readily controllable (e.g., court caseload), there are others that are (e.g., continuance policy). Better measurement of delay and its determinants can help the court administration, for example, to improve the allocation of resources.

Efficiency

Court system managers—presiding judge, court administrator, district attorney, chief of the public defender office—have responsibility for efficiency of operations. They must seek to make resources, including manpower, appropriate to the workload, given some standard for individual productivity.

Attitudes of Lay Participants

Because jurors, victims, and other witnesses represent a bridge between the court system and the general public, practitioners will be concerned that these lay

participants come away from their criminal justice experience with favorable attitudes, other things being equal. The courts can institute policies designed to enhance favorable attitudes; therefore, knowledge of the relationship between characteristics of the treatment of lay participants and their attitudes becomes critical.

Thus, each set of performance measures that we discuss in this report has an "audience" among practitioners (and often in the general public, too). Access to various sets of statistical indicators can assist in the assessment of performance, and in the design and evaluation of new policies and innovations in the myriad aspects of criminal prosecution, defense, adjudication, and sentencing.

STUDY METHODS AND SOURCES OF INFORMATION

The information used in this study was obtained from literature relevant to performance measurement; interviews with practitioners and defendants; case files in various agencies; and mail surveys of victims, witnesses, and jurors.

Practitioner Interviews

Structured interviews with 33 experienced criminal justice practitioners in 13 jurisdictions were conducted to elicit their views on the value of performance measures, the selection of issue areas and the relevant performance measures, and the choice of the two demonstration jurisdictions.

Data Collected from Agency Records and Case Files

Rand data collection teams obtained data manually on approximately 2000 cases from various records made available by officials in the two demonstration jurisdictions and at the state level. In addition, a pilot-case auditing activity was conducted in which a team of outside practitioner-consultants examined 20 burglary-type cases disposed of by plea of guilty in each jurisdiction for the purpose of making judgments about the appropriateness of decisions that were made by participants at various stages in the felony proceeding. The case-audit activity also included extensive interviews with practitioners in both jurisdictions.

Surveys of Lay Participants

Mail surveys, using questionnaires designed for this study, were administered to 1200 individuals—200 to each group of victims, other witnesses, and jurors in each jurisdiction. Questions covered attitudes, experiences, and background characteristics of these lay participants. In addition to analyzing their responses, we tested the efficacy of such questionnaires as potential tools for jurisdictions interested in measuring attitudes and determining which policy factors affect attitudes.

Personal interviews were conducted with upward of 50 defendants, split about equally between the two jurisdictions. The major purpose was to field test the interview questionnaire and contact procedures, although some data were collected on defendants' experiences and attitudes.

Once performance measures were calculated from the raw data elements, standard and specially developed software packages (i.e., computer programs) were used

to cross-tabulate the performance measures and to analyze (i.e., explain) their variation across cases.[3]

THE PERFORMANCE MEASURES

Because of space limitations, we do not list in this summary the many performance measures that were selected to illuminate each of the issue areas. The reader is referred to Chap. 2 of R-1917-DOJ for a discussion of (1) the selection criteria used to screen candidate sets of measures; (2) the selected sets of performance measures and the data elements necessary for computing their value; and (3) the rationale of what they can reveal, as well as conceal, about performance in each issue area.

GENERAL FINDINGS AND IMPLICATIONS

On the Feasibility of Applying Performance Measures

Our study has shown that *it is feasible to apply performance measures* to data already available in court agencies' files, even though incomplete, and *to draw inferences about whether and how performance in specified issue areas changed in a jurisdiction.* To a lesser extent, too, we have shown that it is feasible (within carefully specified limits) to make interjurisdictional comparisons of performance, using the measures specified in this study. The careful collection of specified data elements; the computation, grouping, and cross-tabulation of performance measures; and the analysis (using multivariate statistical techniques) of what factors account for the variation in key performance measures all can provide greatly strengthened informational bases for officials in court, prosecution, and public defender agencies to improve criminal proceedings.

We were more successful in applying performance measures to certain policy issues than to others because of inherent differences in the precision or ambiguity of the performance measures (e.g., in those that measure changes in delay compared with those that measure changes in the charging threshold) or because of differences in the availability of data (e.g., the availability of data on sentence agreements in Multnomah County compared with its unavailability in Dade County for measuring plea bargaining effects).

The actions to be taken jointly by the court, prosecution, and public defender agencies in a jurisdiction to strengthen the informational and analytical base for measuring their performance may be visualized as an *integrated performance measurement program* (IPMP). A fairly comprehensive IPMP would consist of:

- An enumeration of required data elements (or categories) and performance measures.
- Standardized data collection and output forms for each policy issue area of

[3] For example, multivariate regression analysis was employed to isolate the independent effect of selected factors that were hypothesized to affect three key performance measures of outcomes in the felony proceeding: probability of conviction, sentence severity (given conviction), and delay (i.e., elapsed time between arraignment and final disposition).

interest (the ones we considered and/or others of interest to particular jurisdictions).

- Flexible, modular software (i.e., computer programs) packages for computing, displaying, and analyzing performance measures within each issue area (e.g., for performing cross-tabulations and for applying multivariate regression models that help to explain conviction probability, delay, and sentence severity imposed).
- Guidelines for conducting case audits at each major decision point (screening, guilty plea, trial, and sentencing) in the proceeding, using either outside practitioner-consultants or in-house supervisory personnel.
- The administration of sampling plans and standard mail survey questionnaires and the analysis of responses of victims, other witnesses, and jurors (using appropriate software packages).
- The administration of sampling plans and standard personal interview questionnaires and the analysis of responses of defendants (using appropriate software packages).

If data collection procedures and software packages were flexible and modular in design, the scale and scope of an IPMP could be tailored to individual jurisdictions. For example, the three agencies in a jurisdiction could decide whether to embrace all elements (e.g., to include case auditing and defendant interviews) and whether to measure performance in all of the listed issue areas (e.g., to include the measurement of case-processing efficiency in the prosecutor's and public defender's office, as well as in the court). What would be vital to proper tailoring is a clear enunciation by agency officials of the management and policy issues on which performance measurement should focus.

This study is a *first step* toward the design of an IPMP. We have enumerated required data elements and performance measures and, with varying degrees of success, have devised and applied statistical models to explain key performance measures. We have also designed and applied mail and personal interview questionnaires to the four classes of lay participants. More work needs to be done, however, and its nature is discussed below.

On Methodology and Data Availability

Case Audits. Our pilot-case auditing exercises (for cases in which there was a plea of guilty) in the demonstration jurisdictions strongly suggest that they provide *complementary* information about qualitative factors that aid in the interpretations of the statistical performance measures. (By their very nature, case audits are much more expensive per case included than the data collection required to develop statistical performance measures. Thus, with limited resources, audit samples are inevitably too small to stand alone as a *substitute* for statistical performance measures.)

One benefit results because the average practitioner probably regards case auditing as a natural and nontechnical way of revealing performance. His confidence in the correctness of what is shown by statistical performance measures is undoubtedly increased when the results of (even quite limited) case auditing corroborate the statistical story. Another possible benefit of case auditing is that it may help reveal the explanations for the "behavior" of statistical indices. And, finally, it may consid-

erably strengthen the credibility of *inter*jurisdictional comparisons made by means of statistical measures. (Our suggestions for broadening case auditing to test its value more fully are discussed below.)

Data Availability. A salient lesson in our attempt to demonstrate the application of performance measures in two selected jurisdictions was that many necessary or desirable data elements normally recorded in various files were missing from the customary records; some were simply not recorded at all. And this is likely to be the situation in other jurisdictions as well.

Among the data elements that had been (at best) *incompletely recorded* and preserved were defendant-related characteristics, such as ethnicity, prior criminal record, occupation and employment, family status, income, and transiency; the number of appearances per victim or other witness in the course of a proceeding; data describing how judges apportion their time among judicial tasks; and attribution of continuances to the responsible movant(s). However, even with incomplete recording of these data, we were successful in applying performance measures to issue areas requiring these data (with the exception of the judicial weighted caseload).

Among the data elements that were *not recorded* at all were the apportionment of time among the principal activities of prosecutors, public defenders, and jurors; background characteristics of suspects whose cases were screened out before arraignment on felony charges; full information on the outcome of plea bargaining, including the nature of any sentence agreement reached; judicial statements of the rationale for sentences in individual cases; detailed reasons for case dismissals in lower court; duration of appearances of victims and other witnesses; and information on the attitudes of lay participants and defendants toward their experiences and toward the performance of the court agencies. The unavailability of these data not only made it impossible to analyze such issue areas as the use of time by prosecutors and public defenders and evenhandedness in screening but also permitted only partial analysis of the plea bargaining balance and charging accuracy in one jurisdiction. With *special* data collection through surveys of lay participants, it was possible to assess such issue areas as the use of their time and their attitudes toward the court system.

DESIRABLE EXTENSIONS

We feel that a fuller foundation for the design of an operational IPMP would be provided by the following extensions in scope and refinement in methodology to our demonstration work:

- Classes of data that were not recorded or were incompletely recorded in Multnomah and Dade counties should be collected and analyzed elsewhere. *Evenhandedness in screening* should be analyzed with a proper body of data containing appropriate defendant-related characteristics. The *allocation of prosecutors' and public defenders' time* to their various activities is another performance area warranting examination and would need a proper body of data.
- The assessment of case auditing should be broadened in the *screening area* (to include rejected cases) and also extended to the *trial area*, so that our

inferences as to the value of case auditing as a complement to statistical performance measurement can be tested more fully.

- Improved statistical models should be constructed to help explain performance outcomes in criminal proceedings. Those we developed for explaining sentence outcomes and delay in proceedings worked fairly well but need further refinement. Because we were unsuccessful in explaining the determinants of conviction probability, we believe much more theoretical and empirical work is necessary. We speculate that data on the seriousness of the crime incident, on mitigating and exacerbating circumstances of the defendant and the crime incident, and on factors describing the strength of the case at the time of screening, are relevant for constructing better conviction probability models.

How Potential Capabilities of Planned Information Systems Compare with an IPMP

One major consideration for local agencies that may be interested in moving toward an IPMP is that considerable resources already have been or will be devoted to existing or planned information systems such as CCH/OBTS, SJIS, and PROMIS.[4] It is important to know how their potential performance measurement capabilities (which issue areas can be analyzed in what depth?) compare with an IPMP under two conditions: (1) the basic systems with only those data elements that are already collected, assuming that simple software packages (with a cross-tabulation capability) are available; (2) modest, inexpensive upgrading of the basic systems (by adding a few new data elements[5] to be collected, together with a more sophisticated software package, for example, statistical models and standard multivariate statistical analysis routines for estimating the independent effect of important factors on delay and sentence severity imposed).

Capabilities of the Basic Systems Planned.[6] Given the data elements collected by these information systems and the availability of (at best) simple software packages, all of the systems have a valuable capability for measuring performance in the delay estimation and charging accuracy issue areas, and a partial capability in the areas of plea bargaining, sentence variation, evenhandedness, and determinants of delay. *In addition,* SJIS is capable of very gross estimates of the use of judicial time, and PROMIS has a good capability in the charging threshold area and a partial capability in addressing evenhandedness in screening (whereas CCH/OBTS and SJIS have no capabilities in the screening area).

Capabilities If Planned Systems Are Upgraded. Upgrading any of the basic systems (as noted above) would enable better analysis of the plea bargaining balance, the independent effects of important factors on delay, and the independent effects of legitimate and illegitimate factors on sentence severity imposed for all the systems. *In addition,* upgrading of PROMIS would improve the capability to analyze evenhandedness in screening.

Capabilities of an Improved IPMP. If an IPMP were improved and extended

[4] See Sec. XI for definitions and descriptions of these information systems.

[5] See the footnotes to Tables 11.2 and 11.3 in Sec. XI for the few additional data elements that can be collected inexpensively.

[6] We assess system capabilities in terms of the issue areas addressed in this study; capabilities of these systems to address *other* issue areas are not assessed.

in the ways noted above, its performance measurement capabilities would have greater breadth, because many data elements specified for it are not collected by the basic (or upgraded) existing or planned systems. Although each system could function as a partial IPMP, *none* of the systems are designed to measure (as would an IPMP) performance in the following areas: the effect of legitimate factors on conviction probability; continuances (except for PROMIS); the use of lay participant (victim, witness, juror) time; the use of practitioner (judge, prosecutor, public defender) time; and the attitudes (and their determinants) of lay participants.

The Costs and Utility of Various Information Systems

Careful estimates of the range of incremental costs for implementing and operating a partial or full (improved) IPMP or of upgrading existing or planned systems were beyond the scope of this study. However, based on actual resources used in various activities of this study and on rough guesses of costs of activities not covered in this study, we can *bound* the range of likely costs within, say, a factor of two.

For a jurisdiction *with* one of the existing or planned information systems, *incremental* (i.e., over and above the costs of the basic system) annual costs on the order of $10,000 might be incurred for upgrading the system and assessing performance annually. This assumes that appropriate software packages are made available free and that any practitioner time devoted to additional raw data generation is "free." Given the relatively low marginal costs associated with upgrading an existing or planned system and its major benefits outlined above, it is probably cost-effective for a jurisdiction to pursue this alternative.

For a jurisdiction *without* an existing information system (but with access to a computer) that wishes to implement and operate a fully improved IPMP, on the order of $50,000 per year in operating costs are implied, once it is set up. (First-year costs should be considerably higher because of nonrecurring setup costs.) This rough estimate assumes (as with the previous case) that software and practitioner time are free and that the number and size of case file and survey response samples to be collected and analyzed are similar to those collected and analyzed in this study. Of course, additional samples or larger samples would increase costs, and exclusion of certain issue areas from an IPMP would reduce costs.

Whether implementing a full IPMP has adequate utility—that is, whether incremental benefits sufficiently outweigh incremental costs—is a judgment that can be made by an implementing jurisdiction only after such an approach is installed and operated over several years. At that point, the costs will be much less uncertain and its benefits can be assessed by the policymakers involved.

APPLICATION OF PERFORMANCE MEASURES

In the interests of brevity, we do not summarize the findings obtained from the applications of performance measures in the two demonstration jurisdictions, but refer the reader to Secs. VI, VII, IX, and X of this report and Chaps. 3 and 4 of the accompanying report (R-1917-DOJ).

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Interviews with defendants in Multnomah County were conducted by K. Bobzein, M. Craven, and C. Henry, all students in Administration of Justice at Portland State University. In Dade County, they were conducted by T. McDonald, J. Taylor, and D. Weinberger, students or recent graduates from the University of Miami School of Law.

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I. INTRODUCTION

Sorrel Wildhorn

OVERVIEW

This report presents the findings of a broad study of performance measurement of criminal justice agencies involved in the felony proceeding—post-arrest through disposition (and sentencing if it occurs). The primary focus of the study is on the selection, estimation, and analysis of performance measures as *statistical devices* that aid in the interpretation of data drawn from *court system operations* (i.e., from case files and other records in court, prosecution, and public defender agencies). Performance measures may be viewed either as (1) quantitative descriptors of *what* is being done in felony proceedings or (2) as progress indices of *how well* these functions are being performed. Examples of the former are the number and proportion of all felony filings that are disposed of by dismissal, plea of guilty, conviction at trial, and trial acquittal or dismissal; an example of the latter is the proportion of felony trials exceeding the speedy trial standard.

This study emphasizes the latter role of performance measures. Its emphasis is to be contrasted with, for example, the application of standards and goals as articulated in the series of volumes issued since 1968 by the American Bar Association on Standards for Criminal Justice,[1] or those that resulted in 1973 from the work of the National Advisory Commission on Criminal Justice Standards and Goals.[2] Few of these hundreds of individual goals and standards relating to criminal proceedings are couched in quantitative terms or lend themselves to quantitative interpretation, the primary exception being those concerning the "speediness" of the proceeding.

The secondary focus of this study is on performance measures of the court system as viewed through the eyes of *lay participants* in the felony proceeding—victims, other witnesses, jurors, and defendants. That is, their attitudes toward the court system are performance measures of interest, which can be elicited through survey techniques. Moreover, with proper statistical analysis, their attitudes can be related to their individual experiences with, and treatment by, the court system.

In this report we describe the roles or uses of performance measures; the broad purposes and scope of the study; the relationships between the issue areas considered in this study and the interests of agencies and practitioners responsible for making policy; general criteria for selecting performance measures; the performance measures themselves and how they illuminate the issue areas; the data

[1] The individual volumes (and their dates of approval) include *ABA Standards Relating to Pretrial Release* (1968); *Providing Defense Services* (1966); *Fair Trial and Free Press* (1968); *Pleas of Guilty* (1968); *Speedy Trial* (1968); *Joinder and Severance* (1968); *Trial by Jury* (1968); *Sentencing Alternatives and Procedure* (1968); *Appellate Review of Sentences* (1968); *Post-Conviction Remedies* (1968); *Discovery and Procedure before Trial* (1970); *Probation* (1970); *Criminal Appeals* (1970); *Electronic Surveillance* (1971); *The Prosecution Function and the Defense Function* (1971); *The Function of the Trial Judge* (1972); *The Urban Police Function* (1973), *Court Organization* (1974); and *Trial Courts* (tentative draft, 1975).

[2] The six individual reports of the National Advisory Commission are entitled *A National Strategy To Reduce Crime, Criminal Justice System, Courts, Police, Corrections,* and *Community Crime Prevention* (1973).

elements necessary to estimate them; the techniques used to estimate, display, and explain changes in performance measures; the inferences (as well as the qualifications or ambiguities inherent in these inferences) that can be drawn from illustrative applications made in two (county) jurisdictions; the general implications of, and lessons learned from, the study; and the need for certain extensions to this study.

Finally, following a "tri-level approach," we illustrate for jurisdictions that may be interested in applying our performance measurement approach:

1. Which issue areas can be analyzed using information systems *currently installed or planned in some jurisdictions.*
2. To what extent a modest and inexpensive *extension in data elements collected by these systems* could improve the scope and depth of performance measurement capabilities.
3. Those performance measurement applications to issue areas that would require *new* (and more costly) data collection and analysis efforts, because current or planned information systems do not suffice for such applications.

It must be emphasized that certain software packages (such as those applied in this study) would be required for analyzing performance, *whichever* of the three alternatives above is pursued.

ROLES AND USES OF PERFORMANCE MEASURES

The potential roles and uses of statistical performance measures in the felony proceeding can be categorized as follows:

Within a Jurisdiction

1. Routine tracking or administrative monitoring of *pending* cases in the prosecution, defense, and court agencies.
2. *Retrospectively* comparing the performance of the full court system, its component agencies, and its individual practitioners at different times:
   a. When no major policy change or innovation is introduced.
   b. When a major policy change or innovation (procedural, legislative, administrative) is to be evaluated.
3. *Prospectively estimating the performance effects of a major policy change or innovation.*

Among Jurisdictions

1. *Retrospectively* comparing the full court systems and component agencies in different jurisdictions:
   a. Comparing different jurisdictions within a state (e.g., of interest to a state-level judicial council or supreme court).
   b. Evaluating whether a particular policy or innovation in one jurisdiction has similar effects in another.
2. Helping to show the condition of the criminal justice process at the *state* and *national* levels.

Other

1. Prompting and guiding research on ways to enhance the administration of justice in felony proceedings.

Even though the collection and use of statistical data describing the operations of court, prosecution, and defense agencies have increased over the last decade, the major use of these data has been to provide (often rudimentary) assessments of how speedily and efficiently a jurisdiction disposes of its pending caseload. There has been less inclination to employ statistical indicators to measure other aspects of felony proceedings relating to the quality of justice, notwithstanding the upsurge in production of (largely qualitative) standards and goals bearing on these proceedings, as mentioned above. Basically, statistical descriptors have been widely used to depict *what* is going on in felony proceedings. Thus, there is a marked gap between the articulation of (largely qualitative) goals and standards on one hand and the measurement of progress toward goals on the other. Given this observation, the broad purpose of our study was to reduce that gap.

PURPOSES AND SCOPE OF THE STUDY

The specific purposes of the study were:

- To identify, screen, and evaluate sets of performance measures as *indices of progress.*
- To demonstrate the applicability of these performance measures in two selected (county) jurisdictions.

As "outside" analysts, we could, at best, aim to demonstrate the *feasibility of applying* performance measures. Whether this application is "practical" and whether the benefits of applying performance measures would outweigh the incremental costs can thereafter be assessed by having officials in one or more jurisdictions adopt the approach, use it over some period of time, and then make the necessary cost/benefit judgments. Incremental costs of applying the full range of performance measures would vary from jurisdiction to jurisdiction, depending on the type of information system that was planned or already installed and the extent to which the jurisdiction would desire to measure performance in issue areas *outside* of the capabilities of existing information systems. Careful estimates of the range of such incremental costs were beyond the scope of this study. However, we provide (in Sec. XI) very rough incremental cost estimates for two bounding cases: a gross estimate of costs for implementing a relatively comprehensive performance measurement system from scratch, assuming that a jurisdiction has access to a computer but has not installed (or does not plan to install) an information system; and a gross estimate of the incremental cost associated with "upgrading" (i.e., adding a few data elements that can be collected inexpensively and special software packages) existing or planned information systems such as CCH/OBTS, SJIS, or PROMIS.[3]

Resource limitations necessarily limited the scope of this study. Attention was confined to *adult felony proceedings;* thus, we did not consider misdemeanor proceed-

[3] See Sec. XI for definitions and descriptions of these systems.

ings or civil proceedings. We addressed only the *primary* activities of the court system, excluding supporting activities and functions performed by court clerks, court reporters, bailiffs, or paralegals in the prosecutor's office or the public defender's office. Two broad types of performance applications were made: *retrospective* comparisons of the full court system and of its component agencies (rather than of individual practitioners) *within* the jurisdiction at different times; and *retrospective interjurisdictional* comparisons of court systems and component agencies at the same time. To the extent that our statistical modeling was successful in predicting and explaining certain performance measures of outcomes, we developed a very limited capability to do *prospective* performance analysis.

The study focused on a set of persistently important issue areas. Our view of their importance was confirmed by the results of personal interviews with 33 practitioners (judges, prosecutors, defense counsel, court administrators, and legal scholars) in 13 large urban court systems throughout the United States and by the views of an advisory panel of distinguished practitioners. These issue areas were:

- Prosecutorial (or other) case screening: limited to the subissues of adherence to charging standards and charging accuracy.
- Plea bargaining: viewed as a balance between gains to, and other "operational effects" (some possibly harmful) on, the court system on one hand and system "concessions" to defendants on the other.
- Sentencing variation: how much variation; to what extent "legitimate" as opposed to "illegitimate" factors[4] explain the variation.
- Evenhandedness or consistency of disposition and sentencing: to what extent such outcomes are affected by illegitimate factors.
- Delay (or measures of elapsed time) between major events in the felony proceeding.
- Case processing efficiency (as reflected in the use of judicial, prosecutorial, and defense counsel time) and the use of lay participant (jurors, victims, and other witnesses) time.
- Attitudes of lay participants toward the court system and its practitioners.

RELATIONSHIPS BETWEEN ISSUE AREAS AND THE POLICY INTERESTS OF PRACTITIONERS AND AGENCIES

The issue areas considered in this study are important in the administration of criminal justice because they involve significant aspects of the performance of the court system. For some of these issue areas—delay, efficiency, evenhandedness, charging accuracy, attitudes of lay participants—practitioners and observers all agree on the direction of improvement to be sought. For others—the charging threshold, the effects of plea bargaining, sentence variation—they can agree only that further clarification is desirable. Also, certain agencies and types of practi-

[4] There is considerable controversy over whether *certain* factors are legitimate or illegitimate in sentencing decisions; for others, there is general agreement. For purposes of this study, we have assumed that the defendant's age, prior criminal record, community ties, and the nature of the original and convicted charges and counts are legitimate factors; and that ethnicity, pretrial custody status, type of defense attorney, type of disposition (trial or guilty plea), and correctional facilities crowding are illegitimate factors. See the discussion in Sec. IV.

tioners find particular issues to be of greater interest than others, either because of relevance to their own performance or because of concerns about current policy and how current policy effects compare with effects of past policy.

Charging Standards

Prosecutors' offices in most jurisdictions need objective evidence of the standards being implemented to discern whether they conform to policy and what the effects on the system are if policy changes. For example, if the charging threshold is lowered, how are court workload, delay, and plea bargaining affected? Measures of the operation of the charging threshold over time also can reveal trends in police performance.

Charging Accuracy

Is the nature of the disposition of cases being unduly affected by the accuracy with which charges are filed against defendants? Is the court workload being magnified by the consequences of inaccurate charging? Is inaccuracy in the charging process abusing defendants' rights? Such questions concern not only prosecutors, but also judges, defense counsel, court administrators, and others in addressing charging policy and practices in their jurisdiction.

Plea Bargaining

What are the nature and frequency of the practice of plea bargaining in the jurisdiction? Quantitative evidence available to practitioners is often scant on this question. And the public rarely has seen even the rudiments of an objective picture. What is the court system gaining from plea negotiation? How are delay and efficiency of resource use being affected? Is punishment significantly lighter than in the absence of such negotiation? All practitioners, and the public as well, have a vital stake in these questions, even though plea bargaining policy is primarily a prosecutorial responsibility.

Sentence Variation

Judges and other practitioners want to know the degree of consistency in sentencing in one court as compared with others, how sentencing practices change over time, and how they vary among judges within a court system. If, in addition, quantitative evidence were available to explain how much of the observed variation was accounted for by various legitimate and illegitimate factors, such clarification may enhance the effectiveness of various devices (e.g., sentencing panels or appellate review of sentences) aimed at reducing sentence disparity.

Evenhandedness

All practitioners and lay participants in the system, as well as the general public, are concerned that the courts be evenhanded in the delivery of justice, although the bench has the primary responsibility in ensuring that it occurs. If illegitimate factors (e.g., the defendant's ethnicity, pretrial custody status, or type

of defense counsel) have significantly affected how cases were disposed or the sentences imposed, steps can be taken to guard against such occurrences in the future.

Delay

Although it is universally recognized that justice should be speedy, few jurisdictions have comprehensive objective evidence on the duration of their cases. They are even less prepared to isolate the effects of the separate factors (e.g., nature of offense, type of disposition, type of defense counsel, backlog problems) that tend to delay individual cases. Although some factors that might cause delay are not readily controllable (e.g., court caseload), there are others that are (e.g., continuance policy). Better measurement of delay and its determinants can help the court administration, for example, to improve the allocation of resources.

Efficiency

Court system managers—presiding judge, court administrator, district attorney, chief of the public defender office—have responsibility for efficiency of operations. They must seek to make resources, including manpower, appropriate to the workload, given some standard for individual productivity.

Attitudes of Lay Participants

Because jurors, victims, and other witnesses represent a bridge between the court system and the general public, practitioners will be concerned that these lay participants come away from their criminal justice participation with favorable attitudes, other things being equal. Lay participants *ought* to feel that justice was done, that the system performs effectively, that they themselves were treated satisfactorily, and they ought to be willing to cooperate again in the future. Court agencies can institute policies designed to enhance favorable attitudes; therefore, knowledge of the relationship between characteristics of the treatment of lay participants and their attitudes becomes critical.

Thus, each set of performance measures we discuss in this report has an "audience" among practitioners and often in the general public. Access to various sets of statistical indicators can assist in the assessment of performance, the design of new policies, and the evaluation of innovations in the myriad aspects of criminal prosecution, defense, adjudication, and sentencing.

STUDY METHODS AND SOURCES OF INFORMATION

In the initial period of the study, we constructed a hierarchy of recognized goals of the criminal justice system as a whole, and of felony proceedings as a whole. This goal structure was intended to be the framework to which the individual performance measures would be related. They would then be assembled into sets that would assess how closely the proceedings in a jurisdiction approached the goals.[5] In an early review by our Advisory Group (made up of distinguished jurists, prosecu-

[5] For a discussion of goals and their links to individual performance measures, see Sec. II.

tors, defense attorneys, law professors, analysts, and experts in court administration),[6] it was clear that no general consensus among practitioners on any specific goal structure was forthcoming. Thus, a "fail-safe" quality was lacking: If any specific goal structure was not accepted, the credibility of this study would suffer. In response, we reshaped the study's scope by focusing on several persistently important areas (noted above) to which we could apply performance measures to assess at least the desired direction of movement.

A series of interviews with experienced criminal justice practitioners and the analysis of their responses comprised the second phase of this study. There were six interviews with judges, seven with court administrators, five with defense counsel, six with prosecutors, and two with academicians—the interviews being distributed in 13 jurisdictions across the country.[7] The interviews, which ranged from several hours to a full day in duration, informed us about the views of experienced practitioners toward the use and value of performance measures, the selection of issue areas and the relevant performance measures, and focused our consideration on the choice of the two demonstration jurisdictions. The results of these interviews were as follows:

- There was some controversy as to the value and acceptability of statistical performance measures standing by themselves; the more experience a practitioner had had with statistical data, the less he distrusted them and the more realistic he was about their use. Many suggested—and we implemented—a complementary (to the mainstream effort of statistical performance measurement) pilot approach we call "case auditing." We asked a team of experienced consultant-practitioners (in this instance, prosecutors) to audit 20 burglary-type cases disposed of by pleas of guilty in each jurisdiction. They made judgments about the appropriateness of decisions and actions by practitioners at various stages in the felony proceeding, given the information available to them at the time.
- The issue areas that we selected for analysis were demonstrated to indeed be important, and there was no consensus on *additional* ones to be analyzed.
- Our choice of performance measures was enriched, and we expanded the check list of "comparability features" that govern the validity of *interjurisdictional* comparisons by means of performance measures.[8]
- We were led to choose Multnomah County (City of Portland), Oregon, and Dade County (City of Miami), Florida, as cooperating jurisdictions in the demonstration phase.

In the demonstration phase, we collected operational data from records in the prosecution, public defender, and court agencies and survey data (through mail questionnaires and personal interviews) from lay participants in both jurisdictions. The information collected is given below.

From Agency Records and Case Files

Rand data collection teams obtained data directly from a variety of agency

[6] See App. B for a roster of the Advisory Group members.

[7] See App. B for identification of the interviewees. The results of the interviews are given in Sec. III.

[8] See Sec. IX.

records and case files made available by officials in the demonstration jurisdictions and at the state level.[9] These included 1200 cases filed in felony court (samples of 100 each of burglary or breaking and entering, robbery, and all felonies in each jurisdiction in each of two years) containing data on the nature and number of original and convicted charges, plea bargaining information, dates of major events in the case, disposition, sentence, and a variety of defendant-related characteristics. In addition, separate samples were collected for continuances and for victim and other witness appearances, where necessary, as were samples for screening actions (100 each of police-booked burglary and robbery cases in two years in both jurisdictions) and for rejection reasons (samples or census of burglary and robbery "rejections" or "no-information" in two years in both jurisdictions). In addition, a pilot-case audit activity was conducted as noted above, including extensive interviews with practitioners in both jurisdictions.

From Surveys of Lay Participants

Mail Surveys. Using questionnaires designed for this study, 1200 individual mail surveys were administered—200 to each group of victims, other witnesses, and jurors in each jurisdiction. Questions covered attitudes, experiences, and background characteristics of these lay participants. In addition to analyzing their responses, we wanted to test the efficacy of such questionnaires as potential tools for jurisdictions interested in measuring attitudes and determining which policy factors affected attitudes.[10]

Personal Interviews. We interviewed somewhat less than 50 defendants split about equally among the two jurisdictions. The major purpose was to field test the interview questionnaire and contact procedures, although some data were collected on defendants' experiences and attitudes.[11]

Once performance measures were calculated from the raw data elements, standard and specially developed software packages (i.e., computer programs) were used to cross-tabulate the performance measures, to estimate whether observed changes were statistically significant, and to analyze the performance measure variation across cases. For example, multivariate regression analysis was employed to uncover the independent effect of selected factors that were hypothesized to affect performance measures of outcomes in the felony proceeding. These outcome measures included probability of conviction, sentence severity (given conviction), and delay (i.e., elapsed time between arraignment and final disposition).

A GUIDE TO THE STUDY REPORTS

R-1917-DOJ (A Guide to Practitioners)

Chapter 2 discusses the selection criteria used to screen performance measures, lists the sets of performance measures selected for each issue area together with the

[9] For a complete description of the methods and sources used to collect these data, see App. D.

[10] For a complete description of the methods and results of the mail surveys, see App. F.

[11] For a complete description of the methods and results of the defendant interviews, see App. I.

data elements needed to compute their values, and provides a brief rationale on what the measures reveal, as well as conceal, about performance. Chapter 3 illustrates one of the roles of performance measures—an application in one jurisdiction to illuminate how performance changes from year to year. Chapter 4 illustrates another role of performance measures—an application comparing performance in two jurisdictions in one year. Finally, Chap. 5 presents the general findings and implications that emerged from this study.

R-1918-DOJ (Analysis and Demonstration)

This companion report provides a comprehensive and detailed description of all aspects of the work of this study. Section I discusses the roles and uses of performance measures, the study purposes, and scope and provides an overview of the methods and sources of information used. Section II provides a background discussion and a limited literature review. Section III is a discussion of professional views on statistical performance measures as gathered from a series of interviews with practitioners. Section IV discusses the selected performance measures that were found useful. It includes a description of the selection criteria used to screen and select candidate sets of performance measures; a brief rationale on what the measures reveal, as well as conceal, about performance in each of the selected issue areas; and a description of the data elements necessary for computing values of the performance measures.

Section V provides our rationale for selecting the two demonstration jurisdictions and a (largely qualitative) description of the component agencies of the court systems in both jurisdictions. Sections VI and VII apply the performance measures to each issue area in Multnomah and Dade counties, respectively, to illuminate how performance changes from year to year. Section VIII discusses the role of criminal case auditing in performance measurement in general terms and then describes the results of a pilot application made in the two jurisdictions. Section IX compares performance in the two jurisdictions in one year. Section X discusses the general procedures and results of the mail surveys of victims, other witnesses, and jurors and of the defendant interviews. Finally, Sec. XI, which is identical with Chap. 5 of the *Guide to Practitioners*, presents the general findings and implications that emerged from this study.

Various appendixes discuss the links between goals and performance measures of criminal proceedings (App. A), various considerations for making interjurisdictional comparisons by means of statistical performance measures (App. C), data collection methods and sources used in the two jurisdictions (App. D), and the mail survey procedures used (App. F). Others display results of the statistical analyses of the case file data (App. E) and of the lay participant survey data (App. H), and show representative questionnaire instruments developed and used in the study (Apps. G and I). Appendix B lists the criminal justice practitioners interviewed and the study Advisory Group members.

II. BACKGROUND AND LITERATURE REVIEW

Marvin Lavin

CONTEXTS FOR THE USE OF CRIMINAL JUSTICE DATA

This study focuses on the use of statistical performance measures in the context of criminal (felony) proceedings—post-arrest through disposition (and sentencing if applicable). Our central concern is with statistical indicators that measure the performance of the primary criminal court agencies—namely, the court itself, the office of the prosecutor, and the office of the public defender and other defense counsel. For the moment, we characterize *performance measures* simply as statistical devices that help in the interpretation of quantitative information about system operations. Later, we expand and deepen this characterization.

Collecting and processing criminal justice data are burgeoning activities. A leading writer in the field tells why:

> Despite the uncertainty of objectives, the inherent difficulty of the crime problem itself, and limited resources to cope with it, there are more pressures on criminal justice agencies to measure and evaluate their own activities than on almost any other public system in our society. . . . (C)orrections and other criminal justice agencies have strong mandates not only to report their activities but to assess their effectiveness, and thus to justify in measurable form their very existence. Normally it is not sufficient for the police to report how many crimes they investigated; they must also indicate how many they solved. Prosecutors often feel obligated to give a win-loss report in order to be reelected; judges face the responsibility of appellate reversal of their actions if they are deemed improper by a higher court; and prisons must make effectiveness claims of both security and rehabilitation. The time spent by criminal justice agencies counting and evaluating their activities probably exceeds that of any other public system, yet, as pointed out earlier, the amount and reliability of criminal justice reporting is less than adequate. One consequence of this pressure to report is that illusory improvements or failures can be shown simply by changing methods of record keeping, making rates appear to increase or diminish. . . . In any event, by selective statistics or by other means, there is great pressure within and between criminal justice agencies toward self-maintenance, toward winning a larger share of available tax-based resources . . . .
>
> There are other minor intra-agency objectives that may become important in understanding or evaluating a criminal justice agency or its processes. Because they are public agencies, all criminal justice offices and bureaucracies have public relations needs. Agency image is important not only for receiving budget support, but also for maintaining respect of the public at large, particularly if their cooperation is sought in helping to achieve crime control goals. Image making is not directed outside exclusively, but is equally important for those who work in the agencies, to maintain the morale and *esprit de corps* necessary to attain even limited success in achieving objectives.[1]

[1] From *Introduction to Criminal Justice*, by Donald J. Newman, pp. 47-48. Reprinted by permission of the publisher, J. B. Lippincott Company, Philadelphia, 1975.

To reiterate, the primary thrust of this study is to the question of how criminal justice data and more particularly how statistical performance measures may be used to analyze the operations in the criminal proceeding. As an aid to placing this research in a larger context, we summarize in Table 2.1 a comprehensive discussion of alternative approaches to analyzing the criminal justice process given by D. J. Newman.[2] Each identified approach differs in the degree to which it relies on the use of operational data and in the nature of data used. Our study approach most closely relates to the *Agency Practices* Approach and the *Recurrent Themes* Approach as characterized by Newman.

HISTORICAL PERSPECTIVE ON JUDICIAL STATISTICS

Efforts, historically, to collect *court data* reflect a snail's pace growth of the recognition that this type of criminal statistics has rewarding uses. J. A. McCafferty describes the primitive beginnings at the federal level in the 19th century, involving mostly a handcounting of cases docketed and disposed of by the separate courts.[3] Apparently, there was no significant advance in collecting and processing federal court data until the 1930s when punched-card methods were introduced. A few state jurisdictions began collecting court statistics in the 1920s; in 1931 a landmark Ohio study indicated the feasibility of a permanent state reporting program.[4] The Bureau of the Census, pursuant to a 1931 act of Congress, initiated the collection and compilation of statistical information on state court operations, with the participation of about 30 states (at one time or another). This program was abandoned in 1946, because of poor cooperation by state courts and limited resources.[5]

Advances in the collection and use of judicial statistics during the 1930s and 1940s were meager despite the farsighted recommendations of the National Commission on Law Observance and Enforcement (the Wickersham Commission) given in its *Report on Criminal Statistics*. But in the 1950s and the 1960s, as the business of the criminal courts grew increasingly heavy, and as substantive and procedural law reforms were made and felt, an impetus developed to obtain informational bases by which to improve the administration of the courts. Yet, even as late as the mid-1960s, the availability of judicial statistics, especially on a national scale, was limited.[6] In the past decade, forces for change—including those exerted by the President's Commission and by the National Advisory Commission—have constructively reshaped the situation. One effect has been such research and development programs as Project SEARCH,[7] which has shown the *technological* feasibility of computerized information systems that can meet the needs of the criminal justice

[2] Ibid., pp. 115-130.

[3] J. A. McCafferty, "The Need for Criminal Court Statistics," *Judicature*, Vol. 55, No. 4, November 1971.

[4] *Ohio Criminal Statistics, 1931*, Johns Hopkins Press, Baltimore, Md., 1932.

[5] H. Alpert, "National Series of State Judicial Criminal Statistics Discontinued," *Journal of Criminal Law and Criminology*, Vol. 39, No. 2, 1968.

[6] See the discussion in P. Lejins, *National Crime Data Reporting System: Proposal for a Model*, Appendix C, Task Force Report: *Crime and Its Impact—An Assessment*, Task Force on Assessment, The President's Commission on Law Enforcement and Administration of Justice, 1967.

[7] Project SEARCH (System for Electronic Analysis and Retrieval of Criminal Histories) was launched in 1969 by LEAA funding. It has sought to integrate the criminal statistics reporting systems of a number of states by means of an on-line information system for the exchange of offender files.

Table 2.1

Approaches to Criminal Justice Analysis[a]

I. *Criminal Procedure* (Statutes and Case Law) Approach

Studying procedural law that relates to each decision point from the police investigation of crimes to parole and discharge from sentence; highlighting issues that are current and controversial.

II. *Agency Practices* Approach

Studying the day-to-day practices of police, prosecutors, judges, correctional personnel, defense counsel, and other participants in the criminal justice process; seeking to ascertain the criteria applied by these practitioners in performing their functions and to explain deviations from normal practices.

Three methods of analyzing agency practices:

- Statistical--tabulate how frequently each discretionary alternative occurs at major decision stages of the criminal justice process.
- Descriptive--describe the qualitative range of discretionary alternatives at each major decision stage and identify reasons for variations.
- Evaluative--assess the effectiveness of decisions made at the major stages.

III. *Models* Approach

Constructing simplified, aggregative representations of the criminal justice process that purport to show how it operates according to a dominant proposition or analogy.

Examples: H. L. Packer's *Crime Control* and *Due Process* Models; J. Griffiths' *Family* Model.

IV. *Managerial Styles* Approach

Isolating and contrasting the styles of managerial behavior within criminal justice agencies.

Examples: (J. Q. Wilson) police enforcement styles--watchman, legalistic, and service; (L. Ohlin et al.) probation and parole officers--punitive, protective, and welfare; (V. O'Leary and D. Duffee) correctional organizations--reform, rehabilitative, restraint, and reintegration.

V. *Recurrent Themes* Approach

Identifying issues that are pervasive across the criminal justice process.

Example: Approach of F. J. Remington et al., using five themes:

- Evidence sufficiency
- Consent
- Fairness and propriety of procedures
- Effectiveness and efficiency of procedures
- Discretion.

VI. *Analysis of Dominant Functions* Approach

Perceiving each decision point in the criminal justice process as it relates to the different purposes assigned to crime control efforts, which purposes become functional imperatives in the daily operation of the system.

Example:

- Punitive function
- Deterrent function
- Community protection function
- Corrective function
- Due process function.

[a]Derived from the discussion in Newman, op. cit., pp. 115-130.

system and its component agencies. But even if no serious technological impediments remain to the collection and processing of criminal justice data, economic and bureaucratic obstructions persist to the present time and limit the availability of court data in particular. Of course, such limitations differ widely in the various jurisdictions.

That it is now a propitious time to expand the uses of criminal justice data has been widely expounded. The following extract from the *Report on the Criminal Justice System*, National Advisory Commission on Criminal Justice Standards and Goals, January 1973, speaks to this point:

> All criminal justice agencies, those with operational responsibilities and those with planning or policy responsibilities, require substantial data to function properly as part of the overall criminal justice system. In general, criminal justice agencies require information on the events that initiate and terminate criminal justice processes; on people (suspects, victims, offenders, etc.) who are relevant to the operation of the criminal justice system; . . . and *on the operation of the agencies themselves.* [p. 37, emphasis added]

And to conclude this brief historical review, we draw from a modern text, Dean D. Nelson's *Cases and Materials on Judicial Administration and the Administration of Justice*, a quotation provided by its author, at p. 863, to show how judicial statistics ought now be viewed.

> The compiling and publishing of intelligently gathered and adequately organized judicial statistics is an important item in a program of improving our administration of justice. But at the outset one must enter a *caveat* that we must not expect too much from such statistics. . . . We cannot use even the best and most scientifically compiled statistics to solve the fundamental problems of jurisprudence. They cannot give us a measure of values of competing claims, or a criterion of justice, or a theory of what we are seeking to bring about by means of law. But it does not follow that we have no use for statistics. . . . We must learn how to use statistics to control the quality of the output of the operations by which the legal order is maintained and carried on. . . . A workable system of intelligent gathering, compiling, and reporting statistics is one of the first steps toward making justice effective for its purpose. [Pound, *Judicial Councils and Judicial Statistics*, 28 A.B.A.J. 98, at 102-104]

PERFORMANCE MEASURES IN THE CRIMINAL COURT SYSTEM: ROLES AND USES

The foregoing discussion has been concerned with the roles and uses of statistical information in the criminal justice context generally and in the criminal court system particularly. Henceforth our attention will focus on the statistical device of *performance measures*. Their roles and uses are to:

- Facilitate the routine administrative monitoring of the criminal proceeding and the prosecutorial, defense, and judicial agencies that participate in it.
- Help to evaluate judicial, legislative, or administrative modifications of the system.

- Compare court systems in different jurisdictions, either in the context of administrative monitoring or of evaluating policy or legislative changes.
- Prompt and guide research on ways to enhance the administration of justice.
- Better enable the *national* condition of the criminal justice process to be shown.

Performance measures may be regarded as having dual aspects. On the one hand, they are convenient and concise *quantitative descriptors* of the outputs from various stages of a criminal proceeding and of supporting activities. On the other hand, they are *indices* of how well the criminal justice process is working—that is, of progress toward goals. But their utility in the latter role is limited unless they are buttressed by the application of revealing statistical tools such as regression analysis, factor analysis, cross-tabulation, etc. Such further analyses should help to explain what underlying factors are responsible for observed changes in the magnitudes of the performance factors. Later in this report we shall show how the application of statistical estimation techniques to performance data enables one to better understand the significance of variations in performance measure magnitudes.

THE PRESENT STUDY IN THE PERSPECTIVE OF PRIOR EFFORTS: A LIMITED LITERATURE SURVEY

We have chosen not to present a comprehensive compilation and critique of the existing criminal justice studies and writings that bear upon statistical performance measures for criminal proceedings. Rather, by means of a limited literature review, we provide a perspective on the work that has preceded our study and, thereby, an understanding of where our efforts fit.

As noted above, statistical performance measures in the field of criminal justice (and criminal proceedings in particular) may be regarded as *quantitative descriptors* or as *indices of progress*. In plain terms, quantitative descriptors help to answer the question, *What are we doing?*; progress indices, the question, *How well are we doing?* —all in the context here of criminal proceedings. It will be helpful to retain this broad dichotomy for the purposes of our review.

Performance Measures as Quantitative Descriptors: What Are We Doing?

Descriptors Unrelated to Quantitative Models. Quantitative descriptors of criminal court operations may be viewed as falling generally into two classes (with some important middle-ground exceptions). First, there are measures that describe the activities of a criminal court system as it actually is organized and operates without attempting to depict it in terms of a *quantitative model*. These measures may present either intermediate or final outputs of the proceedings—for example, the number of defendants charged, the number of case dispositions, the number of jury trials, the number of defendants incarcerated, etc., in a specified period. Or they may describe resource inputs to the process—for example, overall expenditures, payroll expenditures, employment levels, use of physical facilities such as courtrooms, numbers of practitioners of various types (judges, prosecutors, public defend-

ers, etc.) engaged in the proceedings. *Input* descriptors are not of themselves "performance" measures, but they do enable output data (which are performance measures) to be further refined into measures of productivity and efficiency. This first class of quantitative descriptors is found with varying degrees of completeness in national-level publications,[8] not only applying to the federal courts, but also individually and aggregatively to the states and lower jurisdictions; state-level reports;[9] and local-level publications emanating from the county (or its governmental equivalent), the municipality, the individual court system, and even an agency within a court system.[10] Although these statistical descriptors provide a "snapshot" of court system operations at one point or period in time, it is customary to present them with corresponding operations in preceding periods, and thereby to suggest trends in activities—the descriptors thus may serve as "indicators." Many types of users (government executives, legislators, court administrators, the media, the public, etc.) use this first class of statistical descriptors for policymaking, management, administration, public relations, political purposes, etc.

Descriptors Related to Quantitative Models. A second class of statistical descriptors derives from, or is associated with, the *quantitative modeling or simulation* of a court system. The presence of this analytical formulation or representation of the court system distinguishes the second class from the first. Jennings has briefly surveyed quantitative models of criminal courts and has identified three types that address case flow, case scheduling, or courtroom activities.[11] Case-flow models, which have been especially applied to the problem of allocating court resources, range in complexity and depth from descriptive types that clarify the interrelationships within a court system (and thereby help to identify bottlenecks) to types that relate the flow of cases to the specific use of resources in the system (and thereby help to explore the effects of alternative uses of resources).[12] Jennings also reviews the use

[8] An important example would be *Sourcebook of Criminal Justice Statistics—1974*, National Criminal Justice Information and Statistics Service, LEAA, U.S. Department of Justice (prepared by M. Hindelang et al., Criminal Justice Research Center, Albany, N.Y.). To illustrate, one would find a tabulation therein of criminal cases filed in U.S. district courts by offense and by fiscal year; and a tabulation of the disposition of persons formally charged by the police (in 2832 cities) by offense. An example of a resource-input report that could aid the preparation of performance measures is *Expenditure and Employment Data for the Criminal Justice System*, LEAA and Bureau of the Census. To illustrate, this report contains a tabulation of the judicial expenditures of 312 large county governments, by character and object, for fiscal year 1972-1973.

[9] See, for example, *Crime and Delinquency in California*, Bureau of Criminal Statistics, Division of Law Enforcement, Department of Justice, State of California (issued annually), which contains statistical information about crimes, arrests, court dispositions, jail populations, and personnel engaged in law enforcement activities; or see *Judicial Statistics, Fiscal Year 1973-1974*, Office of Court Administrator, Supreme Court of Michigan, which contains, for example, a tabulation of the average elapsed time in months from date of filing to date of trial for criminal jury and nonjury cases in the 46 circuit (county) court systems in that state.

[10] An example would be the *Annual Report, The Recorder's Court of the City of Detroit, Michigan*, which includes tabulations such as total felony dispositions for a calendar year by type of disposition and by individual judge and felony arraignments for a calendar year by offense and by month. Examples of reports at the court agency level that present statistical descriptors of performance are two from Multnomah County, Oregon: *The Public Defender, A Program Analysis*, Office of Planning, Evaluation and Program Development, Board of County Commissioners (1974), and *Your District Attorney's Office*, Harl Haas, District Attorney (1974). These both contain, for example, workload measures for the staff attorneys.

[11] J. B. Jennings, *Quantitative Models of Criminal Courts*, The New York City-Rand Institute, P-4641, May 1971.

[12] Instances of (case-flow) simulation models of court operations, primarily applied to resource allocation problems, can be found in the following publications: J. Navarro and J. Taylor, "Data Analyses and Simulation of the Court System in the District of Columbia for the Processing of Felony Defendants," *Task Force Report: Science and Technology*, President's Commission on Law Enforcement and the Ad-

of models that simulate court operations to improve case scheduling or calendaring.[13] And, finally, he touches upon the applications of simulation to other aspects of court activities, e.g., the efficient use of jurors.[14]

For our purposes, the application of quantitative models to a specific court system requires actual operating data to be collected from the system and incorporated into the model. The significance to us of the availability of analytical models of specific or generalized criminal court systems is that statistical descriptors are generally an explicit part of their analytical structure or are readily implied by it. Depending on the type of model, these may be measures of the relative flow of cases in various branches of the model, times consumed between events in the proceeding, workloads, backlogs, relative frequencies of various outputs, etc.

In this vein, the work of Blumstein and his associates in constructing flow models of the criminal justice system should be cited.[15] Numerous court system descriptors stem from their analytical representations, including workload measures for case processing (e.g., prosecutor-hours per guilty plea, judge-days per trial) and cost measures (e.g., cost to the court for processing a defendant by type of offense charged). Such models generally entail the determination of branching ratios within the system (e.g., the likelihood that a defendant at point A in the criminal proceeding will move to point B); these ratios characterize the relative flows of cases and serve as quantitative descriptors of the court system simulated.

A narrower example is that of a simulation model addressing the vital issue of court delay.[16] Although this model suggests informative measures of delay, it is not a source for measures of other aspects of court performance.

A final example is a statistical model of judicial productivity and elapsed time to disposition in U.S. district courts, in which the author devises several statistical descriptors of productivity and delay and attempts to explain the independent effects of important variables in these descriptions.[17] This model, like the preceding one, is not a source for measures of other aspects of court performance.

Quantitative Descriptors from a Middle Ground. The two classes of descriptors—related and unrelated to quantitative models—are not exhaustive. The literature contains significant studies in which quantitative descriptors were identified or

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ministration of Justice, Washington, D.C., 1967; J. Jennings, *The Flow of Arrested Adult Defendants through the Manhattan Criminal Court in 1968 and 1969*, The New York City-Rand Institute, R-638-NYC, January 1971; and J. Jennings, *The Flow of Defendants through the New York City Criminal Court in 1967*, The New York City-Rand Institute, RM-6364-NYC, September 1970.

[13] *Final Report on the Development of a Criminal Court Calendar Scheduling Technique and Court-Day Simulation*, Programming Methods, Inc., New York, N.Y., March 1971, is cited as containing a detailed simulation model of the operations of a portion of the New York City Criminal Court, which was developed primarily to test the effects of alternative methods of scheduling cases.

[14] A court is modeled in terms of the number of courtrooms in which jury trials are held each day, the times required for voir dire, the probability of accepting a prospective juror, and the lengths of trials in an article by F. Merrill and L. Schrage, "Efficient Use of Jurors: A Field Study and Simulation Model of a Court System," *Washington University Law Quarterly*, Vol. 1969, No. 2, cited by Jennings.

[15] Alfred Blumstein and Richard Larson, "Models of a Total Criminal Justice System," *Operations Research*, Vol. 17, No. 2, 1969; J. Belkin and A. Blumstein, "Methodology for the Analysis of Total Criminal Justice Systems," Urban Systems Institute, Carnegie-Mellon University, 1970; J. Belkin, A. Blumstein, and W. Glass, "JUSSIM, An Interactive Computer Program for Analysis of Criminal Justice Systems," Urban Systems Institute, 1971.

[16] *System Study in Court Delay: LEADICS (Law Engineering Analysis of Delay in Court Systems)*, Notre Dame University Law and Engineering Schools (in four volumes), prepared for LEAA, January 1972.

[17] R. W. Gillespie, *Judicial Productivity and Court Delay: A Statistical Analysis of the Federal District Courts*, Final Report: Grant Number 74-NI-0025, National Institute of Law Enforcement and Criminal Justice, LEAA, U.S. Department of Justice, March 5, 1975.

devised not strictly on the basis of analytical models but nevertheless with a clear recognition of court processes and agencies as a "system." The need for a full and balanced statistical picture of criminal proceedings suggests an array of measures that reveal what the system is doing. A classic study of this type was conducted by Subin, who sought to show how a judicial process (felony and serious misdemeanor proceedings in the District of Columbia Court of General Sessions) might be described quantitatively.[18] Two similar instances (again reflecting the efforts of Subin and others) are presented in an appendix to the President's Commission Task Force Report on the Courts.[19] These studies produced descriptions of municipal criminal court systems mainly by means of a statistical framework.

An important example of the middle-ground approach is that of Greenwood, Wildhorn, et al., who viewed the prosecutor's office in a systems context; they described in statistical terms how the felony prosecutorial process functioned in Los Angeles County and attempted to demonstrate the value of analysis in informing policymakers how the policies of the prosecutor's office were working.[20] They used a variety of statistical descriptors in analyzing the prosecutor's screening decision; the lower court's decision to hold the defendant to answer on felony charges, treat him as a misdemeanant, or dismiss the case and discharge him; the plea bargaining process; the trial outcome; the court's sentencing decision; the effect of such factors as the defendant's prior record, race, type of defense counsel, and pretrial custody status on type of disposition and outcome; and the consistency or evenhandedness with which defendants were treated in various branches of the district attorney's office and the superior court within the county. (The study also hypothesized two polar descriptive models of prosecutorial management and philosophy and attempted to explain differences in felony dispositions and outcomes across branch offices in terms of these polar models.)

A contrasting middle-ground approach is that of DonVito, who views urban criminal courts in a system context (but without a quantitative model).[21] DonVito advances a set of seven measures and then attempts to ascertain how well these measures, as a set, describe the performance of the courts in a number of urban areas from which he draws data.

Another distinctive example in the middle ground is provided by the *Report of the State's Attorney's Office of Baltimore City*,[22] which portrays that office as an element of a larger system and explains how specified statistical descriptors indicate its performance as an agency of the court system.

[18] H. Subin, *Criminal Justice in a Metropolitan Court—The Processing of Serious Criminal Cases in the District of Columbia Court of General Sessions*, Office of Criminal Justice, U.S. Department of Justice, Washington, D.C., October 1966.

[19] "Administration of Justice in the Municipal Court of Baltimore" and "Administration of Justice in the Recorder's Court of Detroit," App. B, *Task Force Report: The Courts*, The President's Commission on Law Enforcement and Administration of Justice, Washington, D.C., 1967.

[20] P. W. Greenwood, S. Wildhorn, et al., *Prosecution of Adult Felony Defendants in Los Angeles County: A Policy Perspective*, The Rand Corporation, R-1127-DOJ, March 1973.

[21] P. DonVito, "An Experiment in the Use of Court Statistics," *Judicature*, Vol. 56, No. 2, August-September 1972. The selected measures are: the amount of time taken to dispose of criminal cases; the extent to which those convicted had entered pleas of guilty; the percentage of jail prisoners awaiting trial; the amount of time prisoners spend awaiting trial; the backlog of criminal cases relative to the court's caseload; the average number of cases disposed of per judge; and the extent to which probation is used as an alternative to imprisonment. The number of cities from which data were available varied from 3 to 28, depending on the descriptor.

[22] *Report of State's Attorney's Office of Baltimore City*, under the Administration of Milton B. Allen, January 1971 to July 1974. The office appears to rely on measures such as disposition rate, conviction rate, jury trial rate, and plea rate, with appropriate breakouts of some gross measures.

Performance Measures as Indices of Progress Toward Goals: How Well Are We Doing?

Assessing progress toward goals and standards is the second function that we have attributed to statistical performance measures (as a basis for classifying relevant literature).[23] Again we perceive a dichotomy that facilitates the discussion: goals that are associated with a (primarily qualitative) model and those that are set forth without specific reference to a model or conceptual representation. This review will touch upon selected references that articulate various criminal justice goals toward which the system should progress and will lead us to conclude that there is an important gap in the literature: how to select or devise performance measures to assess progress toward criminal justice goals.

Goals and Standards Unassociated with Models. The leading sources of standards and goals unassociated with a specified model of the criminal justice system or its components are the ABA Standards for Criminal Justice, which appeared in a series of volumes since 1968,[24] the *Standards and Goals of the National Advisory Commission on Criminal Justice Standards and Goals*,[25] which resulted from the work of a commission appointed in 1970 by the Administrator of LEAA, and from the ABA Standards of Judicial Administration, being drawn up by an ABA commission.[26] Reference to statements of goals and standards has been facilitated by the preparation of a standard-by-standard comparative analysis of the first two works.[27] A substantial proportion of these hundreds of individual goals and standards relate to the criminal proceeding, but few are couched in quantitative terms or lend themselves to a quantitative interpretation, the primary exception being those concerning the "speed" of the proceeding. For example, an ABA standard relating to pleas of guilty sets forth, as to the relationship between defense counsel and client, that: "Defense counsel should conclude a plea agreement only with the consent of the defendant, and should ensure that the decision whether to enter a plea of guilty or *nolo contendere* is ultimately made by the defendant."[28] By contrast, an NAC standard relating to the time frame for prompt processing of criminal cases states that: "The period from arrest to the beginning of trial of a felony prosecution generally should not be longer than 60 days."[29]

[23] Our purposes here do not require that we make a distinction between *goals* and *standards*. We may regard a standard as being one type of goal that has been established by authority or that has been set forth officially (in some sense), for example, by constitution, legislation, court decision, court rule, governmental commission, professional committee.

[24] The individual volumes (and their dates of approval) are *ABA Standards Relating to Pretrial Release* (1968); *Providing Defense Services* (1968); *Fair Trial and Free Press; Pleas of Guilty* (1968); *Speedy Trial* (1968); *Joinder and Severance* (1968); *Trial by Jury* (1968); *Sentencing Alternatives and Procedure* (1968); *Appellate Review of Sentences* (1968); *Post-Conviction Remedies* (1968); *Discovery and Procedure Before Trial* (1970); *Probation* (1970); *Criminal Appeals* (1970); *Electronic Surveillance* (1971); *The Prosecution Function and The Defense Function* (1971); *The Function of the Trial Judge* (1972); *The Urban Police Function* (1973).

[25] The six individual reports of the National Advisory Commission are entitled: *A National Strategy to Reduce Crime*, *Criminal Justice System*, *Courts*, *Police*, *Corrections*, and *Community Crime Prevention*.

[26] The first completed volume is *Standards Relating to Court Organization* (approved 1974); a second volume, *Standards Relating to Trial Courts* is in tentative draft form; the third and final volume, *Standards Relating to the Administration of Appellate Courts*, is in rough draft.

[27] *Comparative Analysis of Standards and Goals of the National Advisory Commission on Criminal Justice Standards and Goals with Standards for Criminal Justice of the American Bar Association*, American Bar Association Section of Criminal Justice, Washington, D.C., 1973.

[28] Ibid., p. 240.

[29] Ibid., p. 249.

An excellent example of an expression of goals (and subgoals) for a court system, unassociated with a qualitative model, is to be found in an internal memorandum of The Institute of Judicial Administration, Inc.[30] In this instance, the selection and application of performance measures have been facilitated by rephrasing the general goals as more specific issue statements. The goals, subgoals, and corresponding issue statements are as follows:

- Goal: Efficient use of resources

  Subgoal: Court unification and reorganization
  Issue: Is the caseload reasonably well allocated among judicial districts and personnel? Are there districts too large or too small to operate efficiently?
  Subgoal: Reduction of unnecessary idleness of resources
  Issue: Are judicial and other resources standing idle because of insufficient resources or poor management?

- Goal: Accurate and conscientious decisionmaking

  Subgoal: Adequate time and resources devoted to individual cases
  Issue: Are individual cases receiving adequate time and attention?
  Subgoal: Equal protection of the laws
  Issue: Are judges or other decisionmakers letting personal bias unduly influence decisionmaking?
  Subgoal: Due process of law

- Goal: Neutrality in decisionmaking

  Subgoal: Elimination of unnecessary delay
  Issue: Is there unnecessary delay in the processing of cases which is favoring particular classes of litigants?

- Goal: Minimize social and economic costs to outside participants in the judicial process

  Issue: Are the demands of the judicial system on witnesses, jurors, and litigants too great in terms of time, money, and dislocation?

- Goal: Maintenance and growth of judicial system

  Issue: Is the judicial system receiving enough financial and other support to continue to serve the public effectively and efficiently?

Still another approach is described in App. A of this report, which presents a paper prepared in the first phase of our study. There a multilevel goals structure for the criminal justice system (and the criminal proceeding within it) is developed as a check list to appraise the usefulness of current gross performance measures.

Goals and Standards Associated with Qualitative Models. An articulation

[30] Internal Memorandum, D. Weinstein to Requirements Analysis Subcommittee, April 24, 1974, The Institute of Judicial Administration, Inc., New York, N.Y.

or at least an implication of goals is usually entailed in the qualitative or descriptive models approach to the characterization and analysis of criminal justice. We shall rely on a recent article by J. Senna to provide a concise classification of qualitative models of criminal justice.[31] Senna identifies three broad classes: *systems* models, which typically characterize a criminal justice system as an interrelationship of police, court, and corrections agencies; *process* models, which in the attempt to achieve some expressed goals perceive a series of decisionmaking points and emphasize the various sequential stages through which an offender passes; and *organization* models, which embody applications of organization and management. The *systems* model approach has generally been frustrated, Senna feels, by the fact that criminal justice is implemented by a loose federation of agencies with some mutual concerns, but which tend to operate more or less independently with a multiplicity of sometimes conflicting objectives.

By contrast, *process* models have been more fruitfully pursued. H. Packer has developed two widely discussed models termed "Crime Control" and "Due Process."[32] The Crime Control model is founded on the belief that repression of criminal conduct is the most important function of criminal justice; the Due Process model is based on the concept of the primacy of the individual along with limited official power. The first emphasizes the need to maintain a high rate of apprehension and conviction of offenders, with a premium on the speed and finality of the criminal proceeding; the second underscores the protections for the accused at every stage of the criminal proceeding.

J. Griffiths has devised a "Family" model in opposition to both of Packer's models.[33] In Griffiths' model, the interests of society and the offender need not be incompatible, and the state should seek to act in the best interests of the offender, as a juvenile court attempts to do. By contrast, Griffiths suggests Packer's models are two versions of a "battle" model that depicts a conflict of interests.

Other process modeling has been contributed by W. Miller[34] (who distinguishes a "Left" from a "Right" ideological pattern in implementing the criminal justice process), by F. Remington[35] (who identifies five basic analytic themes for viewing the criminal justice process—see Sec. V of Table 2.1 above), and by A. Goldstein[36] (who contrasts "Inquisitorial" models with "Accusatorial/Adversarial" models).

Senna's survey identifies several leading examples of the *organizational* model approach. One such development was given by the Organization for Social and Technical Innovation.[37] Another is a management model of criminal justice proposed by McCaffey, wherein the emphasis is primarily on roles, functions, skills, programming, budget control, and other management considerations.[38]

[31] J. Senna, "Models in Criminal Justice: Building Blocks for Change," *Judicature*, Vol. 59, No. 1, June-July, 1975.

[32] H. Packer, *The Limits of the Criminal Sanction*, Stanford University Press, Stanford, Calif., 1968.

[33] J. Griffiths, "Ideology in Criminal Procedure or a Third Model of the Criminal Process," *Yale Law Journal*, Vol. 79, January 1970.

[34] W. Miller, "Ideology and Criminal Justice Policy: Some Current Issues," *Journal of Criminal Law and Criminology*, Vol. 64, June 1973.

[35] F. Remington, D. Newman, et al., *Criminal Justice Administration*, Bobbs-Merrill, Indianapolis, 1969.

[36] A. Goldstein, "Reflections on Two Models: Inquisitorial Themes in American Criminal Procedure," *Stanford Law Review*, Vol. 26, May 1974.

[37] *Implementation Report Submitted to the President's Commission on Law Enforcement and the Administration of Justice*, The Organization for Social and Technical Innovation, 1967.

[38] A. McCaffey, *Administration of Criminal Justice—A Management Systems Approach*, Prentice-Hall, Englewood Cliffs, N.J., 1974.

What do these models express in the nature of goals as the directions for criminal justice efforts, and which goals generate requirements for progress measurement? Senna finds in his survey that *systems* models seek to accomplish a wide span of criminal justice objectives including crime prevention, deterrence, rehabilitation, reintegration, and punishment. But the core of the difficulty in developing systems models is that these goals tend to be conflicting, and different agencies within a criminal justice system pursue them with different emphases.

Process models are found generally to underscore both deterrence and restraint of offenders while emphasizing fairness and strict procedural safeguards. And Senna observes that *organizational* models, by focusing on goals of efficiency and effectiveness, seek to avoid moral dilemmas posed by the imposition of criminal sanctions.

Measuring Progress Toward Goals and Standards. The main thrust of our limited review of the literature serving as a source of goals and standards for criminal justice (and for criminal proceedings in particular) is that one generally obtains *gross and qualitative* expressions or inferences of goals from these references, which do not suggest how progress might be measured by performance data. At a minimum, the analyst finds it necessary to seek or devise a structure of more specific and measurable subgoals and then perhaps to translate these subgoals into concrete issue statements that clarify what operational data are relevant.[39] And then there remains the problem of selecting specific performance measures or sets of measures that reveal the appropriate informational content of these data (this problem will be considered in Sec. IV).

So far as we have become aware, the literature is mostly lacking in studies that address the problems of translating general statements of objectives into specific measures of progress. One important purpose of our study is to help close this gap. In particular, in Sec. IV we set forth a set of criteria for selecting preferred performance measures from among the wide choices that may be available to assess performance relative to goals. Our study as a whole illustrates the process of arriving at and applying sets of performance measures that illuminate several important issue areas widely acknowledged to be closely linked to criminal proceeding goals.

SUMMARY

The focus of the present study has not been on the use of performance measures simply as quantitative descriptors of criminal proceedings—that is, as aids to answering the question, *What are we doing?* Rather, our study's thrust has been toward the role of performance measures as progress indices—toward their contribution to answering the question, *How well are we doing?* We do not advance a fresh set of goals and standards (except to the limited degree given in App. A, a product of the first exploratory stage of our work); neither does it propound another model, either quantitative or descriptive, of the court system. What it does set out to do is to lessen the research gap between articulating goals and standards on the one hand, and selecting and applying progress indices on the other.

As to its links with previous work, our study may be viewed as a natural outgrowth, both in scope and depth, of a previous Rand study (described above) of adult

[39] See, for example, Internal Memorandum, P. Weinstein to Requirements Analysis Committee, op. cit.

felony proceedings in Los Angeles County.[40] The present study is broader in scope in that certain issues (such as delay) are considered here, whereas the earlier study simply did not address them at all. It provides more depth by advancing a conceptual basis for selecting performance measures and for drawing inferences about performance, by analyzing some specific issue areas (such as plea bargaining) in greater detail, and by applying multivariate statistical estimation techniques to attempt to explain the *independent* effects of important variables on some of the major performance measures.

[40] P. Greenwood, S. Wildhorn, et al., op. cit.

III. PROFESSIONAL VIEWS ON PERFORMANCE MEASURES: INTERVIEWS WITH PRACTITIONERS

Marvin Lavin

PURPOSE OF INTERVIEWS

A series of interviews with experienced criminal justice practitioners (including academicians in the field) was conducted early in the study after our research efforts had been reshaped on the basis of the counsel given by the Advisory Group organized for the study. The purposes of the interviews were set out to be the following:

- To learn at first-hand the views and attitudes of a variety of judges, court administrators, defense counsel, prosecutors, and academicians toward the use and value of statistical performance measures in criminal court systems.
- To elicit reactions from these experienced professionals about our (reoriented) study program, including their views on
  1. Our tentative selection of significant issue areas in which to demonstrate applications of performance measures.
  2. Our preliminary choices of specific performance measures purporting to illuminate these issue areas.
  3. Our insights concerning the validity of interjurisdictional comparisons by means of performance measures.[1]
- Finally, to appraise possible choices of two jurisdictions in which the study team would seek to demonstrate the application of performance measures.

Primarily, we saw these interviews as a foundation for decisions and choices in subsequent phases of our work, not as a source from which we would derive final study output. The interviews, in fact, provided the first empirical data of the study.

IMPLEMENTATION OF INTERVIEWS

A set of brief discussion papers was written to be sent beforehand to the interviewees.[2] In addition, an interview instrument of nearly 100 questions (keyed to a set of exhibits) was constructed. These questions were to be flexibly administered by an interviewer as seemed appropriate to him, given the available interview time, the interests of the practitioner, and his role in the system. Our next step was to pre-test these materials with a sample of practitioners in the Los Angeles area—a trial court judge, a senior prosecutor, a federal public defender, two court administrators, and an academician with both prosecution and defense experience. The interview

[1] See App. C for an outline of key factors in making interjurisdictional comparisons of performance measures.

[2] These background papers presented definitions and ideas in the areas of prosecutorial screening, plea bargaining, sentencing variation, evenhandedness, case processing efficiency, delay, and interjurisdictional comparisons, respectively. Their content is reflected in Sec. IV of this report.

materials and the technique of applying them were both refined as an outcome of these pre-test interviews.

Next came a series of countrywide inquiries to a list of well-known practitioners, whose names had been suggested mostly by the Advisory Group and by study consultants. Their replies were almost invariably affirmative when asked: "Would you be willing to submit to an interview of several hours or more for the described purposes of Rand's study?" Five study team members then conducted interviews over a period of roughly two weeks in the jurisdictions listed in Table 3.1. By design, the interviewing was almost entirely concentrated in major metropolitan areas. Most quarters of the country were represented.

Table 3.1

JURISDICTIONS IN WHICH PRACTITIONER INTERVIEWS WERE CONDUCTED

Maricopa County (Phoenix), Arizona
Los Angeles County (City of Los Angeles), California
District of Columbia
Dade County (Miami), Florida
Fulton County (Atlanta), Georgia
Cook County (Chicago), Illinois
Wayne County (Detroit), Michigan
New York County (City of New York), New York
Clackamas County (Oregon City), Oregon
Multnomah County (Portland), Oregon
Philadelphia County (City of Philadelphia), Pennsylvania
Harris County (Houston), Texas
Milwaukee County (City of Milwaukee), Wisconsin

Including the pre-test interviewees, 33 professionals were queried in the course of 26 interviews, which ranged in length from an hour and a half to an entire day.[3] We invited respondents to choose the areas they preferred to discuss (among those we had proposed), in addition to replying to our other areas of inquiry. The distribution of their choices, shown in Table 3.2, reveals that the judges we interviewed preferred to express their views most frequently in the subject areas of sentencing and of case processing efficiency and delay; court administrators tended to parallel judges in their emphases but with greater concentration on efficiency and delay, which could be expected; defense counsel showed less propensity to concentrate than the other practitioners in our sample; and prosecutors gravitated to two areas of special concern to them, namely, screening and plea bargaining.

The practitioners comprising our interview sample spanned the primary professional roles within criminal proceedings; furthermore, they were geographically diverse. But they did not purport to be a representative cross section of the national

[3] With the exception of the pre-test sessions, which were conducted jointly by all interviewers, each session was conducted by a single interviewer who manually recorded notes as the dialogue proceeded, usually in the office or chambers of the interviewee during the business day.

Table 3.2

NUMBER OF INTERVIEWS IN WHICH A GIVEN ISSUE AREA WAS ELECTED

Practitioner	Efficiency and Delay	Evenhand-edness	Screening	Plea Bargaining	Sentencing
Judges (6 interviews)	5	3	0	1	6
Court administrators (7 interviews)	7	3	0	2	3
Defense counsel (5 interviews)	2	3	1	3	1
Prosecutors (6 interviews)	1	0	4	5	1
Academicians (2 interviews)	0	0	1	2	1
Total	15	9	6	13	12

population of criminal justice practitioners. By contrast with the nominal practitioner, those whom we selected were more experienced and had evinced greater interest in the problems of analyzing criminal justice operations. Such a sample bias was consistent with the purposes of the interviews, which were to obtain professional guidance and not to produce an accurate nationwide picture of practitioners' attitudes. Therefore, we must underscore that, *because of the small size of the interview sample and its special qualities, one should not regard the views expressed to us (summarized below) as being attributable to practitioners in general.* Nevertheless, when the interviewees were nearly unanimous in their responses to a query, we are inclined to give that position special weight and to regard it as likely to be representative.

This section gives some general findings from the interviews, indicates how they influenced our subsequent undertakings, and then presents specific illustrative findings that affected our work.

GENERAL OBSERVATIONS, FINDINGS, AND IMPACTS

Our interviews revealed a wide diversity of views about most substantive issues that were raised—not only differences among types of practitioners but also within each type. We had anticipated this variety and were, in fact, reassured by its presence.

We were particularly struck by the division of views and the strength of feeling about the role of *case auditing* relative to the role of *statistical performance measures* as informational devices in criminal proceedings.[4] Some interviewees asserted that case auditing was indispensable, even when other informational tools such as statistical performance measures were used. Others felt that case auditing was the most reliable technique for revealing how a court system had been performing. Still

[4] See Sec. VIII for a characterization and application of case auditing.

others placed a higher value on performance measures but thought that their main utility was for internal management control. And finally, some were performance-measure enthusiasts who perceived important benefits not only in their application to internal control, but also in measuring progress toward external goals and in facilitating interjurisdictional comparisons of criminal justice performance.

Practitioners who had had more experience with the operational use of performance data and statistical performance measures were less distrustful and more specific and realistic in their reservations about such use.

Nearly all interviewees recognized our need to limit the scope and aspirations of the study and concurred, at least by default, in its design. Some expressed misgivings about the omission of misdemeanor proceedings and of support staff operations, both of which they felt were inseparable from the elements included in the study.

There was a weak consensus that, among the five areas that we identified, the least promising for our research purposes would be evenhandedness or possibly case processing efficiency and delay. This relative disaffection with evenhandedness seemed to stem from three bases: (1) Even where the problem was recognized, there tended to be little policy leverage on it; (2) in most jurisdictions the problem was regarded as not being significant; and (3) some practitioners felt that a practical definition of the problem was difficult to frame and have accepted. It was less apparent why some interviewees ranked case processing efficiency and delay lower; evidently they felt that this topic was already the subject of enough work so that our contribution would be marginal. But, observing the absence of a clear consensus and the lack of suggestions as to alternative areas,[5] we interpret the interviewing results as not, of themselves, calling for the elimination of any of our five areas from further attention.

As a whole, the interviewed practitioners seemed to oppose complexity in the measures that purported to capture plea bargaining performance and the accuracy of screening.

If a formal management information system (which included the production and distribution of statistical performance information) were to be implemented in a jurisdiction that lacked one, the interviewees tended to believe it should be operated on a routine periodic basis under centralized control, but not necessarily with centralized data collection. Nearly all doubted the availability of additional funding in their own jurisdiction for such purposes; some surmised that a reallocation of current resources might be feasible, if such an information system clearly promised an overall reduction in court system costs.

In general, the practitioners anticipated that having demonstrations in (two) selected jurisdictions, as we contemplated doing, would be useful, but there was no consensus that these demonstrations would be instrumental in persuading other jurisdictions to adopt a more extensive program of collecting and applying performance data. Rather, the feeling was that local budgetary factors would be almost completely determinative in any decision to undertake such activities.

The foregoing general findings led us to conclude that:

- *We would add a case auditing effort of modest dimensions to the demonstra-*

[5] The only alternative area which the practitioners shared a view of importance was the court itself, namely, judicial background, judicial appointment method, judicial competence, judicial temperament, etc., and the relations among these factors. But this area is not amenable to quantitative analysis.

tion phase of the project, primarily to show the complementarity of the two approaches. But this would not reflect a diminution of our belief (shared by many interviewees) that statistical performance measures, objectively employed, have a rich and insufficiently tapped potential for policymakers, managers, practitioners, and the public.

- While we concurred that a picture of the performance of felony proceedings would not be as complete and accurate as might be desired if measures of support staff operations and of misdemeanant and juvenile processing were omitted, *our resource limitations continued to preclude the expansion of the study scope to include the latter areas.* We would, however, accept the lessened applicability of the study in some situations.

- As mentioned earlier, an absence of a persuasive consensus about eliminating any of the proposed five areas and a lack of suggestions for alternative or additional areas amenable to statistical analysis were observed in the interview responses. Therefore, *we would continue consideration of all five through at least the demonstration phase.* Difficulties in collecting relevant data or a paucity of useful results might impel later abandonment of an area.

- Despite an abundance of technical difficulties in measuring plea bargaining performance and screening accuracy, as underscored by the interviewees, *we would continue to seek measures that were more informative than the gross ones now employed,* while sacrificing simplicity only when we could not avoid doing so.

- We would not count on our work in the two demonstration jurisdictions as being of such scope and detail that it could prove compellingly the advantage of having an information system that used statistical performance measures, at various alternative levels of implementation cost. *The size of our data bases in the demonstration systems would have to be governed to an important degree by constraints on our study resources.* We might fortuitously uncover instances of gross inefficiency, whose elimination could help underwrite a better information system in the demonstration jurisdiction. But even this result could not be expected to persuade other jurisdictions to act.

OBSERVATIONS, FINDINGS, AND RESULTS IN SPECIFIC AREAS

Case Processing Efficiency and Delay

Our interviewees generally felt that performance measures were relatively informative and useful in monitoring case processing efficiency and delay.

All interviewed judges and court administrators, the practitioner classes most immediately concerned, favored the use of "weighted" caseload data and, in particular, the measure *weighted dispositions per available judge-year* to show judicial

productivity, although acknowledging that few jurisdictions are as yet making use of this measure.[6]

Whether the efficiency of criminal proceedings should be monitored only for criminal cases as a whole or in terms of individual offense types (or classes of offenses) tended to be viewed differently by the different practitioner classes. Judges opted for aggregation; court administrators and defense counsel favored a more detailed monitoring. A frequent caveat was that measures of criminal case processing efficiency should not stand alone, but should be presented jointly with measures of the quality of justice delivered.

Nearly all of the interviewed practitioners commended the use of the delay measures shown to them, namely:

- Median elapsed time between major court events, by type of disposition.
- A measure of lengthy court delay, such as minimum time to disposition for the most lengthy 10 percent of cases.
- Percentage of convictions, by time in months to sentencing date.
- Average number of continuances per disposition.
- Percentage of continuances on defense motion, on prosecution motion, etc.
- Average number of days consumed per continuance.

A few dissented vigorously to reliance on such gross performance measures for monitoring delay; they generally believed that delay is better controlled by auditing individual slow cases. Many practitioners preferred that delay be monitored in terms of broad offense classes rather than, on the one hand, all criminal cases as a group or, on the other, individual offense types. The majority felt that delays between the major steps of a proceeding should be monitored in addition to the overall time to trial or to disposition. And nearly all felt that in collecting delay data, the responsibility for the delay should be attributed.

While most interviewees approved the employment of measures to help monitor the treatment of lay participants (i.e., victims, witnesses, jurors) in criminal proceedings, many avoided specific suggestions pending the outcome of programs to improve handling of jurors and witnesses within their jurisdiction. Few were impressed with the Juror Usage Index (JUI) employed in the federal court system.

Almost without exception, practitioners displayed distrust of interjurisdictional comparisons of case processing efficiency and delay by means of performance measures. Careful identification of essential similarities and differences between jurisdictions was felt to be a necessary (but possibly not sufficient) prerequisite to such comparisons.[7]

Many decisions about what data we would seek in the demonstration jurisdictions were modified by the practitioners' views on case processing efficiency and delay. The affected data collection objectives are discussed in App. D. They include:

- Collect data that enable weighted caseload measures to be calculated, preferably by offense type if feasible.
- Obtain elapsed time data for all major events of criminal proceedings and thereby show frequency distributions for the time until disposition for all

[6] The weighted measure is discussed in Sec. IV.

[7] See App. C for a listing of these factors, as discussed with the interviewees.

types of dispositions. Associate the defendant's custody status with the elapsed-time data.

- Classify continuance data by the practitioner responsible for the continuance.
- Use case data to estimate frequency and duration of appearances of victims and other witnesses. Where such data would be unavailable, we decided to provide rough estimates from responses to our mail surveys.
- Seek a data source by which to estimate the fraction of a juror's time that is consumed in sitting actively on criminal cases, with a breakdown into voir dire time versus trial time if feasible. (Juror surveys may be the only practical source.)
- Identify procedural differences between the two demonstration jurisdictions that would significantly affect an interjurisdictional comparison of case processing efficiency and delay.

Evenhandedness

Interviewees disputed the adequacy of a proposed set of four factors—namely, nature of criminal conduct, prior record, age, and sex—as the primary legitimate sources of disparities in the outcomes of criminal proceedings, particularly pretrial release and sentence severity. Additional factors suggested most frequently included employment history, education, and community ties.

Interviewees also disputed that defendant's race, pretrial custody status, and type of counsel was a sufficient set of illegitimate sources of disparities in case outcomes, but they gave no consistent suggestions to expand the list. Some felt that there were strong correlations among these three factors, and between them and various legitimate ones.

Interviewees tended to believe that statistical measures purporting to disclose the presence of unevenhandedness were susceptible of other explanations.

Among the specific impacts that the practitioners' responses made on our plans for the demonstration phase were the following:

- Collect data, if obtainable, on employment status, educational level, and familial ties of defendants.
- In collecting sentencing data, distinguish between supervised and unsupervised probation, and between suspended and nonsuspended sentences.

Prosecutorial Screening

Measuring the output of prosecutorial screening only in terms of a few broad classes—e.g., proportion rejected, proportion charged with felonies, proportion handled otherwise—was regarded by all interviewees as inadequate for assessing the application of charging standards. At the very least, reasons for case rejection would have to be recorded and analyzed. Even so, some interviewees would question the accuracy of reasons given as the true reasons for some case rejections.

Statistical descriptions of what happened at major decision points subsequent to screening tended to be regarded as ambiguous indicators of the accuracy of screening.

While our respondents believed that performance measures of prosecutorial screening could be helpful management tools, most doubted that current record-keeping in their jurisdictions was sufficiently accurate and reliable to implement use of these measures, because standardized data on strength of case and reasons for rejection were not collected.

Interviewees were strongly negative on the potential of statistical performance measures to monitor overcharging (if they were willing to express any opinion). Case auditing was advanced as a better way of exposing such screening abuse.

The way that a prosecutor's office sees and performs its role was felt to be an important factor that must be accounted for in interjurisdictional comparisons of screening.[8]

Practitioner views affected or confirmed a number of elements in our plans to collect screening data in the demonstration jurisdictions, including the following:

- Obtain reasons for declining to file felony complaints or for disposing by means of misdemeanor proceedings in such cases.
- Investigate the capability of case auditing to reduce ambiguity in statistical performance measures on charging accuracy, particularly those showing plea bargaining outcomes.
- Ascertain (qualitatively) the nature of, and changes in, police arrest policies vis-à-vis prosecutorial charging standards.
- Clarify the relevance of *how the prosecutor's office is organized* and *how prosecutors are assigned to cases* to comparisons of case screening between the demonstration jurisdictions.

Plea Bargaining

A majority of interviewed practitioners agreed that summary statistical measures would help managers and policymakers to appreciate the effects of plea bargaining on system operations. They also agreed that monitoring a few representative classes of offenses should suffice for this purpose. On the other hand, they were divided on the meaningfulness and implementability of our proposed approach of measuring punishment foregone by the plea agreement, by means of a statistical technique of estimating what the defendant's punishment would have been in the absence of the agreement and with punishment being expressed in terms of a sentence severity index. Opposition to this way of measuring plea bargaining concessions to defendants was grounded either on the great variety of plea bargain types and purposes or on the noncomparability of defendants who gained bargains and those who did not.

Opinion about the validity of a sentence severity index—that is, a one-dimensional sentence weight scale—was evenly divided when we presented this device to the practitioners in the context of measuring plea bargaining concessions. Few had prior exposure to this statistical tool. The relative weights to be accorded different types of punishment (prison time, jail time, fines, probation, community service, etc.) were widely controversial. Interviewees concurred, however, that historical experience on sentences actually served for specified offenses would be an appropriate basis for assigning weights to indeterminate sentences.

[8] The descriptions in Sec. V of the prosecutor's office in Multnomah County, Oregon, and Dade County, Florida, support this point.

Unfamiliar and relatively technical measures of performance for a heretofore subjective activity such as plea bargaining were generally resisted by our sample of practitioners. Further, given that some plea agreements in most jurisdictions are not prompted by efficiency motives, a number of interviewees questioned the sometimes artificial interpretation of plea bargaining as a balancing of concessions to the defendant and gains to the system.

Interview responses emphasized that two jurisdictions must be similar in a substantial number of ways if interjurisdictional comparisons of plea bargaining performance measures were to be valid. In addition to the factors we had enumerated, the following "comparability" factors were mentioned: whether or not sentencing was by a jury; defense counsel was provided; how ample were the prosecutor's resources; what alternatives were available for handling minor felonies; jail conditions (especially crowding); the organization and procedures of the prosecutor's office and the public defender's office; and the caseload and backlog situation.

Various planned activities in the demonstration phase were affected by the above views on plea bargaining, including the following:

- Develop alternative sets of relative weights for various punishment types for the purpose of calculating a sentence severity index.
- Collect imposed sentence data by individual offense type for the estimation of plea bargaining concessions.
- Obtain data on sentences served as well as sentences imposed and correlate the two in the demonstration jurisdictions.
- Add factors suggested by interviewees as tests of plea bargaining comparability of different jurisdictions.
- Use case auditing to explore whether gross statistical measures seem to provide sufficiently sensitive and accurate insights into the plea bargaining process.

Sentencing Variation

Judges in our sample, when presented with gross statistical measures of sentencing, generally preferred that these measures have detailed breakouts (e.g., by type of conviction, by whether or not probation was supervised, etc.). Also, they wished to see feedback measures of sentencing (e.g., measures that would reveal the serving of sentences imposed and would show other impacts of sentencing on criminal justice goals, such as the effect of sentence type and severity on the rate of recidivism).

Sentence severity indices tended to be disfavored for demonstrating undue sentencing variation mainly on the grounds of their crudeness.

The proposed list of factors by which the relative harshness of sentencing might be explained (namely, age, sex, prior record, type of conviction, judge's sentencing formula, case publicity) was found to be generally acceptable but incomplete, with the interviewees suggesting additions such as: the relationship between victim and defendant, jail conditions, the relative culpability among multiple defendants, defendant's physical appearance, availability of the pre-sentence report, etc.

The majority of practitioners interviewed on this issue felt that type of conviction, type of trial, and case publicity ought *not* to be the basis for differences in sentences for a given convicted offense (except in rare instances) although conceding that these factors commonly caused such differences.

Interviewees generally accepted our proposed list of jurisdiction-linked factors that were causes of sentencing variation—namely, statutory sentencing structure, probation policy, plea bargaining practices, charging threshold policy, parole eligibility and practice, availability of sentence review, local attitudes toward the gravity of various offenses and appropriate sentences for them, post-sentencing control by judges, the role of the media, and the availability of alternatives to sentencing. Proposed additions included jury or judge sentencing, method of filling judgeships, and use of sentencing councils.

Among the various devices for eliminating undue sentencing variation (e.g., exchange of sentence data among judges, use of sentencing guidelines, sentencing institutes for judges, written justifications for felony sentences, sentence review panels, appellate review of sentences), the use of sentence review panels of trial judges was the most favored, with preferences divided between limiting such panels to an advisory role or giving them power to overrule the sentencing judge.

As in other issue areas, our planned activities in the demonstration phases reflected the views of the interviewed practitioners in a number of ways, including the following undertakings:

- In collecting sentencing data, distinguish between types of probation and show such conditions as restitution of victims and community service; also, segregate suspended from nonsuspended sentences.
- Examine the validity of type of conviction and type of trial as explanatory factors for sentence severity in the demonstration jurisdictions.
- Explore how changes in relative punishment weights affect the application of a sentence severity index to actual sentence data.
- Expand the list of interjurisdictional "comparability" factors (see App. C) to include practitioner-suggested items such as method of filling judgeships, post-sentencing powers of trial judges, availability of pre-sentence reports, sentencing review mechanisms, overcrowding of correctional facilities, etc.

IV. THE PERFORMANCE MEASURES: SELECTION, RATIONALE, LIMITATIONS, AND DATA ELEMENTS REQUIRED

Marvin Lavin, Anthony Pascal, and Sorrel Wildhorn

Our exposition of performance measures in this section is not intended to be a complete handbook that presents a full spectrum of available performance measures for criminal proceedings and methods for devising additional ones. Indeed, depending on the data base that is constructed, there would be an almost unlimited number of statistical descriptors of those data; and many, if not all, of those descriptors would be interpreted as performance indicators for some purposes. The scope and objectives of this study lead us to focus our treatment of statistical performance measures more narrowly, that is, on exemplary sets of measures (whose data elements are collected from case files and other agency records) that we find to be useful in illuminating the specific issue areas with which the study is concerned: prosecutorial case screening, case processing efficiency and delay, plea bargaining, evenhandedness, and sentence variation. The discussion in this section is, almost in its entirety, from the *intra*jurisdictional point of view. (See App. C for *inter*jurisdictional considerations.)

First, we present the set of criteria that shaped our choices of performance measures in particular and that can be used as a guide in general. Next we discuss in detail some illustrative complexities that are involved in devising a set of performance measures for an issue area—in this instance, prosecutorial screening. This discussion is intended to suggest problems in the other issue areas, which we do not consider in the same depth. Finally, we present and explain sets of performance measures for each issue considered within the specified areas, including a specification of the data elements contained in these measures. We explain how changes in the values of the given measures reveal information about the related issue.[1] Also, we note where difficulties were encountered in collecting the required data elements in the demonstration jurisdictions.[2]

SELECTION CRITERIA FOR PERFORMANCE MEASURES TO BE APPLIED TO CRIMINAL PROCEEDINGS

A performance measure should be *relevant;* and the more *proximate, directly linked,* and *applicable* the measure is, the better it is. The meaning of these terms as we use them is as follows:

1. A *relevant* performance measure is one with significant probative value concerning a matter that is in issue or that requires illumination. For

[1] Sections VI and VII contain a fuller discussion of how to use performance measures to draw inferences about performance issues (in the context of the demonstration jurisdictions).

[2] See App. D.

example, the average elapsed time from arraignment to disposition is a measure relevant to issues of court resource use. The proportion of cases dismissed before trial is a measure that has little relevance to issues of sentence variation.

2. A performance measure may be more or less *proximate*, depending on how close the events it captures are to the matter of interest. For example, the proportion of complaints rejected by the prosecutor for reasons of insufficient evidence is an immediate output of screening and is therefore proximate to the matter of screening performance. By contrast, the rate at which trial dismissals occurs is a measure of events more remote from the screening process and is therefore less proximate to the prosecutor's screening performance.
3. A performance measure is more *directly linked* to an aspect of the criminal proceeding if its magnitude is more strongly correlated with that aspect. For example, the guilty plea rate would be more directly linked with judicial case processing efficiency than would be, say, a measure of sentencing severity.
4. The *applicability* of a performance measure refers to its usefulness in the analytical task undertaken. For example, if one's purpose is to make interjurisdictional comparisons of judicial productivity, the number of *weighted* cases processed per available judge-year may be a more useful measure for this purpose than would be simply the *unweighted average* number of dispositions per judge per year.

Two further criteria are that a performance measure should be *specific* and *clear*. We intend that these terms be given the following interpretation:

5. The fewer the aspects of the proceeding on which the informational content of a performance measure is focused, the more *specific* it is. For example, case rejection rate is a measure more specific to the screening process, on which it is singularly focused, than is a measure such as the proportion of cases in which the defendant pleads to charges different from the ones originally filed, which reflects both the charging and plea bargaining aspects.
6. *Clarity* connotes that the construction and usage of a performance measure is readily understood by the average practitioner. For example, median number of days between arrest and final disposition of all felony cases is a clear measure of delay. However, a sentence severity "score" is a less clear performance measure of sentence severity than the elements that comprise it (e.g., two years of prison followed by five years on probation).

It is appropriate in the use of *sets of performance measures* to apply the criteria of *consistency* and *complementarity:*

7. Performance measures are *consistent* if they can properly be used to measure the same element of performance. For example, the average severity of sentencing broken down by defendant's race for convicted charge A might not be consistent, for the purpose of revealing a lack of evenhandedness, with the same measure for convicted charge B if there were a statutory minimum sentence for one charge and not for the other. However, the

complete set of screening outputs produced by a prosecutor's office during a specified period would be a consistent set of measures of screening performance.

8. Performance measures are *complementary* when they mutually contribute to the analysis of an issue. For example, the proportion of jury trials among all trials and the average number of continuances in a proceeding are two measures that are complementary for purposes of analyzing the duration of criminal proceedings.

A most important final criterion is that of *implementability*, which brings into consideration the cost and availability of data embodied in a performance measure:

9. When a jurisdiction can, with reasonable cost and effort, collect and process the data necessary to employ a specified performance measure, we say that this measure is *implementable*. For example, the median time elapsed between arraignment and final disposition is a measure that is readily implementable in most jurisdictions, because the dates of major case events are nearly always recorded in standard case files. By contrast, there may be formidable difficulties in obtaining reliable data on the economic status of defendants in some jurisdictions; measures (say, of evenhandedness) that require such information might not be implementable in these circumstances.

For brevity, we shall not discuss the specific application of these selection criteria each time that we introduce sets of performance measures in the discussion. Instead, they will be examined in selected instances.

PERFORMANCE MEASURES FOR SELECTED ISSUE AREAS

Prosecutorial Case Screening

As mentioned above, we shall discuss the application of performance measures to the prosecutorial case screening function in some depth, with attention to two salient aspects of this screening, namely, the *charging threshold* and the *accuracy of charging*. These matters are important in their own right, but they also are richly revealing about limitations in the application of statistical performance measures more generally. We shall first briefly review the nature of the screening function.

The Nature of the Screening Function. The responsibility for initiating formal criminal proceedings belongs to the prosecutor in most jurisdictions for most types of offenses.[3] Traditionally, the screening function involves broad prosecutorial discretion (with which the courts have been highly reluctant to interfere). "The primary responsibility of a prosecutor in charging is to determine whether or not there is sufficient evidence to convict the accused of the particular crime in question and to authorize the filing of appropriate charges."[4] A decision to charge must meet

[3] That this should be the responsibility of the prosecutor is recommended in *Standards Relating to the Prosecution Function*, Approved Draft, ABA Project on Standards for Criminal Justice, Institute of Judicial Administration, New York, 1971, p. 32.

[4] *Uniform Crime Charging Standards*, California District Attorneys Association, December 1974, p. 13.

four criteria: the evidence satisfies the prosecutor that the accused is guilty of the crime to be charged; there is legally sufficient and admissible evidence of a corpus delecti; there is legally sufficient and admissible evidence of the accused's identity as the perpetrator; and the nature of the admissible evidence makes the probability of conviction by a reasonable and objective fact-finder sufficiently high notwithstanding foreseeable plausible defenses.[5] In addition, there are other important dimensions to the charging decision. First, the prosecutor will consider whether charging a suspect in a specific case would serve one or more goals of the system. In his consideration, he may weigh many factors that are largely external to the merits of the evidence, such as:[6]

- Attitude of the victim.
- Cost to the criminal justice system (including possible loss of public support and respect) for initiating prosecution.
- Undue harm to the suspect.
- Adequacy of the incarceration potential in alternative nonprosecutorial procedures (e.g., insanity commitment).
- Effectiveness of criminal sanctions (relative to available noncriminal sanctions).
- Willingness of suspect to cooperate in achieving other enforcement goals.

The prosecutor's charging decision sometimes will be complicated by the fact that, at the time the decision must be made, the criteria for charging cannot be fully applied because of uncertainties that cannot be resolved at that time.[7] The prosecutor may then rely on local prosecutorial policy. For example, he may at all times follow a policy of filing the most serious charge that the available evidence would support were the uncertainties to be resolved in favor of the prosecution; or he may apply a policy not to charge if a clear violation of constitutional rights has occurred in making the arrest. Also, when the charging decision is complicated by uncertainties in the case, the prosecutor may be particularly influenced by the current workload in his office and the court, because his decision directly affects the future load.

We next address the application of statistical performance measures to the two selected aspects, the charging threshold and the accuracy of charging.[8]

The Charging Threshold. A pervasive concern with the prosecutorial screening function is the translation of charging policies and standards into practice. Because it is highly discretionary, the decision to charge or not to charge a felony suspect reflects many policy elements, even though they may not be explicitly and fully articulated in some jurisdictions.[9] In this section, we shall regard policies or

[5] Ibid.

[6] F. W. Miller, *Prosecution: The Decision To Charge a Suspect with a Crime*, Report of the American Bar Foundation's Survey of Criminal Justice in the United States, Little, Brown & Co., Inc., Boston, 1969. The items listed are taken from chapter and section headings.

[7] For example, uncertainty in making the charging decision may exist because a legal issue requires resolution by a later court ruling; or because the availability of affirmative defenses is unknown; or because the admissibility of decisive evidence cannot be ascertained short of a judicial determination; or because the reliability of prosecution testimony cannot be verified without follow-up police investigation, etc.

[8] See N. Abrams, "Prosecutorial Charge Decision Systems," *UCLA Law Review*, Vol. 23, No. 1, October 1975, for an in-depth discussion of a third aspect, namely, the *timing* of the charge decision.

[9] *Uniform Crime Charging Standards*, is a leading example of charging standards developed for statewide use. Another work giving prosecutorial standards, including charging policies, is *The Prosecu-*

standards that govern the "charging threshold" as being in the nature of goals or objectives. We may then ask how well these goals are being realized, that is, Is the charging threshold that is applied the one that is intended? One approach to answering this question could be purely procedural; for example, all charging decisions by screening deputies in cases that meet specified criteria are immediately reviewed at the supervisory level and possibly modified. Another approach might be to rely on case auditing, that is, on the in-depth review after-the-fact of the charging decision in appropriate samples of the case flow.[10] And a third approach could be the statistical monitoring of the entire case output (or a sample thereof) of the screening operation, using performance measures with a level of detail that would depend on the specificity of the charging policies being monitored. Whatever the approach, maintaining a gross statistical picture of the case output from screening serves the purpose of alerting officials to changes in the nature of this output over time, and thereby to possibly unintended movement of the charging threshold.

This gross statistical picture is most simply depicted in terms of a classification of the case output from screening. The corresponding performance measures may be taken to be the *size of each class, expressed as a proportion of the number of suspects screened.* The set of classes might be as follows:

- Suspects unconditionally rejected for felony prosecution because of evidence deficiencies.
- Suspects unconditionally rejected for felony prosecution for reasons other than evidence deficiencies (further classified by major reason).
- Suspects rejected for felony prosecution in favor of misdemeanor prosecution.
- Suspects for whom prosecution is deferred in favor of diversionary (rehabilitative) programs.
- Suspects charged with at least one felony count.[11]

These are such natural measures for the purpose of monitoring the effect of charging threshold policies that we need not discuss the application of the suggested selection criteria (i.e., relevance, proximity, direct linkage, applicability, specificity, clarity, consistency, complementarity, and implementability), except to note that implementation may entail a more complete and precise recording of screening data than had previously been the practice. The desired information is, however, readily available to the screening deputy.

Proceeding from the gross statistical monitoring of the screening function in order to show adherence to charging standards, we next address a more specific and subtle charging issue, namely, *abuse of discretion to reject prosecution of felony suspects.* The decision to reject may, for simplicity, be viewed as having one of three bases:

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tor's Discretion: A Statement of Policy of the Office of District Attorney of Harris County by Carol S. Vance, District Attorney, Harris County, Texas, April 1974.

[10] Section VIII discusses a demonstration of case auditing conducted in our study in Multnomah County, Oregon, and Dade County, Florida.

[11] This class would ordinarily be broken down in greater detail to reflect the seriousness of the charges as defined in the jurisdiction (e.g., capital; 1st, 2d, or 3d degree; class A, B, or C, etc.) and the number of charges.

1. The case has significant (and incurable) evidentiary defects.[12]
2. The case conforms to a recognized specific reason for rejection, other than insufficiency of evidence.
3. The case, whether or not it might culminate in a conviction, evokes a judgment call from the prosecutor that to initiate prosecution would not be in the "interests of justice."

Applying such a classification to help reveal abuse of charging discretion requires that in each instance of rejection there be recorded the (accurate) reason for its occurrence. Most prosecutors' offices already use a more or less standardized list of evidence flaws in substantiating the reasons for rejection. For example:[13]

- No corpus delicti
  - No specific intent (if this is an element of the offense)
  - No criminal act
- No connecting evidence
  - Statement deficiency
  - Witness deficiency
  - Physical evidence deficiency
- Insufficient evidence[14]
  - Facts weak
  - Evidence not available
  - Incomplete investigation
  - Witness not available
  - Evidence inadmissible

Similarly, in connection with the second basis for rejection, a set of *nonevidentiary* reasons is widely recognized. These include:[15]

- Contrary to legislative intent
- Antiquated statute
- Victim requests no prosecution
- Need for granting immunity to suspect (to support other prosecutions)
- De minimus violation
- Present confinement on other charges
- Pending conviction on other charges
- Highly disproportionate cost of prosecution

For the third rejection basis, however, one cannot expect to find objective, standardized reasons because the rejection results from a judgmental balancing of factors in

[12] Or the case may have curable evidentiary defects and is tentatively rejected and sent back to the police for further investigation.

[13] Extracted from "Prosecutorial Discretion in the Initiation of Criminal Complaints," unsigned comment, *Southern California Law Review*, Vol. 42, 1969.

[14] The meaning of "insufficient" does, of course, vary among prosecutors' offices. The legal burden of proof on the prosecution at the charging stage is "probable cause" to believe that the suspect committed the alleged crime. Some prosecutors look ahead to the probability of being able to present proof of guilt beyond a reasonable doubt; they may also look to the expected reaction of the trier of fact to the defendant and to the evidence presented. These prosecutors are thereby applying a charging standard of whether or not a jury (or judge) would convict this suspect of this crime.

[15] *Uniform Crime Charging Standards*, pp. 42-46.

an exercise of true prosecutorial discretion. The result is typically characterized only as a rejection in the "interests of justice."

Here, then, is a potential impediment to the use of statistical performance measures for revealing abuse of discretion in rejecting felony suspects. If the broad, undifferentiated "interests of justice" reason were frequently employed by the prosecutor, it might effectively conceal the presence of abuse. Another impediment would be, of course, the inaccurate recording of the true reasons for rejection.

The charging standards issue on which our study primarily focused in its demonstration phase was whether and how the charging threshold might be changing over time within a jurisdiction. To this end, we adapted the foregoing theoretical discussion as follows.

We viewed the gross output of case screening (whether done by the prosecutor's office or the court) as falling into one of three classes: cases filed on the most serious charge(s) booked by the police, cases filed on a lesser charge or charges (broken down by level of seriousness), and cases rejected unconditionally. These outputs were taken to be the appropriate performance measures. The scheme is shown in Table 4.1, along with the data elements implied by these measures.

How do inferences about a shift in charging threshold over time follow from the use of these measures? For example, if the police's arrest policies and booking standards remained unchanged, one would expect to see a rise in the rejection rate and the filing rate at lesser levels and a decline in the filing rate at the booked-charge level *when the charging threshold for a specified offense category were elevated from one period to another.* But if police arrest practices changed too and, say, responded to the change in the charging threshold by less frequent presentation of weaker cases, then changes in the gross measures of case screening shown in Table 4.1 would be more ambiguous and difficult to interpret. Knowledge of changes in police arrest practices would be required to remove their effect on the inference about a charging threshold shift. A fuller discussion of the interpretation of these charging threshold performance measures is given in Secs. VI and VII, where their application to the demonstration jurisdictions is presented.

Table 4.1 also indicates a breakdown of the rejection rate by reasons for rejection, which we sought in order to obtain insights about the possible abuse of discretion in case rejection and about the quality of cases submitted by the police. To illustrate the interpretation of this more detailed performance measure, we observe (as noted earlier) that a dramatic rise in the frequency of rejections in the "interests of justice" might signal an abuse of discretion. Or if the proportion of rejections for inadequate evidence lessened and the proportion of rejections for other specific reasons increased, while the "interests of justice" rejections remained constant, this might be a signal that the quality of cases submitted by the police had improved. And if the overall rejection rate rose at the same time, an elevation in charging threshold might also be indicated thereby. (See the discussion in Secs. VI and VII.)

The required data elements for the performance measures shown in Table 4.1 would generally lead to a straightforward collection approach. In some jurisdictions, to judge from our experience in the demonstration phase,[16] reasons for previous rejection may not have been recorded in sufficiently clear, complete, and accurate form; thus, attention to this collection problem would be required. However, it is

[16] See App. D.

Table 4.1

PERFORMANCE MEASURES IN CASE SCREENING: CHARGING STANDARDS OR THRESHOLD

Performance Measures	Required Data Elements
By category of highest offense at police booking as a percentage of cases screened over a specific time period:	Number of cases screened
Filing rate on most serious booked-charge level	Number of cases filed at most serious booked charge
Filing rate on less than most serious booked-charge level[a]	Number of cases filed at less than most serious booked-charge level
Gross rejection rate	Number of cases rejected
Reasons for rejection (in percentage of cases rejected):[b]	Number of cases rejected
Evidence deficiency (illustrative subcategories: no corpus delicti, no connecting evidence, insufficient evidence, inadmissible evidence, returned to police for need of more investigation)	Number of cases rejected for evidence deficiency
Nonevidence deficiency: specific reasons (illustrative subcategories: victim requests no prosecution, need to grant immunity, suspect currently confined or pending conviction on other charges, contrary to legislative intent)	Number of cases rejected for specific nonevidence deficiency reasons
Nonevidence deficiency: general reasons (illustrative category: interests of justice)	Number of cases rejected for general or interests of justice reasons
Case audit results	Judgment about the charging standards used, given the observed screening decision

[a]May be broken down in greater detail to reflect the seriousness of the charge. For example, if there are three felony levels of seriousness (A, B, and C) and if the most serious charge level is, say, Felony A, this measure may be broken down into three categories: Felony B, Felony C, and misdemeanor.

[b]Depending on whether the screening agency (prosecution, lower court) has a pretrial diversion program, which agency makes the diversion decision, and at what point in the felony proceeding diversion may occur, an additional category of "rejection" reasons may be added.

clearly possible to record these reasons completely and accurately, as evidenced in those jurisdictions that have implemented PROMIS.

The Accuracy of Charging. We have selected charging accuracy as a second issue area to illustrate the use and limitations of statistical performance measures applied to prosecutorial screening. The question to be considered is, How accurate are the charges as originally filed, given the decision to initiate a (felony) prosecution? First, what is *accurate* charging? For the discussion here, we adopt a simplified definition: *A prosecutor has charged a defendant accurately if there is sufficient evidence to convict him of the most serious charges filed.*[17] Second, given this definition, we may ask, What data are relevant to the measurement of charging accuracy? Is the collection of these data feasible?

[17] We have already noted that at the time of the charging decision various uncertainties may exist on the law governing the case or on the availablity and admissibility of evidence. These uncertainties may prevent the prosecutor from making a reliable determination of the most accurate charges to file. Also, note that our definition requires that a prosecutor, in seeking to charge accurately, go beyond his minimal legal duty of charging when there is "probable cause." (In contrast with our definition, Abrams, pp. 49-55, views the accuracy of the charge decision as being the correctness with which the future "disposition" of the defendant is predicted.)

The key to answering these questions is that *errors in filing charges affect later stages of a criminal proceeding and must be inferred from events subsequent to the charging.* But this proposition involves an intrinsic difficulty; namely, the outcomes of subsequent events in criminal proceedings are governed by a variety of factors, only one of which would be the accuracy of the charges as originally filed. For example, an acquittal may occur not only because of an inaccurate charging decision, but also because of prosecutorial shortcomings in preparing the case, arguing motions, or conducting the case at trial. Or superior defense counsel performance may be the explanation. The occurrence of an acquittal by itself thus lacks specificity as a charging accuracy datum. But we also observe that this ambiguity could be largely eliminated by accurately recording the true reason for the acquittal.

Trial outcomes, however, are but one class of the many events that may be affected by the accuracy of original charging. Others include the outcomes of the preliminary hearing, pretrial hearings on motions of various types, plea negotiations, etc. And each of these events is also affected by processing factors other than charging accuracy, often to a greater degree than by the latter. In consequence, we are led to the use of a *set* of performance measures that collectively captures the effects of inaccurate charging, as well as information extraneous to this issue.

Table 4.2 presents this set of performance measures, together with the required data elements. Basically, these measures reflect four types of dispositions subsequent to charging: nonconvictions (includes pretrial dismissals; nolle prosequi cases, if applicable; pretrial diversions or interventions, if applicable; and trail acquittals, dismissals, and mistrials); convictions on all charges as filed (both by guilty plea and trial); conviction at the most serious filed charge level, but with some charge or count reductions; and conviction at lesser charge levels than initially filed.[18]

The measures given in Table 4.2 are *relevant* to the issue of charging inaccuracy in the sense mentioned earlier; that is, they capture information on how this inaccuracy affects subsequent defendant-related events. They are more or less *proximate* depending on when these events occur in the chain of consequences of the charging decision. Their *directness of linkage* would depend on the relative importance of factors other than charging accuracy in the occurrence of the events. Because these measures are constructed from mutually exclusive and exhaustive counts of defendant dispositions, they satisfy the criteria of *consistency* and *complementarity*. Their meaning seems clear, but (as discussed above) shortcomings in *specificity* are present. For example, the final measure could be dominated by the plea bargaining process, which may be motivated by reasons other than the redress of charging inaccuracy. Finally, because these measures involve only counts of defendant dispositions, they should be readily *implementable*.

Given the ambiguity of these measures, one would have to observe large changes in their magnitudes over time to conclude that charging accuracy has changed; or if it is known from other sources that other factors affecting the measures have remained essentially constant, smaller changes in the measures over time would suffice to indicate a movement in charging accuracy. However, should one of the "extraneous" factors change substantially, for example, as did the plea bargaining policy in one of our demonstration jurisdictions between the periods studied, then

[18] These measures may be broken down in greater detail to reflect the seriousness of the convicted charge. See footnote a, Table 4.1.

Table 4.2

PERFORMANCE MEASURES IN CASE SCREENING: CHARGING ACCURACY

Performance Measures	Required Data Elements
By category of highest offense at charging as a percentage of cases charged or disposed over a specified time period:	Number of cases charged or number of dispositions
Overall nonconviction rate	Number of nonconvictions
Pretrial dismissal rate	Number of pretrial dismissals
Nolle prosequi rate (if applicable)	Number of nolle prosequi
Pretrial diversion or intervention rate (if applicable)	Number of pretrial diversions
Trial acquittal, dismissal, and mistrial rate	Number of trial acquittals, dismissals, mistrials
Conviction rate on all charges as filed	Number of convictions on all charges as filed
By plea of guilty	As above, by plea
By trial	As above, by trial
Conviction rate on at least one of the most serious charges, but with charge or count reductions	Number of convictions on at least one of the most serious charges, but with charge or count reductions
By plea of guilty	As above, by plea
By trial	As above, by trial
Conviction rate on lesser charges than the most serious filed	Number of convictions on lesser charges
By plea of guilty	As above, by plea
By trial	As above, by trial
Case audit results	Strength and appropriateness of filed charges in average case

this change could nullify the applicability of one or more of these measures to charging accuracy performance. (See Sec. VI for a further discussion.)

Dispositional data that comprise the required data elements shown in Table 4.2 for the charging accuracy measures tend to be among the most available, accurate, and complete items of information in the criminal proceeding. Our experience in the demonstration jurisdictions was that the individual case folders generally sufficed as sources of such dispositional data. (See App. D.)

Finally, we sought to broaden our understanding of the performance measures proposed in Table 4.2 by postulating theoretical (statistical) equations that related these conviction and nonconviction probabilities (or rates) as dependent variables to a broad set of possible determinants. The latter independent variables reflected the characteristics of the defendant, his pretrial status and type of counsel, the original charges filed against him, whether he was tried, the influence of case backlog, etc. We then tested these models (by means of regression analysis), using the data obtained from case samples drawn from the two demonstration jurisdictions. We found that our postulated equations did not provide an adequate explanation of the determinants of conviction probability. (By contrast, similar statistical models worked rather well in explaining the determinants of sentence outcomes and delay in proceedings.) It is likely that our failure to include (because of lack of data) such possible determinants as the seriousness of the crime incident, the mitigating or exacerbating aspects of the crime incident and the defendant, and case-strength factors at time of charging was responsible for the disappointing outcome. It is clear

that more theoretical and empirical work must be done if a useful statistical model of conviction probabilities is to be constructed.

Case Processing Efficiency and Delay

Within the issue area termed *case processing efficiency and delay*, our study and particularly its demonstration phase have focused on a limited set of topics, primarily judicial efficiency in criminal proceedings[19] and delay in proceedings, and the use of lay participant (jurors, victims, and other witnesses) time. For these topics, we next set out the performance measures selected for the conduct of this study, together with the data elements they imply and explanatory comments.

Judicial Efficiency. One way of assessing judicial efficiency in criminal proceedings could be simply to divide the total number of dispositions in a period (say, a year) by the number of judge-years devoted to the criminal caseload. But this measure is very gross and unrevealing. When its magnitude changes, one does not know whether this was caused by a change in the relative frequencies of different types of cases processed (e.g., the mix of crime types) or by changes in policies or procedures affecting the frequency with which various judicial activities occur in a specified case type or affecting the time consumed in a specified type of judicial activity.

A more reliable measure of judicial efficiency is available and is already being employed in some jurisdictions (including California, where the judicial council employs it to help establish the required number of judicial positions in local jurisdictions). This measure has the virtue of compelling the collection of those data and the performance of those calculations that generally reveal what is responsible for changes in judicial efficiency. Specifically, the measure is *the number of weighted cases processed (counted either by filings or dispositions) per available judge-year.*

To explain the nature of this performance measure, we first describe what is meant by *weighted caseload processing time.* For each activity (or step) in a criminal proceeding, one may associate both an average duration of that activity and the relative frequency with which it occurs per disposition. (Table 4.3 shows the classifications of judicial activities, which have been used in three weighted caseload studies.) The weighted processing time for a judicial activity is the product of its average duration and its relative frequency of occurrence per disposition. For example, taking a guilty plea might on the average consume 20 minutes of the court's time and occur six times per ten dispositions. If so, the weighted processing time per disposition would be 12 minutes for a guilty plea. By summing the weighted processing times for the various activity types, we obtain the weighted caseload processing time per *disposition* (for all types of dispositions). Then, to estimate the weighted caseload processing time per *filing,* we multiply the preceding result by the ratio of dispositions to filings.[20]

[19] Although we discuss only judicial efficiency here, similar approaches are available for measuring prosecutorial and public defender case processing efficiency. For example, Peat, Marwick, Mitchell & Co. has used a statistically based approach to ascertain prosecutorial and public defender staffing requirements in several California counties (*Staffing Requirements Projection Approach for Professional Prosecution and Defense Services,* September 1974, a study done for the county of Santa Clara, Calif., under LEAA funding). Unquestionably, case processing efficiency is a product of the efficiency with which *all* of the court agencies perform, rather than the bench alone.

[20] This simple procedure rests on the assumption that nondisposed cases resemble disposed cases in the consumption of judicial time.

Table 4.3

CLASSIFICATIONS OF JUDICIAL ACTIVITIES IN CRIMINAL PROCEEDINGS

California Judicial Council Study[a]	Florida Weighted Caseload Statewide Study	Multnomah County (Oregon) Circuit Court[b]
Short matters (plead not guilty, continuance, calendar call, sentencing and probation hearing, diversion hearing, other pretrial motions, trial confirmation conference)	Case-related, with party and counsel present:	Arraignment
Plead guilty	First appearance hearing	Motion hearing
Dismissal transfer	Preliminary hearing	Plea hearing
§995 PC (penal code) motion	Arraignment	Other hearings
§1538.5 PC motion	Motion hearings	Court trial
Court trial (regular, transcript, transcript and testimony)	Plea hearing	Jury trial
Select jury and jury trial	All other hearings	Sentencing hearing
Habeas corpus hearing	Pretrial conference	
	Other conferences	
	Detention hearing	
	Adjudicatory hearing	
	Disposition hearing	
	Nonjury trial	
	Jury selection	
	Jury trial	
	Sentencing/presentence investigation	
	Postdisposition/trial hearing, motions	
	Other case-related	
	Case-related, office work:	
	Predisposition (legal research, drafting, etc.)	
	Postdisposition (legal research, drafting, etc.)	
	Conferences	
	Jury-related:	
	Grand jury	
	Mass jury selection	
	Statewide grand jury	
	Coroner's jury	
	Non-case-related:	
	Correspondence	
	Travel	
	General research and study	
	Conferences	
	Court administration	
	Ex officio	

[a]*Judicial Weighted Caseload System Project*, Final Report Prepared for the Judicial Council of California, Arthur Young & Company, Sacramento, May 1974. This list of activities pertains to criminal (felony) proceedings in superior court. The cited report gives similar lists for other types of proceedings in superior court as well as the various proceedings in municipal courts.

[b]See Sec. VI.

The second ingredient of the judicial efficiency measure for criminal proceedings is the *available judge-year time.* This is the total time per year per judge less the time consumed by civil matters, vacations, sick leave, official traveling, professional meetings, etc.

The judicial efficiency measure is then calculated simply by dividing the available judge-year time by the weighted caseload processing time per filing. This measure generally satisfied the selection criteria that we gave earlier, although implementation may present difficulties.

In jurisdictions where the nature of the case mix by offense tends to vary significantly over time or locality, it would be desirable to maintain the judicial efficiency measure separately for different classes of offenses[21] and possibly for different geographic divisions of the court system. By so doing, one makes the measure more sensitive to changes in efficiency.

For the purposes of this study, we felt that the next important step forward in the use of judicial performance measures would be the wider application of the measure *the number of weighted cases processed per available judge-year.* This would enable court systems to better determine the effect of policies that alter the relative mix of activities within the proceeding (e.g., the effect of a change in plea bargaining policies and a consequent change in the frequency of related activities); to better translate projections of future caseloads into requirements for practitioners and other court personnel; and to better estimate the effect of procedural changes (e.g., the adoption of omnibus hearings) that would alter the time consumed in affected court activities.

We attempted to apply the weighted-caseload approach in one of the two demonstration jurisdictions,[22] but rejected the results because of deficiencies in the available raw data. The key difficulty was the unwillingness of the judges of this court to have the use of their time monitored directly. Indirect sources of data (e.g., logs of the clerk's activities while court was in session) and inferences about the allocation of off-bench time devoted to various matters turned out to be too inaccurate. (See Sec. VI and App. D for a discussion of the data collection and analysis.) The lesson appears to be that the cooperation of the bench is essential, and this cooperation probably hinges on persuading the judges beforehand of the value of weighted-caseload information and on preserving their anonymity in the data recording process.

Delay and Use of Lay Participant Time. The speediness of felony proceedings is a highly visible attribute by which criminal justice is necessarily judged, and it is a matter of urgent concern to officials and the public. In many jurisdictions, delay has increased because resources allocated to court system agencies have not kept pace with rising caseloads or because practitioner time is not used efficiently.

[21] Currently, jurisdictions that collect the necessary data and calculate weighted criminal caseload for judges do so only for criminal cases as a whole and not by offense type or class. However, different types of *proceedings* are already being handled separately, as in the superior courts of California for criminal, juvenile delinquency, probate, personal injury-death-property damage, eminent domain, civil complaints, civil petitions, and appeal proceedings; and in the municipal courts for felony preliminary-felony reduction, traffic, intoxication, other misdemeanor, civil, small claim, juvenile traffic, and illegal parking proceedings. See *Judicial Weighted Caseload System Project,* Final Report Prepared for the Judicial Council of California, Arthur Young & Company, Sacramento, Calif., May 1974.

[22] In the second demonstration jurisdiction (Dade County, Florida), our effort was preempted by a study that the court itself had recently completed as part of a statewide effort. The data of this official study were not available to us.

We adopted three basic gross measures of delay or elapsed time between specified events: median[23] number of days, minimum time for the lengthiest 10 percent of cases (i.e., the shortest of the longest 10 percent of cases), and the percentage of cases exceeding some standard (set by court rule or statute). The specified intervals we selected were from arrest and from arraignment to dismissal; to plea of guilty; to trial; to sentencing; and to final disposition (the sum of cases dismissed, acquitted, and sentenced). In addition, the time between conviction (by any means) and sentencing is of salient interest. Speedy trial standards usually refer to the arrest-to-trial period, but it is also revealing to estimate the percentage of cases exceeding the standard that are disposed of otherwise. These performance measures should be calculated for *all* felonies and for *each* major offense category as defined at the point of charging to determine which offense categories account for more or less delay.

In addition, it is useful to apply multivariate statistical techniques to determine the independent effects of certain factors on delay. We have hypothesized that four factors may influence delay: pretrial custody status (because defendants out on bail or O.R. (own recognizance) may have incentives to delay proceedings); type of defense attorney (because court calendar conflicts or incentives stemming from attorney compensation arrangements may lead to more delay by private attorneys or because one category of attorney might know the system better than another); type of disposition (cases tried may take longer than guilty plea cases); and heavier or lighter caseloads or more or less court backlog as measured directly or as reflected in a time trend (proxy) variable.

Continuance measures, which are indirect measures (and causes) of delay, provide additional insights into performance. They should be estimated separately for contested and uncontested cases (because of large differences between the two types of cases) as well as for the sum of contested and uncontested cases. Where possible, the "movant" or "requestor" should be identified (e.g., prosecution, defense, court, joint) to reveal which agencies are most responsible for continuance-induced delays.[24] The continuance measures we adopted were the percentage of cases continued, the number of continuances per case, and the average number of days continued per case.

Measures of the use of victims' and other witnesses' time that we adopted were the number of appearances per case disposition, the time consumed per appearance, and the number of total appearance-hours consumed per disposition (the product of the previous two measures). Each measure was calculated separately for victims and other witnesses. At best, court records may contain information on the number of victim and witness appearances per disposition. Data on time per appearance are generally not recorded, at least in the two jurisdictions we examined. It can be

[23] Median instead of mean or average number, because the mean is too sensitive to the presence of a few cases of *very* long duration.

[24] This complete classification of continuances in terms of the responsible party would be advantageous for many purposes. At the same time we must recognize the complex nature of the reasons for continuance. For example, in being "responsible" for a continuance, a defense attorney may have legitimate reasons (e.g., to prepare and file a motion) or illegitimate reasons (e.g., to collect a fee), or reasons that cut both ways (e.g., "judge-shopping" to avoid unduly harsh judges). The usual prosecutor's reason is the unavailability of witnesses, which might mean that they cannot be found, despite his efforts, or that he has not tried sufficiently to find them. If the complete attribution of continuances is impractical, the identification at least of defense-moved continuances would be the next-best level of detail. In our application of continuance measures in one jurisdiction, the data were adequate to attribute responsibility; in the other, the data were incomplete (see Secs. VI and VII).

measured directly (the preferred approach) by filling out time sheets for each appearance, or it can be estimated from the response of victims and witnesses to mail surveys (as we did). However, the latter approach is flawed because it relies on the memories of respondents. These performance measures should be calculated for the entire (or a sample thereof) felony caseload to obtain a general picture, but it may also be estimated for particular offense types that are thought to consume substantial victim or witness time.

Conventional measures of juror usage essentially measure the oversupply of jurors in an indirect fashion. For example, the Juror Usage Index (JUI) used in federal courts is defined as the available number of jurors per day (summed over a month) divided by the number of juries in trial per day (summed over a month). We adopted a more informative and direct set of measures—simply the average fraction of time a juror spends in idleness (although in the courthouse), in jury selection, and in trial. The latter two measures should be calculated separately for civil and criminal matters in jurisdictions that use the same juries for both types of cases. Data on the use of juror time are normally not recorded, at least in the two jurisdictions we examined. Again, it can be measured directly (the preferred approach) or it can be estimated from juror responses to mail questionnaires (as we did).

Table 4.4 presents the performance measures that were used in this study for the topics discussed above in the issue areas of delay, continuances, and the use of lay participant time, as well as the data elements that are required to apply the measures. In both jurisdictions we worked in, data from case files and agency records were adequate to estimate measures of delay and continuances (but not attribution in one jurisdiction). No data were available in agency records to estimate the use of juror time or the time consumed per appearance of victims and other witnesses; these data were collected in surveys of these lay participants. Data from agency records on the number of witness or victim appearances per disposition were fragmentary; in one jurisdiction the data were available only for trials, whereas in the other the data were available for both contested and uncontested cases but only in a small fraction of the cases sampled. Thus, we also collected frequency of appearance data using the victim and witness mail surveys. Additional discussion can be found in Secs. VI, VII, and App. D.

Plea Bargaining

Plea bargaining (the terms *plea negotiation* and *case settlement* will be used here synonymously) is currently an issue area attracting much professional and lay attention and engendering wide public debate. Numerous jurisdictions have conducted or are conducting experiments to ascertain the effects of curtailing or eliminating some or all forms of plea bargaining.[25]

To simplify, we shall define a *plea bargain* to be an agreement between the defendant on one side and the court system on the other that elicits a plea of guilty from the defendant.[26] The result of this agreement is generally (though not neces-

[25] For example, the Office of the U.S. Attorney in San Diego, California, the District Attorney's Office in Fresno County, California, the District Attorney's Office in Los Angeles County, California, and the District Attorney's Office in Multnomah County, Oregon.

[26] This definition excludes straight pleas of guilty to the specified charges for which no consideration is offered to the defendant.

Table 4.4

PERFORMANCE MEASURES OF DELAY AND THE USE OF LAY PARTICIPANT TIME

Performance Measures	Required Data Elements
Elapsed Time between Events	
For all felonies and specific offense categories over a specified time period:	
1. Median number of days	
2. Minimum number of days for longest 10 percent of cases	
3. Percent of cases exceeding standard[a]	
These measures to be calculated between the following events:	*For each case in each sample, applicable dates of arrest, arraignment, dismissal, guilty plea, trial (commencement and end dates), sentencing*
Arrest and arraignment } and { Dismissal, Guilty plea, Trial, Final disposition	
Conviction and sentencing (Meas. 1 only)	
Continuances	
Separately for all, contested and uncontested, cases in a random sample of all felonies over a specified time period:	
4. Percent of cases continued	Number of cases continued ÷ number of cases
5. Continuances per case	Number of continuances ÷ number of cases
6. Number of days continued per case	Number of continued days (= number of continuances × number of days per continuance) ÷ number of cases
7. Percent of continuances attributed to defense, prosecution, court, joint	Identity of "movant" or "requestor"
Use of Lay Participant Time	
Victims and Other Witnesses	
For all felonies over a specified time period:	
8. Number of appearances per disposition	Separately for victims and other witnesses
9. Time consumed per appearance	Separately for victims and other witnesses
10. Total number of appearance-hours per disposition	(Measure 8) × (Measure 9)
Jurors	
For all trials over a specified time:	
11. Percent of time idle	
12. Percent of time in jury selection	Separately for civil and criminal trials
13. Percent of time in trial	Separately for civil and criminal trials

[a]The speedy trial standard (defined by court rule or statute) usually applies only to the arrest-to-trial period, but it is of interest to compute this measure as if the same standard applied as well to the period between arrest and dismissal, arrest and guilty plea, and arrest and final disposition.

sarily) a less severe punishment than that which might otherwise have been imposed on the defendant. A plea agreement may entail a dismissal of some of the original charges (or counts), possibly producing a reduction in the gravity of the most serious charge. It may provide for the dismissal of other pending cases against the defendant or a commitment by the prosecutor not to file other cases. Or it may entail a sentence commitment by the court or a limitation on the prosecutor in recommending or opposing sentences. Some plea bargaining issues are not quantitative in nature and cannot be clarified by performance measures. But some aspects of plea bargaining are quantifiable. We shall discuss an analytical framework in which to view the quantitative side of the plea bargaining process.

What the defendant has gained from a plea agreement (even if only the removal of uncertainty about his sentence) we call a *concession* by the criminal justice system. At the same time, the system tends to gain from the agreement, for example, by conserving its resources. This is not to say that plea agreements are necessarily prompted by the system's desire to conserve resources, for there are many other reasons within the accepted administration of criminal justice that account for plea agreements.[27] Whatever may be the motivation of the prosecutor for entering into a plea agreement, increasing the proportion of cases that are settled by plea negotiation has the effect of creating operational advantages for the system; for example:

- The average time to reach final disposition would tend to shorten.
- The rate of trials and the trial backlog would tend to lessen.
- The use of witnesses and jurors would be reduced.

For analytical purposes, we can thus regard a plea agreement as an *exchange* or *balance* (though not necessarily a conscious one) of concessions to the defendant and operational advantages and/or disadvantages to the system resulting from a certain conviction by a plea of guilty rather than a less certain conviction by trial. This exchange or balance is the quantitative framework for plea bargaining that our study has adopted.

But how might concessions to the defendant be measured? The outcome of plea bargaining to the defendant might be characterized, for example, as a changed description of his alleged criminal conduct; or a change in the number and legal nature of the charges against him, possibly reflecting other pending or potential cases; or a change in the punishment that he might otherwise incur. We have concluded that, among these and other possibilities for measuring concessions, the preferred approach to assessing the "amount" of concession to the defendant is to

[27] See D. J. Newman, *Conviction: The Determination of Guilt or Innocence without Trial*, Little, Brown & Co., Inc., Boston, 1966, for a comprehensive treatment of this subject. Newman gives the following reasons (other than conserving resources) for granting concessions to the defendant:

- Avoidance of a repugnant social label (e.g., sex deviant) as a result of conviction on the original charge.
- Avoidance of a felony record.
- Avoidance of a mandatory sentence.
- His youth and inexperience.
- His "respectability."
- His low mentality.
- The disrepute of the victim, complainant, or witnesses.
- His previous illegal relationship with the victim.
- The view that his conduct is regarded as normal within his subculture.

use *the resulting change in imposed punishment* (i.e., the sentence change) attributed to the plea agreement. One compelling reason for this choice is that a direct sentence commitment is frequently used as the form of the concession in many jurisdictions (and seems to be the leading objective of the typical defendant).

For analytical purposes, we define the concession to the defendant to be the punishment forgone by him; that is, the concession is the difference between the following two levels of punishment severity:

- The sentence imposed on the defendant as a result of his guilty plea in return for the prosecutor's reducing the gravity of the charges; or lessening the number of counts; or arranging or consenting to a sentence commitment; or agreeing to drop or not to file other cases; or providing some combination of the above.
- The sentence that would have been imposed in the absence of the plea agreement, that is, had the defendant gone to trial (or, alternatively, had he made a straight plea to the original charges).[28]

To apply this definition we are compelled to simplify the multidimensionality of sentences and to devise a means of estimating the sentence that would have been imposed on the defendant in the absence of the plea agreement.

First, consider the problem of sentence complexity. Sentence elements include prison time, jail time, supervised or unsupervised probation, registration, fines, restitution, community services, and rehabilitative programs with conditions on the defendant (e.g., entering a drug therapy program); furthermore, any given sentence may contain several of these elements. Given the complex forms that sentences take in practice, it is difficult to perform arithmetical operations on them or even to compare them in severity. The analysis of sentences for many purposes is greatly facilitated by the use of a Sentence Severity Index, that is, a one-dimensional numerical scale onto which the various elements of a sentence can be projected and combined. But this process of integrating different kinds of punishment into a single measure requires that they be given weights relative to one another. For example, what should be the relative severity of one year of prison incarceration compared with a $10,000 fine? Any single set of these weights is controversial, for if it is appropriate for one class of defendants in one community, it is inevitably inappropriate for another class in that community or the same class in another community. Although this is a major difficulty, we feel it can be overcome by the use of several alternative sets of weights that span reasonable differences in belief about how onerous one type of punishment is relative to another. We next describe the four alternative sets used in this study; their application is illustrated in Secs. VI, VII, and IX.

The first of our four weighting schemes (Index A) approximates one developed by the California Bureau of Criminal Statistics (BCS), but never applied in their

[28] In his review of the draft of this report, Harry Subin pointed out that the concession to the defendant would be better measured by taking the reference level of punishment to be that which the judge and/or prosecutor *threatened* to have imposed. Granting this, it seems clear that the collection of data on "threatened" punishment presents formidable difficulties. See in this connection Michael O. Finkelstein, "A Statistical Analysis of Guilty Plea Practices in the Federal Courts," *Harvard Law Review*, Vol. 89, No. 2, December 1975, pp. 293-315, wherein the argument is developed that prosecutors may be using threats of lengthy sentences to obtain convictions in cases in which the government's evidence is insubstantial.

publicly available summaries of sentencing. Our Sentence Severity Index A was defined to be the sum of the following component scores:[29]

Jail incarceration score	1 per month of jail time imposed
Custody time (credit) score	−1 per month of custody before disposition
Probation time score	.167 per month of probation imposed (i.e., 2 per year)
Fines score	1 per $1000 in fines (with maximum score of 5)
Prison incarceration score	18 plus 1 per year of prison time imposed

Sentence Severity Index B differs from Index A in that both the probation and fines scores have only half the magnitude of those of Index A, but the increase in prison incarceration score per year imposed is three times as large. Index C differs from Index A in that both the probation and fines scores are taken to be zero, while the increase in the prison score per year imposed is five times as large as for Index A. Finally, Index D differs from Index A in that again the probation and fines scores are taken to be zero, while the prison incarceration score is the same as the jail incarceration score per month of incarceration imposed (i.e., the incarceration score is 12 per year imposed whether jail or prison). Indices C and D embody the view that fines and probation times are not punishment (even though they may involve "costs" to the defendant, perhaps in the sense that bail bond fees and legal fees are costs but technically not punishment).

Table 4.5 summarizes the weighting formulas by which the four severity indices are calculated. By way of illustration, for a sentence of 6 months in jail plus 5 years' probation, the magnitude of A would be 16; B would be 11; and both C and D would be 6. For a sentence of 10 years in prison, A would be 28; B, 48; C, 68; and D, 120.

Next, consider the problem of estimating the sentence that would have been imposed on a defendant in the absence of the case settlement—the second difficult aspect of applying our definition of concession to the defendant. Conceptually, given reliable historical data concerning trial outcomes for defendants who are similar (in traits and charges) to this defendant, one could accurately estimate the hypothetical outcome had the latter gone to trial. In practice, however, the appropriate data are sparse in most court systems, one reason being that relatively few criminal cases culminate in trials and, of these, relatively few involve defendants similar to this one. This paucity of data is exacerbated because plea bargaining tends to be inherently selective; that is, some defendants who go to trial do so because their traits and criminal conduct have precluded their obtaining a concession in return for a plea. Thus, trial outcomes are appropriate for deriving the desired estimates only to the extent that trial defendants resemble defendants obtaining concessions.

One way of circumventing this data base problem is to adopt the view that defendants enter the plea bargaining process with the option of making a straight plea to the original charges and with the expectation that punishment resulting from a straight plea, if made, would be less severe than that imposed after conviction

[29] Translated into words, this formula reflects the value judgment that one month's incarceration in jail is as harsh as 6 months' probation time or a $1000 fine; or that one year of prison time (score = 19) is 58 percent harsher than one year of jail time (score = 12), while two years of prison time (score = 18 + 2 = 20) is 67 percent more onerous than one year in jail. We have followed the BCS approach in defining the components of the Index to be additive.

Table 4.5

FOUR ALTERNATIVE SENTENCE SEVERITY INDICES[a]

Components	Description of Indices A: Modified BCS	B: Probation and Fines Half of A; Prison Rate 3 Times A	C: Probation and Fines Zero; Prison Rate 5 Times A	D: Probation and Fines Zero; Prison and Jail Equal and Heavy
Jail time score (per month imposed)	1	1	1	1
Custody time score (credit per month of custody before disposition)	−1	−1	−1	−1
Probation time score (per month imposed)	.167	.083	0	0
Fines score (per $1000 in fines; maximum score = 5)	1	.5	0	0
Prison time score	18 plus 1 per year	18 plus 3 per year	18 plus 5 per year	12 per year
Illustrative sentences:				
6 months jail + 5 years probation	16	11	6	6
10 years prison	28	48	68	120

[a]The Sentence Severity Index is the sum of the five component scores.

at trial.[30] Thus, at least from the defendant's viewpoint, the concession to him could be properly measured by reference to his straight plea option. From the prosecutor's viewpoint in his prosecutorial role, it might be argued that the concession should be measured by reference to the potentially most severe punishment, that is, that imposed after conviction by trial. But this view should be moderated to allow for the likelihood that the defendant might be acquitted or be convicted of lesser charges as the result of trial. It can be further moderated by the prosecutor's responsibility to be also a "minister of justice," which may encourage him to feel that the most severe punishment obtainable is not necessarily the most desirable. The point of using the straight plea punishment as a reference level for calculating the amount of concession is, of course, that it enables the analyst to apply a larger data base than when using the trial-conviction alternative.

In the demonstration phase of this study we viewed performance measures of plea bargaining from the two perspectives: those that indicate *what* happens over

[30] E. J. Younger, Attorney General of California, addressed this point by citing the responses of federal judges to a *Yale Law Journal* questionnaire which indicated that 66 percent thought that the fact that the defendant had pled rather than been tried was a relevant factor in sentencing. Also, 87 percent of these judges agreed that it is accepted practice to give lighter sentences to those pleading guilty. "Change Plea Bargaining Law," *Los Angeles Times*, Part X, "Opinion," December 14, 1975.

time and those that indicate *how* the systemwide exchange or balance of gains or other (possibly harmful) operational effects on one hand and concessions to defendants on the other hand has changed over time. Table 4.6 displays performance measures relating to what happens over time to the gross plea rate and the frequency of different types of plea bargains, that is, the straight plea (to all charges and counts) rate (with no *other* bargain); the straight plea rate with *other* bargains (such as a sentence agreement or an agreement by the prosecutor not to oppose a defense-recommended sentence or an agreement to drop other pending cases); the plea rate to at least one count of the most serious charge, with other charges and/or counts reduced (with and without *other* bargains); and the plea rate to lesser charges (with and without *other* bargains).

Table 4.7 displays measures of the systemwide effects of plea bargaining. On one hand are measures of operational effects, some of which may be viewed as "gains" and some as "losses," depending on the direction of the changes in the performance measures and one's philosophical view. A change in plea bargaining policy (and the resulting change in plea bargaining rates) could affect pretrial dismissal rate (and diversion or nolle prosequi rate, where applicable), trial rate, trial conviction rate, overall conviction rate, measures of sentence severity imposed (e.g., percent of guilty pleaders incarcerated and the average sentence severity score imposed), delay in proceedings (e.g., measures such as the period from arrest to final disposition or arraignment to final disposition), and the use of witnesses' and jurors' time (e.g.,

Table 4.6

PERFORMANCE MEASURES IN PLEA BARGAINING: THE FREQUENCY OF PLEA BARGAINING

Performance Measures	Required Data Elements
By category of highest offense charged *as a percentage of dispositions over a specified time period:*	Number of dispositions
1. Straight plea rate (to *all* charges and counts) with no *other* bargain	Number of guilty plea dispositions in each designated category
2. Straight plea rate With sentence agreement With agreement to drop other cases With combination of the above	
3. Plea rate to at least one count of most serious charge with other charges and/or counts reduced With no *other* bargain With sentence agreement With agreement to drop other cases With combined sentence/drop other cases agreements	
4. Original charge plea rate (Sum of Items 1, 2, 3)	
5. Plea rate to lesser charges (charge bargaining rate) With no *other* bargains With sentence agreement With agreement to drop other cases With combined sentence/drop other cases agreements	
6. Gross plea rate (Sum of Items 4 and 5)	Total number of guilty pleas

Table 4.7

PERFORMANCE MEASURES IN PLEA BARGAINING: BALANCE OF SYSTEMWIDE EFFECTS AND SYSTEMWIDE CONCESSIONS TO DEFENDANTS

Performance Measures	Required Data Elements
Systemwide Operational Effects	
By category of highest offense charged over specified period of time:	
Pretrial dismissal, diversion, nolle prosequi rates	Number of cases in each category/number of dispositions
Trial rate (total, bench, jury)	Number of trials (total, bench, jury)/number of dispositions in each category
Trial conviction rate (total, bench, jury)	Number of trial convictions (total, bench, jury)/number of trials in each category
Overall conviction rate	Number of convictions/number of dispositions
Sentence severity imposed:	
Percent of guilty pleaders incarcerated	Number of guilty pleaders incarcerated/number of guilty pleaders
Average sentence severity score imposed	Sentence elements by type and amount imposed on guilty pleaders
Delay or elapsed time measures:	
Median arrest to final disposition period Median arraignment to final disposition period	Median elapsed time (days) between these events
Number of victim-hours per disposition	(Number of victim appearances/disposition) × (time per appearance)
Number of other witness-hours per disposition	(Number of witness appearances/disposition) × (time per appearance)
Percentage of juror time in jury selection and trial	Juror time in jury selection and trial ÷ total time (including idleness)
Systemwide Concessions to Defendants	
Sentence concession per guilty pleader	Difference between average sentence severity score of all straight pleaders and of all guilty pleaders
Sentence concession:	Difference between average sentence severity score of all straight pleaders and
Per charge bargainer	Average severity score of all charge bargainers
Per count bargainer	Average severity score of all count bargainers
Per sentence bargainer	Average severity score of all sentence bargainers
Per combination of above	Average severity score of all combinations

measures such as the number of victim and witness hours consumed per disposition and the fraction of time jurors spend in jury selection and trial). Most observers would agree that if delay were reduced by a change in plea bargaining policy this would constitute a gain to the system, as long as the quality of justice was not adversely affected. However, if a change in plea bargaining policy led to a decrease in average sentence severity imposed, there is considerable controversy as to whether this constitutes a system gain or loss.

In contrast are concessions granted to defendants. In the demonstration phase of the study, we used the straight plea sentence as our reference level of punishment in the application of the performance measures shown in Table 4.7.[31] Nevertheless, even when this reference level is used, we believe it is still important for a jurisdiction to determine whether and to what extent imposed sentences differ for similar cases that go to trial if it is to fully appreciate plea bargaining effects particularly because the conventional wisdom holds that the system exacts a penalty in punishment imposed on those who exercise their rights to a trial.

Tables 4.6 and 4.7 display the data elements implied by our choice of measures. A serious limitation to our plea bargaining performance analysis in one demonstration jurisdiction turned out to be the failure of the system to record the occurrence and nature of the principal type of plea bargain in that system, that is, sentence agreements. Without this data element, only a fragmentary picture of plea bargaining performance could be drawn, as discussed in Sec. VII. In the other jurisdiction, the analysis was much more successful because the occurrence of sentence agreements *was* recorded (as well as other types of plea bargains), although the nature was not.

Evenhandedness

Evenhandedness is a quality of court operations[32] that is characterized by the following question: Do defendants in *similar* circumstances fare comparably in criminal proceedings? In devising a statistical approach to this issue area, we sought to classify defendants so that within each class they would be sufficiently similar that the intermediate and final outcomes within a class would not be significantly different. It would then be appropriate, given outcome data in a specific jurisdiction, to examine the differences within a class for significant indications of unevenhandedness.

We first identified various outcomes within a criminal proceeding that might manifest unevenhandedness:

- The proportion of suspects rejected.
- The proportion of suspects charged with at least one felony count.
- The proportion of defendants dismissed (including nol-prossed):
  - Before arraignment.
  - Following arraignment.
- The proportion of defendants making a straight plea.
- The proportion of defendants entering a plea agreement.

[31] The alternative approaches mentioned in the definition of concession given earlier proved to be infeasible because of data availability problems.

[32] Note that lack of evenhandedness in police practices is beyond the scope of this study. For example, police rejection of cases may reflect uneven treatment of suspects before the involvement of the court system.

- The proportion of defendants tried.
- The proportion of defendants acquitted.
- The proportion of defendants convicted in trial of charges less serious than the original charges—by type of trial.
- The magnitude of the Sentence Severity Index:
  For conviction by plea (also separately for straight pleas and plea agreements).
  For conviction by trial.

Next we identified those defendant qualities that appear to justify differences in the outcomes reflected by the above list, for example, rejected or not, charged or not, or dismissed or not. These qualities (which we term "legitimate") would thus serve to classify defendants so that, within each class, the defendants are sufficiently similar that the above outcome measures should not differ significantly (among groups of defendants within a class). On the basis of Rand's earlier work[33] and the views of practitioners interviewed in the course of this study (see Sec. III), we designated the following factors to be the defendant qualities for which differences in outcomes were justified:

Legitimate Factors

- Nature of the criminal conduct:
  Most serious charges (i.e., those for which the most severe punishment could be imposed)
  Strength of the evidence[34]
- Prior record:[35]
  None
  Minor
  Major
  Prison

[33] Reported in Peter W. Greenwood et al., *Prosecution of Adult Felony Defendants in Los Angeles County: A Policy Perspective*, The Rand Corporation, R-1127-DOJ, March 1973.

[34] We treat this factor only in the case auditing substudy of the demonstration phase of this study (see Sec. VIII). It would affect disposition but not (in theory at least) sentencing.

[35] Our definitions, which are simplifications of the classification scheme developed by BCS, are as follows:

1. No prior record: Fewer than three prior arrests and no convictions.
2. Minor prior record: Three or more arrests or some convictions but none imposing more than 90 days of jail time.
3. Major prior record: Any convictions with more than 90 days' jail time or more than two years' probation but no prison time.
4. Prison record: Any prior prison incarceration.

More elaborate categories were used in Greenwood, again based on the BCS scheme (see Table 21, p. 40, of that reference). Prior record could be expressed in terms of specific types of offenses or (as we have done) nonspecifically. For some purposes, for example, analyzing sentence variation, specific-offense prior records would be desirable, but would engender greater complexity both in data collection and analysis. In our demonstration work we used the nonspecific-offense prior record categories.

The fact that a defendant has a prior criminal record may have several different effects at a number of points in a felony proceeding. The police may be prompted to make a more thorough investigation of the offense; the charging may be performed with greater care; the defendant may be more likely to remain in jail during the proceeding because of ineligibility for O.R. release or inability to meet a higher bail requirement; the defendant may elect not to testify because of his vulnerability to impeachment by evidence of his prior record; and the sentencing judge may be strongly influenced by the defendant's history of criminal conduct—all these and other effects could lead to an outcome significantly different from that for a defendant with no prior record.

- Age:
  - Youth
  - Adult
  - Senior
- Sex[36]
- Defendant's community ties, as reflected by an index that includes income, education, transiency, employment status, marital status, and number of dependents[37]

For example, one class of defendants defined by this list of factors would be composed of male youths who were charged with armed robbery, had no prior record, and had weak community ties. We would expect similar defendants to fare comparably in their criminal proceedings, as indicated by the performance measures elected in the first step, if the court system is operating evenhandedly.

Finally, what are the appropriate factors within a class of similar defendants that should be examined to show *lack* of evenhandedness in outcomes? In other words, what defendant-related and other factors are illegitimate or suspect bases for differences in the outcome of criminal proceedings for similar defendants? We identified the following factors:[38]

Illegitimate Factors

- Defendant's ethnicity (in affecting disposition and sentencing).
- Defendant's pretrial custody status (in affecting disposition and sentencing).
- Type of defense counsel (in affecting disposition and sentencing).
- Type of conviction—by trial or straight plea of guilty (in affecting sentencing only).
- Crowding of correctional facilities (in affecting sentencing only).

Most observers would agree that ethnicity alone should not affect dispositional or sentence outcomes under our system of justice. They would also concur that pretrial custody status per se should not affect whether an arrestee is charged or rejected; dismissed, convicted, or acquitted; or (if convicted) given a more or less severe sentence. In practice, pretrial custody status may relate to the defendant's disposition and sentence. One argument holds that, compared with defendants in custody, those who are released are better able to strengthen their defense by locating witnesses. Another contends that defendants held in jail (particularly for offenses in which the probability of receiving a nonincarceration sentence is high) have more incentive to plead guilty, because this cuts short the total (pretrial and

[36] There is a marked division of view on whether "sex" is a justifiable ground for distinction. This dispute is inconsequential for our purposes here because female defendants are relatively infrequent.

[37] Some practitioners would regard "community ties" as being a legitimate basis for differences in sentences (e.g., probation versus incarceration) but not necessarily for other aspects of the proceeding. For a discussion of the community ties index, see App. E, which contains our analysis of data from the demonstration jurisdictions for estimating the relative weights of the "independent variables" listed above in their relationship to the "community-ties dependent variable."

[38] Another factor, "geographical evenhandedness," was investigated in an earlier Rand study (Greenwood) of the performance of the Office of the District Attorney, Los Angeles County, which has numerous branch offices distributed over the County. This factor is not included in the present study because the criminal proceedings in the two demonstration jurisdictions are centralized.

postconviction) incarceration period. In addition, the failure to obtain pretrial release may be a proxy for the viciousness of the offense or the evilness of the defendant (in ways not fully reflected by the actual original charges, the convicted charges, and the defendant's prior record).

Most observers would also agree that whether a defendant has the services of a public defender, a privately retained counsel, or a court-appointed attorney should not influence his disposition or sentence. But in reality, disposition or sentence may relate to the type of defense counsel. One argument is that a wealthier defendant who retains private counsel can provide more resources in building a defense. Another view is that public defenders (compared with the usually less specialized court-appointed or retained counsel) may achieve better results for defendants because they know the court system and its principals better. For example, the public defender may be more skilled at judge-shopping to avoid the more severe sentencers.

Most observers would also agree that, all other things being equal, the type of conviction, whether by trial or straight plea of guilty, should not affect sentence severity. The conventional wisdom, however, is that conviction by trial leads to more severe sentences; that is, the system penalizes defendants who exercise their right to a trial. (However, in our application of measures of evenhandedness in the two demonstration jurisdictions, the analysis of a small sample of trial convictions revealed no penalty in sentence severity for these defendants compared with others who pleaded guilty to the original charges against them.)

Finally, the degree of crowding in correctional facilities should not affect sentences, but in reality, it undoubtedly does. In some jurisdictions where correctional facilities are extremely crowded, judges may feel compelled to impose probation or other nonincarceration sentences on the least dangerous defendants who would have otherwise been incarcerated.

In short, the approach taken by this study in its demonstration phase was to take as indicators of a lack of evenhandedness the direction and magnitude of effects of each illegitimate factor on a set of performance measures of dispositional and sentence outcomes.[39] These effects of illegitimate factors may be estimated by cross-tabulations or by multivariate statistical techniques. The former can indicate only the presence of gross effects. For example, if the straight plea rate of black burglary defendants is 70 percent and that of white burglary defendants is 50 percent, one cannot be sure that all of the difference is due to ethnicity, because some of the difference may be due to other factors. Continuing with this example, if a much larger proportion of the black burglary defendants remain in jail and have public defender representation, the effects of these factors may confound the ethnicity effect. Multivariate statistical techniques, however, permit the identification of the unique or independent effect of each of these factors.

For dispositional evenhandedness, we used three illegitimate factors: ethnicity, pretrial custody status, and type of defense counsel. For sentencing evenhandedness, we added type of disposition (straight plea, trial) to these factors, in the belief that jurisdictions might wish to test the proposition that exercising the right to trial involves a penalty in punishment compared with a straight pleader. We also added

[39] We believe that an examination of the evenhandedness of charging would also have been desirable. Unfortunately, data on defendant characteristics were too sparsely recorded at the screening stage in both demonstration jurisdictions.

degree of crowding in correctional facilities as an illegitimate sentencing evenhandedness factor, even though in practice crowding may lead to lighter sentences.

Table 4.8 gives the performance measures, together with the comparisons that need to be made to assess the effects of the illegitimate variables. (Required data elements are not repeated here since they were indicated in previous tables.)

In the demonstration jurisdictions, we found the principal data element problem in this issue area to be the incompleteness of case files and other data sources in recording defendant ethnicity (and other biographical information).

Sentencing Variation

That sentencing vitally affects the achievement of criminal justice system goals is universally recognized. Notwithstanding, there seems to be scant understanding and agreement about relationships between the type and severity of punishment imposed and how progress toward system goals is made. To say that no generally accepted theories or models exist and that judges tend to accord little weight to accumulated experience in sentencing is not an unfair assessment. This situation obviously reduces the opportunities for useful application of performance measures in the general area of sentencing.

One quality of sentencing activity is, however, relatively visible and readily lends to measurement. This is *sentencing variation*, that is, the sentencing disparities that occur within statutory punishment limits. What is the significance of this variation? Generally, judges have some breadth of discretion in imposing sentence in individual cases. In exercising this discretion, judges concern themselves with *appropriateness* of punishment, which requires that they balance the gravity of the

Table 4.8

MEASURING EVENHANDEDNESS IN DISPOSITIONAL AND SENTENCE OUTCOMES

Performance Measures	Magnitude and Direction of the Illegitimate Factor Effects
By category of highest offense as booked by police and charged by prosecutor over a specified time period:	*(Each of the illegitimate factors must be identified for each case in the sample)*
Dispositional Outcomes	
Rejection rate (based on number screened)	
Nonconviction rate (based on number of dispositions)	Black, Spanish, other, minority vs. majority
Pretrial dismissal rate	Held in jail vs. released on bail vs. released on O.R.
Pretrial diversion rate	
Nolle prosequi rate	
Conviction rate (based on number of dispositions)	Public defender vs. retained counsel vs. court-appointed counsel
By straight plea	
By any plea bargain (gross plea rate)	
By trial (and trial conviction rate)	
Overall	
Sentence Outcomes	
Sentence severity score	*As above but add:*
	Trial conviction (all, bench, jury) vs. straight pleas
	Crowding in correctional facilities

criminal conduct, the characteristics of the convicted defendant, the goals of criminal justice (both locally and nationally), the expectations of the victim and the public, and other factors. At the same time, our system of justice includes an *even-handedness* standard for sentencing, which requires that judges sentence not only rationally but also consistently.

Sentencing disparities are, seemingly, ambiguous. They might reflect an intended and justified adaption of a flexible punishment structure to individual cases. Or they might signify the presence of sentencing flaws, for example, the presence of unjustifiable inconsistencies among sentences or unreasonable interpretations of the appropriateness of punishment.

We undertook the limited task of identifying measures that could reveal sentencing variation in informative ways. Despite the complexity of issues in this area, we believe that sentencing performance measures are useful if they simply reveal in gross terms what the prevailing sentencing practices are in a specific jurisdiction. They then serve to "tip-off" the appearance of abrupt sentencing aberrations or of major changes occurring more gradually over time.[40]

Our task in the issue area of sentencing variation had two aspects: identifying criteria that measure *how much* sentence variation for a specified convicted charge actually exists and *explaining* how much of the total observed variation is accounted for by various factors that should (i.e., legitimately) or should not (i.e., illegitimately) cause the variation. An appropriate approach begins by measuring the proportion of defendants receiving sentences in each of the following categories:[41]

- Probation only
- Rehabilitative program (possibly as a probation condition)
- Community service (possibly as a probation condition)
- Fine only
- Probation and fine
- Jail only
- Jail and fine
- Jail and probation
- Jail, fine, and probation
- Prison

[40] What a jurisdiction should do to remedy undue sentence variation, given statistical evidence of its presence, is far from settled. See, for example, C. A. Korbakes, "Criminal Sentencing: Should the 'Judges' Sound Discretion' Be Explained?" *Judicature*, Vol. 59, No. 4, November 1975, where the author discusses the results of a survey of the chief judges of fifty states by the American Judicature Society. The survey addresses several measures (e.g., stating reasons for sentences in writing, using sentencing panels, providing for appellate review of sentences) aimed at "better" sentencing. Opinions given by these judges were widely and sharply divided on these measures.

[41] In the earlier Rand study of adult felony defendants in Los Angeles County (Greenwood), we also used a measure that is appropriate to a jurisdiction which gives the court the discretion to treat specified offense types as alternate misdemeanor/felony offenses, namely:

> The proportion of defendants receiving sentences at each of the following levels of conviction/sentence for a specified most serious charge:
>
> Convicted of felony as charged; received felony sentence.
>
> Convicted of lesser felony; received felony sentence.
>
> Convicted of felony as charged; received misdemeanor sentence.
>
> Convicted of lesser felony; received misdemeanor sentence.
>
> Convicted of misdemeanor (although charged with felony).

These sentence categories should be broken down by magnitude classes of their elements. This can be done for each major category of *convicted* offense, if one believes that the convicted charge most accurately reflects the criminal conduct of defendants. Or it can be done for each major category of highest *charged* offense, given the belief that the latter most accurately reflects the criminal conduct. This provides a gross statistical picture of the distribution of sentences, that is, sentence variation. But to facilitate further analysis, each of the sentences can be assigned a corresponding sentence severity score (or a set of such scores for alternative Sentence Severity Indices, as discussed earlier within the issue area of plea bargaining). The result would then be a sentence distribution in the form of the proportion of defendants receiving sentences that correspond to each specified interval of scores of a stated Sentence Severity Index. A useful statistical measure of how much variation is reflected by this distribution of sentences (for a category of convicted offense or else of highest charged offense) is the so-called coefficient of variation, namely, the ratio of the standard deviation of the sentence severity scores divided by the average sentence severity score.

Sentences vary for many reasons.[42] As we noted above in the discussion of evenhandedness, some factors accounting for variation are acceptable (or legitimate); they may include the defendant's age, prior record, an index of community ties (including employment, education, family status, transiency), the nature and number of charges against the defendant, and judges' sentencing philosophies. Other factors are not so acceptable (or illegitimate); that is, most observers would agree that they should *not* affect sentencing. These may include defendant race or ethnicity, pretrial custody status, type of defense counsel, method of conviction (guilty plea, trial), and changes over time in the crowding of correctional facilities. By applying standard multivariate statistical techniques, it is possible to determine how much of the explained variation is accounted for by each of these factors (with the exception of judicial philosophy). (Data on all but the latter factor are normally recorded or could be easily recorded in agency files. Data on judges' sentencing philosophies would be very difficult to quantify.) The remaining unexplained variation is then attributable to factors that could not be identified or measured or to random effects. Sections VI and VII illustrate, for the demonstration jurisdictions, how this approach is applied to measure how much sentence variation is present and how much of this variation can be accounted for by various factors.

MEASURING LAY PARTICIPANT ATTITUDES AND THEIR DETERMINANTS

The attitudes of jurors, victims, other witnesses, and defendants toward the

[42] One important study on unevenhandedness was submitted by LEAA's National Institute for Law Enforcement and Criminal Justice in June 1972 to the U.S. Senate Subcommittee on Criminal Law and Procedure (McClellan Subcommittee). The Institute, reporting on sentencing variations in the federal courts during the four-year period 1967-1970, concluded that, among other findings, there was a significant lack of evenhandedness in sentence length according to the race of the defendant. Another such study was the "1972 Sentencing Study, Southern District of New York," unpublished report, compiled May-October 1972 by the Office of the U.S. Attorney, SDNY (published in part in the *American Criminal Law Review*, Vol. 11, No. 4, Summer 1973, pp. 826-834). The latter study found that during the six-month period covered there was a significant disparity in sentences (both as to rate and to length of incarceration) among classes of offenses, with "white-collar crime" being treated much more leniently than other types of theft, not only in the Southern District of New York but also in all federal courts.

court system and its practitioners is the last broad issue area we considered. These attitudes are important because they affect the atmosphere in which practitioners function and can even affect their decisions (e.g., a judge's decisions on sentencing often mirror the community's views, and, more particularly, the views of lay participants). Then, too, the attitudes of jurors and witnesses can have direct operational consequences for court systems if they affect their willingness to cooperate in felony proceedings. Attitudinal data of this sort are not normally collected, even occasionally, in local jurisdictions. Thus, we were led to the use of mail surveys of victims, witnesses, and jurors, and personal interviews, using a structured questionnaire, with defendants. The mail and personal interview questionnaires we developed seemed to have worked fairly well; they are reproduced in Apps. G and I, respectively, for jurisdictions that wish to consider their use. (As an alternative to mail questionnaires, jurisdictions may use an exit questionnaire for jurors, victims, and other witnesses.)

For the first three classes of lay participants (victims, witnesses, jurors), we found that questions dealing with the following topics adequately captured the attitudes of interest:

- Overall opinion of the court system (both before and after lay participants' experiences).
- Whether justice was done in the case(s) in which they were involved.
- Their degree of satisfaction with their treatment and experience.
- Their attitudes toward the practitioners they dealt with.
- Their willingness to cooperate again in the future.
- Whether their understanding of the system increased as a result of their experience.

The hypothesized determinants of these attitudes include lay participants' background characteristics (age, sex, ethnicity, income, employment, etc.), the characteristics and outcome of the case they were involved in, the problems they encountered in serving, and the treatment they received by the court system. Standard statistical techniques (such as correlational analysis and multivariate regression analysis) should be applied to determine the relationships that do or do not exist between these "independent" factors and their professed attitudes. In particular, jurisdictions should focus on analyzing *policy* factors that could be manipulated to improve attitudes. These include reducing the number of problems encountered (such as parking, transportation, idleness, lack of facilities, and concern for their safety in the court building); improving the system of notifying and transporting (if applicable) witnesses when to appear and informing them of reschedulings; informing them of case outcomes; treating them with courtesy and respect; and instilling a feeling that their participation helped bring about justice.

For defendants, we found that questions dealing with the following topics adequately captured the attitude of interest:

- Perceived fairness of procedures and outcome in their case.
- Their attitudes toward prosecutors, judges, and the court system.
- Their attitudes toward and perceptions of defense counsel.
- Their attitudes toward plea bargaining.

Possible determinants of these attitudes include defendant background characteristics (including prior record), type of offense in the present case, type of disposition and nature of sentence (if any) imposed, type of defense counsel, and circumstances of the case and defendant.

Tables 4.9 and 4.10 classify questions in the lay participant questionnaires (see App. G) and the defendant questionnaire (see App. I), respectively, according to the attitudes of the respondents and the determinants of those attitudes. These questions in effect correspond to data elements presented in earlier tables giving performance measures and data elements for other issue areas.

Table 4.9

CLASSIFICATION OF QUESTIONS IN LAY PARTICIPANT QUESTIONNAIRES ACCORDING TO ATTITUDES AND DETERMINANTS OF ATTITUDES
(Questionnaires are contained in App.G; entries here are question identifiers)

	Lay Participants		
Category	Jurors	Victims	Other Witnesses
Attitudes (dependent variables)			
Overall opinion of court system after present experience?	A2	A2	A2
Was justice done in cases participated in?	B16	B21	B22
Degree of satisfaction with treatment and experience?	B15	B31	B32
Attitudes toward practitioners dealt with?	B9	B14	B15
Willingness to cooperate in the future?	B17	B32,B33	B33
Increased understanding resulting from present experience?	B11	B29	B30
Determinants of attitudes (independent variables)			
Background characteristics	A1,A3,B10, C1—C3, D1—D7	A1,A3,B5,B17, B23—B26,B28, C1—C3, D1—D7	A1,A3,B3,B4,B6, B24—B27,B29, C1—C3, D1—D7
Case characteristics and outcome	B1,B2 B3,B12	B1,B2,B11,B12, B13,B16,B19,B30,	B1,B2,B12,B13, B14,B19,B31
Problems encountered	B8	B4,B6,B7,B8,B9, B10	B5,B7,B8,B9,B10, B11
Treatment received	B13	B3,B22	B23

Table 4.10

CLASSIFICATION OF QUESTIONS IN DEFENDANT QUESTIONNAIRE ACCORDING TO ATTITUDES AND DETERMINANTS OF ATTITUDES
(Questionnaire is contained in App. I; entries here are question numbers)

Category	Question Number
Attitudes (dependent variables)	
Perceived fairness of procedures and outcome in their case?	4,5,6,7,15,16,36,37,38,41,42,43,44,46
Attitudes toward prosecutors, judges, and the court system?	56,57,58,59,75,76
Attitudes toward and perceptions of defense counsel?	52,53,54,55,59
Attitudes toward plea bargaining?	27,28,29,77
Determinants of attitudes (independent variables)	
Background characteristics	1,60,61,62,63,64,65,66,68,69,70,71,72, 73,74,78,79,80,81,82,83
Type of offense	2,3
Type of disposition	13,14,17,25,33
Nature of sentence	34,35
Type of defense counsel	49
Circumstances of the case and defendant	8,9,10,11,12,18,19,20,21,22,24,26,30,31, 32,33,39,40,45,47,48,50,51

GAUGING OVERALL PERFORMANCE

We have, as declared at the opening of this section, restricted our treatment to the selection, rationale, and limitations of statistical performance measures in clarifying several salient issue areas within criminal proceedings. Our intent was not to consider performance measures as routine informational devices in support of day-to-day management and control of court systems, but much of what has been presented here could be recast in such a context.

Neither have we undertaken to gauge the "overall" performance of a court system. The scope of this study was not overall in breadth. In some jurisdictions, the selected issue areas might encompass most operational problems being encountered; in others, the coverage could be significantly incomplete. In some jurisdictions, performance in these issue areas might be highly interrelated; in others, markedly independent.

Nevertheless, for completeness, we should touch on the question of objective approaches to the *aggregate interpretation* of an entire battery of performance measures (and sets of performance measures) used in a jurisdiction to clarify various aspects of criminal justice operations. We shall confine our comments to two possible approaches, neither of which appears to be a satisfying solution to this often felt

need of characterizing, in some overall sense, the criminal justice posture of a specific jurisdiction at some given point in time.

One approach might be the construction of an aggregate performance measure out of separate indices of performance in selected areas of criminal justice activity. Of course, this approach immediately confronts us with a multitude of "apples and oranges" issues, the resolution of which tends to be strongly subjective. What activities within the process should be selected for inclusion in the aggregate measure? How may they be made commensurate? And, most difficult of all, how much importance should be attributed to one activity compared with another? For example, what weight should case processing efficiency be given relative to charging accuracy in the construction of an aggregate performance measure? Such dilemmas might be resolved by statements of position by responsible policymakers. Or they might be circumvented by the use of a small set of alternatives, spanning reasonable differences of opinion. But, in practice, a single composite measure of performance for a complex process serving a multitude of (sometimes conflicting) objectives would not gain acceptance. The criminal justice community shows a clear preference for retaining the separate identities of different functions and agencies and for integrating their performance (if any integration is attempted) subjectively according to the evaluator's view of their relative contributions.

In the rare case where *clear* changes are observed over time in performance measures (for, say, the better) in *all* or *almost all* of the issue areas of interest, it is then possible to draw a *qualitative* inference that overall performance changed for the better. Our application of performance measures in one of the two jurisdictions uncovered such a case.

Another approach to obtaining a collective interpretation of a battery of performance measures might be to compare their magnitudes with "standards" that are implied by various descriptive models of the criminal justice process, for example, Packer's Due Process and Crime Control Models, Goldstein's Inquisitorial and Accusatorial/Adversarial Models, and Davis's Administrative Model. Conceptually, one could observe changes in magnitude occurring over time in a battery of performance measures in a jurisdiction and then infer, if possible, the presence of trends toward one or another of the descriptive models. But there are serious difficulties, both conceptual and practical, in implementing this approach, as we learned in unsuccessfully attempting to apply it in the present study.

V. THE DEMONSTRATION JURISDICTIONS: SELECTION AND DESCRIPTION

Marvin Lavin and Sorrel Wildhorn

THE SELECTION CRITERIA

Our study, even at its earliest formulation, was not envisaged as a purely theoretical investigation of statistical performance measures. Rather, it has sought a fuller understanding of how the improved application of performance measures might contribute to the betterment of criminal proceedings, as well as to a deeper appreciation of practical impediments to such use. Given this study orientation, our analyses seemed to be most appropriately performed in the context of actual criminal court systems. We felt that a single jurisdiction would not suffice for this purpose, for criminal court systems are far too variable from one jurisdiction to another. But resource limitations on our study program limited field work of substantial depth to only two jurisdictions.

If we were to work in only two jurisdictions, we felt that they should be widely separated geographically and markedly contrasting in their treatment of criminal proceedings. Also, it was desirable that neither jurisdiction be so large nor so overburdened by its caseload as to be highly atypical. Nor was it thought desirable to select a small jurisdiction with a very light caseload, since the small number of cases would then make less interesting the application of statistical techniques. Small jurisdictions also inevitably generate small case samples which would make it difficult to explain why certain performance measures vary and which factors most importantly affect that variation. Within these bounds we sought to select two jurisdictions that were more typical of medium to large (in terms of population, crime rate, and court caseload) urbanized felony court systems.

A further, exceedingly vital criterion was the quality of the data sources at our disposal. Since we would be drawing most of our data from closed case files and related records, it was essential that these materials be reasonably complete, accurate, and consistent. Still another consideration was that the existence of a recent court experiment or substantial policy change would facilitate our demonstration objective, in that it would show how performance measures could help to illuminate the consequences of system changes. Finally, a necessary but not sufficient selection factor was simply the receptiveness of the jurisdiction to our presence—would the bench, the office of the prosecutor, and the office of the public defender all join in extending permission and assistance to our delving into their case-related files?

Given these criteria, the soundest approach to selecting two demonstration jurisdictions would have entailed in situ investigations of a number of candidates (including various possibilities suggested by our Advisory Group). But such preliminaries were plainly too costly. Instead, we conducted the practitioner interviews described in Sec. III with the additional objective of exploring the choice of two from among the 13 jurisdictions visited. As it turned out, Multnomah County, Oregon (containing the City of Portland) and Dade County, Florida (containing the City of

Miami) together appeared to meet our selection criteria, and we subsequently arranged for them to be the subjects of our demonstration analyses.

Below, we present concise overviews of the criminal justice process in the two counties, emphasizing the aspects most related to our studies.[1] These overviews will serve to contrast the two jurisdictions without comparing their performance per se in criminal proceedings. Statistical overviews based on data samples that we ourselves collected from case files are given in Secs. VI, VII, and IX.

THE FELONY COURT SYSTEM IN MULTNOMAH COUNTY, OREGON

The Flow and Timing in Felony Cases

Arrests in Multnomah County are made primarily by the Police Bureau of the City of Portland, a force of 700 officers, and the Sheriff's Office, a 250-officer force. A small proportion of the felony arrests in the county are produced by the state police and the Police Department of the City of Gresham. A felony suspect, within 24 hours after his arrest (unless a weekend intervenes), makes his first appearance in district court, the lower court of a two-tiered system. Within five days of arrest, a preliminary hearing[2] and bind-over (if any) will have occurred in district court. Until recently, all but the most routine cases would then have proceeded by way of grand jury indictment (if any) to arraignment in circuit court. In 1975, however, a markedly increased use of informations began,[3] whereby the case moved into circuit court without indictment. On the first-mentioned route the defendant would, by approximately the twentififth day after arrest, have been arraigned in circuit court, have entered a plea of not guilty, and have been assigned dates for a pretrial conference and a trial; on the second route, which omits the grand jury hearing, a circuit court arraignment would generally ensue within two weeks of arrest. Another alternative requires the return of a (secret) indictment by the grand jury prior to an arrest, which is then made under warrant. In this sequence of events, the circuit court arraignment usually comes within three days after the defendant has entered custody. A pretrial conference would customarily be held within two weeks after arraignment and important pretrial motions would be filed and heard a few (three to five) days later. Elapsed time between the conference and the beginning of trial would be roughly three to four weeks; between the end of trial and sentencing, about 30 days.

There is no formal pretrial intervention (diversion) program in Multnomah County.

Oregon has a statutory requirement that a defendant be tried no more than 60 days after his arrest if he is in custody.[4] Motions for setover (continuance) of the trial

[1] This background information was provided by personnel of the two systems, by internal and external publications of their various agencies, and by our observations during data collection and case auditing.

[2] In Multnomah County the preliminary hearing is relatively informal. No court reporter is present, but the use of a cassette recorder has recently been initiated.

[3] An information is a formal accusation of a crime made by a prosecuting officer as distinguished from an indictment presented by a grand jury. The marked increase in their use was a result of a change in the Oregon constitution and statutes.

[4] In Multnomah County, the circuit court judges, by philosophy and policy, attempt to meet this requirement in all felony cases, whether or not the defendant is in custody.

date are readily granted provided the new date requested is within this statutory period and does not cause a loss of witnesses to the other side. Otherwise, a full-scale hearing is required by the court to rule on the motion. If defense counsel is the moving party, he must show that the defendant's rights will be prejudiced unless the setover is granted. If the prosecutor is the moving party, he must justify his request by showing that it is necessitated by unforeseeable events beyond his control. That the two sides join in asking the delay is of itself given no weight. For the past several years, the time from first arrest to beginning of trial for felony cases in the circuit court has generally been meeting the statutory standard, but not all individual cases do so.[5] (See Table 5.1.)

The Multnomah County Circuit Court

Organization and Calendaring. Judges are elected for six-year terms on a nonpartisan basis. The presiding judge is chosen by the judges of the court and serves two successive one-year terms. He hears all pretrial civil matters and assigns all civil and criminal trials. The chief criminal court, presided over by a circuit court judge for a term of three to four months, was established in its present form in 1972. The chief criminal judge conducts recognizance and bail hearings, hears motions to dismiss and all dispositive motions, conducts trials on stipulated facts, receives all pleas except those taken in the course of a trial, and generally sentences pleas of guilty. The 11 trial judges conduct both civil and criminal trials, hearings on trial motions, and sentencing hearings after a judgment of guilty in a trial or a plea of guilty taken in the course of trial. A trial judge is assigned to a criminal case on the day preceding the trial date by a random-assignment method that virtually precludes "judge-shopping."[6] And there is a judicial policy in opposition to allowing a defendant to appear in trial court for the purpose of pleading guilty and being sentenced there rather than in chief criminal court. Table 5.1 conveys an impression of the scale of criminal case activity in the circuit court.

Juror Handling. The term of jury duty is four to five weeks. A pool of 200 veniremen is formed by calling about 800 candidates. A panel of 20 veniremen is sent from the pool to a criminal courtroom for the voir dire examination. In the selection of the 12-person jury, defense counsel is accorded six peremptory challenges and the prosecutor has three, except in capital cases wherein each side has double this allowance.

Witness Handling. Each side is responsible for the service of subpoenas on its own witnesses, who are directed to appear at a specified date and time. Typically, the attorneys in criminal cases require their witnesses to appear at the beginning of either a morning or an afternoon session and then wait for their case to be called. They feel that this procedure does not unduly extend a witness's commitment of time.

Court Involvement in Plea Bargaining. As a matter of judicial policy current in mid-1975, the chief criminal judge eschews involvement in plea negotiation. Infrequently the parties may confer with the court to obtain his views on possible

[5] A marked increase in case intake in 1975 may significantly elevate the percentage of cases failing to meet the 60-day standard in the current year.

[6] Either defense counsel or the prosecutor could cause the transfer of a case (but no more than twice) by "affidaviting" the judge for prejudice—thus, there is potential for judge-shopping by this means. It is infrequently done because of its abrasive nature.

Table 5.1

STATISTICAL INFORMATION PROVIDED BY THE CIRCUIT COURT OF MULTNOMAH COUNTY
(Criminal proceedings)

Item	1972	1973	1974	January-April 1975	April 1975
Total criminal case intake	2897	2730	2869	1341	369
Felony arraignments	2412	2037	2317	1144	303
District court appeals	485	693	552	197	66
Average monthly case intake	241	227	239	335	369
Felony arraignments	201	170	193	286	303
District court appeals	40	57	46	49	66
Average monthly pleas of guilty	183	197	224	248	262
Average monthly trials (verdict)	60	50	38	40	33
Circuit court cases	42	29	25	25	18
District court appeal cases	18	21	13	15	15
Elapsed days from arrest to beginning of trial, circuit court cases					
Average	58	46	54	--	61
Median	44	41	52	--	60

SOURCE: May 15, 1975, internal memorandum from the chief criminal judge.

sentencing solutions in a specific case but no sentencing commitment will be made. A negotiated plea of guilty tends to be the object of judicial inquiry when a very serious crime or a very substantial reduction in charges is involved; otherwise, the court is mainly concerned with the voluntariness of the plea. Plea agreements must be committed to writing.

Sentencing. Statutory maxima for imprisonment and fines are set under the Oregon Criminal Code, revised in 1971. Nearly all felonies are now classified as one of three types (A, B, or C) with corresponding maxima being specified.[7] (At the court's discretion, Class C felonies may be regarded as an alternate felony/misdemeanor type.) The sentencing judge may impose a maximum sentence less than the statutory limit. The choice between consecutive and concurrent sentences in the event of multiple convictions rests within his discretion, with a presumption of concurrency if not otherwise specified. Written pre-sentence reports are not prepared for all defendants, so sentencing without this report does occur. An informal, advisory-only sentencing panel consisting of current, past, and prospective chief criminal judges has been used in Multnomah County for plea-of-guilty cases, which comprise 75 to 85 percent of the total to be sentenced. Sentencing in "Impact" crime cases has been a particular focus of the panels.[8] But at least one judge who presided over the chief criminal court did not elect to use the panel arrangement.

[7] Class A felonies incur a maximum prison term of 20 years and a maximum fine of $2500; Class B, 10 years and $2500; Class C, 5 years and $2500.

[8] Within the "Impact" program, the district attorney's office has received a grant to implement a No Plea Negotiation Experiment for three crimes—dwelling burglaries, serious robberies, and fencing (selling stolen property).

Bail and O.R. Practices. Subsequent to January 1974, an arrestee may gain release from custody by posting with the court 10 percent of the bail set for his case. He ultimately recovers 90 percent of the posted amount by making his required appearances.[9] This procedure has virtually eliminated bondsmen. A prefixed bail schedule applies to some felonies (and to misdemeanors in general). Most pretrial release is, however, on the basis of own recognizance (O.R.). The arrested suspect is interviewed in jail by the O.R. staff. If he does not qualify for O.R. release, he is so informed at his first appearance in district court, where his bail is set or reviewed. Some arrestees avoid O.R. interviews by posting bail at the time of police booking, provided the prefixed bail schedule is applicable.

Office of the Multnomah County District Attorney

Size and Workload. The office serves as the prosecutor of both felony and misdemeanor cases arising in Multnomah County; such cases comprise about 45 percent of the state's criminal cases. Currently, its caseload is composed of roughly 2500 felonies and upwards of 6000 misdemeanors per year, DUIL (drunk driving) and domestic relations cases each numbering into the thousands, district court appeals and extradition cases each numbering into the hundreds, and a variety of juvenile court, consumer protection, and negligent homicide matters. This caseload is borne by a staff of nearly 50 attorneys, four interns, and 75 support personnel.

Organization. To handle its felony caseload, which is our concern here, the office of the district attorney divides its duties broadly as follows. The responsibilities of a district court (lower court) prosecutorial staff include the preliminary hearings to which felony cases move after they are initially screened and a complaint issued.[10] An Intake Unit, attached to the district court, draws up complaints, obtains arrest warrants, reviews search warrants, prepares evidence in direct presentation cases, deals with citizens' complaints, and so forth. A Pretrial Unit has the task of presenting evidence to grand juries, both for direct cases and those moving from preliminary hearing along the grand jury indictment route rather than the information route. This unit also argues in opposition to pretrial motions by defense counsel and handles extradition and appeals. There are four trial teams in the circuit court Trial Unit which take cases as they move from the Intake and Pretrial Units, each being assigned a broad offense category. Team A is assigned homicides, rapes, and serious assaults; Team B is assigned felony drug and vice cases; Team D is responsible for "non-Impact" burglaries and robberies; and Team E handles fraud, bad check, and welfare offenses. These teams are involved in the pretrial conference, plea negotiations, motion hearings, and trial. However, only the team heads have authority to enter into a plea agreement.

The fifth team, Team C, is unique, for it has responsibility for cases from intake to disposition when they involve the three Impact crimes of residential burglary, serious robbery, or fencing. This team is the core of the district attorney's No Plea Negotiation Experiment begun in December 1973. (See Sec. VI for a description of this project.)

[9] The court administrator estimates the current "skip" rate to be in the neighborhood of 5 percent, lower than it was before January 1974.

[10] Felony cases in which the suspect is not in custody may be presented directly to the grand jury for indictment. If an indictment is returned, an arrest warrant will be issued. Once the defendant is in custody, he is arraigned immediately in circuit court; thus, no preliminary hearing occurs in these cases.

The scale of the felony (and district court appeals) case activity of the office of the district attorney is suggested by the data provided by them and presented in Table 5.2.

Policy Considerations. Prosecutorial policy about the intake and prosecution of cases related to the No Plea Negotiation Experiment is discussed in Sec. VI. For other cases, no standard filing policies guide the Intake Unit.[11] Nevertheless, it is impractical for the police to exploit the absence of standardized filing standards to their advantage since one deputy has a nearly exclusive assignment of the felony screening function.

Three types of concessions are made in return for pleas of guilty: to reduce charges (the most frequent type), to dismiss pending charges in other unrelated cases or not to file these other charges, or to recommend a (relatively favorable) sentence or not to oppose a defense-recommended one. No standard policy governs how much will be offered to effect an agreement. The office seeks to dispose of cases at the pretrial conference but also remains "willing to try cases."

With one exception, there is no uniform policy concerning whether or not a deputy should make a sentencing recommendation after obtaining a conviction.[12] It is generally left to his discretion to make a recommendation at the probation and sentencing hearing.

The office posture toward cooperation with defense counsel is substantially shaped by Oregon's liberal discovery statutes, which give the defendant full access to police reports, witnesses, physical evidence, and other relevant items.

The Public Defender's Office: The Metropolitan Public Defender

Size and Workload. The Metropolitan Public Defender, a private nonprofit corporation, provides (under contract) legal defense services to indigent defendants in Multnomah County and, to a lesser extent, in neighboring Washington County and in the federal district court. Multnomah County funds defense counsel (public defenders plus court-appointed counsel) in roughly 60 percent of the felony cases and 20 percent of the misdemeanor cases (plus a small percentage of the traffic cases). The Metropolitan Public Defender handles about 45 percent of all felony cases and about 10 percent of all misdemeanors (excluding traffic). Twelve attorneys are assigned to Multnomah cases. Eight are assigned to felonies, three to misdemeanors, and one is a "swing" attorney to even out the workload. The annual total of felony cases handled by the eight attorneys is about 1600 (i.e., 200 cases per attorney) of which approximately 5 percent go to trial. (The misdemeanor caseload per attorney is about twice as large.) The public defenders take cases on first-come first-served basis up to the point where the quota is fulfilled each month.

Organization. The eight attorneys assigned to Multnomah felony cases are divided into four two-man teams, each consisting of a senior and a junior attorney. A team defends a case from appointment to disposition.[13] A blind case assignment system is used, with each team drawing appointments that are made initially in

[11] One exception is the consistent policy of discouraging domestic-relations complaints.

[12] There is now a policy of recommending restitution to the victim in every case where appropriate.

[13] If the defendant appeals, the Oregon State Public Defender's Office assumes responsibility for his representation. On the other hand, if the district attorney appeals a dismissal (say, after a successful defense motion to suppress essential evidence), then the Metropolitan Public Defender team will continue its representation.

Table 5.2

Statistical Information Provided by the District Attorney's Office of Multnomah County

Type of Activity	1971	1972	1973	1974
Number of indictments	2264	2340	2247	2428
Number of defendants indicted	2313	2429	2280	2368
Number of informations	221	86	169	127
Number of district court appeals	178	405	459	401
Number of defendants convicted either by trial or plea of guilty	2164	1982	1765	1711
Total number of defendants tried	445	650	428	429
Number tried by jury	236	294	194	215
Number tried by court	209	356	234	214
Total number of defendants convicted by trial	336	485	316	339
Number convicted by jury trial	180	207	130	153
Number convicted by court trial	156	278	186	186
Percent judgments of guilty after trial	75.5	74.9	73.3	79.0
Defendants convicted, as percent of defendants who were tried or pled guilty[a]	95.2	92.4	94.0	95.0
Defendants disposed of by trial, as percent of defendants who were tried or pled guilty	19.6	30.3	23.0	24.0

Felony Case Screening

Type of Activity	Jan. 75	Feb. 75	Mar. 75	Apr. 75	May 75
Number of complaints issued	182	142	167	154	144
Number of cases with "No Complaint"	93	81	97	87	58
Percent rejected	33.4	36.3	36.7	36.1	28.7
Direct presentations to grand jury:					
Number of cases logged in	69	81	83	112	103
Number of cases sent to GJ	59	55	51	62	58
Number of cases referred back	--	26	34	50	35
Number of cases pending	20	20	19	20	28
Cases referred back, as percent of cases either sent to GJ or referred back to source	0	32.1	40.0	44.6	37.6

High Impact Project: Dec. 1973 to Dec. 1974 (13 months)

Cases tried	56
Guilty	39
Not guilty	12
Not guilty/insanity	5
Cases pending (Dec. 1974)	51
Cases pled to charge	172
Cases pled to lesser charge	4
Cases returned to police	149
Cases dismissed	46
Cases not indicted by grand jury	14

SOURCE: Office of the District Attorney, internal memoranda and 1974 year-end report.

[a]Does not reflect dismissals or other terminations of prosecution.

circuit court one week a month, appointments that are made initially in district court two weeks a month, and no new cases during the fourth week. An unusually important case appointment might be assigned by the public defender himself outside of the regular assignment system. A defender team has the support of an "alternatives" staff, which is familiar with existing community programs for offenders and which helps to organize new ones; of trial assistants, who perform initial interviews with defendants, retrieve their property from the police, handle subpoenas, and maintain trial books; and of an investigations unit. Table 5.3 shows the nature of the felony case activities of the Metropolitan Public Defender.

Defense Counsel Compensation. Attorneys of the Metropolitan Public Defender are compensated by salary. By contrast, court-appointed private attorneys are paid a fee per appearance, with the amount depending on the type of appearance. Defendant-retained private attorneys are generally remunerated by a fee per case.

Oregon Corrections Background

A felon, if not classified a dangerous offender, is eligible for parole six months after admission to prison. The parole authorities set reviews at a time slightly before one-third of the imposed maximum time has been served. Consequently, parole is frequently granted after the felon has served one-third of the imposed maximum term. According to the State Department of Corrections, the typical Oregon felon actually serves roughly 33 percent of the maximum prison sentence imposed by the court.

The State Department of Corrections provides local probation and parole services. Probation could be either under court supervision or state supervision, at the discretion of the sentencing judge. Incarceration for younger offenders is generally at the Oregon Correctional Institution; for older offenders and the most serious offenders, regardless of age, it is at the state prison.

THE FELONY COURT SYSTEM IN DADE COUNTY, FLORIDA

The Flow and Timing in Felony Cases

Most felony arrests in Dade County are made by officers of the Police Department of the City of Miami and the Public Safety Department of the County, which respectively number about 750 and 1200 sworn personnel. Provided the arrestee does not immediately post bond (according to a prefixed schedule that does not include capital offenses, which are nonbailable), he is given a bond hearing in the magistrate's division of the county court generally within 24 hours. This is a nonadversarial hearing in which the magistrate purports to make a probable-cause determination on the basis of the police affidavit and sometimes, if necessary, the arresting officer's testimony. The affidavit may be prepared after arrest or, in some instances, before arrest to enable the issuance of an arrest warrant.

Most felony cases (perhaps 80 percent) then have a second lower-court hearing to determine whether or not the suspect should be bound over to the circuit court. Prior to April 1975, this was in the nature of an adversarial (though informal) preliminary hearing. Currently, as one of the procedural changes engendered by the

Table 5.3

Statistical Information on Felony Cases Provided by the Metropolitan Public Defender, Multnomah County[a]

Type of Activity	May 1975		July 1974 to May 1975	
Case Flow	Number		Number	
Open at start	363		274	
Refiled	10		66	
Appointed during period	193		1796	
Total open during period	566		2136	
Closed during period	207		1777	
Open at end	359		359	
Case Closings	Number	Percent	Number	Percent
Total closed	207	100.0	1777	100.0
Adjudicated	155	74.9	1272	71.6
To probation/extradition	31	15.0	281	15.8
To other defense counsel	11	5.3	142	8.0
Retained private counsel	3	1.4	35	2.0
Appointed private counsel	8	3.9	107	6.0
Bench warrant issued	10	4.8	82	4.6
Adjudications	Number	Percent	Number	Percent
Total cases adjudicated	155	100.0	1272	100.0
Total not convicted	56	36.1	543	42.7
Dismissed	48	31.0	504	39.6
Not guilty/jury trial	1	0.6	14	1.1
Not guilty/bench trial	--	--	1	0.7
Not guilty/insane	7	4.5	24	1.9
Total convicted	99	63.9	729	57.3
Plea as charged	43	27.7	308	24.2
Plea to lesser felony	43	27.7	207	16.3
Plea to misdemeanor	10	6.4	151	11.9
Guilty/jury trial	2	1.3	51	4.0
Guilty/bench trial	1	0.6	12	0.9
Trials	Number	Percent	Number	Percent
Total number of trials	11		102	
Percent of adjudications by trial		7.1		8.0
Win rate (not guilty/guilty)				
Jury trials		33.3		21.5
Bench trials		--		7.7
Sentences	Number	Percent	Number	Percent
Total number of sentences	99	100.0	729	100.0
No jail after sentence	72	72.7	489	67.1
Jail after sentence	27	27.3	240	32.9
Penitentiary	11	11.1	114	15.6

SOURCE: Office of the Metropolitan Public Defender, internal statistical reports.

U.S. Supreme Court's decision of *Gerstein v. Pugh*, 43 L. Ed. 2d 54 (1975), a nonadversarial probable-cause hearing is conducted.[14] If the suspect is bound over to circuit court, he is arraigned in that forum within one week if in custody; within two weeks, otherwise. In other cases (roughly 5 percent of the total), the state attorney's office may directly file an information in circuit court without further proceedings in county court (other than the bail hearing, if any). In the remaining cases, the grand jury will be used, either to return an indictment after the arrest and bail hearing, followed by arraignment in circuit court if the indictment is returned; or to indict prior to arrest, whereupon the defendant is arrested pursuant to a warrant and then directly arraigned in circuit court. The state attorney's office has authority to "no action" a case before the probable-cause (preliminary) hearing or to "no information" it after a bind-over. Furthermore, it can file an information even though the magistrate does not hold the suspect to answer. However, these powers are not frequently exercised.

There is considerable employment of nolle prosequi (perhaps 15 to 20 percent of the cases) once felony cases have been filed in circuit court.[15] Continuances of trial date are common, usually justified by schedule conflicts and discovery needs. Most cases are set for trial from 30 days to six weeks after arraignment, but trials do not often occur before 60 days. A court-imposed speedy trial rule of 180 days between arrest and trial applies in Florida. In addition, a defendant may demand a trial within 60 days after the information is filed in circuit court. Both of these rules can be waived by the defendant or, under exceptional circumstances, disregarded by the court.

The Criminal Division of the Circuit Court

Organization and Calendaring. Judges sit in the circuit court by virtue of nonpartisan election to six-year terms. Prior to October 1974, a system of individual calendaring applied to all 10 criminal trial courts. Subsequent to that date, five trial courts retained individual calendaring with "blind" assignment of cases to them and thenceforth responsibility for all aspects of a case; the remaining five courts were organized into an experimental master calendar system, with a master calendar court handling arraignments, pretrial motions, and other pretrial matters and with four trial courts being assigned trials on a "blind" basis. Currently two courts that are usually civil have been added to the criminal division to help alleviate caseload pressures; the total number may soon rise to 14. Further experimentation with master calendar arrangements is planned. Table 5.4 displays descriptive statistics that suggest the scale of felony proceedings in Dade County.

Juror Handling. Except for capital cases, criminal juries in Florida have six members (plus an alternate). In the circuit court, the practice is to call a panel of 15 veniremen to the courtroom for jury selection, each side being allowed six peremptory challenges. Twelve-person juries sit on capital cases, with each side being

[14] Under the revised procedures, an adversarial preliminary hearing is available before the administrative judge of the magistrate's division but only after bind-over and on defense motion. On the other hand, the magistrate has discretion to permit the probable-cause hearing to be adversarial.

[15] We were informed that most nolle prosequi entries occur as a result of restitution made in worthless check cases in which the complainant no longer wishes to prosecute. Another frequent reason is the admission of the defendant into the Pretrial Intervention Program. (In October 1975, 174 cases were nol-prossed, of which 72 were worthless check cases and 42 were diversions.)

Table 5.4

STATISTICAL INFORMATION PROVIDED BY THE CRIMINAL DIVISION OF DADE COUNTY CIRCUIT COURT

Felony Proceedings	1974			January–July 1975		
Felonies filed	11,000			7,000		
Cases closed	9,600			5,000		
Cases open at end of period	3,400			3,000		
Disposition of closed cases:						
Probation without adjudication	2,150	(22%)		1,200	(24%)	
Convicted	3,450	(36%)		1,650	(33%)	
Acquitted	650	(7%)		400	(8%)	
Nol-pros	1,350	(14%)		650	(13%)	
Dismissed	1,300	(14%)		650	(13%)	
Other (including pretrial diversion)	700	(7%)		450	(9%)	
Total	9,600	(100%)		5,000	(100%)	
How cases closed:						
Guilty plea	4,400	(72%)	(47%)	2,400	(72%)	(48%)
Nolo contendere plea	500	(8%)	(5%)	300	(9%)	(6%)
Nonjury trial	600	(10%)	(6%)	450	(13%)	(9%)
Jury trial	600	(10%)	(6%)	200	(6%)	(4%)
Subtotal	6,100	(100%)	(64%)	3,350	(100%)	(67%)
Other (dismissals, nol-pros, transfer, etc.)	3,500	(36%)		1,650	(33%)	
Total	9,600	(100%)		5,000	(100%)	

SOURCE: Criminal Division of Dade County Circuit Court, internal statistical reports.

given 10 peremptory challenges. Initial panels of 25 to 30 veniremen are subjected to voir dire examinations in these cases. The term of jury duty is one week. Each week 400 candidates are notified to appear, roughly 250 appear, and about 150 to 160 are empaneled.

Witness Handling. Subpoenas for witnesses (including victims) are prepared by a computer system and then are served either personally or by mail. The witness is usually required to appear in court at 9:00 a.m. on a specified date. In some instances, witnesses are placed in a standby, on-call status. The Greater Miami Crime Commission is presently sponsoring a witness control project whose purpose is to maintain liaison between witnesses and the offices of the state attorney and the public defender, as well as to provide liaison police officers on location in the court building. The latter assure the timely appearance of individual police witnesses.

Court Involvement in Plea Bargaining. The degree of court involvement in plea bargaining is discretionary with the individual court, given the absence of general court policy. Occasionally, to deal with caseload emergencies, the chief judge has directed that intensified plea negotiation be utilized to dispose of appropriate cases. The type of plea agreement is a sentence bargain rather than a charge bargain by a large margin. When court involvement occurs, it is often in chambers, where the court will or will not ratify a sentence agreed to by the attorneys or will state what his sentence will be when there is no attorney agreement. Obtaining sentence commitments from judges appears to be facilitated by the practice in Dade County of not ordering a pre-sentence investigation report (PSI), which might reveal information that would deter judicial commitment to a sentence in return for a plea. The

views of both the victim and the police are an important ingredient in Dade County plea bargaining.

Sentencing. Sentencing in Florida may be characterized as indeterminate in nature, with a statutory general minimum of six months for a noncapital felony. The judge may impose a maximum sentence less than the maximum prescribed for the offense. Sentences for multiple convictions may be concurrent or consecutive at the sentencing court's discretion; without specific designation, these sentences are rendered concurrent. In addition to capital felonies for which (in the absence of the death penalty) the punishment is life imprisonment with parole eligibility after 25 years, the other classes of felonies are the following: life, for which imprisonment is a life term or a term of years not less than 30; first degree, for which the prison term shall not exceed 30 years and the fine, $10,000, unless the statute specifically provides greater punishment; second degree, for which the maxima are 15 years and $10,000; and third degree, 5 years and $5,000. Sentencing advisory panels are not used in circuit court, nor are sentences reviewable by appeal. Florida law provides a right to a pre-sentence investigation only for first offenders or for those younger than 18 years. In Dade County, even when these conditions are met, the PSI report is commonly waived. Overall, perhaps 5 percent of the sentences are based, in part at least, on the PSI report. Sentencing alternatives include supervised and unsupervised probation (where either the judgment or the sentence may be suspended), jail, fines, combinations of jail, probation, and fines, and state prison. "Probation without adjudication," i.e., probation granted prior to any judgment, is a prominent category of disposition. In Dade County the views of the victim are an important ingredient in sentencing.

Bail and O.R. Practices. Dade County provides a standard bail schedule for felonies other than those punishable by life sentence, which are denied bail under the Florida constitution. Standard commercial bail bondsmen procedures are available to arrestees who do not meet the standards for release on recognizance. If the suspect is not released on O.R. or in the custody of another person, or fails to meet the scheduled bail, then he remains in custody for his first appearance in the magistrate's division of county court, namely, a bail hearing where the probable cause for arrest is reviewed by a magistrate on the basis of a police affidavit and bail is set specifically for this case. One estimate (by a judge) is that 40 to 45 percent of arrestees are released on O.R. or in the custody of another, 30 percent are released by means of bail bond, and the remainder continue in custody.

Office of the State Attorney of Dade County

Size and Caseload. The state attorney's office has responsibility for the prosecution of all felony and misdemeanor cases arising in Dade County. The staff numbers 80 attorneys, of whom 12 are assigned to misdemeanor cases, four to consumer protection cases, one to obscenity matters, five to juvenile court, three to organized crime prosecutions, five to appellate activities, and 46 to adult felony cases (exclusive of organized crime prosecutions). There are approximately 11,000 felony cases arraigned in circuit court per year and roughly 1200 felony trials. Some felony cases (perhaps 20 to 25 percent of the total felony arrests) do not survive to arraignment, and these too involve the state attorney's staff at the county court level. There are 15 investigators and a large secretarial staff to aid the attorneys in carrying their caseload.

Organization. The 46 attorneys committed to adult felony cases are assigned as follows: five to each of the five courts with individual calendars, 16 to the courts within the master calendar system, and five to comprise an Intake Group which screens cases wherein the arrest is preceded by the preparation of a police affidavit and by the issuance of a warrant. The only significant specialization by crime type in the state attorney's office is the Major Crimes Unit, which handles all capital cases and other aggravated and serious offenses. Beyond this specialization and the intake assignments, the deputies committed to adult felony cases are generalists. Cases are assigned to them in advance of the probable cause (or preliminary) hearing. They then have authority to "no action" a case, to offer a misdemeanor disposition, or to offer a plea to a lesser felony, but some deputies exercise this power infrequently. Should a case be bound over and an information filed, the initially assigned deputy then becomes the trial deputy, responsible for all aspects of the proceeding. But the office of the state attorney can elect not to file an information even if the defendant is bound over (or can file an information if the defendant is not held to answer).

Table 5.5 presents some gross statistics on prosecutorial activity and performance based on data supplied by system personnel.

Table 5.5

SELECTED STATISTICS ON FELONY PROSECUTIONS IN DADE COUNTY

Average number of felony arraignments per year	11,000
Average number of felony trials per year	1,200
Conviction rate (percent of disposed cases)	60
Plea rate (percent of cases disposed by trial or plea)	80
Trial rate (percent of cases disposed by trial or plea)	15-20
Nolle prosequi rate (percent of disposed cases)	20

SOURCE: Office of the State Attorney of Dade County, internal statistical reports.

Policy Considerations. In two major prosecutorial areas—screening and plea bargaining—the policies of the incumbent state attorney uniquely shape the activities of his office. Traditional prosecutorial screening is not conducted. That is, the prosecutor does not (with some exceptions) exercise discretion to file or not to file an accusatory pleading before judicial involvement in a prosecution. In the ordinary case, after a police officer has arrested a suspect, the officer initiates the prosecution by filing an arrest affidavit with the court.[16] The prosecutor has a screening role in noncustody cases, in citizen complaints, and in (complicated and unusual) cases where the police seek prosecutorial counsel. The incumbent state attorney does not feel a responsibility to systematically exercise the screening function, since it is the magistrate who formally makes the bind-over decision in the bulk of felony cases.

[16] All misdemeanor arrest affidavits are screened in the state attorney's office and approximately 20 to 25 percent are rejected at this time for failure of the arrest affidavit to state probable cause.

His power to "no action" cases before the preliminary hearing or to "no information" them afterwards is not commonly exercised. While the county court magistrates typically discharge 20 to 25 percent of the felony suspects, the state attorney does eliminate an additional 15 to 20 percent after arraignment by the use of nolle prosequi.

The incumbent state attorney, who has directed the office for 19 years, is a vociferous public critic of plea bargaining; his announced policy is not to permit plea bargaining without the approval of the victim and the police and never to invade the court's province by bargaining over sentences.[17] By contrast, no such opposition is evidenced by the courts or, of course, by the public defender. In practice, charge bargaining seems indeed to be rare, but sentencing agreements or assurances are frequent. Often a prosecutor and a defense attorney will have an understanding ratified in chambers by the trial judge or will be told what his sentence will be if the attorneys are in disagreement. The court and the prosecutor continue to regard the punishment views of the victim and the police as central.

The Pretrial Intervention (Diversion) Program

Initiated by means of Law Enforcement Assistance Administration (LEAA) funding in 1972, the pretrial intervention program is now county-funded under the Administrative Office of the Courts. The diversion decision is made jointly by the program director and the state attorney's office. The latter then agrees to defer prosecution at the arraignment hearing in circuit court. The defendant waives only his right to a speedy trial and enters the three-to-six month program.[18] Upon successful completion of the program, the office of the state attorney will nol-pros the information if one has been filed or will file a "no information" if an information has not been filed. Restitution is a condition of the program if property was taken from the victim.

The original criteria for entrance into the diversion program were that the defendant be a first offender, a resident of Dade County, between 17 and 25 years of age, and charged with a nonviolent offense (including drug possession, breaking and entering an automobile, carrying a concealed weapon, and grand larceny). The success of the program has promoted changes in the criteria for admission: Defendants may now be older than 25, need not be residents of Dade County, and are not precluded by a juvenile record. Futhermore, given the concurrence of the victim and the police, defendants charged with robbery, aggravated assault, or drug sale may be admitted. And the program has been expanded to include some misdemeanants.

About 7 percent of all felony defendants are currently being diverted. The program claims an 80 percent rate of successful completion. The rate of recidivism (any rearrest) for those completing the program is estimated to be under 10 percent. Among the 20 percent failures to complete the program, the recidivism rate has been found to be about 38 percent; among a control group of eligible, but nonparticipating defendants (who were granted probation instead), the recidivism rate was 32 percent. The relatively low rate of recidivism of those completing the program is regarded by observers as a persuasive indication of its value.

[17] See 17 Crim. Law Rptr. 2438 (August 27, 1975).

[18] Twice-a-week counseling sessions are conducted by professional counselors, who also provide social service assistance to the participants. Their individual caseload is approximately 30 offenders.

Office of the Public Defender

Size and Caseload. The public defender is an elected public official whose office is funded by a state appropriation. The staff of 46 attorneys is assigned as follows: two to misdemeanor cases, two to traffic, four to juvenile, one to bond hearings, six to appeals, three to mental incompetence hearings and administrative work, and 27 to the felony division. The public defender is appointed to roughly 5000 felony cases per year (about half of the total but most of the indigent cases) and an even larger number of misdemeanor cases.

Organization. The 27 attorneys in the felony division are assigned as follows: 14 to the master calendar system, including two senior attorneys; and the remaining 13 to the courts that are individually calendared, again including two senior attorneys, who supervise the defenders in three and two courts, respectively. All attorneys are generalists and handle cases from assignment to disposition. Case assignments are generally "blind," but in the master calendar system, one defender may take an entire week of cases as a single assignment. Florida has a recoupment statute, but recovery of incurred defender costs is slight.

The public defender estimates that approximately 20 percent of his cases are disposed of by a dismissal or nolle prosequi, 60 percent by a plea of guilty, and 15 to 20 percent by trial. (His policy is never to enter a plea of guilty for a client without advance assurance of the sentence that will be imposed.) Table 5.6 provides further statistical insights using public defender-supplied data.

Defense Counsel Compensation. Public defenders are remunerated by salary. Compensation for court-appointed or defendant-retained private counsel is generally by a fee per case. A maximum of $750 per case, including trial, applies to court-appointed attorneys. This limit is construed by some judges in some cases,

Table 5.6

SELECTED STATISTICS PROVIDED BY THE DADE COUNTY PUBLIC DEFENDER'S OFFICE

Type of Case and Disposition	In Process July 1, 1974	Cases Added by Information and Indictments in Quarter Ending-- September 30, 1974	December 31, 1974
Capital offenses	16	22	65
Other felonies	1383	1253	1191
Misdemeanors	821	1750	1693
Total	2220	3025	2949
Dispositions, all cases			
Nol-prossed	--	331 (17%)	348 (18%)
Public defender relieved	--	130 (7%)	227 (12%)
Plea of guilty	--	1015 (51%)	931 (48%)
Convicted by trial	--	300 (15%)	299 (15%)
Acquittal by trial	--	212 (11%)	154 (8%)
Total	--	1988 (100%)	1959 (100%)

SOURCE: Dade County Public Defender's Office, internal statistical reports.

especially involving a capital crime, to be $750 per count. Some private attorneys charge defendants a retainer plus a daily fee; others charge a flat fee per appearance, depending on the type of appearance.

Florida Corrections Background

A felon is immediately eligible for parole if his cumulative sentences total at least 12 months; eligibility for felons convicted of capital crime is at the discretion of the parole board. The practice of the parole authorities is to set the initial review at six months served and subsequent reviews at intervals of six months when the imposed sentence is five years or less; for longer sentences, the initial review occurs when one year has been served and subsequent reviews are at intervals of one year. On the average, felons are released after serving roughly 40 percent of the imposed maximum term.

The overcrowding of the Dade County Jail in which defendants await trial has impelled emergency court measures from time to time to relieve the trial backlog, including increased and accelerated plea negotiation. Jail terms are served in the "Stockade," reputedly an excellent penal institution. The state prison is severely overloaded and inadequate. It is a major source of pressure on the court system and has a substantial effect on the policy toward probationary sentences.

VI. APPLICATION OF PERFORMANCE MEASURES IN MULTNOMAH COUNTY

Sorrel Wildhorn

In Sec. V we described in largely qualitative terms the organization, operation, and policies of the court, prosecution, and public defender agencies in Multnomah County. In this section, using data collected from the agencies' records and from mail surveys of victims, witnesses, and jurors, we apply the statistical performance measures in this jurisdiction. Our analysis is built around a preliminary evaluation of the systemwide effects of a No Plea Negotiation Experiment introduced by the district attorney's office in late 1973. First, we briefly describe the objectives and nature of the experiment. Next, we provide the reader with a statistical overview of system resources, defendant-related characteristics, and the outputs of felony proceedings before and during the experiment. Then, taking each of the major issues discussed in Sec. IV, we attempt to show how performance has changed between these two time periods and to what extent performance changes are associated with the experiment or with other identifiable factors. Finally, we summarize these findings and comment on the extent it is possible to characterize changes in "overall" performance.

THE DISTRICT ATTORNEY'S NO PLEA NEGOTIATION EXPERIMENT

Toward the end of calendar year 1973 a special trial unit, known as the Impact Unit, was formed in the district attorney's office. It was partially funded through the LEAA as part of Portland's (Oregon) High Impact Anti-Crime program. There were three broad operating goals: to improve the quality of cases coming to trial by providing legal advice and casework assistance to police investigators, to provide swift and appropriate prosecution of target crimes, and to reduce the frequency of negotiated pleas. The target (or Impact) crimes were dwelling burglaries, serious robberies, and receiving and selling stolen goods (fencing).

Six additional deputy prosecutors and five support people were funded under the grant and assigned to the Impact Unit. In a departure from the usual procedure, in which a case moves between DA units, an attorney in the Impact Unit retains a case from filing to final disposition. Also, deputies work with detectives daily, assisting with case preparation whenever requested. We observed two other departures in Impact cases from the usual procedures: Case folders are in a distinctive bright orange (clearly conveying to the judge and defense attorney that this is an Impact case), and more extensive pre-sentence investigation reports are prepared more frequently in Impact cases than in other cases.

The district attorney's objective within the broad program goal of reducing the number of negotiated pleas was to reduce only the number of plea agreements involving charge reduction. The program had no effect on other forms of plea agreements, such as reduction of the number of counts (but not the charge level), recom-

mendation of (or not to oppose) specific sentences, consent not to file or not to prosecute other cases pending against the defendant, or combinations of the foregoing, in return for guilty pleas.

More specific objectives within the broad goals were also delineated. These included maintaining a high "original charge" conviction rate of 85 percent, maintaining a rate of negotiated pleas (i.e., charge reduction plea bargains) of less than 5 percent, reduction of the dismissal rate for insufficient evidence, and maintaining an arrest-to-trial period for Impact offenses equal to that for a comparison group of offenses. Since the nature of any plea bargain is recorded on a special form filed with the court and since we captured this information in our data collection effort, it was possible (as is discussed below) to determine how plea bargaining changed during the experiment and to trace its consequences on other outcomes.

A STATISTICAL OVERVIEW

In Tables 6.1 through 6.6 we provide a statistical overview of caseloads and dispositions in the circuit court, inmate population of Oregon felony correctional institutions, and a variety of defendant-related and other characteristics. The information was collected from two sources: statistics provided by various criminal justice agencies in Multnomah County and the State of Oregon, and from data samples collected by research team members from agency records. The collection procedures for the latter are described in App. D together with descriptions of the samples themselves.

General characteristics of the court, prosecution, and public defender agencies are summarized in Table 6.1. Most of the information summarized is taken from the descriptions provided in Sec. V. The numbers of criminal case filings and backlog (i.e., pending or open cases) in circuit court are shown in Table 6.2, and the numbers of felony inmates confined in Oregon correctional institutions are shown in Table 6.3.

Notice that case filings have steadily increased between 1971 and 1975 and that the backlog doubled between the end of 1973 and the end of 1975. The number of felony inmates in Oregon correctional institutions (excluding those on work release) declined somewhat between 1972 and 1974, but increased steadily thereafter. By June 30, 1975, the number of inmates (2054) exceeded the single cell capacity of 1918 beds (including work release inmates) in the entire system; and by December 1975, inmates exceeded capacity by almost 300. During the past few years there has been no increase in state correctional facilities for felony inmates. Multnomah County facilities (Rocky Butte Jail) were nearly full, too. Until mid-1973 the average daily count ran somewhat over 400 inmates. After a remodeling in mid-1973, which reduced capacity to 340, the average daily count rose to near-capacity (288 in August 1974 and 335 in December 1974).

Table 6.4 displays the mix of felony cases, by offense type, closed in 1973 and 1974. Offenses against persons other than robbery declined from 16 percent in 1973 to 9 percent in 1974. Offenses against property also declined during that period—45 to 37 percent—mainly because of a decrease in burglary and theft offenses, while the relative proportion of drug offenses increased sharply from 26 to 38 percent. However, none of these changes were statistically significant.

Table 6.1

CHARACTERISTICS OF CRIMINAL JUSTICE AGENCIES IN MULTNOMAH COUNTY

Criminal Justice Agency	Characteristics
Circuit court	
Number of courts	1 chief criminal court (no trials except on stipulated facts) 10 civil/criminal trial courts
Number of felony cases filed per year	2500 (approx.)
Number of felony trials per year	300 (approx.)
Method of electing judges	Nonpartisan; 6-year term
Juries	
Term of duty	4-5 weeks
Pool of venirepersons	200
Jury size	12
Sentencing	
Statutory maxima by felony level	Life: life, no minimum A: 20 years B: 10 years C: 5 years
Court discretion	May impose maximum lower than statutory maximum; concurrent or consecutive sentences
Typical percentage of imposed sentence served (all felonies)	33
District attorney's office	
Number of prosecutors	
Total (felonies and misdemeanors)	50
Felonies only	27
Number of felony cases filed per year ÷ number of felony-assigned deputies	100 (approx.)
Public defender's office	
Number of felony case adjudications	1400
Number of attorneys	
Total	12
Felonies only	8
Number of felony cases adjudicated per year ÷ number of felony-assigned defenders	175 (approx.)

Table 6.2

CRIMINAL CASES[a] FILED AND PENDING IN MULTNOMAH COUNTY CIRCUIT COURT, 1971-75

Fiscal Year	Number of Filings	Date	Cases Pending (backlog)
1971-72	2466	Dec. 1972	597
1972-73	2928	Dec. 1973	533
1973-74	3250	Dec. 1974	774
1974-75	3657	Nov. 1975	1008

SOURCE: Multnomah County Circuit Court records.

[a]Includes criminal appeals from lower court.

Table 6.3

Inmates[a] Confined in Felony Correctional Institutions in the State of Oregon, 1972-75 Selected Dates

Date	Number of Inmates
Jan. 1, 1972	1899
Jan. 1, 1973	1595
Jan. 1, 1974	1659
Jan. 1, 1975	1886
June 30, 1975	2054
Dec. 1, 1975	2205

SOURCE: Oregon Department of Corrections.

[a]Excludes inmates on work release.

Table 6.4

Distribution of Felony Cases by Type of Offense in Multnomah County, 1973-74 (Percent of all cases)

Type of Offense	1973	1974
Offenses against persons	25	20
Robbery	9	11
Other	16	9
Offenses against property	45	37
Burglary	21	16
Theft	16	13
Other	8	8
Drug offenses	26	38
All other offenses	4	5
Total	100	100

SOURCE: Based on a random sample of 100 felony cases in each year (the "general" samples--see App. D).

In Table 6.5 we display trends in the characteristics of felony defendants processed in circuit court. Compared with 1973, burglary and robbery defendants in 1974 tended to be somewhat younger and less educated and had resided longer in Multnomah County, although these differences were not substantial. In 1974 fewer burglary and robbery defendants were black. The number of defendants with prior prison records changed only slightly over the two years; about half had no prior record and 15 to 17 percent had prior records. In 1973 about half of all felony defendants had public defenders whereas in 1974 only a third did. In 1973 over 80 percent of all felony defendants made bail or O.R., whereas in 1974 only 70 percent did.

Table 6.5

SELECTED CHARACTERISTICS OF FELONY DEFENDANTS IN MULTNOMAH COUNTY, 1973-74
(Percent of all defendants)

Characteristics of Defendants	1973	1974
Age[a]		
Under 21	40	46
21-29	47	39
30 and over	13	15
Ethnic group[a]		
Black	45	30
Spanish surname	4	0
Other minority	8	13
Nonminority	43	57
Transient[a] (i.e., less than 2 years in county)	15	11
Less than high school education[a]	57	50
Prior record[a]		
None	47	52
Minor	20	12
Major	18	19
Prison	15	17
Type of defense attorney[b]		
Public defender	50	33
Private (court-appointed or defendant-retained)	50	67
Pretrial custody status[b]		
In jail (or combination of jail and bail or O.R.[c])	17	30
Released on bail or O.R.	83	70

[a]Based on 100-case random samples for each exemplary offense in each period, and weighted by their relative frequency of occurrence as shown in Table 6.4.

[b]Based on a 100-case random sample of all felony defendants in each period.

[c]The defendant is in jail part of the time and free part of the time.

In Table 6.6 we present an overview of felony case dispositions, sentences, and delays based on an examination of a random sample of almost 100 circuit court case folders in each of the two years. Pretrial dismissal rates remained almost constant over the two years at 35 to 38 percent, although the proportions dismissed for other reasons and for insufficient evidence changed somewhat in 1974 compared with 1973. The trial rate also declined from 13 percent in 1973 to 7 percent in 1974; this decline was fairly evenly split between jury and court trials. Trial conviction rates, however, remained constant.

For all felonies, the overall rates of pretrial guilty pleas showed little change over the two years (52 to 53 percent), and the proportion pleading guilty to original charges as opposed to lesser charges also showed little change over this time period. (However, as we shall demonstrate below, the picture for the specific Impact offenses of Dwelling Burglary I and Robbery I was very different.) With pretrial dismissal, guilty plea, and trial conviction rates fairly constant over the two years, overall conviction rates, too, showed little change—remaining at roughly 60 percent of all felony dispositions.

Table 6.6

DISPOSITIONS, SENTENCES, AND DELAYS IN FELONY PROCEEDINGS IN MULTNOMAH CIRCUIT COURT[a]
(Disposition and sentence entries in percent)

Type of Disposition, Sentence, and Delay	1973		1974	
Dispositions				
Pretrial dismissal rate	35		38	
For insufficient evidence		16		8
For other reasons		19		30
Trial rate	13		7	
Jury		5		2
Court		8		5
Trial conviction rate[b]	75		72	
Trial acquittal rate[b]	25		14	
Trial dismissal, mistrial rate[b]	0		14	
Pretrial plea of guilty rate	52		53	
To original charges		22		19
To lesser charges		30		34
Overall conviction rate	62		58	
N (dispositions)	(94)		(95)	
Sentences				
Suspended	2		0	
Nonincarceration (probation, fine, restitution, etc.)	66		61	
Incarcerated	32		39	
Jail (and any lesser punishment)		11		28
Prison (and any lesser punishment)		21		11
N (convictions or sentencings)	(57)		(53)	
Median elapsed time (days)				
From arrest to trial	56		84	
From arrest to final disposition	62		77	
From arraignment to final disposition	34		63	

[a]Based on random sample of 100 felony cases from each period.

[b]Based on number of trials.

Somewhat over 60 percent of all felony defendant sentences were nonincarcerations—i.e., probation, fines, restitutions, or combinations of these elements—and little change in these rates occurred over the two years. Of the 32 to 39 percent incarcerated, the proportion receiving jail sentences (rather than prison sentences) declined markedly in 1974 compared with 1973. Although we cannot demonstrate it conclusively, this phenomenon may be related to the fact that the total number of confinements in state felony correctional institutions was rising rapidly (see Table 6.3) during this period and approaching the single cell capacity of the entire system. (Again, as shown later, the sentencing picture for felony defendants charged with Impact offenses was very different.)

Between 1973 and 1974, median elapsed time from arrest to trial and from arrest to final disposition increased by 50 and 25 percent, respectively. In 1973, the median arrest-to-trial period was barely within the statutory standard of 60 days for individual cases; in 1974 it rose to 84 days. Even more significant for the operation of the circuit court, the median time from arraignment to final disposition almost doubled between 1973 and 1974. As we shall indicate below in a more detailed discussion of the delay issue, part of the rise in elapsed time seems to be related to the steady rise in the number of case filings and amount of backlog. For example, between the end of 1973 and the end of 1974, the number of cases pending rose by almost 50 percent (see Table 6.2).

Finally, to provide a general context for the subsequent analysis of the issues outlined in Sec. IV, we show in Figs. 6.1 and 6.2 what happened to robbery and burglary arrestees at the various stages between arrest and final disposition for 1973 (figures in italics) and 1974 (figures in roman). Our focus is on the flow of those defendants originally arrested on robbery or burglary charges, charged by the prosecutor with at least one count of these offenses, and arraigned in circuit court.

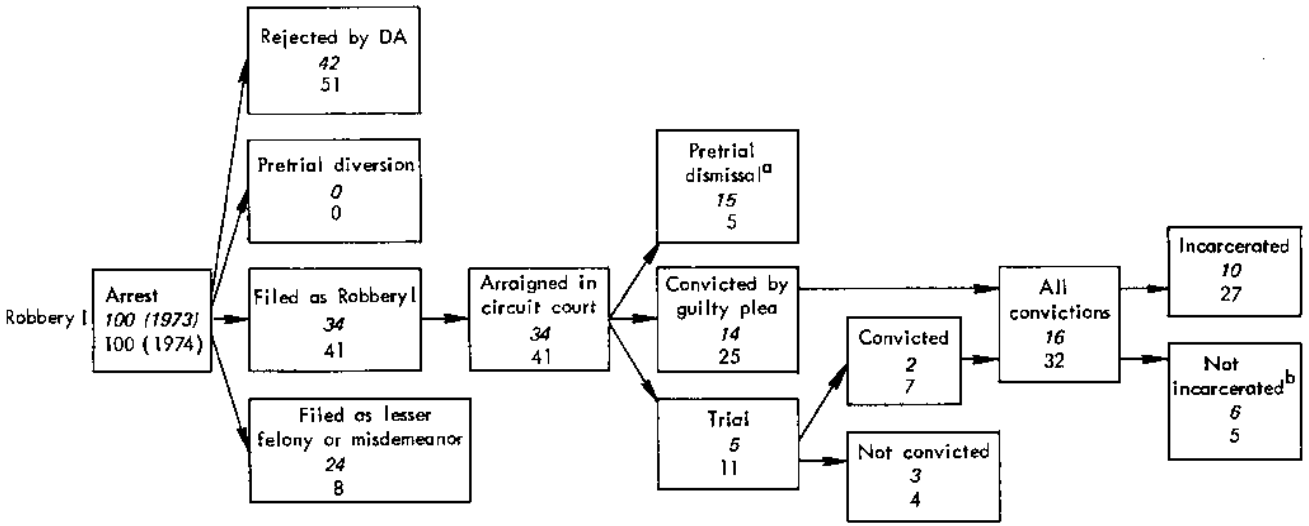
PROSECUTORIAL SCREENING

Here we apply the performance measures to the two prosecutorial issues discussed in Sec. IV—charging threshold[1] and charging accuracy.

Charging Threshold

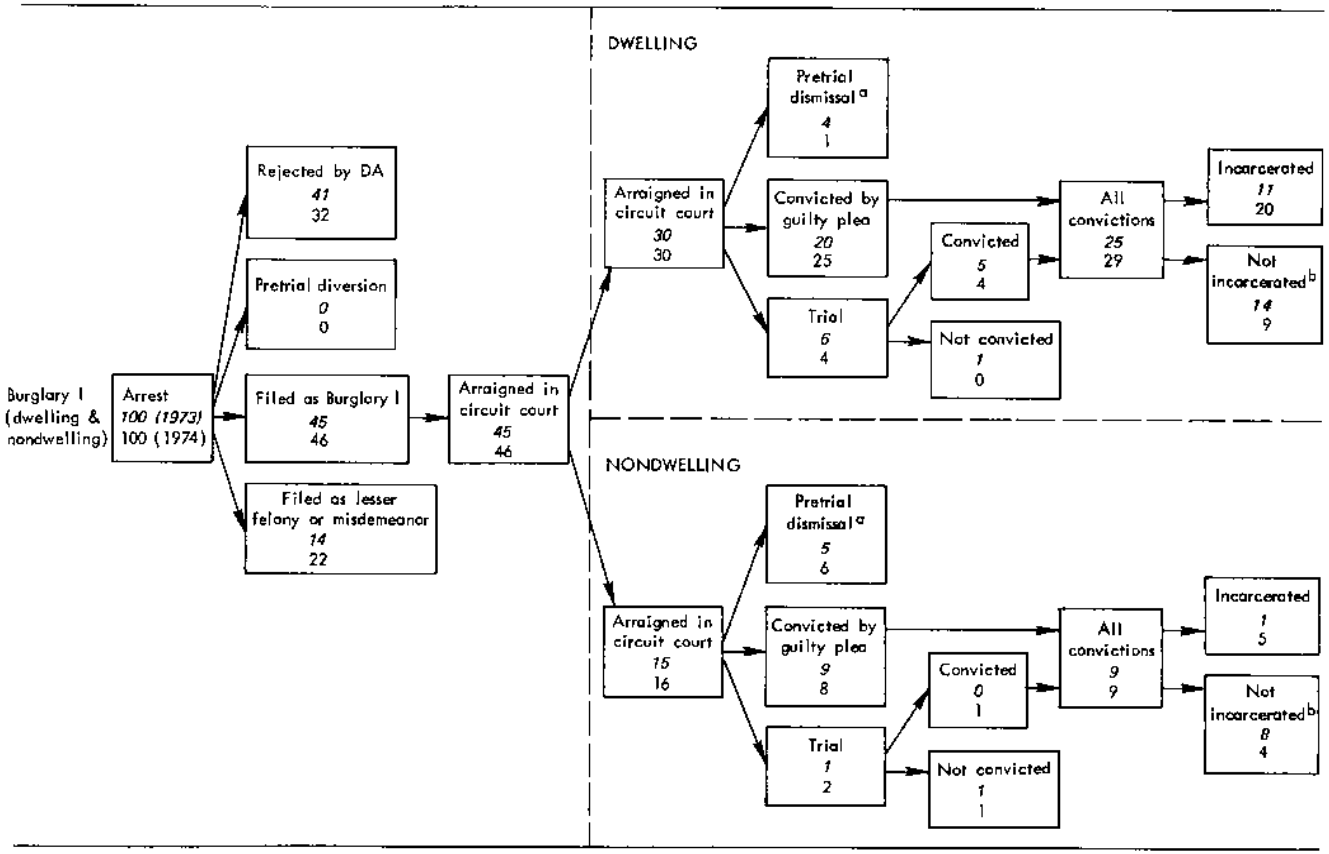
In Tables 6.7 and 6.8 we give performance measures relevant to gauging changes in the prosecutorial charging threshold. Table 6.7 shows the screening actions taken in 1973 and 1974 by the prosecutor's office on cases booked by the police in which the highest police booking charge was Robbery I (an Impact offense in 1974). Some cases contained only one Robbery I count, others contained multiple Robbery I counts, and still others had one or more Robbery I counts plus one or more lesser charges. Table 6.8 shows similar data for cases booked by the police in which the highest booking charge was Burglary I. (In Oregon, Burglary I includes all dwelling burglaries as well as some nondwelling burglaries.) As mentioned above, the No Plea Negotiation Experiment focused on *dwelling* burglaries in 1974. However, because police booking records do not distinguish between Dwelling Burglary I and Non-

[1] That is, the case strength that suffices for the filing of felony charges.



NOTES: [a]Includes cases charged as Robbery I that are dismissed in lower court or in circuit court before trial on a motion by the prosecutor or after a hearing on a motion by the defense.
[b]Includes suspended sentences.

Fig. 6.1—Movement of Robbery I cases from arrest through final disposition in the circuit court of Multnomah County, 1973 and 1974 (in percent of arrests)



NOTES: [a]Includes cases charged as Burglary I that are dismissed in lower court or in circuit court before trial on a motion by the prosecutor or after a hearing on a motion by the defense.
[b]Includes suspended sentences.

Fig. 6.2—Movement of Burglary I cases from arrest through final disposition in the circuit court of Multnomah County, 1973 and 1974 (in percent of arrests)

Table 6.7

PROSECUTORIAL CASE SCREENING FOR ROBBERY I BOOKINGS IN MULTNOMAH COUNTY, 1973-74

Charging Threshold	Percent of All Police Bookings	
	1973	1974
Rejected outright	42	51
Pretrial diversion[a]	--	--
Filed on (at least) the most serious charge (Felony A)	34	41
Filed on lesser felony charge(s) (Felony B or C)	21	5
Filed as a misdemeanor	3	3
All police bookings (N)	24 100 (58)	8 100 (100)

[a]There are no pretrial diversion programs in Multnomah County.

Table 6.8

PROSECUTORIAL CASE SCREENING FOR BURGLARY I BOOKINGS IN MULTNOMAH COUNTY, 1973-74

Charging Threshold	Percent of All Police Bookings	
	1973	1974
Rejected outright	41	32
Pretrial diversion[a]	--	--
Filed on (at least) the most serious charge (Felony A)	45	46
Filed on lesser felony charge(s) (Felony B or C)	8	16
Filed as a misdemeanor	6	6
All police bookings (N)	14 100 (112)	22 100 (112)

[a]There are no pretrial diversion programs in Multnomah County.

dwelling Burglary I, we could not show screening actions for each suboffense category separately. The data are based on samples for each year and each offense taken from police booking records. Data on prosecutorial screening actions on cases in which an information was filed were gathered from card files in the district attorney's office. If a card was missing, our interpretation was that the case had been rejected outright by the prosecutor.

Turning first to police-booked Robbery I cases, we see that between 1973 and 1974 there was no substantial change in either the outright rejection rate or in the filing rate on the most serious charge. Taken *alone*, these two performance measures suggest that no major changes in prosecutorial charging policies and standards for robbery occurred as a result of the No Plea Negotiation Experiment.

It has been suggested that an improper way of furthering one of the objectives of the experiment—to lower the frequency of reduced charges for guilty pleas in Impact cases—would be for the prosecutor to screen out potential Impact cases at the charging stage by reducing the booking charge from Robbery I to some lesser charge. These cases then enter the system as non-Impact cases; and even if they were subsequently charge bargained, they would not be counted as charge bargained Impact cases. The filing rate on lesser charges (Felony B, Felony C, or misdemeanor) actually *decreased* significantly (from 24 percent in 1973 to 8 percent in 1974), suggesting, at the very least, that the prosecutor's office did not attempt this subterfuge.

Turning next to Burglary I bookings, we see that no salient changes occurred in any of the prosecutorial filing actions over the two-year period. Based on these performance measures alone, we infer that the charging threshold for Burglary I as a whole did not shift materially. (But since the data could not be estimated separately for dwelling and nondwelling burglaries, we cannot infer from these data whether the Impact experiment affected charging standards in dwelling burglaries differently from charging standards in nondwelling burglaries.) However, the results of the case auditing exercise (see Sec. VIII), in which small samples of dwelling and nondwelling burglaries were examined, revealed that for both years and both types of burglaries there was no discernible change in the strength of the average case. The audit suggested that almost all of the filed cases were strong. Since the case audit was designed primarily to illuminate plea bargaining, and since no rejected cases were audited, it cannot be used to assess adherence to charging standards.

As we argued in Sec. IV, to illuminate the charging threshold issue one should examine the changes in the frequency of reasons for rejection. If, for example, the relative proportion of outright rejections because of evidence deficiency declines, while the overall rejection rate remains fairly constant between two periods of time, it is fair to infer that the police investigations have improved *and* that the prosecutor has raised his charging threshold. Had the prosecutor not raised it, he would have rejected outright a *smaller* proportion of cases in the latter time period, other things being equal.

In Table 6.9 we display the relative frequency of rejection reasons Robbery I and Dwelling Burglary I in the two time periods. These data were gathered and classified (by study team members) from narratives in "prosecution declined" memoranda on file in the prosecutor's office. Notice first that the percent of rejections calling for more investigation declined for both Impact crimes in 1974. This is a strong indicator that one of the experiment's goals was being achieved—improving the quality

Table 6.9

FREQUENCY OF REASONS GIVEN FOR REJECTION OF POLICE-BOOKED ROBBERY I AND DWELLING BURGLARY I CASES, MULTNOMAH COUNTY, 1973-74

Reason	Robbery I		Burglary I (dwelling only)	
	1973	1974	1973	1974
Evidence deficiency				
Insufficient evidence and absence of indispensable parties	53	40	59	60
No corpus of crime	0	0	0	2
Evidence inadmissible	0	0	0	1
Good case but needs more investigation	31	15	30	20
Total	84	55	89	83
Proper reasons other than evidence deficiency	8	19	7	7
Interests of justice (discretionary refusal to prosecute)	8	26	4	10
All rejections (N)	100(13)	100(48)	100(27)	100(80)

of cases presented by the police by providing legal advice and assistance to police investigators. Notice further that robbery rejections for evidence deficiencies declined dramatically, from 84 percent in 1973 to 55 percent in 1974. Proper nonevidentiary reasons were more frequent in 1974 as was the exercise of discretion by the prosecutor, but the latter rose only from 8 to 26 percent between years—much less than the decline in evidence deficiency rejections. This, coupled with the fact that overall rejection rates and filing rates at the most serious charge level remained fairly consistent, indicate that the charging threshold for Impact robberies was raised as well. For Dwelling Burglary I the evidence is more ambiguous: Since rejections for evidence deficiencies declined only slightly in 1974, this one indicator seems to suggest that the charging threshold is not noticeably higher under the experiment. But as we noted above, data were not available to estimate *overall* rejection and filing rates for dwelling burglaries separately, so we cannot tell whether they have risen, declined, or remained unchanged over the time period. All we know is that such performance measures showed little change for *all* Burglary I. However, the case audit conclusions support the inference that the charging threshold for both dwelling and nondwelling burglaries was high, and did not change materially over the two years.

We have drawn inferences about changes in charging standards from our observations of year-to-year changes in the performance measures. But some of the observed changes in the performance measures might be explained by changes in factors *other* than charging standards, such as arrestee-related characteristics. For example, if it is thought that the arrestee's age or prior record might affect the

prosecutor's screening actions and if these characteristics change from one year to the next for the average burglary or robbery arrestee, not all of the observed changes in the performance measures could be attributed to charging threshold differences. It is here that one needs statistical tools such as multivariate analysis to reveal any such effects. Although we supply such statistical tools (see App. E for results and a methodological discussion) in attempting to explain conviction probability, sentence severity, and elapsed time, we were not able to apply them to the analysis of prosecutorial screening actions because few, if any, data are collected by the court or prosecutorial agencies on background characteristics of arrestees whose cases are rejected by the prosecutor.

Charging Accuracy

Table 6.10 displays a set of performance measures relevant to charging accuracy. The measures (dispositions subsequent to screening actions) were calculated from samples of data collected from circuit court felony case files and other court, prosecutor, and public defender agency records. Approximately 100 cases filed by the prosecutor in which the highest charge was Burglary I were selected for each year; similar samples were collected for cases in which the highest charge was Robbery I. Measures for dwelling and nondwelling burglaries are shown separately, because the former was an Impact crime in 1974 whereas the latter was not.

What inferences can we draw as to changes in charging accuracy? One ambiguous measure is the proportion of defendants *not* convicted. For the Impact crimes of Robbery I and Dwelling Burglary I there was a dramatic reduction in nonconvictions, particularly in the robbery pretrial dismissal rate; for the non-Impact crime of Nondwelling Burglary I there was no such decline. This is one indicator that the case quality of Impact crimes improved markedly in 1974 compared with 1973, whereas no such change seems to have occurred in nondwelling burglaries. To what extent better case quality is a result of better police investigative work or improved charging accuracy cannot be ascertained from these data. Our examination of the reasons for case rejection indicated that police investigation *did* improve. But whether charging accuracy improved also is unclear. The case audit exercise (see Sec. VIII) suggests at least that the strength of both dwelling and nondwelling burglary cases *did not* decrease from year to year. To the extent that subjective judgment by practitioners regarding the strength of cases is a measure of charging accuracy, we can conclude that charging accuracy did not decline in 1974.

The fact that Robbery I and Dwelling Burglary I convictions on original charges rose markedly in 1974 and, concomitantly, convictions on reduced charges declined must be attributed to the policy ground rules of the No Plea Negotiation Experiment and not to improved charging accuracy. (If, however, the same year-to-year changes in all of these performance measures had occurred *without* introducing such an experiment, one could conclude that charging accuracy had indeed improved dramatically.)

Of some note is the fact that nondwelling burglary convictions on original charges also rose in 1974, but not as greatly as for dwelling burglaries. Convictions on reduced charges also declined in 1974. This pattern seems to suggest that a spillover effect was at work in non-Impact crime cases, even though the nondwelling and dwelling burglary cases are handled by two different units within the prosecutor's office.

Table 6.10

PERFORMANCE MEASURES RELATED TO CHARGING ACCURACY: MULTNOMAH COUNTY BURGLARY I AND ROBBERY I CASES, 1973-74

	Percent of All Dispositions					
	Robbery I		Dwelling Burglary I		Nondwelling Burglary I	
Disposition of Cases	1973	1974	1973	1974	1973	1974
Not convicted						
Pretrial dismissal	44	12	14	5	33	38
Trial acquittal, dismissal, hung jury	9	12	3	0	9	6
	53	24	17	5	42	44
Convicted on all original charges						
By guilty plea	10	54	19	67	6	23
By trial	6	13	11	12	0	3
	16	67	30	79	6	26
Convicted on at least one most serious original charge (with other charges and/or counts reduced						
By guilty plea	7	4	11	2	3	0
By trial	0	0	3	0	0	0
	7	4	14	2	3	0
Convicted on lesser charge(s)						
By guilty plea	24	4	39	12	49	27
By trial	0	1	0	2	0	3
	24	5	39	14	49	30
All dispositions (N)	100(95)	100(86)	100(64)	100(56)	100(33)	100(32)
Trial conviction rate[a]	36	52	82	100	0	50
Trial acquittal, dismissal, mistrial rate[a]	64	48	18	0	50	50

[a]Based on number of trials.

As discussed in App. E, we attempted to explain and predict conviction probabilities (no conviction, i.e., dismissals; conviction on original charges; conviction on fewer counts, with no level reduction; and conviction at reduced charge level) as related to nonaccuracy factors such as defendant-related characteristics, number and level of original charges, and so on. But the attempt was unsuccessful in that none of the factors we chose seems to be related to conviction probability. If our technical approach was correct, this suggests that variables or factors *other* than the ones we hypothesized may be related to the probability of conviction and that future research should be directed along these lines.

PLEA BARGAINING

We turn now to the measurement of the effects of plea bargaining. Before we display the sets of performance measures (discussed in Sec. IV) for gauging the extent of concessions to defendants, on the one hand, and the effects ("gains" or other impacts) to the system, on the other hand, it is instructive to show in some detail the changes in the *nature* of plea bargaining as affected by the No Plea Negotiation Experiment. In other words, we must first show to what extent the major policy objectives of the experiment have been achieved—reducing the frequency of charge bargains to virtually zero and increasing dramatically the frequency of guilty pleas to the highest original charge. Recall that the experiment's goals did not mention other types of plea bargains, such as reduction in the number of counts of lesser charges, reduction in number of counts (of highest) original charges, sentence agreements, or agreements not to prosecute other cases.

In Table 6.11 we display how guilty plea rates, by type of plea, changed between 1973 and 1974 for the two Impact offenses of Robbery I and Dwelling Burglary I and the non-Impact offense of Nondwelling Burglary I. The most striking change is that negotiated pleas in Impact cases in which the charge was *reduced from the original level* (with or without other bargains such as sentence agreements or agreements to drop other cases) dropped dramatically in 1974. In the case of robbery, the charge reduction rate dropped from 59 percent in 1973 to only 6 percent in 1974—almost meeting the experiment's target of 5 percent. In the case of dwelling burglary, the rate declined from a similar level in 1973 to 16 percent in 1974—somewhat in excess of the 5 percent target, but a dramatic decline nevertheless. For the non-Impact offense of nondwelling burglary there seemed to be a spillover effect; whereas 85 percent of pleas were charge bargained in 1973, only 53 percent were so disposed of in 1974, and all of the decline was in charge bargains with *other* types of plea bargains.

Concomitantly, straight plea rates with or without other plea bargains rose considerably in 1974, not only for Impact offenses, but for non-Impact offenses as well. In 1973 straight pleas with or without other plea bargains accounted for roughly 25 percent of robbery or dwelling burglary guilty pleas. In 1974 they accounted for over 80 percent, and roughly 50 percent involved sentence agreements or agreements to drop other charges. A similar, but less pronounced, increase occurred in 1974 nondwelling burglary guilty pleas as well, indicating the presence of a spillover effect to non-Impact offenses. Incidence of count bargaining—a guilty plea to at least one count of the highest original charge, but with the number of counts of highest original and/or original lesser charges reduced—was low for all three offense categories and showed little year-to-year change.

In summary, implementation of the No Plea Negotiation policy resulted in a much lower incidence of charge bargaining, a much higher incidence of pleas to at least one count of the highest original charge, and a sizeable increase in other forms of plea bargaining for Impact offenses. One non-Impact offense showed similar spillover tendencies.

Given this changed pattern of plea bargaining, what was the pattern of system-wide concessions and "gains"? Tables 6.12 and 6.13 summarize these effects in gross terms. The set of performance measures in Table 6.12 is composed of dispositional, imposed sentence severity, and delay measures; the measures show the operational

Table 6.11

RESULTS OF PLEA BARGAINING IN MULTNOMAH COUNTY, 1973-74

Level of Plea and Disposition	Percent of All Pleas of Guilty					
	Robbery I		Dwelling Burglary I		Nondwelling Burglary I	
	1973	1974	1973	1974	1973	1974
No plea bargain of any kind						
Straight plea (to all original charges and counts)	5	43	11	30	5	27
Straight plea with other plea bargain types						
Sentence agreement	0	11	5	28	0	0
Agreement to drop other cases	18	28	9	22	5	20
Combination of the above	0	6	2	2	0	0
	18	45	16	52	5	20
Plea to at least one count of most serious charge with other charges and/or counts reduced						
With no additional plea bargain types	5	2	0	0	0	0
With sentence agreement and/or agreement to drop other cases	13	4	6	2	5	0
	18	6	6	2	5	0
Plea to lesser charge(s)						
With no additional plea bargain types	13	4	16	8	21	20
With sentence agreement and/or agreement to drop other cases	46	2	41	8	64	33
	59	6	57	16	85	53
Total guilty pleas (N)	100(39)	100(53)	100(44)	100(46)	100(19)	100(16)
Gross plea rate (N ÷ number of dispositions)	41	61	69	82	58	50

effects of plea bargaining. The experiment's effect was to substantially reduce dismissal rates and increase overall guilty plea and conviction rates for Impact offenses, whereas these measures showed little year-to-year change for other felony offenses. Only robbery trial rates increased notably, whereas other Impact and non-Impact offenses showed little change. We cannot explain why only robbery trial rates increased except to speculate that if defendants in 1974 were aware that robbery sentences had risen considerably (see below), they might have been more willing to go to trial on the chance that they would be acquitted.

We have displayed in Table 6.12 five measures of sentence severity imposed: the percent incarcerated (in jail or in state prison) and the Sentence Severity Index scores for the four sets of weights described in Sec. IV, (sentence severity values are

Table 6.12

MEASURES REFLECTING THE OPERATIONAL EFFECTS OF PLEA BARGAINING IN MULTNOMAH COUNTY, 1973-74

Dispositions, Sentences, and Delays	Robbery I		Dwelling Burglary I		Nondwelling Burglary I		All Burglary I		All Felonies	
	1973	1974	1973	1974	1973	1974	1973	1974	1973	1974
Dispositional measures (%)										
Dismissal rate	44	12	14	5	33	38	20	17	35	38
Trial rate	15	27	17	13	9	13	14	13	13	7
Guilty plea rate	41	61	69	82	58	50	65	70	52	53
Overall conviction rate	47	77	83	95	58	56	75	80	62	58
Sentence severity measures										
Percent incarcerated	67	87	36	67	15	53	33	64	32	39
Sentence severity imposed										
(Index A score)	16.7	26.5	--	--	--	--	11.4	16.9	--	--
(Index B score)	20.9	43.9	--	--	--	--	10.4	20.4	--	--
(Index C score)	25.0	61.2	--	--	--	--	9.4	23.9	--	--
(Index D score)	43.8	116.5	--	--	--	--	19.0	47.0	--	--
Median elapsed time (days)										
Arraignment to guilty plea	37	21	33	31	29	21	31	29	23	29
Arraignment to trial	35	52	30	79	25	72	30	76	30	49
Arraignment to final disposition	71	64	68	68	61	61	58	66	34	63
Continuances and witness appearances										
Number days continued/total cases										
Per uncontested case	--	--	--	--	--	--	--	--	7	9
Per trial	--	--	--	--	--	--	--	--	14	13

not shown separately for dwelling and nondwelling burglaries). A greater proportion of defendants pleading guilty to robbery charges were incarcerated in the later period and received stiffer sentences. Defendants pleading guilty to either dwelling or nondwelling burglary charges also were more likely to be incarcerated (notice that the year-to-year *rate* of increase in percent incarcerated is greater for nondwelling burglary) and receive more severe sentences. Even though a greater proportion of dwelling, as compared with nondwelling, burglars were incarcerated in the later period, the multivariate regression analysis using Sentence Severity Index A (see App. E) showed that the independent effect of burglary in a dwelling on Sentence Severity Index score was not significant.

The experiment's goals were silent on sentence severity. That is, no explicit policy was enunciated regarding more severe or less severe sentencing in Impact offenses. Nevertheless, it is of interest to ask whether there was a discernible effect on sentence severity that could be associated with the experiment—that is, did judges appear to be sentencing like defendants more harshly during the experiment? To answer this question requires that we adjust for year-to-year random sampling errors in the population characteristics of defendants and cases. Using Sentence Severity Index A, which is closest to the weighting scheme devised by the California Bureau of Criminal Statistics, we can estimate what the mean Sentence Severity Index score would have been had there been no plea bargaining or sentence policy change *and* no other changes. To do this, we apply the 1973 sentence severity equations (shown in App. E) in conjunction with the 1973 average charge bargaining

Table 6.13

CONCESSIONS AS A RESULT OF PLEA BARGAINING IN MULTNOMAH COUNTY, 1973-74

Sentence Severity Index	Type of Defendant	Reductions in Sentence Severity Index Score from Straight Plea Score			
		Burglary I		Robbery I	
		1973[a]	1974[b]	1973[c]	1974[d]
A	Per convicted defendant (including those tried)	5.2	2.1	9.1	1.3
	Per defendant with charge bargain	9.3	9.3	12.4	10.7
	Per defendant with count bargain	NS	NS	16.8	22.2
B	Per convicted defendant (including those tried)	6.8	15.1	23.0	1.9
	Per defendant with charge bargain	12.2	18.0	31.2	31.3
	Per defendant with count bargain	NS	34.9	43.8	NS
C	Per convicted defendant (including those tried)	8.5	23.4	41.0	3.1
	Per defendant with charge bargain	15.1	26.8	49.9	51.9
	Per defendant with count bargain	NS	57.3	70.9	NS
D	Per convicted defendant (including those tried)	17.0	37.7	57.8	NS
	Per defendant with charge bargain	30.2	50.7	72.6	NS
	Per defendant with count bargain	NS	86.7	126.2	NS

NOTE: NS = not significantly different from zero at the 90 percent confidence level.

[a] 56 percent of those convicted pled to reduced charges; 15 percent pled to reduced counts at same level.

[b] 23 percent of those convicted pled to reduced charges; 30 percent pled to reduced counts at same level.

[c] 51 percent of those convicted pled to reduced charges; 17 percent pled to reduced counts at same level.

[d] 6 percent of those convicted pled to reduced charges; 3 percent pled to reduced counts at same level.

rate (no policy change), the 1973 time trend proxy variable, and the average of the 1973 and 1974 means for defendant and case characteristics to estimate the average 1974 score had there been no changes. We hypothesize that the 1974 estimate will be lower than the 1974 observed score—the difference being associated with the experiment or other factors.[2]

[2] Other factors which could influence sentence severity between the two years include changes in crowding in correctional facilities and changes in individual sentencing judges. However, our procedure adjusts for the former by utilizing the *1973* mean value for the proxy variable which reflects crowding. (In any case, crowding in both state and local correctional facilities increased in 1974, which ought to have resulted in lower average sentence severity.) Most of the sentences upon which our analysis is based were imposed by the chief criminal judge, since most defendants are convicted through pleas of guilty. In Multnomah, this position rotates every two to three months among circuit court judges. It is *possible* therefore, that the 1974 chief criminal judges simply sentenced more severely than those in 1973 for reasons having nothing to do with the experiment.

Table 6.14 summarizes these results. For Robbery I, it appears that the observed increase in Sentence Severity Index score was 9.8 (or 37 percent of the 1974 observed value), but after adjusting for random sampling errors in defendant and case characteristics, the increase associated with the experimental period was 13.8 (or about 50 percent of the 1974 observed value). For Burglary I as a whole the observed increase was 5.5 (or 33 percent of the 1974 observed value), but after adjusting for sampling errors in population characteristics, the increase attributable to the policy change is 3.3, or about 29 percent of the 1974 observed value. Thus, our hypothesis that the experiment did indeed (albeit perhaps unintentionally) induce more severe sentences is not rejected. However, we cannot rule out the possibility that more severe sentencing judges presided in 1974.

Table 6.14

DIFFERENCES IN SENTENCE SEVERITY IMPOSED ASSOCIATED WITH THE NO PLEA NEGOTIATION EXPERIMENT IN MULTNOMAH COUNTY, 1973-74

Average Score of Sentence Severity Index A for Defendants Pleading Guilty	Robbery I	Burglary I
1973 observed score	16.7	11.4
1974 observed score	26.5	16.9
1974 predicted score, assuming no policy change and no other changes, with adjustments for sampling errors	12.7	13.6
1973 to 1974 total observed increase	9.8	5.5
1973 to 1974 increase associated with the experiment	13.8	3.3

Returning to other effects of plea bargaining on the system, Table 6.12 also indicates that, by and large, the experiment had little effect on median elapsed times from arraignment to guilty plea or from arraignment to final disposition for Impact offenses. But for felony cases as a whole, the period to final disposition almost doubled, probably as a consequence of the growing caseload and backlog pressures noted above. This suggests that Impact cases (and Nondwelling Burglary I cases as well) were treated more expeditiously than the average felony case. The arrest-to-trial period, however, increased by 50 to 100 percent for offense categories shown, but the estimates are based on very small samples.

Some rise in continuance-induced delay was apparent in uncontested cases (from seven days per average case in 1973 to nine in 1974), but such delay declined slightly in 1974 for the few relevant trials.

Turning next to plea bargaining concessions, Table 6.13 displays three different concession measures, all expressed in terms of sentence severity score reductions from *straight plea* scores. (Our statistical analysis using Index A revealed that electing a court or jury trial did not generally affect sentence severity when compared with that received on a straight plea. However, our trial sample was very

small and we cannot make accurate estimates of this hypothesized effect.) Using each of the sentencing severity indices, we show for each year and for both Robbery I and Burglary I the average amount of sentence severity score conceded per *convicted defendant* (those who plead guilty and those who are convicted at trial), per defendant who *pled guilty to lesser charges*, and per defendant with a *count bargain* (i.e., a plea to at least one count of the highest original charge, but with one or more counts of original charges dismissed in return for a guilty plea). (Concession values are shown only if they differed from zero at the 90 percent confidence level.)

It is very clear that, in Robbery I offenses, the sentence severity conceded per *convicted* defendant (measured by any of the indices) fell dramatically in 1974. In any given year it is also clear that a robbery defendant who is successful in obtaining a reduction in charge level in return for a plea of guilty receives more of a concession than does the average convicted defendant.

For reasons we cannot explain, count bargaining in Robbery I seems to produce even greater concessions than does charge bargaining. There also seems to be a little year-to-year change in concessions per defendant who enters a charge or count bargain, which suggests that the effect of the experiment was not to change the concession for any specific type of plea bargain, but to change the proportion of defendants pleading within each type. We note further that compared with pure straight pleas, our statistical analysis revealed no significant independent effect of *other* plea bargain types (sentence agreement or agreement to drop pending cases) on sentence severity.[3] This is perhaps to be expected for the latter but not for the former, for the very nature of a sentence agreement plea bargain should be to reduce the expected severity. But in our data collection we could capture only the *fact* that sentence agreements were reached or that the prosecutor agreed not to oppose a defense recommendation, not the actual sentence discussed by the practitioners involved (since these were never recorded). This suggests that such data must be recorded routinely in court files if their effects are to be analyzed and interpreted.

In our statistical analysis we also found that the independent effect of the number of original Robbery I (or other charges of equivalent level) charges was positively related to the sentence severity score, but that the number of additional charges at lesser levels was not. (The independent effects of other variables such as defendant background characteristics, prior record, custody status, and type of defense attorney on the sentence severity score are discussed later in this section when we address sentence disparity and evenhandedness.)

The sentence concession results for Burglary I are somewhat different. We noted above that the independent effect of burglary in a dwelling on the sentence severity score was not significant. It is apparent from Table 6.13 that the concession per *convicted* defendant as measured by Index A fell significantly in 1974 compared to 1973, but rose when measured by the other three indices. Index A weights nonincarceration sentences (years of probation, dollars of fine) relatively higher compared to jail or prison sentences than do the other three indices. Apparently more burglars who obtained a charge or count bargain in 1974 were receiving nonincarceration sentences; since these receive less (or zero) weight in Indices B, C, and D, the amount of concession measured against straight pleaders (who were receiving incarceration

[3] In discussing the independent effects of other types of plea bargains, number of original charges at each level of seriousness, etc. on sentence severity, we use the estimates based on Sentence Severity Index A as being illustrative of the others.

sentences more frequently) rose. As with robbers who pled guilty, burglars who obtained a charge or count bargain received less severe sentences than did straight pleaders. And the same pattern existed, in that count bargaining for burglary seemed to produce even greater sentence concessions than did charge bargaining. As with robbery, the sentences of burglars who pled guilty and were able to have other charges dropped or to make sentence agreements were not significantly different from those of straight pleaders. Unlike robbers, the number of counts of original felony charges (at any level) did not affect the sentence severity score.

SENTENCE VARIATION

Tables 6.15 and 6.16 display the frequency of sentence type and amount imposed, by conviction level, for Robbery I and Burglary I cases in both periods. For clarity, sentence categories have been aggregated into two nonincarceration categories (probation alone, and probation plus other—e.g., fine, restitution, community service, or rehabilitation program) and two incarceration categories (jail alone or with any nonincarceration sentence and prison alone or with any nonincarceration sentence). In addition, we show the average sentence severity score for Index A and a measure of its variability (the standard deviation).

For Robbery I cases, in which all defendants were initially charged with at least one count at the Felony A level, about half of the defendants were convicted at the Felony A level in 1973, whereas in 1974 almost all were so convicted. In 1973, 16 percent of those convicted were given nonincarceration sentences, whereas in 1974 this category decreased to 7 percent. Concomitantly, sentences with some prison time for Felony A level convictees rose from 73 percent in 1973 to 80 percent in 1974. The average sentence severity score increased about 30 percent from year to year, but the standard deviation showed little change over that period. In 1973, 33 percent and 50 percent of defendants convicted at the Felony B and C levels, respectively, received nonincarceration sentences.

For dwelling burglary cases, in which all defendants were initially charged with at least one count at the Felony A level, about 55 percent were convicted at the Felony A level, but in 1974 almost 90 percent were so convicted. At the Felony A conviction level, those nonincarcerated fell from 36 percent in 1973 to 20 percent in 1974; there was a corresponding increase over the two-year period in percent of prison sentences imposed. But the average sentence severity score rose only 15 percent over the time period, with some increase in variability (as measured by the standard deviation).

Different trends in average sentence severity and sentence variability were apparent in nondwelling burglaries. Most nondwelling burglars in 1973 were convicted at lesser charge levels; in 1974 most were convicted at the higher charge levels. At the higher conviction levels, the year-to-year average sentence severity score decreased or remained relatively constant, whereas at the lesser conviction levels it increased.

For dwelling and nondwelling burglaries together, conviction at the highest level rose from 45 percent in 1973 to almost 80 percent in 1974, with an accompanying rise in average sentence severity score and variability.

Tables 6.15 and 6.16 display *what* sentences were imposed and how often. Now we turn to *why*, i.e., how much of the observed variation in severity is accounted for

Table 6.15

DISTRIBUTION OF CONVICTEES IN MULTNOMAH COUNTY ROBBERY I CASES, BY CHARGE LEVEL AND TYPE OF PUNISHMENT, 1973-74

Charge Level	Percent of All Convictions at a Given Level													Sentence Severity Score (Index A)	
	Probation Only			Probation + Other			Jail Alone and Jail + Other			Prison Alone and Prison + Other					
	<2 yr	2-4 yr	≥5 yr	<2 yr	2-4 yr	≥5 yr	<6 mo	6-11 mo	≥12 mo	<2 yr	3-4 yr	5-10 yr	11-20 yr	≥21 yr	Mean	Standard Deviation
Felony A																
1973 (N = 19)	--	--	--	--	--	16	--	--	11	--	21	47	--	5	20.8	11.0
1974 (N = 59)	--	--	--	--	2	5	2	2	9	--	2	44	32	2	26.8	10.9
Felony B																
1973 (N = 9)	--	--	--	11	--	22	--	--	33	--	11	22	--	--	16.8	8.9
1974 (N = 2)	--	--	--	--	--	100	--	--	--	--	--	--	--	--	10.0	--
Felony C																
1973 (N = 10)	--	--	10	--	20	20	--	--	20	10	20	--	--	--	12.3	8.4
1974 (N = 2)	--	--	--	--	--	--	--	--	50	--	--	50	--	--	22.5	0.7
Misdemeanor																
1973 (N = 2)	--	--	--	--	--	--	100	--	--	--	--	--	--	--	5.5	6.4
1974 (N = 0)	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--

NOTE: Total number of convictions: 1973, N = 40; 1974, N = 63. A dash (--) denotes zero.

Table 6.16

Distribution of Convictees in Multnomah County Burglary I Cases, by Charge Level and Type of Punishment, 1973-74

Charge Level	Percent of All Convictions at a Given Level														Sentence Severity Score (Index A)	
	Probation Only			Probation + Other			Jail Alone and Jail + Other			Prison Alone and Prison + Other						
	<2 yr	2-4 yr	≥5 yr	<2 yr	2-4 yr	≥5 yr	<6 mo	6-11 mo	≥12 mo	<2 yr	3-4 yr	5-10 yr	11-20 yr	≥21 yr	Mean	Standard Deviation
Dwelling Burglary I																
Convicted Felony A																
1973 (N = 28)	--	3	11	--	4	18	4	7	21	--	--	28	4	--	17.7	7.6
1974 (N = 45)	--	2	7	--	--	11	2	--	22	--	11	41	4	--	20.4	8.1
Convicted Felony B																
1973 (N = 2)	--	--	--	--	--	50	50	--	--	--	--	--	--	--	13.0	4.2
1974 (N = 0)	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--
Convicted Felony C																
1973 (N = 13)	--	23	--	8	23	15	8	--	23	--	--	--	--	--	9.4	6.8
1974 (N = 5)	--	--	20	--	40	--	40	--	--	--	--	--	--	--	8.4	3.0
Convicted misdemeanor																
1973 (N = 8)	13	12	--	63	--	12	--	--	--	--	--	--	--	--	3.7	2.9
1974 (N = 1)	--	--	--	--	100	--	--	--	--	--	--	--	--	--	4.0	0
Nondwelling Burglary I																
Convicted Felony A																
1973 (N = 3)	--	--	33	--	--	33	--	--	--	--	--	33	--	--	16.0	10.4
1974 (N = 7)	--	--	14	--	--	14	30	--	14	--	--	14	14	--	13.5	11.7
Convicted Felony B																
1973 (N = 2)	--	--	--	--	--	100	--	--	--	--	--	--	--	--	10.0	0
1974 (N = 3)	--	--	--	--	--	33	33	--	33	--	--	--	--	--	9.6	9.3
Convicted Felony C																
1973 (N = 9)	--	22	--	--	33	22	11	--	11	--	--	--	--	--	7.6	5.9
1974 (N = 2)	--	--	--	--	50	--	50	--	--	--	--	--	--	--	11.1	6.9
Convicted misdemeanor																
1973 (N = 4)	--	25	--	50	25	--	--	--	--	--	--	--	--	--	3.6	1.9
1974 (N = 3)	--	--	--	--	67	--	33	--	--	--	--	--	--	--	4.3	2.9
All Burglary I																
Convicted Felony A																
1973 (N = 31)	--	3	13	--	3	19	3	7	19	--	--	30	3	--	17.5	7.7
1974 (N = 52)	--	2	8	--	--	11	6	--	21	--	10	36	6	--	19.3	9.0
Convicted Felony B																
1973 (N = 4)	--	--	--	--	--	75	25	--	--	--	--	--	--	--	11.5	3.0
1974 (N = 3)	--	--	--	--	--	33	33	--	33	--	--	--	--	--	9.6	9.3
Convicted Felony C																
1973 (N = 22)	--	23	--	5	27	18	9	--	18	--	--	--	--	--	8.6	6.4
1974 (N = 7)	--	--	14	--	43	--	43	--	--	--	--	--	--	--	9.2	4.0
Convicted misdemeanor																
1973 (N = 12)	8	17	--	49	8	8	--	--	--	--	--	--	--	--	3.7	2.6
1974 (N = 4)	--	--	--	25	50	--	25	--	--	--	--	--	--	--	4.3	2.4

NOTE: Total number of convictions: Dwelling Burglary I: 1973, N = 51; 1974, N = 51
Nondwelling Burglary I: 1973, N = 18, 1974, N = 15
All Burglary I: 1973, N = 69, 1974, N = 66.

A dash (--) denotes zero.

by various "legitimate" and "illegitimate" factors. Table 6.17 summarizes these results, which are taken from App. E. (In this discussion we focus only on how much of the variation is accounted for by each factor;[4] when we discuss the evenhandedness issue we will show the *direction* (i.e., more or less severity) of the effect as well.)

Our statistical analysis of sentencing variation in Robbery I cases shows that a very large percentage of the total variation (79 percent in 1973 and 61 percent in 1974) is accounted for by the factors that we included. About 48 percent is accounted for by legitimate factors in both years. The nature and number of original charges and the nature of plea bargaining were the most important of these legitimate factors in 1973, accounting for 30 percent; in 1974 they accounted for 18 percent. Prior criminal record (compared with no prior record) explained 13 percent of the variation in 1973 and 24 percent in 1974. Apparently prior criminal record contributed more to sentencing disparity in the later period. This was also true for the factor of age.

Illegitimate factors as a whole accounted for less of the variation in robbery sentences than did legitimate factors and their effect decreased over time (falling from 29 percent in 1973 to 11 percent in 1974). Minority status or the fact that a defendant had a private attorney (either retained or court-appointed) accounted for little or none of the disparity. Compared with defendants whose pretrial custody status included *both* time in jail and time out on bail or O.R., being in jail or on bail or O.R. exclusively accounted for a significant proportion of the variation explained by illegitimate factors. Compared with straight pleas, electing a jury trial had no significant effect on disparity. But electing a court trial did account for 11 percent of the variation in 1973, whereas in 1974 it had no effect. Our proxy for crowding of the correctional facilities accounted for a small amount of the sentence variation.

The results of our analysis of Burglary I cases were quite different. The factors used in the statistical analysis were able to explain a smaller percent of the total variation in sentencing (31 and 38 percent in the two years, respectively) than in our analysis of Robbery I cases. (This indicates that factors *excluded* from the burglary analysis account for more of the variation.) But *most* of the variation accounted for is explained by legitimate factors (25 and 28 percent in two years) and the effect of illegitimate factors is either very small or not significant. The nature of the charges and counts explains almost all of the legitimate variation accounted for in 1973 and over half of it in 1974; but prior criminal record also accounts for a significant portion in 1974.

In summary, we feel that sentencing variation performance measures are useful and that appropriate statistical analysis helps to reveal the extent to which various factors account for variation. In Multnomah County, we found that illegitimate factors accounted for little or none of the sentence disparity in burglary cases in both years, indicating evenhandedness in sentencing. In robbery cases, however, illegitimate factors (particulary pretrial custody status and election of court trials) accounted for a large, but decreasing over time, portion of sentence variation; such outcomes should trigger judges' attention. If these effects persist, it may be an

[4] The reader will note that in Table 6.17 we have employed rather weak standards for gauging statistical significance of the factors affecting sentencing variation. We have included effects that are statistically significant at the 50 percent level or higher (i.e., the changes are better than even that a particular factor has an effect that is not zero). Had we applied stricter standards, such as a 95 percent level of confidence (i.e., the chances are 95 percent or higher that the effect is not zero), more of the entries would be shown as not statistically significant (NS).

Table 6.17

EFFECTS OF SELECTED FACTORS ON SENTENCE SEVERITY IN MULTNOMAH COUNTY BURGLARY I AND ROBBERY I CASES, 1973-74

Factors Influencing Sentencing	Percent of Accountable Variance in Sentence Severity Score (Index A)[a]			
	Burglary I		Robbery I	
	1973	1974	1973	1974
"Legitimate" factors				
Age	1	3	3	6
Prior criminal record				
Minor	1	1	1	NS
Major	1	NS	1	11
Prison	2	5	11	9
Community ties	0	4	2	4
Nature of charges and counts	20	15	30	18
Aggregate variance explained	25	28	48	48
"Illegitimate" factors				
Minority status				
Black	1	NS	1	NS
Other	NS	--	1	NS
Pretrial custody status				
In jail	2	NS	3	5
On bail or O.R.	0	NS	9	3
Defended by private attorney				
Defendant-retained counsel	NS	NS	1	NS
Court-appointed counsel	NS	NS	1	NS
Convicted at trial				
Court	NS	1	11	NS
Jury	NS	0	0	NS
Proxy for correctional facilities crowding	NS	6	2	3
Aggregate variance explained	3	7	29	11
Total variance explained[b]	31	38	79	61

NOTE: NS = not significant.

[a]Entries are only for variables in regression equations that were statistically significant at the 95 percent level and in which the regression coefficient on that variable was statistically significant at the 50 percent level; entry reads NS otherwise.

[b]Including variance from NS variables and the constant term.

indication of inconsistency or lack of evenhandedness in sentencing over the long term.

EVENHANDEDNESS

In this discussion we focus exclusively on the effects on dispositional outcomes and sentences imposed that are attributable or related to illegitimate factors.[5] If illegitimate factors are influential in affecting these performance measures, one can conclude that defendants in similar circumstances are not being treated consistently or evenhandedly.

Dispositional Measures

We turn first to dispositional measures, such as case rejection rate at screening, felony charging rate, dismissal rate, trial rate, straight plea rate, charge bargain rate, acquittal rate, and so forth. For these measures we postulate that the following are illegitimate factors: ethnicity (or minority status), pretrial custody status, and type of defense counsel. Few would argue that ethnicity alone should be a legitimate influence on dispositional outcomes under our system of justice. Most would agree that pretrial custody status should not affect whether an arrestee is charged or rejected or whether a defendant is dismissed, pleads guilty, or is convicted or acquitted at trial. However, the fact that a defendant is in jail or out on bail or O.R. may *actually* be related to dispositional outcome. One argument holds that defendants out on bail or O.R. can help build a better defense by seeking out witnesses. Another contends that defendants in custody have more incentive to plea bargain, rather than demand a trial, especially in cases for which the probability of receiving a nonincarceration sentence is high—since this is a way of spending less total time in jail (pretrial and postconviction). Also, the failure to obtain pretrial release may be a proxy for the viciousness of the offense or the bad character of the defendant (in ways not fully reflected by the defendant's actual original charges, convicted charges, and prior criminal record).

Most would also agree that whether a defendant has the services of a public defender, a court-appointed attorney, or retains counsel himself should not influence his disposition. Again, the type of defense attorney may in fact influence outcomes. One argument is that a wealthier defendant who retains private counsel may be able to provide more resources in building a defense. Another is that public defenders may be able to achieve better results for the defendant because they know the court system and its practitioners better. Whichever view one holds, it is clearly useful to test whether these effects are present in a jurisdiction and, if so, how they are changing over time.

[5] We do not here consider the "evenhandedness" of delay, since delay (or elapsed time between important stages in adjudication) is jointly determined by court system characteristics (e.g., calendar crowding and management), by prosecutorial readiness, and by defendant-influenced factors (e.g., number of continuances requested by the defense in a case). The latter sometimes are deliberate attempts to slow down proceedings in the belief that it would redound to the defendant's advantage (e.g., the hope that a prosecution witness would be unavailable after the case has been continued several times). Thus, one could not say whether the court system is or is not evenhanded in the speediness of justice, if much of the variation in delay is defendant-induced. (And as we will show in the discussion of the delay issue, a large proportion of continuances granted *are* requested by the defense.)

As mentioned previously, our multivariate regression analysis was not successful in revealing the independent effects of various legitimate and illegitimate factors on the probability of dismissal or conviction (at various levels). Thus, we must turn to cross-tabulations as an analytical device for disclosing the presence or absence of such effects, keeping in mind the caveat that any such observed effects may be partially due to *other* factors. In Table 6.18, we show dispositional measures by pretrial custody status.[6] In robbery cases, most defendants were held in jail during both years. First we note that there seem to be some differences in disposition rates associated with jail or nonjail status, but differences between nonjail categories (bail or O.R.) are small, except for trial conviction rates. (The latter are based on very small trial sample sizes, however, so we cannot place much confidence in these estimates.) There are year-to-year differences in straight plea rates; in 1973 jailed defendants were more likely to plead guilty than nonjailed defendants, whereas in 1974 the differences were small. In 1973 jailed defendants were more likely to have their cases dismissed, whereas in 1974 the reverse was true. And in 1973 jailed defendants were less likely to be convicted (by any means) whereas in 1974 they were more likely to be convicted.

In burglary cases similar observations to those made for robbery cases hold for straight plea rates of jailed defendants in both years. In 1973 jailed defendants were less likely to plea bargain, but in 1974 the differences were more impressive. For most of the other types of disposition, custody status does not seem to have much effect.

Table 6.19 shows similar dispositional measures by type of attorney. Public defenders seem to be able to do better for defendants at burglary or robbery trials (in terms of the fraction of dispositions that are acquittals) than either type of private attorney, but again, most differences are not impressive. Public defenders seem to do marginally better for their clients than retained counsel, in terms of lower overall robbery conviction rates, in both time periods; in burglary cases, this seems to hold in 1974, but not in 1973. However, the differences are generally inconsiderable. In general, overall conviction rates for court-appointed attorneys are somewhere in between. Overall trial rates (i.e., the sum of trial convictions and trial acquittals, dismissals and mistrial cases in Table 6.19) reveal no consistent differences among types of defense counsel, implying that attorney fee compensation systems (described in Sec. V) do not seem to influence dispositional outcomes. But we cannot make high confidence inferences from such small samples. (The number of trials in our samples varied between 2 and 11 for a given combination of offense type, year, and type of attorney.[7]) There are also no consistent differences between straight plea or plea bargaining rates that can be associated with attorney type.

Table 6.20 shows the effect of minority status on dispositional outcomes. In

[6] Tables 6.18, 6.19, and 6.20 do not include *rejection* rate by any of the illegitimate factors. Ethnicity and pretrial custody status are usually not recorded in court or prosecution agency files for rejectees and often a suspect may not have an attorney at the time when the screening decision to reject is made.

[7] It is worth mentioning that manual data collection for rare events that would illuminate, say, the effect of attorney type on trial rate or the effect of trial convictions (compared with straight plea convictions) on sentence severity (discussed below), or the effect of trials on *any* other performance measure is very expensive. For example, if one wanted to be certain to obtain a sample of at least 20 burglars in 1973 who were defended at trial by court-appointed attorneys, and if burglaries represented 20 percent of all felonies, court-appointed attorneys represented 20 percent of all burglars, and only 10 percent of all burglary cases went to trial, one would need to examine 5000 case records for felonies occurring in a one-year period, on the average.

Table 6.18

Evenhandedness: The Relationship between Pretrial Custody Status and Dispositional Measures in Multnomah County Robbery I and Burglary I Cases, 1973-74

Dispositional Measure	Percent of All Defendants in a Given Pretrial Custody Status											
	Robbery I						All Burglary I					
	1973			1974			1973			1974		
	Jail	Bail	O.R.	Jail	Bail	O.R.	Jail	Bail	O.R.	Jail	Bail	O.R.
Not convicted												
Pretrial dismissal	46	23	35	7	14	20	24	29	15	17	25	14
Trial acquittal, dismissal, mistrial	9	15	10	10	14	13	9	0	2	2	13	0
Total	55	38	45	17	28	33	33	29	17	19	38	14
Convicted												
Straight plea	19	0	0	59	58	47	20	0	13	46	50	58
Received plea bargain (reduced charges and/or counts)	22	47	50	9	14	0	36	71	63	25	0	17
Gross plea rate (pleas ÷ N)	41	47	50	68	72	47	56	71	76	71	50	75
At trial	4	15	5	15	0	20	11	0	7	10	12	11
Conviction rate (conv. ÷ N)	45	62	55	83	72	67	67	71	83	81	62	86
All dispositions (N)	100(54)	100(13)	100(20)	100(58)	100(7)	100(15)	100(45)	100(7)	100(40)	100(40)	100(8)	100(36)
Trial conviction rate (trial convictions ÷ trials)	29	50	33	60	0	60	55	0	78	83	48	100

Table 6.19

Evenhandedness: The Relationship between Type of Defense Attorney and Dispositional Measures in Multnomah County Robbery I and Burglary I Cases, 1973-74

Dispositional Measure	Percent of All Defendants with a Given Type of Defense Attorney											
	Robbery I						All Burglary I					
	1973			1974			1973			1974		
	PD	CA	DR	PD	CA	DR	PD	CA	DR	PD	CA	DR
Not convicted												
Pretrial dismissal	45	44	38	12	13	12	17	18	30	22	15	10
Trial acquittal, dismissal, hung jury	7	17	0	18	7	0	4	9	7	5	0	0
Total	52	61	38	30	20	12	21	27	37	27	15	10
Convicted												
Straight plea	13	4	0	51	60	38	20	0	7	49	49	60
Received plea bargain (reduced charges and/or counts)	35	26	39	5	7	25	57	27	48	19	18	20
Gross plea rate (pleas ÷ N)	48	30	39	56	67	63	77	27	55	68	67	80
At trial	0	9	23	14	13	25	2	46	8	5	18	10
Conviction rate (conv. ÷ N)	48	39	62	70	80	88	79	73	63	73	85	90
All dispositions (N)	100(54)	100(23)	100(13)	100(43)	100(30)	100(8)	100(54)	100(11)	100(27)	100(41)	100(33)	100(10)
Trial conviction rate (trial convictions ÷ trials)	0	35	100	44	65	100	33	83	50	50	100	100

[a]PD = public defender; CA = court-appointed; DR = defendant-retained.

Multnomah County, over half of the case files do not identify defendant ethnicity, so sample sizes are small. This means that only very large differences in dispositional rates among ethnic groups will be statistically significant. Compared with nonminority defendants, black defendants tend to have higher pretrial dismissal rates, are somewhat less likely to plead guilty, and are less likely to be ultimately convicted. However, the differences generally are weak. Year-to-year changes in these disparities for robbery are small; but in 1974 burglaries the effect of minority status disappears. To the extent that such small samples permit *any* inferences, our findings suggest either that cases against blacks tended to be weaker, reflecting overarrests by the police or overprosecution by the district attorney's office, or that a double standard is applied to black defendants. Given the data at our disposal we could not resolve the question of which hypothesis best explains the observed differences. Moreover, one must keep in mind the caveat that whatever differences are revealed by cross-tabulating dispositional rates by ethnic group, some of the observed differences may be due to *other* factors.

Sentence Severity Measures

In our discussion of sentencing disparity we showed how much of the total variation explained in our statistical analysis was attributable to each of the illegitimate factors, including ethnicity, pretrial custody status, type of defense attorney, and method of conviction at trial. Here we try to show, in addition, the *magnitude* and *direction* of each significant effect, by addressing the question: What is the magnitude and direction of the change in average sentence severity score (for Index A) associated with a given illegitimate factor? Table 6.21 summarizes the results of the multivariate statistical analysis for the offenses of robbery and burglary for 1973 and 1974. This procedure makes it possible to hold constant the influence on sentence severity (as measured by Index A) of all specified factors other than the factor being examined. That is, we can estimate the independent effect on sentence severity score imposed, for example, of having obtained a pretrial release, while holding constant the influences of other potential causative factors—such as charges and counts, age, prior record, type of counsel, and method of conviction. For each independent causative factor, Table 6.21 indicates both the direction of influence and its magnitude, expressed as a proportion of the average sentence severity score (Index A) imposed in the given offense/year combination.

No clear trends emerged on the effect of minority status in 1973, partly because of the small samples. Black burglars fared 24 percent worse than white burglars in 1973, but black robbers fared 24 percent better, in terms of the average Sentence Severity Index A score. Other minorities (mainly Oriental and American Indian in Multnomah County) were treated the same as whites in 1973 burglary cases, but fared 40 percent worse in robbery cases. However, in 1974 minority status was *not* significantly related to sentence severity in either offense, suggesting more even-handed treatment of offenders.

Independent effects of pretrial custody status are also mixed. Compared with defendants who spent part of their pretrial time in jail and part out on O.R. or on bail, burglars in 1973 who were in jail all the time fared 50 percent worse, whereas defendants who were never jailed fared no better or worse. For burglaries in 1974, custody status had no significant effect. In robberies, jailees fared worse in both years, but inexplicably, those out on bail or O.R. all the time fared even worse. These

Table 6.20

Evenhandedness: The Relationship between Ethnicity and Dispositional Measures in Multnomah County Robbery I and Burglary I Cases, 1973-74

Dispositional Measure	Percent of All Defendants in a Given Ethnic Group[a]											
	Robbery I						All Burglary I					
	1973			1974			1973			1974		
	NM	B	OM	NM	B	OM	NM	B	OM	NM	B	OM
Not convicted												
Pretrial dismissal	36	63	25	5	50	0	9	42	0	25	25	100
Trial acquittal, dismissal, hung jury	21	6	25	23	0	0	9	8	0	0	0	0
Total	57	68	50	28	50	0	18	50	0	25	25	100
Convicted												
Straight plea	29	19	0	58	50	50	18	17	0	50	37	0
Received plea bargain (reduced charges and/or counts)	14	13	50	5	0	0	46	16	100	8	25	0
Gross plea rate (pleas ÷ N)	43	32	50	63	50	50	64	33	100	58	62	0
At trial	0	0	0	9	0	50	18	17	0	17	13	0
Conviction rate (conv. ÷ N)	43	32	50	72	50	100	82	50	100	75	75	0
All dispositions (N)	100(14)	100(16)	100(4)	100(22)	100(6)	100(2)	100(11)	100(12)	100(1)	100(12)	100(8)	100(1)
Trial conviction rate (trial convictions ÷ trials)	0	0	0	28	0	100	67	67	--	100	--	--

NOTE: In Multnomah County, well over half of the case files and other records do not identify defendant ethnicity.

[a]NM = nonminority; B = black; OM = other minorities.

Table 6.21

EVENHANDEDNESS: THE INDEPENDENT EFFECT OF ILLEGITIMATE FACTORS ON SENTENCE SEVERITY IMPOSED IN MULTNOMAH COUNTY BURGLARY I AND ROBBERY I CASES, 1973-74

Illegitimate Factor	Percent Change in Average Sentence Severity Score (Index A) with a Change in a Given Factor[a]			
	Burglary I		Robbery I	
	1973	1974	1973	1974
Ethnicity[b]				
Black	+24 (57%)	NS	-24 (71%)	NS
Other	NS	--	+40 (71%)	NS
Pretrial custody status[c]				
In jail	+50 (86%)	NS	+111 (93%)	+39 (96%)
On bail or O.R.	NS	NS	+250 (90%)	+50 (72%)
Defended by private attorney[d]				
Defendant-retained	NS	NS	+37 (85%)	NS
Court-appointed	NS	NS	+20 (61%)	NS
Convicted at trial[e]				
Court				
Jury	+18	+8	--	-9

NOTE: NS = not significant. + = sentence severity score increased; - = score decreased. Entries in parentheses are the levels of statistical significance of the regression coefficient.

[a]Entries are only for variables in regression equations which were statistically significant at the 95 percent level of confidence and in which the regression coefficient on that variable was statistically significant at the 50 percent level; entry NS (not significant) otherwise.

[b]Measured against white (majority) status.

[c]Measured against mixed pretrial custody status.

[d]Measured against defense by public defender.

[e]Measured against straight plea conviction. Entries are computed from matching pairs of defendants and cases that are similar in most respects; however, one was convicted at trial and the other pled guilty to all original charges (see discussion in text).

data must be interpreted as being inconclusive in gauging trends in evenhandedness in sentencing as affected by custody status.

Burglars defended by the public defender's office fared no better and no worse in both years than those defended by a private attorney. This was also true for robbery defendants in 1974. But robbery defendants in 1973 had sentences imposed that were 20 or 37 percent more severe if they were defended by court-appointed or retained counsel, respectively. From these data we can conclude that the type of defense attorney had little independent effect on sentence severity score.

Unfortunately, the number of trial convictions in our yearly random samples of 100 burglaries and 100 robberies was very small; therefore, we cannot rely on the regression results reported in App. E. Alternatively, we attempted to select matched pairs of convictees in each sample differing only in whether they were convicted at

trial or pleaded guilty to all original counts and charges. These cases were matched exactly on the following characteristics: year, offense (burglary, whether dwelling or nondwelling), number and level of original and convicted charges, and prior criminal record. If possible, the cases were also matched on defendant custody status and age. We then computed the percentage difference in mean sentence severity score for the group convicted at trial and the the group who made straight pleas of guilty. These results are shown in Table 6.21. Because of too few trials, we could not make meaningful estimates for robberies in 1973. For the other three entries, numbers of trials (both court and jury) varied between six to eight. From these small samples it would seem that convictions at trial result in an increase in sentence severity of about 8 to 18 percent over those imposed on straight pleaders. However, this "penalty" imposed by the court system does not account for the probability that a defendant will be acquitted at trial or have the charges reduced (presumably resulting in a lesser sentence). For 1974 robberies, the trial effect is to slightly *reduce* sentence severity. Thus, to the extent we can conclude *anything* from these small samples, it would seem that trials have little effect on sentence severity compared with straight pleas.

In summary, our findings regarding evenhandedness of dispositional and sentencing outcomes as affected by minority status, pretrial custody status, type of attorney, and choice of trial or straight plea are:

Minority Status. Because ethnicity data were not recorded in well over half of the cases examined, sample sizes by ethnic group were quite small and statistically reliable inferences were difficult to draw. For these small samples, black robbery defendants in both years tended to have higher dismissal rates and lower guilty plea and overall conviction rates, suggesting either overarrests by the police or overprosecution by the district attorney's office or the application of a double standard. Given these data, we could not resolve the question of which hypothesis best explains the observed differences. These same ethnic differences were also present in burglaries in 1973 but disappeared in 1974, indicating a trend toward more evenhandedness in dispositions. Imposed sentence severity differences associated with ethnic group were mixed in both offenses in 1973 but these differences disappeared in 1974, indicating a trend toward more evenhandedness in sentencing.

Pretrial Custody Status. Some differences in dispositional rates associated with being in custody or not (out on bail or O.R.) were observed in burglaries and robberies in 1973. But these differences generally disappeared in 1974, indicating a movement toward more evenhandedness in dispositions. Jailed burglars and robbers tended to have more severe sentences imposed in 1973, but pretrial custody status had no effect in 1974 burglaries. However, custody status effects were mixed in 1974 robberies. Consequently, these data must be interpreted as being inconclusive in gauging custody status effects on the evenhandedness of sentencing.

Type of Defense Attorney Compared to private attorneys, public defenders seemed to achieve higher dismissal rates for burglary and robbery defendants in both years and somewhat lower likelihood of overall conviction in

robbery cases, but little differences associated with type of attorney were observed in trial or straight guilty plea or plea bargaining rates. Sentence severity scores were not affected by the type of attorney in burglary cases in both years and in 1974 robberies, but 1974 robbery defendants defended by private attorneys received somewhat more severe sentences. In short, although there were some dispositional and sentencing outcomes that were somewhat more favorable for defendants having public defender representation, our general conclusion is that type of defense attorney had little effect.

Trial versus Straight Plea. To the limited extent we can conclude *anything* from our small samples of defendants convicted at trial, it would seem that conviction at trial imposed little or no penalty in terms of sentence severity imposed compared with similar defendants making straight pleas of guilty. A more statistically reliable analysis would require larger sample sizes of defendants who choose a trial.

HOW THE COURT SYSTEM TREATS DEFENDANTS WITH PRIOR CRIMINAL RECORDS

Although not an issue that fits neatly into one of the categories we have addressed in this study, it seems useful to examine how a jurisdiction treats defendants with prior criminal records as compared with those with no prior record. National concern with the "habitual offender" or the "career criminal" is evidenced by recent LEAA action grants aimed at focusing special prosecutorial resources on the career criminal in several jurisdictions, and by at least one LEAA research grant aimed at examining the nature and number of habitual offenders, their impact on criminal behavior, their contacts with public agencies, the impact of public agencies on their behavior, and alternative programs for dealing with segments of the habitual offender population.

Dispositional Measures

In Table 6.22 we show dispositional measures by the four criminal record categories[8] (none, minor, major, prison) employed by the California Bureau of Criminal Statistics. We show only dispositional measures for defendants arraigned in circuit court. Prosecutorial rejection rate, by prior record, is not shown because data on the suspect's prior criminal record seldom appear in the prosecutor's files for those who are rejected. Again, the reader must keep in mind the caveat that observed differences shown in cross-tabulations of performance measures by prior record may be partially due to *other* factors.

From Table 6.22 it appears that the only prominent difference associated with prior record is that 1974 robbery defendants with more serious prior records are not convicted more often than those with less serious records. Otherwise, observed differences were insubstantial. From Table 6.22, then, it is fair to conclude that no special attention was focused on burglary or robbery defendants with prior criminal

[8] See Sec. IV for simplified definitions of these categories.

Table 6.22

THE RELATIONSHIP BETWEEN PRIOR RECORD AND DISPOSITIONAL MEASURES IN MULTNOMAH COUNTY ROBBERY I AND BURGLARY I CASES, 1973-74

Dispositional Measure	Percent of All Defendants with a Given Prior Record Category															
	Robbery I								All Burglary I							
	1973				1974				1973				1974			
	None	Minor	Major	Prison	None	Minor	Major	Prison	None	Minor	Major	Prison	None	Minor	Major	Prison
Not convicted																
Pretrial dismissal	39	47	32	31	12	21	9	6	17	28	7	33	17	14	11	27
Trial acquittal, dismissal, mistrial	4	20	16	0	5	21	9	11	0	11	14	9	2	0	0	9
Total	43	67	48	31	17	42	18	17	17	39	21	42	19	14	11	36
Convicted																
Straight plea	7	6	15	23	65	37	82	55	15	6	36	8	52	43	66	28
Received plea bargain (reduced charges and/or counts)	46	27	37	31	6	7	0	11	62	44	36	42	21	14	17	27
Gross plea rate (pleas ÷ N)	53	33	52	54	71	44	82	66	77	50	72	50	73	57	83	55
At trial	4	0	0	15	12	14	0	17	6	11	7	8	8	29	6	9
Conviction rate (conv. ÷ N)	57	33	52	69	83	58	82	83	83	61	79	58	81	86	89	64
All dispositions (N)	100(26)	100(15)	100(19)	100(13)	100(34)	100(14)	100(11)	100(18)	100(47)	100(18)	100(14)	100(12)	100(48)	100(7)	100(18)	100(11)
Trial conviction rate (trial convictions ÷ trials)	50	--	0	100	67	40	--	60	100	50	33	50	80	100	--	50

records and that no improvements in conviction rate or conviction level of these defendants resulted in either year. The higher conviction rates and conviction levels for *all* burglary and robbery defendants in 1974 were associated with the No Plea Negotiation Experiment (as discussed under Screening Accuracy and Plea Bargaining). But the experiment did not intend to focus special attention on defendants with heavy prior criminal records, and thus we would not have expected differential treatment for defendants with prior records.

Sentence Severity Measures

Table 6.17 showed the percentage of the total variation in sentence severity score (Index A) accounted for by prior criminal record. In Table 6.23, we show the size and direction of the independent effect of prior record, in terms of the percent of change in sentence severity score (Index A) associated with a minor, major, or prison record. The change in the score is measured against defendants with *no* prior criminal record.

In robbery cases in both years, the effect of a prior record was to increase the sentence severity score by 30 to 69 percent, depending on year and category of prior record. Although there were some inconsistencies, the more serious prior records tended to be associated with higher sentence severity scores. This observation also held for 1974 burglary defendants. Compared with defendants with no prior record, 1973 burglary defendants with a minor prior record received somewhat more severe sentences, but defendants with more serious prior records received *less* severe sentences. This latter anomalous result is not explainable in terms of the gross data we collected, but one may speculate that for 1973 burglary defendants with prior major or prison records, there may have been mitigating circumstances associated with their background, case, or behavior that would account for their lower sentences.

In summary, then, dispositional outcomes in burglary and robbery cases were generally not affected by the prior record of defendants in either year. There was a trend toward more severe sentences for defendants with some prior criminal record, and, in general, the more serious the prior record, the more severe the sentence. This kind of analysis is useful in establishing "current practice" with regard to treatment of "habitual offenders" in a jurisdiction. If a special program were introduced that focused on offenders with serious prior records, dispositional and sentence outcomes under the program could be evaluated by comparing them with "current practice" outcomes.

DELAY

We next apply several performance measures to illuminate the speediness of justice from a variety of viewpoints. First we show *what* delays there were in Multnomah County, in terms of three measures of elapsed time between major events—median number of days, minimum number of days for the longest 10 percent of cases, and the percent of cases exceeding some elapsed time standard—and in terms of continuances. Then, we attempt to analyze *why*—that is, we estimate the magnitude and direction of the change in one elapsed time measure associated with selected factors that we hypothesized would influence the speediness of justice.

Table 6.23

THE INDEPENDENT EFFECT OF PRIOR CRIMINAL RECORD ON SENTENCE SEVERITY IN MULTNOMAH COUNTY BURGLARY I AND ROBBERY I CASES, 1973-74

	Percent Change in Sentence Severity Score (Index A) Associated with a Given Category of Prior Record[a]			
	Burglary I		Robbery I	
Prior Record[b]	1973	1974	1973	1975
Minor	+20	+19	+39	NS
Major	-33	NS	+30	+54
Prison	-44	+59	+69	+45

[a] Entries are only for variables in regression equations that were statistically significant at the 95 percent level and in which the regression coefficient on that variable was statistically significant at the 50 percent level; entry NS (not significant) otherwise.

[b] Measured against no prior record.

Table 6.24 displays the several measures of delay for four offense categories (a random sample of all felonies, robberies, dwelling burglaries, and nondwelling burglaries) in both years. For all felonies, the median number of days from arrest or arraignment to dismissal, or to guilty plea or to final disposition[9] showed fairly consistent year-to-year increases. Very large year-to-year increases in time between conviction and sentencing are also apparent, probably largely because of the more frequent use of pre-sentence investigation reports in 1974, particularly in the more serious offenses. The median number of days between arrest and trial and between arraignment and trial also rose consistently from year to year. And the minimum time for the longest 10 percent of cases showed fairly consistent year-to-year increases between major events, although the small sample size (e.g., the longest 10 percent of 20 cases dismissed in a given offense year sample equal two cases) makes for low confidence in this statistic.

For the two Impact offenses of robbery and dwelling burglaries, year-to-year trends were mixed. Some elapsed time measures showed year-to-year decreases or no change (e.g., median time between arrest and dismissal, from arrest or arraignment to final disposition or to guilty plea for robberies; from arrest or arraignment to guilty plea, and from arraignment to final disposition for dwelling burglaries). Still others rose in 1974, such as times between arrest or arraignment to trial. Elapsed time measures for dwelling burglaries behaved similarly to elapsed time measures for all felonies.

The major point to note is that Impact offenses did *not* generally contribute to

[9] Final disposition is taken as the date of dismissal, nonconviction at trial, or sentencing as a result of a guilty plea or a conviction at trial.

Table 6.24

Measures of Elapsed Time in Multnomah County Felony Cases, 1973-74

Type of Disposition	All Felonies 1973	All Felonies 1974	Robbery I[a] 1973	Robbery I[a] 1974	Dwelling Burglary I[a] 1973	Dwelling Burglary I[a] 1974	Nondwelling Burglary I[a] 1973	Nondwelling Burglary I[a] 1974
	Median Number of Days							
From arrest to--								
Dismissal	29	41	63	26	42	47	17	71
Guilty plea	57	51	65	54	65	51	66	82
Trial	56	84	51	81	52	93	61	118
Final disposition	62	77	86	86	85	97	61	97
	Minimum Number of Days for Longest 10 Percent of Cases							
From arrest to--								
Dismissal	197	154	--	--	--	--	--	--
Guilty plea	90	93	--	--	--	--	--	--
Trial	107	149	--	--	--	--	--	--
Final disposition	159	151	--	--	--	--	--	--
	Percent of Cases Exceeding 60-Day Standard							
From arrest to--								
Dismissal	31	38	51	38	40	--	25	55
Guilty plea	48	44	66	34	52	44	58	63
Trial	50	100	36	69	38	83	--	--
All cases[b]	42	46	57	42	50	46	46	61
	Median Number of Days							
From arraignment to--								
Dismissal	29	46	79	--	78	--	69	84
Guilty plea	23	29	37	21	33	31	29	21
Trial	30	49	35	52	30	79	25	72
Final disposition	34	63	71	64	68	68	32	61
	Minimum Number of Days for Longest 10 Percent of Cases							
From arraignment to--								
Dismissal	184	110	--	--	--	--	--	--
Guilty plea	85	82	--	--	--	--	--	--
Trial	53	92	--	--	--	--	--	--
Final disposition	145	130	--	--	--	--	--	--
	Median Number of Days							
From conviction to--								
Sentencing	2	34	41	35	19	33	2	31

[a] Entries for minimum number of days for longest 10 percent of cases are omitted because of the small size of the sample.

[b] Excluding elapsed time between conviction and sentencing.

year-to-year increases in delay experienced by the felony caseload as a whole, suggesting that Impact cases were expedited, consciously or unconsciously.

In Table 6.24 we have also included a measure of the extent to which the 60-day time standard is being met. The standard actually applies only to the time from arrest to trial (for individual cases); however, it is useful to show separately the percent of cases exceeding 60 days from arrest to dismissal, to guilty plea, to trial, and to "final adjudication" (i.e., dismissals, guilty pleas, and trials together, *excluding* the time between conviction and sentencing). Although the *median* number of days from arrest to dismissal, guilty plea, or trial was less than 60 days for all felonies in 1973, 31, 48, and 50 percent of those dispositions, respectively, exceeded the 60-day standard. And in 1974 larger fractions of those dispositions exceeded the 60-day standard.

In general, a greater fraction of 1973 robbery and burglary dispositions exceeded the standard compared with all felonies taken together. In 1974, fewer robbery dispositions exceeded the time standard. One point of interest is that in 1974, *most* trials for robbery, dwelling burglaries, and all felonies exceeded the standard, whereas half or less did so in 1973. In summary, a very large proportion of adjudicated cases exceeded the time standard and, in general, delay was worse during 1974.

Although they provide only indirect measures of overall delay, continuance measures provide additional insight, particularly on which classes of practitioners are responsible for delay and how continuance policy is being applied. Table 6.25 displays a number of continuance measures, by contested (trials) and uncontested disposition, for all felony cases in both periods. For uncontested cases, about one-third of all cases were continued in both periods, but the average number of continuances per case rose in 1974. Since the average continuance was 15 to 16 days in both periods, this meant that the average number of continued days per uncontested case (continued and noncontinued) rose from 7 in 1973 to 9 in 1974. Half or more of the continuances were attributed to the defense in both periods; those attributed to the prosecution declined in 1974.

For contested cases, the record was somewhat different. Compared with uncontested cases, a greater fraction of contested cases are continued and more continuances are granted in the average case; moreover, both measures increased in 1974 over 1973. But since the average continuance declined from 14 to 9 days over the two years, the number of continued days per average contested case remained relatively constant. Unlike uncontested cases, only about 25 to 30 percent of trial case continuances are attributable to the defense; the percent attributable to the prosecution declined over time. In contested cases, a large proportion of continuances are attributable to the court and jointly to the prosecution and defense in both periods.

We turn next to an analysis of what affects delay. We selected average elapsed time between arraignment and final disposition as a reasonable overall measure of the delay introduced into felony proceedings in circuit court. We hypothesized that four factors could influence this measure of delay.[10] Pretrial custody status could affect elapsed time; defendants on bail or O.R. might seek to delay proceedings for their advantage, whereas defendants in jail might have less incentive to ask for a

[10] Since we had no prior hypotheses as to why factors *other* than the four selected should influence delay, we do not show their effect. The results of the statistical analysis displayed in App. E, do, however, include the effects of other (control) variables on elapsed times.

Table 6.25

CONTINUANCE MEASURES IN MULTNOMAH COUNTY, 1973-74
(Based on a 100-case sample of all felony cases in each period)

Continuance Measure	1973	1974
For uncontested cases		
Number of cases continued ÷ all cases (%)	33	31
Number of continuances ÷ all cases	0.46	0.59
Number of days continued ÷ number of continuances	15	16
Number of days continued ÷ all cases	7	9
Percent of total number of continuances attributed to--		
Defense	57	49
Prosecution	26	15
Court and other, including unidentified cases[a]	17	36
(N)	(73)	(80)
For contested cases		
Number of cases continued ÷ all cases (%)	45	60
Number of continuances ÷ all cases	1.00	1.40
Number of days continued ÷ number of continuances	14	9
Number of days continued ÷ all cases	14	13
Percent of total number of continuances attributed to--		
Defense	25	29
Prosecution	35	19
Court and other, including unidentified cases[a]	40	52
(N)	(20)	(15)

[a]Attributed to court alone, defense and prosecution jointly, and unidentified attribution.

continuance because it would increase pretrial jail time. Type of defense attorney could affect delay, especially if one category of attorney tended to know the system better than another; however, we had no prior hypothesis as to which type of attorney would be associated with longer or shorter elapsed times. A trend over time toward heavier caseloads (or court calendar crowding), we hypothesized, should result in increased delay. Finally, the type of disposition—dismissal, plea bargain, or trial—could affect delay. Compared with straight pleas, we hypothesized that dismissed cases should be shorter and cases disposed by plea bargain or trial should be longer on the average.

Table 6.26 shows these results for the times between arraignment and final disposition in all felonies and in robberies for both periods. Pretrial custody status had little or no effect on court delay overall (i.e., for the entire felony caseload) in either year. Compared with robbery defendants who spent part of pretrial time in jail and part time out on bail or O.R., defendants in jail exclusively or out of jail

Table 6.26

THE INDEPENDENT EFFECT OF HYPOTHESIZED INFLUENCES ON ELAPSED TIME FROM ARRAIGNMENT TO FINAL DISPOSITION FOR ALL FELONIES AND FOR ROBBERY I IN MULTNOMAH COUNTY, 1973-74

	Percent of Change in Average Elapsed Time[a]			
	All Felonies		Robbery I	
Influence on Elapsed Time	1973	1974	1973	1974
Pretrial custody status[b]				
Jail	-13	NS	+95	+44
Bail or O.R.	NS	NS	+74	+88
Defended by private attorney[c]	+76	+48	+84	-37
Type of disposition[d]				
Dismissed	NS	-93	NS	-66
Plea bargained	NS	-68	+98	+110
Tried	-60	+51	-67	+98
Proxy for court calendar crowding	+9	+5	+7	+2
Percent of variance explained by all factors considered	(12)	(18)	(26)	(33)

[a]Entries are only for variables in regression equations which were statistically significant at the 95 percent level and in which the regression coefficient on that variable was statistically significant at the 50 percent level; entry NS (not significant) otherwise.

[b]Measured against mixed custody status.

[c]Measured against defense by public defender.

[d]Measured against straight plea conviction.

exclusively tended to suffer more delay, and the relative effect varied from year to year. In 1974, being out on bail or O.R. introduced more delay than being held in jail. In 1973, the effects where inexplicably reversed, but the difference in magnitude was small.

The independent effect of being represented by a private attorney (whether retained or court-appointed) compared with public defender representation was to lengthen the arraignment-to-final-disposition period by approximately 50 to 75 percent (depending on year) in all felony cases. This effect held true for robbery cases in 1973, but was reversed in 1974. In general, though, we can conclude that private attorneys introduce more delay in felony proceedings. Two hypotheses come to mind to explain these findings: Either private attorneys deliberately ask for, and are granted, more continuances in the hopes of more favorable (to the defendant) sentences, or they ask for, and are granted, continuances because of more calendar conflicts among the cases they handle. Given these data, we cannot choose between these hypotheses.

We found that there was a small, but a highly (statistically) significant positive effect of court calendar crowding on elapsed time. This effect varied between 2 and 9 percent depending on year and type of offense. Since backlog and filings steadily

increased over this two-year period (see Table 6.2 above) our hypothesis is confirmed. However, the small size of the effect is somewhat surprising.

Having a case dismissed, compared with a straight plea, had no effect in 1973 for either robbery cases or all felony cases, but in 1974 there was a large decrease in delay associated with this type of disposition, confirming our hypothesis. Plea bargaining was associated with more delay in robbery cases in both periods, but in the average felony case the effects were mixed (no effect in 1973, but *less* delay in 1974). Inexplicably, for both offense categories, going to trial was associated with *less* delay in 1973, but *more* delay in 1974. Overall, therefore, these data must be viewed as inconclusive with respect to the independent influence of type of disposition on delay.

USE OF VICTIMS, OTHER WITNESSES, AND JURORS

As indicated in App. D, essentially no data were recorded in available court records that would allow us to estimate measures of the use of victims, other witnesses, or jurors. The sole exception was data on the number of victims and witnesses called per trial,[11] which is only one ingredient necessary for estimating the number of witness and victim appearances per disposition. Consequently, we used responses from these lay participants to our mail survey questionnaires to make rough estimates of such measures.[12]

Table 6.27 displays the resulting measures of the use of victims and witnesses. The data reflect cases that were active during March through August 1974; we selected older cases to be certain that they would be closed by the time the mail surveys were administered (early fall 1975), since we were interested in the victims' and witnesses' knowledge of the case outcome, among other things.

The survey responses indicated that the overwhelming proportion (about 90 percent) of victims and other witnesses were cooperative in the proceeding and only a few percent indicated that they were not asked for their cooperation. The average number of appearances by the victim (2.5) was slightly higher than that for other witnesses (1.9). But since the number of victims called per trial (about 1.0) was less than the number of other witnesses called per trial (2.5), the resulting victim or witness appearances per disposition were 2.5 and 2.9, respectively. Average victim time per appearance (1.8 hours) was about the same as that of other witnesses (1.9 hours).

Table 6.28 displays measures of the use of juror time based on responses from jurors who served during the month of June 1975. The major finding is that about 40 percent of juror time (on the average) was spent unproductively waiting in the jury room or elsewhere. About half their time was spent on criminal cases, split fairly evenly between voir dire and in trial. And about 40 percent of their time, split evenly between voir dire and in trial, was spent on civil cases. (Notice that the average time per activity, when summed over all activities, is in excess of 100 percent; apparently responding jurors neglected to allocate their time accurately across activity categories. Thus, *ratios* between categories is a more meaningful

[11] Data were not recorded on the number of witnesses or victims called per *uncontested* case.

[12] Since the mail surveys rely on the memories of victims, witnesses, and jurors, the measures must be viewed as very rough approximations which cannot be checked for accuracy.

Table 6.27

Measures of the Use of Victims and Witnesses in Multnomah County, March-August 1974

Type of Response from Lay Participants	Victims	Other Witnesses[a]
Cooperativeness		
Cooperative (%)	89	93
Not cooperative (%)	6	3
Not asked (%)	5	4
Total responses (N)	100 (105)	100 (89)
Average number of appearances by lay participants	2.5	1.9
Average number of appearances by lay participants per disposition	2.5[b]	2.9[c]
Duration of appearances by lay participants		
Less than 1 hour (%)	24	14
One to two hours (%)	50	48
Three hours (all morning or afternoon) (%)	21	34
Six hours (all day) (%)	5	4
Total responses (N)	100 (93)	100 (85)
Average duration of appearance (hr)	1.8	1.9

SOURCE: Responses of victims and other witnesses to Rand mail surveys, except for number of victims or other witnesses called per trial (see below).

[a]Primarily witnesses for the prosecution.

[b]Assumes one victim per disposition times 2.5 victim appearances per victim = 2.5 victim appearances per disposition.

[c]1.5 witnesses per trial disposition (calculated from trial court records) times 1.9 witness appearances per witness = 2.9 witness appearances per disposition.

Table 6.28

Measures of the Use of Juror Time in Multnomah County, June 1975 (Percent of time spent)

Activity	Percent of Jurors Responding					Average Percent of Time Spent
	None	Less Than 25	25-49	50-74	75-100	
Waiting in jury room or elsewhere (N = 163)	0	29	37	26	8	41
Jury selection: criminal cases (N = 159)	0	70	24	3	3	22
In trial: criminal cases (N = 173)	6	46	31	12	5	28
Jury selection: civil cases (N = 155)	3	71	20	3	3	20
In trial: civil cases (N = 168)	18	50	23	5	4	19

measure.) If we take time in civil trial as an index of 100 percent, jurors tend to spend about the same time in voir dire for civil trials, about 220 percent as much time in voir dire for criminal trials, 280 percent as much time in criminal trials, and about 400 percent as much time waiting unproductively.

THE USE OF JUDICIAL TIME: THE WEIGHTED CASELOAD APPROACH

One objective of our study was to analyze the use of judicial time in various court activities in felony criminal proceedings. Although the analysis is described below, we rejected the results because of deficiencies in the available raw data, as explained later. Our experience illustrates how difficulties can be encountered in working with court data generated for another purpose. Our failure to obtain acceptable results does not imply that the objective was infeasible, but only that its implementation required data collection efforts beyond the means of our study.

The vehicle of analysis was the so-called "weighted caseload" approach (described in Sec. IV), a procedure in which various activities comprising a criminal proceeding are measured by their respective average durations and frequencies of occurrence per proceeding. These in turn are combined into a performance measure termed *the average time* (judge-time in this study) *required to process a case to disposition.*

One use of weighted caseload analysis is to determine the impact of policies that alter the relative mix of activities within the proceeding (e.g., the impact of a change in plea bargaining policies and a consequent change in the frequency of related activities). Another use is to translate a projection of future caseloads into requirements for practitioners and other court personnel. And a third use is to estimate the impact of procedural changes (e.g., the adoption of omnibus hearings) that may alter the average time consumed in affected court activities.

Weighted caseload analyses have been and are being performed in a number of jurisdictions.[13] We planned to go a step further than prior applications of this approach by separating the calculations into broad offense classes. These more detailed results could then be used to deal directly with changes in the mix of offense types.

Data Collection

The circuit court in Multnomah County does not routinely collect data of the type required for weighted caseload analysis. Nevertheless, we hoped to collect data ourselves from at least a sample of judges for a period of one or two months. It turned out that a logging procedure for court clerks had been initiated in June 1975 to collect data that would help to resist county efforts to reduce (clerk) personnel. This procedure required clerks to log their workday activities both in and out of the courtroom, to record the time for each activity, and to indicate whether the activity was related to a civil or a criminal matter. Since the court clerk must be present when the judge is on the bench and since we knew the kinds of activities that the

[13] The Judicial Council of California, for example, has implemented regular judicial weighted caseload analyses for the past ten years or more.

judge must preside over, it was possible to infer from the clerk logs how judges used their courtroom time. It was possible also to infer the amount of judicial time consumed off the bench, presumably in chambers, but its allocation to various matters could not be ascertained. At best, the off-bench time could be prorated between criminal and civil matters on the basis of the corresponding division of time on the bench.

The Chief Criminal Court, which handles all pretrial matters as well as guilty pleas and sentencing flowing from guilty pleas, was not included in the clerk logging program. However, a daily schedule routinely prepared for that courtroom showed for each activity the scheduled time, defendant's name, case number, type of activity, and defense counsel.

Offense type, an item of information that we needed, was absent both from the clerks' logs and the schedule of the Chief Criminal Court. We could obtain this information, however, indirectly from a daily schedule of court appearances prepared by the district attorney's office. This schedule could be matched with the Chief Criminal Court schedule on the basis of defendants' names. The match with the trial court clerk logs could be made on the basis of the type of activity, but ambiguities would sometimes arise when a day's activities in a single courtroom were numerous.

Difficulties in using these reports notwithstanding, copies of the Chief Criminal Court schedule and the prosecutor's court appearance schedule were obtained for each judicial day in July 1975. Trial court clerk logs were available for only 119 of the 185 judge-days during that month. Weighted caseload calculations were made separately for the Chief Criminal Court and the trial courts, both because of the missing data problems and because of differences in activities between the two.

The gross number of dispositions for the entire circuit court during the month of July 1975 was taken from the monthly criminal statistics prepared by the chief criminal clerk. Bench warrants, which are included among dispositions for the court's reporting purposes, were excluded for our purposes.

Analysis

We aggregated court activities into seven types: arraignments, motion hearings, plea hearings, other hearings, court trials, jury trials, and sentencing hearings. The average duration of each type of activity in each of the two types of courts was calculated. The relative frequency of each type of activity per disposition was also calculated. The product of these two measures, that is, the average bench time per specified activity type multiplied by the average frequencies per disposition for a specified activity type, provided the total bench time per disposition for that activity type. Summing these bench times over all activity types then provided the total judge time consumed in the courtroom (i.e., the bench time) per disposition. Total time in chambers for trial judges was prorated between civil and criminal matters on the basis of the identifiable split of bench time on civil and criminal matters. Time in chambers for the chief criminal judge was estimated to be 10 percent of bench time.

The results of these calculations for felonies as a whole are displayed in Table 6.29. Similar results, not displayed, were obtained for four felony types. Some of the entries in Table 6.29 have questionable magnitudes. The rates of occurrence of plea hearings and sentencing hearings, for example, seem unduly low. The combined

Table 6.29

WEIGHTED CRIMINAL CASELOAD ANALYSIS, CIRCUIT COURT, MULTNOMAH COUNTY, JULY 1975
(Felonies plus misdemeanor appeals)

Activity	Average Bench Time per Activity (min)		Average Frequency per Disposition		Average Bench Time per Disposition (min)		
	CCC[a]	TC[b]	CCC	TC	CCC	TC	Total
Arraignments	11	--	.88	--	10	--	10
Motion hearings	23	58	.09	.02	2	1	3
Plea hearings	17	--	.48	--	8	--	8
Other hearings	20	24	.14	.16	3	4	7
Court trials	--	101	--	.03	--	3	3
Jury trials	--	394	--	.07	--	28	28
Sentencing hearings	20	20	.15	.16	3	3	6
Total bench time per disposition					26	39	65
Estimated off-bench time per disposition					3	50	53
Estimated total of judge's time per disposition					29	89	118

[a]CCC = Chief Criminal Court.

[b]TC = Trial courts.

judicial time per disposition of 118 minutes is *less than one-half* of the average time reported for superior courts in California.[14] These questionable results underscore our doubts about the adequacy of the available data.

Data Deficiencies

The most serious shortcoming in the data sources was the substitution of the clerks' logs for direct records of the use of judicial time. While the clerks' logs enabled us to infer how judges' bench time was distributed, they gave no indication as to how the judges employed their off-bench time. And even for the purpose of estimating the use of bench time, the clerks' logs were of uneven quality. Some appeared to be complete, to the point of explicitly identifying the parties in both civil and criminal cases. Others, however, contained only a few cryptic entries per day. (We did not use the latter logs, since the inference was strong that some courtroom activities had simply not been recorded.) Between these two extremes, some clerks failed to designate whether the noted activity was a civil or criminal matter. It was usually possible for us to make this identification by using the district attorney's schedule of court appearances, but even so, over 20 percent of total bench time remained unidentified. This data defect could have produced a significant undercounting of criminal case activities, which in turn could have caused a substantial underestimate of judge time consumed per criminal disposition. Also, some clerk logs for some courtrooms were missing. These data gaps might have biased the mix

[14] Final Report, *Judicial Weighted Caseload System Project for the Judicial Council of California*, Arthur Young and Company, Sacramento, Calif., May 1974.

of bench activities in our data base, since we observed that types of cases and types of activities within cases tended not to be uniformly distributed among judges.

Our estimates of the frequencies of some types of courtroom activities could not, unfortunately, be compared with similar items reported in the court's monthly criminal statistics since their definitions of these activities differed from those used in the logging procedure.

The Chief Criminal Court schedules were deficient for our purposes because they contained *scheduled times* for activities rather than *actual times* consumed. In some instances, we elected to use standard time factors prepared by the chief criminal clerk rather than using scheduled times.

We took the count of dispositions directly from the circuit court's monthly criminal statistics summary. Since the court does not report dispositions by offense type, we estimated them for July from a sample of 400 dispositions selected from all dispositions during the first ten months of 1975. (The sampling was derived from the Cumulative Status Report, which specified the type of offense but not the date of disposition.)

Other data shortcomings would have been avoidable if our resources had permitted us to collect data over a longer period (or over several periods). One month's data were too few to permit analyses of less common offense types or to make reliable estimates of the duration of a relatively infrequent activity such as a trial. Also, more extensive data collection facilitates statistical analysis of courtroom activities that tend to be cyclical. For example, when a trial judge takes his turn as chief criminal judge, his case disposition rate immediately increases about twentyfold, but the increase in his sentencing hearings lags because of the time required to prepare pre-sentence investigation reports. Thus, one would expect sentencing hearings to be relatively infrequent during the first month of a new chief criminal judge's term. The month that we studied, July 1975, was the first month of this term.

Concluding Remarks

Most of the data barriers that we encountered could be readily overcome in a future weighted caseload analysis effort. Modest changes in logging procedures and their supervision would markedly enhance the quality of these data sources. While the data collection period should be lengthened to at least several months, the relatively frequent activities need not be exhaustively reported. For example, 10 percent of the arraignments over a period of three months should probably suffice. On the other hand, trials should be completely reported because of their infrequency.

The principal open question is how to obtain reliable data on the amount of off-bench time judges devote to various matters, given their sensitivity to "monitoring." Their cooperation probably hinges on being persuaded beforehand of the value of weighted caseload information. It could be helpful if a data collection scheme were devised to preserve anonymity of information about individual judges.

GAUGING OVERALL PERFORMANCE: SUMMARY AND COMMENTS

Our findings are summarized in qualitative terms. Each major finding or infer-

ence is stated, followed by a discussion noting year-to-year changes (or lack of change) in the relevant set of performance measures. Where appropriate, we also indicate to what extent, and why, each finding must be qualified, because of the nature and sample size of the data and the success of the supporting statistical analysis described in App. E.

I. *Case quality in Impact crimes improved significantly as a result of the No Plea Negotiation Experiment.*

Rationale: For Impact crimes, the experiment resulted in relatively little change in overall rejection rates and felony filing rates on the most serious charge, but much less frequent rejections for evidence deficiency. Moreover, within this broad rejection category, the frequency of cases rejected because they needed more police investigation decreased; this was not so for a comparable non-Impact crime. Also, nonconviction rates (dismissals, trial acquittals, or mistrials) declined importantly for Impact crimes but not for a comparable non-Impact offense. From these indicators we can conclude that both the quality of individual cases (better police investigation) and the relative frequency of good cases (tightened charging standards) improved.

Qualification: From these indicators it is not possible to separate improvement in better police investigations from elevation of the screening threshold.

II. *Plea bargaining objectives of the experiment were achieved.*

Rationale: Guilty plea convictions on reduced charges were virtually eliminated and plea convictions at the highest original level increased markedly for Impact offenses. But one comparable non-Impact offense showed weaker, but similar, changes, thus indicating some spillover effects of the experiment. Moreover, the district attorney achieved the plea bargaining objectives of the experiment without resorting to the subterfuge of reducing the booking charge of a potential Impact case to a lesser charge, thereby making it a non-Impact case for which plea bargaining was not constrained by the experiment. This statement is supported by the fact that filing rates (of Impact-defined cases at booking) on lesser charges *decreased markedly* during the experiment.

III. *Charging standards were tightened for Impact cases.*

Rationale: Since overall rejection rates for one Impact offense remained unchanged *and* the relative frequency of rejection for evidence deficiency declined, it is reasonable to conclude that the charging threshold was raised *and* that police investigations improved. (This assumes that the proportion of cases rejected on nonevidentiary, purely discretionary grounds did not change materially; in fact, this proportion did increase somewhat, but relatively little compared to the decrease in rejections for evidentiary deficiency.) Had the prosecutor not tightened his standards, he would have rejected a *smaller* proportion of cases during the experiment, ceteris paribus.

IV. *Charging accuracy did not lessen for Impact cases.*

Rationale: One ambiguous indicator of possible improvement in charging accuracy or police investigation is that nonconviction rates (dismissals, trial acquit-

tals, mistrials) fell markedly for Impact crimes, but not for one comparable non-Impact crime. But this indicator alone cannot disclose whether one or both are responsible. (From the case audit of burglary guilty plea cases we concluded that case strength was high both before and during the experiment.) Changes in other measures normally relevant to charging accuracy (charge bargaining and straight plea rates) must be attributed to the policy ground rules of the experiment. Thus, we can conclude only that charging accuracy *did not lessen.*

V. *There was a year-to-year shift in the plea bargaining balance: System gains increased and concessions decreased.*

Rationale: *Gains* and other operational effects included lower dismissal rates and higher plea and overall conviction rates for Impact crimes; a large rise in the proportion of defendants incarcerated and in sentence severity imposed for Impact crimes (and some non-Impact crimes as well); an increase in the sentence severity score for Robbery I cases and for Burglary I cases mainly associated with the experiment[15] (perhaps unintentionally); and more expeditious movement of Impact crimes, although delay for felony cases as a whole showed a year-to-year increase. Compared with straight pleaders, *concessions* per convicted defendant fell markedly for both Impact crimes. (Convicted defendants here included those convicted at trial as well as those who received a plea bargain.) Concessions granted robbery defendants who entered a charge bargain or count bargain showed little year-to-year change, no matter which Sentence Severity Index is applied; the direction of year-to-year changes in concessions granted burglary plea bargainers depends on which index is used, because the frequency of nonincarceration sentences was relatively greater in the latter year and the different indices apply different relative weights to nonincarceration as opposed to incarceration sentence components.

VI. *Sentencing variation remained relatively constant from year to year, with illegitimate factors having little effect in one crime and decreasing effects in another.*

Rationale: In Burglary I cases, illegitimate factors contributed little to sentence variation in both years, indicating evenhandedness in sentencing. In Robbery I cases, illegitimate factors, particularly pretrial custody status and the choice of a bench or jury trial (compared with a straight plea) accounted for a large, but decreasing over time, portion of the variation of the sentence severity score explained in our analysis. In both crimes the nature of charges and counts explains a large portion of the total variation, and prior record is next in importance in explaining variation due to legitimate factors.

VII. *Disposition and sentencing were rather evenhanded and are becoming more so.*

Rationale: *Minority status:* Black burglars or robbers in the earlier year had higher dismissal rates and lower plea and overall conviction rates, suggesting over-

[15] The increase may well be associated with the experiment, even though sentence policy was presumably not part of the experiment's ground rules. After adjusting for random sampling errors in case and defendant characteristics between 1973 and 1974 and for crowding in correctional facilities for the two years, we observed an escalation in average sentence severity in the latter year. However, the customary rotation of sentencing judges *could* have also contributed to the escalation in sentences.

arrests, overprosecution, or the application of a double standard. (Our data could not discern which hypothesis best explained the observed differences.) These differences disappeared in 1974 Burglary I cases. Mixed effects of minority status on the sentence severity score existed in 1973, depending on offense, but these differences disappeared in 1974.

Pretrial custody status: Some mixed effects of pretrial status (whether in jail or not) on dispositions existed in 1973, but these differences were reduced or disappeared in 1974, depending on the offense. There were mixed effects of custody status on the sentence severity score, depending on the offense and year; hence, these data must be interpreted as inconclusive.

Type of defense attorney: Although there were some dispositional and sentencing outcomes that were somewhat more favorable for defendants having public defender representation (higher dismissal rate, lower overall conviction probability in robbery, less severe sentences in robbery), our general conclusion is that the type of defense attorney had little effect. We have no reason to believe that taken as a group, public defenders, retained counsel, and court-appointed attorneys were not equally "good."

Trial versus straight plea: Compared with straight pleas, conviction by trial seems to result in little or no penalty in the sentence severity score.

Qualifications: The observed differences attributable to minority status and conviction by trial are based on small sample sizes and inferences cannot be confidently drawn. The "trial effect," especially, needs to be analyzed in more depth, using a large sample of defendants who go to trial.

VIII. *Defendants with more serious prior criminal records fared no worse in the adjudication phase, but once convicted, they were sentenced more severely.*

Rationale: No consistent differences in dispositional measures associated with prior criminal record appeared in either year, suggesting little or no effect of prior record either before or during the experiment. More serious prior records, however, tended to be associated with a higher sentence severity score in both years, suggesting that no special effect was associated with the experiment.

IX. *Elapsed time measures showed year-to-year increases for the general caseload, but Impact cases were expedited. Only type of attorney and court calendar crowding showed consistent effects on delay.*

Rationale: Most elapsed time and continuance measures exhibited year-to-year increases and a significant fraction of cases exceeded the 60-day standard for the felony caseload in general. However, most delay measures for Impact offenses showed little or no year-to-year change, indicating that special efforts were made to expedite Impact cases. There were either no effects or mixed effects on delay associated with pretrial custody status and type of disposition (dismissal, plea, trial), indicating that these effects on delay are inconclusive. Representation by private attorneys and the increasingly crowded court calendar generally introduced more delay.

X. *Based on their mail survey reponses, the use of victims and witnesses seemed to be reasonable. On the other hand, juror idleness was excessive.*

Rationale: Almost all victims and other witnesses indicated that they cooperated with the prosecutor when requested. The average number of appearances per disposition was between 2.5 and 3.0 for victims and other witnesses and each appearance averaged about 1.8 to 1.9 hours. Although jurors' time seemed to be relatively equally split between civil matters, criminal matters, and simply waiting, during the period of duty surveyed, they spent a significant proportion of their time in idleness.

XI. *On the whole, performance improved from year to year. Delay for the entire felony caseload was somewhat worse, but performance on all other issues (for which data were available) was either better or no worse. Additional system costs incurred were modest.*

Rationale: Given findings I through IX, it seems reasonable to conclude that, on the whole, performance improved between 1973 and 1974; that the No Plea Negotiation Experiment's goals were largely achieved; and that this experiment was associated with improving overall performance. These overall performance gains were achieved at a cost increase of about 25 percent in prosecutorial staff assigned to felony cases, or about a 13 percent increase in staff for the entire office. There was no evidence that any additional short-term costs were incurred in the court or in the public defender's office as a result of the experiment.

VII. APPLICATION OF PERFORMANCE MEASURES IN DADE COUNTY

Anthony Pascal

Dade County, Florida, was selected for pilot analysis because it constitutes an interesting comparison with Multnomah County, covered in the preceding section. Although there was no substantial policy change to match the No Plea Negotiation Experiment, which took place in Multnomah during the period from which our data were collected, an experimental master calendaring system was adopted that involved half of the criminal case assigned judges of the Dade County Circuit Court. The Master Calendar Experiment occurred too recently, however, to permit us to analyze its impacts on case processing efficiency and delay, as we had originally hoped.[1]

In other respects, Dade County makes a useful comparison site. It is more populous than Multnomah County and exhibits more of the characteristics of the "crisis in crime" typical of America's large metropolitan areas. It lies in a different region of the country and has different criminal justice traditions and conventions. The minority population is proportionately larger and more diverse. A more detailed discussion of the differences between our selected pilot jurisdictions is given in Secs. V and IX.

A STATISTICAL OVERVIEW

In Tables 7.1 through 7.6 we present a statistical overview of the following aspects of the criminal justice system in Dade County:

- Resources and workloads in the various criminal justice agencies (Table 7.1).
- Felony case backlog and the nature of the caseload (Tables 7.2 and 7.3).
- Use of correctional facilities in Dade County and in the Florida state prisons (Table 7.4).
- Characteristics of felony defendants (Table 7.5).
- Felony case dispositions, sentences, and delays (Table 7.6).

[1] The Master Calendar Experiment began in September 1974. Of the ten circuit courts assigned to criminal cases, five remained individual calendar courts and the other five became a master calendar court with four satellite trial courts. It was hoped that the latter system would accommodate more trials and permit temporarily assigned judges to be more effectively employed. The master calendar court conducts all arraignments for cases that it and the satellite courts process.

The *Miami Herald*, on September 11 and 12, 1975, ran articles concerning the circuit court's Master Calendar Experiment. Many practitioners were quoted as saying that they thought the experiment was a failure because the master calendar system, in which the master judge is responsible for all pretrial motions and hearings, assigning only trials to the other four judges, was less productive in terms of case dispositions. Criticism was leveled at the shortage of clerks for the master calendar component, delay in assigning cases, overload on the master judge, and incentives toward evasion of responsibility. Advocates of the master calendar system pointed to its potential advantages in reducing attorneys' requests for continuances, better use of judges' time, reduction in "judge-shopping," and easier integration of new judges. The legality of the experiment has been challenged in the Florida Third District Court of Appeals.

The data collection period covered the months of December 1973 through May 1974 and December 1974 through May 1975. Information was collected from various sources, including statistics provided by the criminal justice agencies in Dade County and the State of Florida, samples of agency case records obtained by research team members, and mail surveys of lay participants in felony proceedings (victims, other witnesses, and jurors). The collection procedures and the samples are described in App. D.

Dade County is a large jurisdiction and the average workload of the typical practitioner is rather heavy (Table 7.1). The case backlog is estimated to have increased by about 50 percent over the two-year period for which figures are shown (Table 7.2). Table 7.4 indicates that the number of inmates in county jail facilities rose by more than 40 percent in this same period even though there have been no increases in physical capacity. At the same time the inmate population in the state prisons has risen by a similar amount but official capacity has increased by only 20 percent. Prison crowding is so severe that inmates were being housed in tents in prison yards. (The actual population exceeds "normal" capacity by 44 percent.) Serious impacts on sentencing and parole practices are alleged to have occurred.

Table 7.3 gives sample statistics on the mix of felony cases handled by the circuit court in 1974 and 1975: There appeared to be very little change in the overall distribution of cases by broad offense category. The only substantial alteration in individual offenses consisted of a decline in breaking and entering (B&E) cases and a strong increase in other offenses against property.

There also appeared to be little alteration between the two years in the characteristics of felony defendants, as shown in Table 7.5. Very young defendants were somewhat less frequent in 1975 (and defendants in their twenties somewhat more frequent), and the tendency to release defendants from pretrial custody appears to have diminished. The other defendant characteristics, such as ethnicity, transiency, educational status, prior record,[2] and type of defense counsel, seem, by and large, to have remained fairly constant.

Table 7.6 summarizes felony case dispositions, sentences imposed, and delays incurred for the two observation years in Dade County. It is based on a sample of approximately 100 cases drawn from the files in each year. Pretrial dismissal rates changed little over the two years. In both years, most pretrial dismissals were for reasons other than insufficient evidence. The nol-pros plus diversion rate did rise in the second year. For reasons explained in note d to Table 7.6, we consider our data on trials to be biased downward in the second year and thus cross-year comparisons in trial and plea-of-guilty rates are not appropriate. We note, however, that there was little change in the proportions of defendants pleading guilty on either type of plea (i.e., to original or to lesser charges). Even more interesting, the overall conviction rate changed little in the second year despite the fact that the proportion of convictions-by-plea is biased upward. That is, if a normal rate of trials had occurred in 1975, about 40 percent would have resulted in nonconvictions (at least according to the 1974 statistics). Thus we should have expected to observe a *higher* overall conviction rate in the second year. As to sentence outcomes, the only difference that occurs from one year to the next is the rise in the proportion of convictees receiving jail incarceration rather than prison sentences. The explanation may be the severe

[2] Categories defined according to California Bureau of Criminal Statistics criteria. See Sec. IV.

Table 7.1

CHARACTERISTICS OF CRIMINAL JUSTICE AGENCIES IN DADE COUNTY

Criminal Justice Agency	Characteristics
Circuit court	
Number of courts	5 individual calendar courts 1 Master Calendar Court 4 satellite trial courts (plus temporary assignment of civil courts to criminal cases)
Number of felony cases filed per year	11,000 (approx.)
Number of felony trials per year	1,200 (approx.)
Method of electing judges	Nonpartisan election; 6-year term
Juries	
Term of duty	1 week
Pool of venirepersons	150–160
Size	6 (12 in capital cases)
Sentencing	
Statutory maxima by felony level	Capital: life sentence (with 25 years served for earliest parole eligibility) Life: life sentence or a term of years not less than 30 1st degree: 30 years 2nd degree: 15 years 3rd degree: 5 years
Court discretion	May impose maximum sentence lower than statutory maximum but not below statutory minimum Concurrent or consecutive
Typical percentage of imposed sentence served	40
State attorney's office	
Number of attorneys	80
Number of attorneys assigned to felony cases	46
Ratio: number of felony cases filed per year ÷ number of felony-assigned attorneys	250 (approx.)
Public defender's office	
Number of attorneys	46
Number of attorneys assigned to felony cases	27
Number of felony cases represented by public defenders	5,000 (approx.)
Ratio: number of felony cases represented ÷ number of felony-assigned attorneys	200 (approx.)

Table 7.2

FELONY CASES FILED AND PENDING IN DADE COUNTY CIRCUIT COURT, 1973-75

Calendar Year	Number of Filings	Date	Cases Pending (backlog)
1973	9,790	Dec. 31, 1973	7,373
1974	10,552	Dec. 31, 1974	9,202
1975[a]	11,036	Nov. 30, 1975	10,512
1975[b]	12,200	Dec. 31, 1975[b]	11,000

SOURCE: Dade County Circuit Court records.

[a]First 11 months.

[b]Estimated.

Table 7.3

DISTRIBUTION OF FELONY CASES BY TYPE OF OFFENSE IN DADE COUNTY, 1974-75
(Percent of all cases)

Type of Offense	1974	1975
Offenses against persons	30	29
Robbery	8	8
Other	22	21
Offenses against property	48	46
Breaking and entering (B&E)	18	10
Theft	22	18
Other	8	18
Drug offenses	21	25
All other offenses	1	0
Total	100	100

SOURCE: Based on random sample of 100 felony cases in each year.

Table 7.4

INMATES CONFINED IN CORRECTIONAL INSTITUTIONS IN DADE COUNTY AND THE STATE OF FLORIDA, 1973-75 SELECTED DATES

Date	Number of Inmates	
	Dade County Facilities[a]	State of Florida Facilities[b]
Jan. 1973	1,209	10,574
July 1973	1,267	10,437
Jan. 1974	1,319	10,641
July 1974	1,373	11,335
Jan. 1975	1,459	11,713
July 1975	1,606	14,201
Nov. 1975	1,695	NA

[a]Includes stockade, womens' facility, North Dade jail, and detention center data.

[b]Includes major institutions, road prison, vocational training centers, community correctional centers, hospitals, stockades, and drug houses. Data are from the *Chronological Normal and Emergency Bedspace Report*, Florida Division of Corrections, August 14, 1975.

overcrowding in state correctional institutions noted earlier. We note that the median days elapsed from arrest to trial is well within the 180-day standard established by the Florida supreme court (for individual cases).

Finally, to provide a general context for the subsequent analysis of each issue, we show in Figs. 7.1 and 7.2 what happens to robbery and B&E arrestees at the various stages between arrest and final disposition for the two years (1974 figures are in italics and 1975 figures are in roman). Our focus is on those defendants who are originally arrested on these charges, then charged by the prosecutor with at least one felony count, and then arraigned in circuit court.

CASE SCREENING

Here we discuss the application of performance measures to two aspects of case screening—the charging threshold and charging accuracy—considered in Sec. IV.

The Charging Threshold

As has been described in Sec. IV, there are several alternative procedural routes by which a felony suspect may move from arrest to arraignment in the circuit court. Traditional prosecutorial screening does occur in some instances. But in a substantial majority of cases, screening occurs primarily through the device of the lower-court preliminary hearing in which the magistrate determines whether or not the suspect shall be held to answer, provided the hearing is not waived. The state attorney has authority to "no action" a case before the preliminary hearing or to

Table 7.5

Characteristics of Felony Defendants in Dade County, 1974-75
(Percent of all defendants)

Characteristics of Defendants	1974	1975
Age[a]		
Under 21	47	36
21-29	41	52
30 and over	12	12
	100	100
Ethnic group[a]		
Black	54	60
Spanish surname	12	9
Other minority	0	0
Nonminority	34	31
	100	100
Transient[a] (less than 2 years residence in county)	15	11
Less than high school education[a]	73	69
Prior record[a]		
None	30	36
Minor	32	31
Major	28	24
Prison	10	9
	100	100
Type of defense attorney[b]		
Public defender	67	63
Private (court-appointed or defendant-retained)	33	37
	100	100
Pretrial custody status[b]		
In jail (or jail and bail + O.R.)	28	36
Out on bail or O.R.	72	64
	100	100

[a]Based on samples of robbery and B&E defendants only.

[b]Based on a random sample of all felony defendants.

Table 7.6

DISPOSITIONS, SENTENCES, AND DELAYS IN DADE COUNTY FELONY PROCEEDINGS, 1974-75
(Disposition and sentence entries in percent)

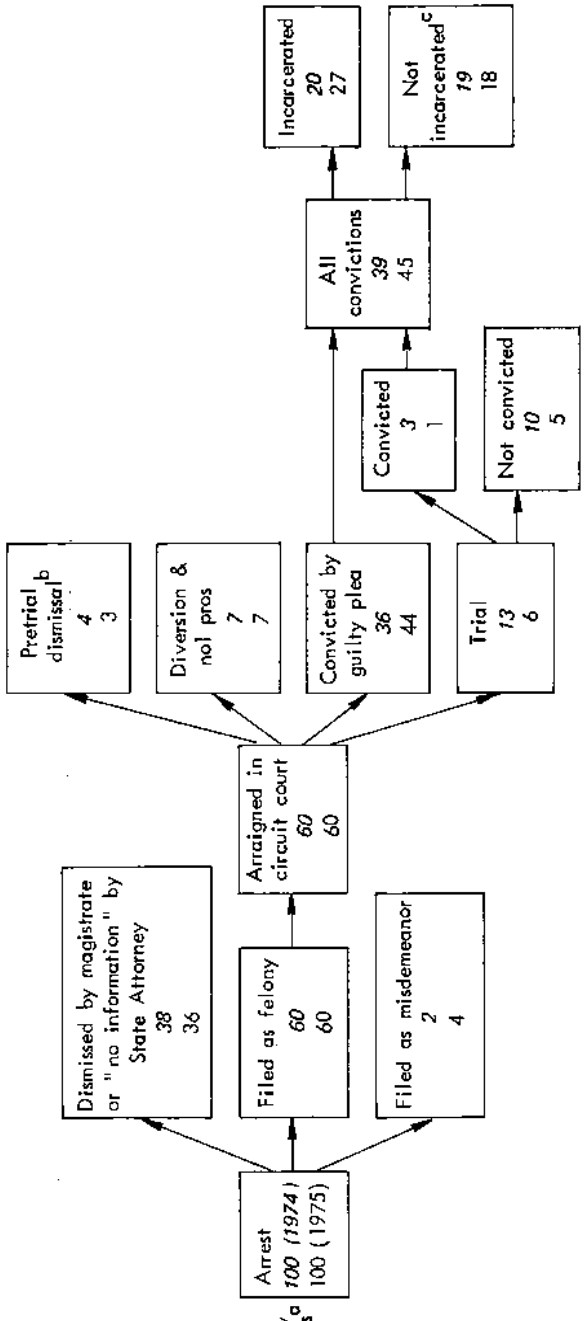
Type of Disposition, Sentence, and Delay	1974	1975
Dispositions[a]		
Pretrial dismissal rate	9	8
For inadequacies of evidence	3	1
For other reasons	6	7
Nol-pros plus diversion rate	6	21
Trial rate	12	(b)
Court	5	
Jury	7	
Trial conviction rate[c]	59	(b)
Trial acquittal rate[c]	34	(b)
Trial dismissal plus mistrial plus hung jury rate[c]	7	(b)
Pretrial plea of guilty rate	63	68
To original charges	53	53
To lesser charges	10	15
Overall conviction rate	69	61
Sentences[d]		
Suspended	0	0
Nonincarceration (probation, fine, restitution, etc.)	60	60
Incarcerated	40	40
Jail (and any lesser sentence)	13	25
Prison (and any lesser sentence)	27	15
Median elapsed times (days)		
From arrest to trial	119	(b)
From arrest to final disposition	109	(b)
From arraignment to final disposition	83	(b)

[a]Based on total dispositions (unless otherwise noted) of *98* in 1974 and *100* in 1975.

[b]Figures not reported because sampling planned called for collection of data only on 1975 cases closed by May of that year; this meant that our sample was severely biased toward short cases, particularly those that did *not* go to trial.

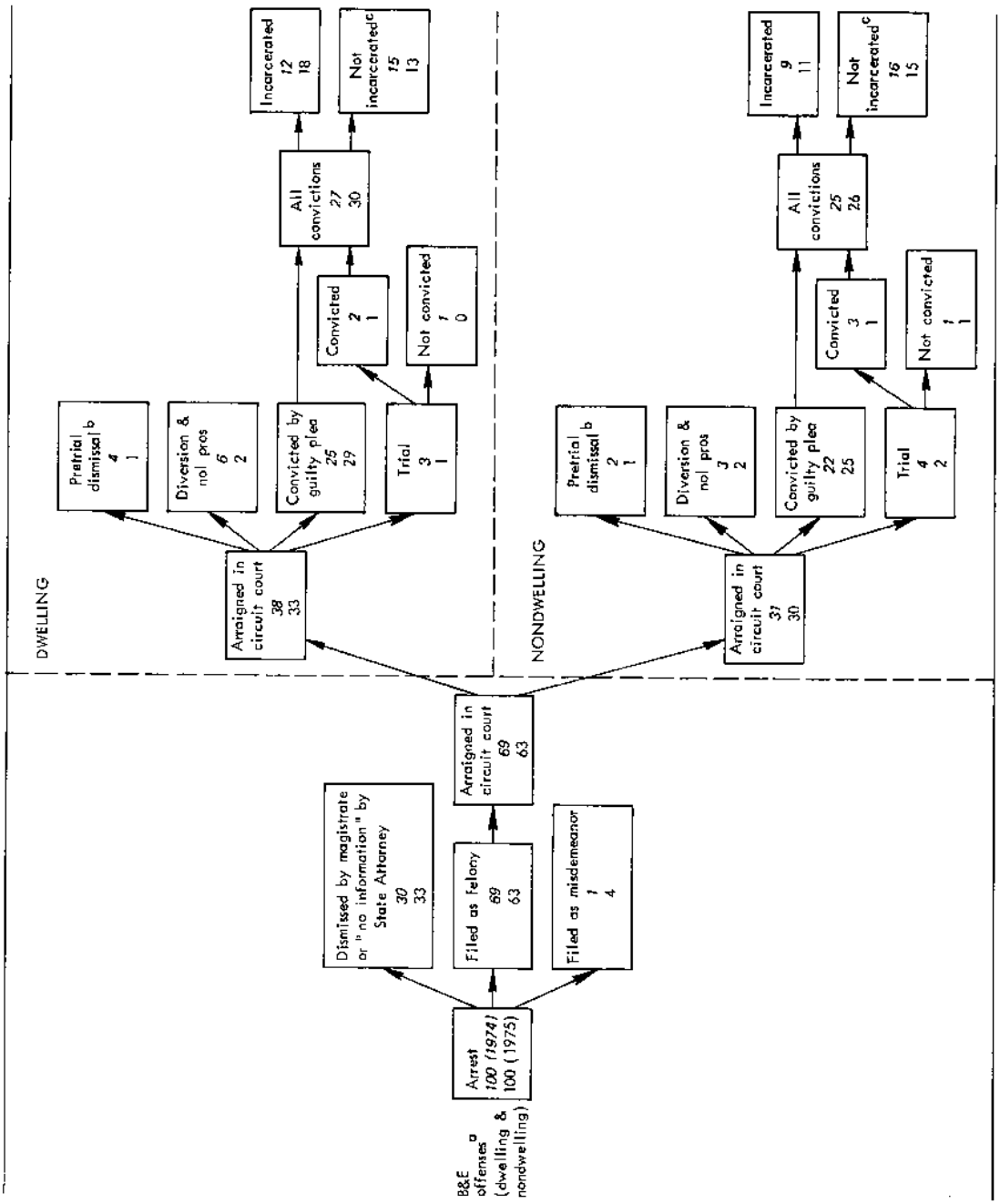
[c]Based on a total number of trials of *12* in 1974.

[d]Based on a total number of sentencings of *70* in 1974 and *67* in 1975.



NOTES: [a]Cases that are predominantly, but not exclusively, robbery; i.e., charges to other offenses may also be included.
[b]Includes cases dismissed in circuit court before trial on a motion by one of the parties.
[c]Includes incarceration sentences imposed but suspended.

Fig. 7.1—Movement of robbery cases from arrest through final disposition in the circuit court of Dade County, 1974 and 1975 (in percent)



NOTES: [a]Cases that are predominantly, but not exclusively, B&E; i.e., charges to other offenses may also be included.
[b]Includes cases dismissed in circuit court before trial on a motion by one of the parties.
[c]Includes incarceration sentences imposed but suspended.

Fig. 7.2—Movement of B&E cases from arrest through final disposition in the circuit court of Dade County, 1974 and 1975 (in percent of arrests)

"no information" it after a bind-over. And he may elect to file an information when the magistrate does not hold the suspect to answer. In this sense, the charging threshold may be regarded as jointly applied by judicial and prosecutorial agencies, although the state attorney—as a matter of policy—does not feel a responsibility to systematically exercise this screening function.

Based on the four samples of exemplary offenses cases drawn by our data collection team from the two periods under study, Table 7.7 shows the gross results of applying the charging threshold in Dade County.

Table 7.7

SCREENING OF EXEMPLARY OFFENSES CASES IN DADE COUNTY, 1974-75

	Percent of Cases			
	B&E Offenses		Robbery	
Charging Threshold	1974	1975	1974	1975
Not held to answer	30	33	38	36
Dismissed by magistrate	26	28	32	30
"No information" by state attorney	4	5	6	6
Reduced to misdemeanor by magistrate	1	4	2	4
Filed as felony by state attorney	69	63	60	60
Number of cases in samples	108	92	108	100

The most serious charges in our samples of cases with B&E offenses varied from third to first degree (by contrast with robbery cases, which by statutory definition are all of first degree). In many of the B&E cases sampled, we were unable to ascertain the degree of the police-booked charges. Thus, the cases shown in Table 7.7, as filed by the state attorney, may include some that were filed as B&E charges at a lower degree than those booked by the police. We observe that 30 to 40 percent of the exemplary offenses cases booked by the police failed to pass the felony filing threshold applied in Dade County and that there appeared to be no substantial gross screening changes between the two periods (this was to be expected, since we knew of no relevant policy change between these periods).[3]

Charging Accuracy

We turn now to sets of performance measures from which, as is argued in Sec.

[3] We had planned to analyze the reasons for "no information" by the prosecutor in order to illuminate the operation of the charging threshold and to perceive interactions between police arrest standards and prosecutorial filing standards. However, because the total population of "no information" forms was very small (see Table 7.8), detailed analysis was not possible. Nevertheless, these data suggest that evidence deficiencies account for most of the robbery "no informations" (but only eight such actions were taken in 1974 and 1975) and that pretrial diversion accounts for most of the B&E "no informations" by the state attorney's office.

Table 7.8

FREQUENCY OF REASONS GIVEN FOR STATE ATTORNEY'S "NO INFORMATIONS" ACTION IN POLICE-BOOKED ROBBERY AND B&E CASES, DADE COUNTY, 1974-75

Reasons Given for "No Informations"	Frequency of Reasons Given			
	Robbery		B&E Offenses	
	1974	1975	1974	1975
Evidence deficiency	57	100	35	11
Other reasons	29	0	6	11
Pretrial intervention (i.e., diversion)	14	0	59	78
Number of cases	(7)	(1)	(17)	(18)

IV, inferences concerning charging accuracy might be drawn. These measures characterize the distribution of the various dispositions of arraigned cases. Table 7.9 presents these disposition measures based on approximately 400 cases involving the exemplary offenses sampled from the two selected periods. The B&E cases have been separated into those whose offenses were committed in a dwelling and those whose offenses occurred in a nondwelling context. This was done to parallel a similar breakdown of Burglary I offenses in Multnomah County, as presented in Sec. VI.

Of the various disposition measures shown in Table 7.9, the one most directly linked to charging accuracy is probably the percentage of cases in which there are *convictions on all charges as originally filed.* We observe in Table 7.9 that, between 1974 and 1975, this measure (which is dominated by plea convictions) increased for all three categories of cases—B&E dwelling, B&E nondwelling, and robbery—although generally not by an impressive amount for the sample sizes used here. The narrower measure of convictions by pleas shows larger increases from 1974 to 1975.[4] We have a statistical indication here that cases of these types may have been charged more accurately in 1975. Such an inference is strengthened by further observing in Table 7.9 that the percentage of cases *not convicted for reason of pretrial dismissal* lessens between 1974 and 1975 for all three disposition categories except that of B&E offenses in dwellings.[5] Generally, the fewer the dismissals, the higher the charging accuracy, provided the application of the screening threshold did not change and thus materially affect the strength of filed cases. We found no evidence,

[4] Using this narrower measure for the comparison between years of the convicted of charges as filed dispositions is probably more valid because of the unreliability of the 1975 trial data caused by a truncated collection period, as mentioned earlier.

[5] However, a note of caution should be registered. The time truncation of our 1975 data may well have led to a bias away from dismissals in our B&E case sample since, in Dade County, dismissals in B&E cases tend to take longer, i.e., the elapsed time to other common dispositions is shorter. (See Table 7.22, below.) This fact, rather than changes in charging accuracy may account for the decline in the 1975 B&E dismissal rate. In robbery cases, time to dismissal is less than to other types of disposition and therefore an observed decrease in the robbery dismissal rate *strengthens* our conclusions on charging accuracy for that offense.

Table 7.9

PERFORMANCE MEASURES RELATED TO CHARGING ACCURACY: DADE COUNTY FELONY DISPOSITIONS FOR EXEMPLARY OFFENSE CASES, 1974-75

	Percent of All Dispositions					
	B&E Offenses					
	Dwelling		Nondwelling		Robbery	
Disposition of Cases	1974	1975	1974	1975	1974	1975
Not convicted	28	10	21	10	34	25
Pretrial dismissal	11	4	7	2	6	5
Nol-prossed	13	6	9	6	10	12
Pretrial intervention	2	0	0	0	0	0
Tried but not convicted	2	0	5	2	16	8
Convicted of all charges as originally filed	46	52	54	66	44	46
By plea	42	52	45	62	41	45
By trial	4	0	9	4	3	1
Convicted of at least one of the most serious charges but with charge or count reductions	18	31	18	21	11	17
By plea	16	29	18	21	10	17
By trial	2	2	0	0	1	0
Convicted of lesser charges than the most serious filed charges	9	8	7	2	11	12
By plea	7	8	7	2	10	11
By trial	2	0	0	0	1	1
Number of cases in samples	55	52	44	47	98	100

either in our statistical sampling (see Table 7.7) or in our case audit exercise, that case strength had changed between the two years. It therefore seems reasonable to regard the changes in the pretrial dismissal measure as strengthening the inference about improved charging accuracy. And we are further persuaded by the observation that the percentage of cases *tried but not convicted* fell in all three offense categories. Given a trial, fewer failures to convict tend to reflect higher charging accuracy, although charging accuracy is not necessarily the decisive factor in trial outcomes.

Some measures in Table 7.9 are less specific and clear, but do not weaken the inference. The *percentage of cases nol-prossed* goes down modestly for the two B&E categories and rises only slightly for the robbery category. We did not find an adequate data source of detailed reasons for nol-prossing these cases, so we cannot conclude that these dispositions reflect a redress of charging inaccuracy by the state attorney. If this latter situation did obtain, then a lower nolle prosequi rate (as in Table 7.9 for the B&E cases) would attest to higher charging accuracy. Similarly, we lacked an adequate source of information as to the reasons why the charge and count

reductions were given in those dispositions measured by the percentage of cases *convicted of at least one of the most serious charges but with charge or count reductions*. The fact that conviction occurred at the most serious felony level charged indicates that the most serious offense was accurately filed. But we are unable to decide whether the change in counts and other charges implies inaccuracy in filing or concessions to the defendant to elicit a plea of guilty. Despite this ambiguity in interpreting the measure, the consistent pattern of increase between the two years for the three offense categories, again dominated by pleas, is consistent with the inference of improved charging accuracy in 1975.

If the *percentage of cases convicted of lesser charges* were to increase significantly, and this increase were not due to a greater propensity toward plea agreements, then this would be evidence contrary to the inference of improved charging accuracy. But Table 7.9 shows no such change. And the remaining disposition measure—*pretrial intervention*—seems to have insignificant linkage with charging accuracy.

We conclude that Table 7.9 tends to support an inference that the accuracy of filed charges in B&E and robbery cases in Dade County was higher in 1975 than in 1974. The inference could be less tentative if we had obtained good "reason" data for the sampled cases that were nol-prossed or were subjected to some changing of charges or counts.

As discussed in App. E, we attempted to explain these Dade County conviction probabilities (and those for Multnomah County as well) in terms of nonaccuracy factors such as defendant-related characteristics, number and level of original charges, and so on. As mentioned earlier in Sec. IV, the set of explanatory factors we chose did not turn out to be significantly related to the conviction probabilities in a statistical sense. Thus, the results of this (multivariate regression) analysis do not contradict the above inference we have drawn from the gross statistical measures presented in Table 7.9.

We advance no hypothesis as to why screening accuracy may have improved in 1975 because we are aware of no specific programs in Dade County seeking to produce this effect and because the unusual nature of the Dade County screening process deters simple explanations.

PLEA BARGAINING

In Dade County, as discussed in Sec. IV, there is an explicit policy in the state attorney's office in opposition to plea negotiation. Table 7.10 shows the results of the process of negotiation on charges and counts that do occur in that jurisdiction and indicates some of the results of that position. Rarely is the defendant permitted to enter a guilty plea at a felony degree level lower than that at which he was originally charged. Sometimes, at least for B&E offenses, other charges and additional counts accompanying a most serious charge are dropped in return for a plea to the latter; this is most likely to occur for burglars originally charged at the lowest felony level. It appears, then, that the more serious the original offense (assuming the relative seriousness descends from robbery to dwelling B&E to nondwelling B&E), the more stringent the no negotiation policy applied.

The central conclusion to be drawn from these measures is that fully 60 percent of defendants charged with the exemplary offenses at all degree levels pled to *all*

Table 7.10

RESULTS OF PLEA BARGAINING ON CHARGES AND COUNTS IN DADE COUNTY, 1974-75

Level of Plea[a]	Percent of All Pleas of Guilty					
			B&E Offenses			
	Robbery		Dwelling		Nondwelling	
	1974	1975	1974	1975	1974	1975
Plea to all original charges and counts:						
First degree felony	80	69	0	2	3	0
Second degree felony	NA	NA	65	49	52	63
Third degree felony	NA	NA	3	9	14	10
Plea to at least one count of highest original felony level with other charges and/or counts reduced:						
First degree felony	20	26	3	0	0	3
Second degree felony	NA	NA	20	27	24	16
Third degree felony	NA	NA	3	9	3	8
Plea to charges at level lower than highest original:						
First degree felony	0	5	0	0	0	0
Second degree felony	NA	NA	6	4	4	0
Third degree felony	NA	NA	0	0	0	0
Total	100	100	100	100	100	100
Total guilty pleas (N)	50	65	34	45	29	38
Gross plea rate[b]	61	73	65	89	70	85

[a]Level of highest original charge shown at each level of plea.

[b]N = number of dispositions.

charges filed; over 90 percent pled to at least one or more charges at the highest level of those filed.

We were informed, however, that sentence bargaining[6] is a common occurrence in Dade County felony proceedings. In fact, some have called it virtually ubiquitous in the taking of guilty pleas (see Sec. V). Unfortunately for the purposes of this study, its occurrence is not recorded in the case folders, unlike the situation in Multnomah County (discussed in Sec. VI). Later in this section we comment on some of the possible effects of sentence bargaining.

The effects of plea agreements that could be identified in court records, i.e., the reduction of charges and counts, are summarized in Tables 7.11 and 7.12. Table 7.11 presents statistical indicators of the impacts on the system, including operational advantages; these are expressed in terms of dispositions, sentences meted out, and delays incurred, so that variations in these measures can be related.

[6] Which may be simply the giving of sentence assurances to the defendant.

Table 7.11

MEASURES REFLECTING THE EFFECTS OF PLEA BARGAINING IN DADE COUNTY, 1974-75

Dispositions, Sentences, and Delays	Robbery		B&E Offenses		All Felonies	
	1974	1975	1974	1975	1974	1975
Dispositional measures (%)						
Dismissal and nol-pros plus diversion rate	18	17	19	9	24	28
Trial rate	20	10	12	4	12	3
Guilty plea rate	62	73	69	87	62	67
Overall conviction rate	67	75	76	91	69	67
Sentence severity measures						
Percent incarcerated	78	80	56	61	40	40
Sentence severity imposed						
(Index A score)[a]	19.3	20.1	13.1	13.2	--	--
(Index B score)	32.1	26.1	14.6	15.7	--	--
(Index C score)	44.8	32.0	16.2	18.3	--	--
(Index D score)	86.3	56.8	23.4	30.1	--	--
Median elapsed time measures (days)						
Arraignment to guilty plea	116	69	73	52	67	(b)
Arraignment to trial	124	87	73	76	131	(b)
Arraignment to final disposition	126	74	90	57	83	(b)
Continuances						
Number of days continued/total cases (non-contested cases)	--		--	--	97	(b)
Number of days continued/total cases (trials)	--		--	--	149	(b)

[a] Index scores are described in Sec. IV.

[b] Time truncation of the data collection implies too few trials and short elapsed times in the sample.

Dismissal rates changed little except in the case of B&E offenses.[7] The fall in trial rates and the rise in plea rates were probably due to the undersampling of trials in the second period. This same fact helps to explain why overall conviction rates increased in 1975; some defendants who go to trial are, after all, not convicted. B&E offenses appear to be somewhat more likely to result in convictions than other offenses. Our case audit analysis (Sec. VIII) revealed that almost 90 percent of B&E defendants (in a small sample) were caught at the scene or gave an admission or were identified by fingerprints.

The measures of sentence severity are the (averaged) indexed scores described in Sec. IV plus the percent of those convicted who are incarcerated. The only important change in sentence severity between the two years seems to have occurred in Index D for robbery, which weights probation and fine sentences relatively lightly compared with prison and jail incarceration. In 1975, there may have been a tendency away from fine and probation sentences (at least when combined with incarceration) for robbery, with the reverse occurring for B&E offenses.

[7] The fall in dismissal rates for B&E may have been a result of the bias toward short cases in 1975. We noted that for B&E offenses in 1974, the median elapsed time for arraignment to dismissal was about two weeks longer than the typical interval from arraignment to guilty plea.

Robbery sentences, not surprisingly, tended to be stiffer than those imposed for the other offense classes. About 80 percent of convicted robbers were incarcerated, whereas only half that number of all convicted felons were incarcerated and an intermediate number of convicted burglars. As measured by all index scores other than type A, robbery sentences were generally about twice as severe as those imposed on burglars, although this multiple was somewhat reduced in 1975. The fact that the Index A measure of sentence severity does not fit this pattern is probably due to the relatively heavy weight it places on probation, etc., and the relatively high frequency with which nonincarceration sentences occur in B&E convictions as compared with robbery. Later we will comment on possible connections between sentence outcomes and the overcrowding in Dade and Florida correctional facilities.

Given the bias in our sample of elapsed times for 1975 cases, we will not comment on these outcomes for that period. It is interesting to note, however, that in 1974, elapsed times were generally longer for robberies than they were for B&E offenses or for all felonies. This situation may be due to the fact that in 1974 fewer charge and count bargains seemed to have been struck with robbery defendants (see Table 7.10). The greatest difference between the robbery case delays and those for the other offense categories, on a proportional basis, occurred in the median elapsed time from arraignment to guilty plea. Such longer delays may be attributable to an anti-plea bargaining posture in an overcrowded court system.

Our attempts to isolate the concessions granted as a result of charge and count bargaining are summarized in Table 7.12. We define a concession to be the *reduction* in the sentence severity score for a defendant who obtains a charge or count bargain compared with the score for an otherwise identical individual (in terms of personal characteristics, original charges, type of counsel, etc.) who pleads guilty to all filed charges and counts.[8] Even though charge and count bargaining is a relatively rare occurrence in Dade County, we were able to calculate the losses in the sentence severity score ("concession to the defendant") for those who did obtain such bargains on pleas. No values for concessions are entered in Table 7.12 when the variable used to represent the plea, in an attempt to isolate the effect of plea bargaining sentence severity, turned out to be insignificant from a statistical standpoint, i.e., in cases where the predictive variable would "explain" no more of the variance in the Sentence Severity Index for an individual than would random factors.

In general, the concessions calculated by this device are rather low, relative to the average sentence severity imposed, possibly for reasons alluded to in footnote 8. Where they are significant, the values generally increase from index type A to D for defendants who struck charge bargains. Count bargaining did not yield statistically significant concession estimates, and cross-year comparisons were not possible. Concessions to robbers appear to be larger than those accorded to burglars.

It was not possible to estimate the effect of trial (by either court or jury) on sentence severity scores. We had hypothesized that, holding constant all aspects of a case and defendant other than disposition, going to trial would increase the sentence severity score. Our statistical analysis did not support the view that the expectation of harsher sentences at trial induces many defendants to enter a guilty

[8] The difficulty here, it should be remembered, is that many pleaders in Dade County get sentence bargains. Thus, comparing sentence severity scores for charge and count bargainers, as compared with straight pleaders, does not at all capture the effect of bargaining in general.

Table 7.12

CHARGE AND COUNT BARGAIN CONCESSIONS IN DADE COUNTY, 1974-75

Sentence Severity Index	Type of Defendant[a]	Reductions in Sentence Severity Index Score from Straight Plea Score			
		B&E Offenses		Robbery	
		1974[b]	1975[c]	1974[d]	1975[e]
A	Per convicted defendant	0.7	0.6	1.8	NS
	Per defendant with charge bargain	6.5	9.3	10.9	NS
	Per defendant with count bargain	NS	NS	NS	NS
B	Per convicted defendant	NS	0.7	4.1	NS
	Per defendant with charge bargain	NS	12.3	24.2	NS
	Per defendant with count bargain	NS	NS	NS	NS
C	Per convicted defendant	NS	0.9	6.4	NS
	Per defendant with charge bargain	NS	15.2	37.5	NS
	Per defendant with count bargain	NS	NS	NS	NS
D	Per convicted defendant	NS	NS	14.0	NS
	Per defendant with charge bargain	NS	NS	82.2	NS
	Per defendant with count bargain	NS	NS	NS	NS

NOTE: NS = not significantly different from zero at 10 percent confidence level.

[a] Including those tried.

[b] 11 percent pled to reduced charges; 25 percent pled to reduced counts at same level.

[c] 6 percent pled to reduced charges; 30 percent pled to reduced counts at same level.

[d] 17 percent pled to reduced charges; 17 percent pled to reduced counts at same level.

[e] 17 percent pled to reduced charges; 20 percent pled to reduced counts at same level.

plea. Our findings showed that trial disposition (as compared to plea) did not increase sentence outcomes in a statistically significant manner.

Because this result may have been due to the very small number of trials included in our sample, we adopted a different technique in an effort to isolate the trial effect. We matched all of those convicted at trial with a group of otherwise similar defendants (in terms of nature of original and convicted charges, prior record, age, etc.) who entered straight pleas and calculated an average sentence severity (Index A) for both groups. But in Dade County, it was only for 1974 B&E offenses that there was a sufficient number of defendants convicted at trial to make this procedure feasible.[9] But even for these convictions the estimated impact of trial (court and jury combined) was to *reduce* the sentence by about 13 percent.

[9] The numbers convicted at trial on B&E and robbery in 1974 and 1975 were, respectively, 8, 3, 3, and 2.

The overall conclusion must be that the performance measures we calculated could not reveal the impact of guilty plea negotiations in Dade County. The relatively small number of trials and the common, but unrecorded, policy of striking sentence agreements with defendants who pled guilty meant that our statistical indicators failed to capture most of the effects that may occur in the system.

SENTENCE VARIATION

In applying statistical performance measures to variations in imposed sentences, we first identify the extent of this variation and then attempt to analyze the factors that might be responsible.

Tables 7.13 and 7.14 show sentence outcomes for Dade County robbery and B&E defendants convicted in circuit court by plea or trial. The columns proceed from the lightest to the heaviest types of punishment, beginning with probation and ending with prison terms. "Probation and Other" signifies probation sentences combined with fines imposed, orders for restitution, requirements for community services and/or assignments to special rehabilitation programs. "Jail and Other" and "Prison and Other" also include these additional types of punishment, plus probation sentences imposed jointly with incarceration. The average score given for Sentence Severity Index A, which assigns weights to each type of sentence imposed and then sums the resultant values, is simply the mean of the sentences in all sampled cases with a particular convicted charge, calculated according to the formula given in Sec. IV for that index. The standard deviation is a common measure of the amount of dispersion of the outcomes around the mean value. The larger the standard deviation, the greater the sentence variation.

We turn first to an examination of robbery sentences in Table 7.13 and notice that most robbery defendants in both years were convicted of first degree felony charges.[10] The frequencies at lower levels of conviction are too small to warrant much discussion. Although the average scores and standard deviations were not too dissimilar between 1974 and 1975, in the earlier period about three-quarters of the robbery defendants were sent to prison as compared with about three-fifths in the latter period. Jail sentences and probation were more common in 1975. The average value of Sentence Severity Index A tends to fall as one moves down the level of conviction (except for the anomolous result for second degree convictions in 1975, caused by the three prison sentences in that year), as would be expected.

B&E sentences (Table 7.14) reflect very few convictions at the first degree or misdemeanor level; upwards of 70 percent of the convictions were at the second degree level, with most of the remainder at the third degree level in both years. The average scores of Sentence Severity Index A were quite similar across both periods and conviction levels, as were the standard deviations. The distributions by sentence type and amount were more disparate. In the 1975 period, second degree felons were more likely to be sent to prison and in general received longer incarceration terms than their 1974 counterparts. For third degree felons, the 1974 entries show lighter sentences, well over half were not incarcerated; but the 1975 pattern was similar

[10] Defendants convicted of first degree robbery numbered 48 of a total of 58 in 1974, and 61 out of 72 in 1975.

Table 7.13

Distribution of Convictees in Dade County Robbery Cases, by Charge Level and Type of Punishment, 1974-75

Charge Level	Percent of All Convictions at a Given Level														Sentence Severity Score (Index A)	
	Probation Only			Probation + Other			Jail Alone and Jail + Other			Prison Alone and Prison + Other						
	<2 yr	2-4 yr	≥5 yr	<2 yr	2-4 yr	≥5 yr	≤6 mo	7-11 mo	≥12 mo	<2 yr	3-4 yr	5-10 yr	11-20 yr	≥21 yr	Average	Standard Deviation
First degree felony																
1974 (N = 48)	--	--	--	--	2	--	--	2	13	2	12	27	19	15	22.0	13.9
1975 (N = 61)	--	3	11	--	3	--	2	2	16	8	11	23	21	--	21.0	10.1
Second degree felony																
1974 (N = 3)	33	--	--	--	--	--	--	--	33	--	33	--	--	--	11.9	3.9
1975 (N = 3)	--	--	--	--	--	--	--	--	--	--	33	--	67	--	28.3	7.6
Third degree felony																
1974 (N = 2)	--	--	--	50	--	--	--	--	50	--	--	--	--	--	9.0	7.1
1975 (N = 3)	--	--	--	--	--	--	--	--	--	33	33	33	--	--	18.6	0.4
Misdemeanor																
1974 (N = 5)	--	--	--	20	--	--	60	--	60	--	--	--	--	--	4.8	4.1

NOTE: A dash (--) denotes zero.

Table 7.14

Distribution of Convictees in Dade County B&E Cases, by Charge Level and Type of Punishment, 1974-75

Charge Level	Percent of All Convictions at a Given Level														Sentence Severity Score (Index A)	
	Probation Only			Probation + Other			Jail Alone and Jail + Other			Prison Alone and Prison + Other						
	<2 yr	2-4 yr	≥5 yr	<2 yr	2-4 yr	≥5 yr	≤6 mo	7-11 mo	≥12 mo	<2 yr	3-4 yr	5-10 yr	11-20 yr	≥21 yr	Average	Standard Deviation
First degree felony																
1974 (N = 2)	--	50	--	--	--	--	--	--	--	--	--	--	--	50	25.7	27.9
1975 (N = 2)	--	--	--	--	50	--	--	--	--	--	--	--	50	--	17.7	16.6
Second degree felony																
1974 (N = 54)	2	25	9	--	4	2	9	--	15	6	8	18	2	--	13.4	8.5
1975 (N = 60)	--	21	7	2	3	5	5	--	13	3	12	27	2	--	14.0	8.9
Third degree felony																
1974 (N = 14)	14	21	14	--	--	7	--	--	15	15	7	7	--	--	12.6	8.2
1975 (N = 23)	4	18	4	13	--	5	13	4	9	4	22	4	--	--	11.7	8.5
Misdemeanor																
1974 (N = 3)	--	--	--	--	--	--	100	--	--	--	--	--	--	--	2.0	1.7
1975 (N = 0)	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--	--

NOTE: A dash (--) denotes zero.

to that for second degree felons, although incarceration terms were shorter for both jail and prison sentences. In general, sentence severity for defendants convicted on second and third degree B&E charges appeared to be increasing from 1974.

To identify the factors responsible for the variation in sentence outcomes, we performed multivariate statistical analysis, a technique that estimates the independent effect of one of a group of factors on an outcome of interest. Since we want to parcel out the explanation of the variance in imposed sentences among a number of separate factors hypothesized to influence sentencing, the entries in Table 7.15 indicate the fraction of variance accounted for by a list of factors categorized as "legitimate" and "illegitimate." The meaning of these categories and the principles by which individual factors were assigned to each are discussed in Secs. IV and VI. Simply stated, differences in the legitimate factors across a sample of defendants "should" cause variation in sentence outcomes; differences in illegitimate factors across the same set "should not" cause their sentences to vary.

In general, we found that about 40 percent of the variance in sentence outcomes was accounted for by the set of factors we hypothesized to be influential in the three out of the four offense/year pairs for which we had data. The results for 1974 robberies failed to meet the test of statistical significance. That is, the group of factors we specified performed no better than random chance in "explaining" sentencing when evaluated at a 95 percent level of confidence.[11] The roughly 60 percent of variation left unexplained in sentence outcomes in the other offense/year pairs is attributable (a) to factors we could not measure (e.g., the differences in sentencing philosophy among a group of judges, differences in publicity surrounding particular cases, exacerbating or mitigating circumstances in the perpetration of the crime); (b) to factors we were unable to specify; (c) to errors in measurement of those factors we did specify and measure; and (d) to pure chance.

Once a particular set of factors had met our criterion of statistical significance, however, we adopted a less rigorous standard in deciding whether a factor within that set was or was not a probable causative influence. If there was an even chance, statistically, that a given factor was associated with variation in sentence outcomes, we report the share attributable to that factor in Table 7.15. If there is a less than even statistical probability that the factor had a causative influence, we entered NS (not significant).

In observing the influence of individual factors, we note that age of the offender was relatively important in robbery (1975), but not in B&E. Having a prior criminal record influenced the sentence in all three cases, but more so in B&E than in robbery. Community ties (represented by an index that captured the employment, occupation, educational attainment, transiency, and family status of the defendant) turned out not to influence the sentences imposed. Original charges and counts (a conglomerate of variables representing the number and types of charges and counts filed against the defendant and those on which he was ultimately convicted) had an appreciable influence, especially for burglars. Together the so-called legitimate factors account for more than a quarter of the variation in sentence outcomes for convicted burglars and a fifth of the variation for convicted robbers.

The illegitimate factors present a less consistent picture. Being a black or belonging to an "other" minority (mostly Cuban), as opposed to being a white Anglo,

[11] That is, at a rate more than 5 times out of 100, pure chance would have done as well as our "model" (a set of variables thought to be related to the outcome of interest) in explaining sentence outcomes.

Table 7.15

EFFECTS OF SELECTED FACTORS ON SENTENCE SEVERITY IN DADE COUNTY ROBBERY AND B&E CASES, 1974-75

Factors Influencing Sentencing	Percent of Accountable Variance in Sentence Severity Score (Index A)[a]			
	Robbery		B&E Offenses	
	1974	1975	1974	1975
"Legitimate" factors				
Age	NS	6	NS	NS
Prior criminal record:				
Major	NS	7	2	2
Minor	NS	1	9	4
Prison	NS	1	3	6
Community ties (index)	NS	NS	NS	NS
Nature of charges and counts	NS	5	13	15
Aggregate variance explained	0	20	27	27
"Illegitimate" factors				
Minority status				
Black	NS	4	2	1
Other	NS	2	3	NS
Pretrial custody status				
In jail	NS	4	1	1
On bail or O.R.	NS	NS	NS	NS
Defended by private attorney				
Defendant-retained counsel	NS	9	1	NS
Court-appointed counsel	NS	NS	1	1
Convicted at trial				
Court	NS	1	NS	NS
Jury	NS	NS	NS	3
Proxy for correctional facilities crowding	NS	(b)	(b)	3
Aggregate variance explained	NS	20	8	9
Total variance explained[c]	NS	40	38	37

NOTE: NS = not significant.

[a]Entries are only for variables in regression equations that were statistically significant at the 95 percent confidence level and in which the regression coefficient on that variable was statistically significant at the 50 percent level; entry reads NS otherwise.

[b]Significant, but very small (<<1%).

[c]Including variance accounted for by NS variables and the constant term.

had an influence on outcomes for 1975 robberies and 1974 B&E offenses, but tended to be unimpressive in the remaining B&E case sample. Failing to obtain a pretrial release had a larger effect for robbery than for B&E offenses. This relatively large effect for robbery was even larger when a defendant was represented by retained counsel (instead of the public defender) in 1975. Being represented by a court-appointed private attorney rather than the public defender had no significant effect on robbers' sentences and only a small one on burglars' sentences. Conviction by trial—as compared with conviction by plea—emerged as a significant, sizable influence only in cases of jury trials for 1974 burglars. Our proxy measure for crowding in correctional facilities (which, some hypothesize, may tend to diminish a judge's inclination to incarcerate a guilty defendant) seemed to be influential only in 1975 B&E cases. Assuming no inadequacies in our analytic technique, we would argue that the entries for all the illegitimate factors "should" read NS; the higher the actual numbers entered, the more reason to suspect that factors which "should not" influence sentencing are doing so. In general, then, Dade County does not seem to be a jurisdiction in which the illegitimate factors play a large role in sentencing.

EVENHANDEDNESS

To measure the degree to which a jurisdiction treats its felony defendants in an evenhanded fashion, we attempt to identify the relationship between selected dispositional and sentence outcomes and various defendant characteristics. These latter we label illegitimate factors because their presence ought not to result in differential treatment of defendants with respect to dispositions and sentences imposed. They are ethnicity, pretrial custody status, and type of defense counsel.[12]

Dispositional Measures

We originally considered presenting our conclusions on evenhandedness as they would have emerged from a multivariate statistical analysis, in which the influence of a given characteristic is shown independently, i.e., with all other hypothesized causative factors "held constant." That technique proved infeasible, however, in the case of dispositional outcomes (although not for sentencing outcomes). The sets of factors we chose to try to "explain" such dispositions as dismissal or conviction at alternative charge levels performed no better than random chance in accounting for the variation in the outcomes. Hence, in the next three tables we present cross-tabulations to show the differential outcomes of groups of defendants categorized according to the illegitimate factors. It must be recognized that this method of presentation does not ensure that the factor in question is responsible for the outcome; other factors, not shown in the table, may be confounded in the result. (For instance, if it were to be shown that black defendants were less often diverted but that they also happened to have disproportionately heavier prior records, then the cause of the low diversion rate is not necessarily due to antiblack prejudice.)

In Table 7.16, defendants are grouped by offense, year, and pretrial custody

[12] We at one point attempted to develop an index that would indicate the strength of a defendant's community ties and would incorporate such separate attributes as employment, occupation, education, transiency, and family status. None of the indices we experimented with, however, showed a statistically significant relationship with outcomes of interest.

status. (See the discussion of evenhandedness in Sec. VI for reasons why pretrial custody status may in fact be a *legitimate* influence on dispositions.) In Dade County in 1974-75, robbery defendants were rarely eligible for pretrial release under the terms of the program in operation there (see the N values in Table 7.16); consequently, here we only discuss the results for B&E defendants among whom the distribution by pretrial status is more evenly spread. (It should be pointed out, however, that robbery defendants who made bail were less likely to plead in 1974.) In 1974, B&E defendants on bail and on O.R. were less likely to be convicted, mostly because pretrial, dismissal, diversion, and nol-pros were the more common dispositional outcomes for these offenses, and defendants who remained in jail were more likely to plead guilty to all charges (although it should be noted that in Dade County sentence agreements or assurances, not recorded in case files, may have been given to a large proportion of all those who pled guilty). In 1975, the outcomes were more mixed, although bailees did tend to do somewhat better. In the latter year, the plea pattern was quite similar for all three classes. None of the comparisons across custody status categories (except as noted for robbery defendants in 1974) reveal substantial differences in outcomes. In cross-year comparisons, 1973 B&E defendants who made bail had higher straight plea and gross plea rates than their 1974 counterparts.

We turn now to a discussion of how the type of defense counsel may influence dispositional measures (and may indeed have *legitimate* effects or outcomes, as pointed out in Sec. VI). From Table 7.17, we see that defense by a court-appointed attorney was quite infrequent in Dade County, whereas representation by an attorney retained by the defendant was somewhat more likely. A comparison of defendants represented by private attorneys with those represented by the public defender shows that except for the early period robberies, where those few court-appointed attorneys who did provide representation seemed to have achieved a better nonconviction record, type of counsel had very little impact on dispositional outcomes. Whether defendants pled to all original charges or obtained a count-or-charge bargain or went to trial, or were convicted at trial, did not seem to be related to type of counsel in any consistent way. For B&E offenses, it should be noted, the record of the public defender appeared to worsen between the early period and the later one; conviction rose, largely as a result of straight pleas of guilty.[13]

Table 7.18 indicates type of disposition by the ethnicity of the defendant. In Dade County, most of the defendants making up the "other minority" category were of Cuban descent. They were relatively infrequent defendants, especially in the case of the more serious offense, robbery. For this offense, the figures show little evidence of discrimination in the jurisdiction. In fact, for robberies, both minorities were more likely to have obtained charge and count bargains in 1974 as compared with nonminority defendants, whereas in 1975, blacks had a lower gross plea rate and Cubans had a higher nonconviction rate than nonminority defendants. No salient interethnic group differences emerged in B&E cases. For both offenses, there was some tendency in 1975 for nonminority defendants to enter more pleas of guilty, as was the case for blacks accused of B&E offenses. Indeed, the year-to-year changes in the conviction rates seem more dramatic than the differences across ethnic groups.

[13] This result may well be attributable to a change in the seriousness of offenses of B&E defendants between the two years and/or to the undersampling of trials and (B&E case) dismissals in the second data collection period.

Table 7.16

EVENHANDEDNESS: THE RELATIONSHIP BETWEEN PRETRIAL CUSTODY STATUS AND DISPOSITIONAL MEASURES IN DADE COUNTY ROBBERY AND B&E CASES, 1974-75

Dispositional Measure	Percent of All Defendants in a Given Pretrial Custody Status[a]											
	Robbery						B&E Offenses					
	1974			1975			1974			1975		
	Jail	Bail	O.R.	Jail	Bail	O.R.	Jail	Bail	O.R.	Jail	Bail	O.R.
Not convicted												
Pretrial dismissal	6	12	0	5	0	0	6	13	6	5	3	0
Nol-pros and diversion	10	12	0	12	50	0	6	13	19	2	12	6
Trial acquittal, dismissal, hung jury	14	38	0	9	0	0	3	4	0	2	0	0
Total	30	62	0	26	50	0	15	30	25	9	15	6
Convicted												
Straight plea	45	25	0	45	25	100	55	35	44	55	59	50
Received plea bargain (reduced charges and/or counts)	22	0	100	27	25	0	24	27	19	33	26	33
Gross plea rate (pleas ÷ N)	67	25	100	72	50	100	79	62	63	88	85	83
At trial	3	13	0	2	0	0	6	8	12	3	0	11
All convictions (conv. ÷ N)	70	38	100	74	50	100	85	70	75	91	85	94
All dispositions (N)	100(83)	100(8)	100(2)	100(92)	100(2)	100(1)	100(33)	100(48)	100(16)	100(42)	100(34)	100(18)
Trial conviction rate (trial convictions ÷ trials)	20	25	0	20	0	0	67	67	100	50	0	100

[a]Jail, bail, or own recognizance (O.R.).

Table 7.17

Evenhandedness: The Relationship between Type of Defense Counsel and Dispositional Measures in Dade County Robbery and B&E Cases 1974-75

Dispositional Measure	Percent of All Defendants with a Given Type of Defense Counsel[a]											
	Robbery						B&E Offenses					
	1974			1975			1974			1975		
	PD	CA	DR	PD	CA	DR	PD	CA	DR	PD	CA	DR
Not convicted												
Pretrial dismissal	6	0	8	5	0	7	12	0	0	2	33	0
Nos-pros and diversion	9	33	17	11	0	22	9	0	25	6	0	13
Trial acquittal, dismissal, hung jury	15	50	0	8	25	7	1	0	10	0	0	12
Total	30	83	25	24	25	36	22	0	35	8	33	25
Convicted												
Straight plea	42	17	42	46	25	43	46	50	30	59	33	25
Received plea bargain (reduced charges and/or counts)	23	0	25	28	50	21	24	50	25	29	34	50
Gross plea rate (pleas ÷ N)	65	17	67	74	75	64	70	100	55	88	67	75
At trial	5	0	8	2	0	0	8	0	10	4	0	0
All convictions (conv. ÷ N)	70	17	75	76	75	64	78	100	65	92	67	75
All dispositions (N)	100(79)	100(6)	100(12)	100(79)	100(4)	100(14)	100(76)	100(2)	100(20)	100(85)	100(3)	100(8)
Trial conviction rate (trial convictions ÷ trials)	25	0	100	25	0	0	86	0	50	100	0	0

[a]PD = public defender; CA = court-appointed; DR = defendant-retained.

Table 7.18

Evenhandedness: The Relationship between Ethnicity and Dispositional Measures in Dade County Robbery and B&E Cases, 1974-75

Dispositional Measure	Percent of All Dependants in a Given Ethnic Group[a]											
	Robbery						B&E Offenses					
	1974			1975			1974			1975		
	NM	B	OM	NM	B	OM	NM	B	OM	NM	B	OM
Not convicted												
Pretrial dismissal	5	4	25	3	6	0	3	16	0	3	4	0
Nol-pros and diversion	19	12	0	10	11	40	14	8	23	3	5	10
Trial acquittal, dismissal, hung jury	9	17	12	0	11	20	3	2	8	4	0	0
Total	33	33	37	13	28	60	20	26	31	10	9	10
Convicted												
Straight plea	57	36	38	23	39	0	41	43	46	66	55	40
Received plea bargain (reduced charges and/or counts)	5	25	25	64	30	40	28	25	15	24	34	30
Gross plea rate (pleas ÷ N)	62	61	63	87	69	40	69	68	61	90	89	70
At trial	5	6	0	0	3	0	11	6	8	0	2	20
All convictions (conv. ÷ N)	67	67	63	87	72	40	80	74	69	90	91	80
All dispositions (N)	100(21)	100(69)	100(8)	100(31)	100(64)	100(5)	100(36)	100(49)	100(13)	100(29)	100(56)	100(10)
Trial conviction rate (trial convictions ÷ trials)	50	25	0	--	22	0	80	75	50	0	100	100

[a]NM = nonminority; B = black; OM = other minorities.

Sentence Severity Measures

Another potential indicator of the absence of evenhandedness in the treatment of felony defendants involves a comparison of the kind of sentences meted out to defendants classified, again, by ethnicity, pretrial custody status, type of defense counsel, and method of conviction. Here, fortunately, we were able to adjust for influence on sentence severity (as measured by Index A) of all specified factors, other than the one in question, through the use of multivariate statistical analysis. In other words, we estimated the independent effect on sentence, for example, of retaining a private counsel when holding constant the effects of other potential causative factors, such as charges and counts, age, ethnicity, prior record, pretrial status, and method of conviction. The entries in Table 7.19 show whether these independent effects had a positive (+) or negative (-) effect on sentence severity; the magnitude of the effect is expressed in terms of the mean value of sentence as measured by the average Index A score for that offense/year combination.

For robberies in 1974, the set of factors specified to affect sentence severity performed no better than random chance; therefore no independent effects are reported. For the following year, however, the effect of both black and Cuban status was to increase severity, the former in the three remaining year/offense combinations and the latter in only two, but with a larger effect. Failing to obtain pretrial release seemed to result in heavier sentences, ranging from 40 percent in the case of robberies to about 30 percent in the case of B&E offenses. To the extent that the pretrial release factor incorporated the heinousness of the crime or the bad character of the defendant in dimensions not captured by the original charges and by prior record factors, then those effects are confounded here. The use of privately retained lawyers increased sentences by as much as two-thirds for 1975 robbers and by as little as one-fourth for 1974 burglars. Court-appointed counsel was too rare an event in Dade County to make a statistical impact. Once more, trials were so infrequent as to fail to produce statistically interesting results in a multivariate framework. By means of the device of matching defendants convicted at trial and defendants with the same characteristics who entered straight pleas, and then calculating average values of sentence severity, we were able to estimate that the effect of jury and court trials combined was to *reduce* sentence severity somewhat.

In summary:

- Minority ethnic status appeared not to have negative effects on dispositions but did tend to increase sentence severity.
- Pretrial custody status, in which substantial variation occurred only for B&E defendants, emerged as an association between nonrelease and a higher plea rate in 1974. By 1975 this tendency had moderated, but jailees got heavier sentences in both years.
- There was little consistent difference between public defenders and private counsel (court-appointed and defendant-retained) in terms of the dispositional outcomes for their clients. Public defenders of B&E defendants were involved in more convictions in 1975 than in 1974. Those defendants represented by retained counsel, when convicted, tended to draw heavier sentences at least in half the offense/year combinations and, again, in comparison with public defender clients.

Table 7.19

EVENHANDEDNESS: THE INDEPENDENT EFFECT OF ILLEGITIMATE FACTORS ON SENTENCE SEVERITY IMPOSED IN DADE COUNTY ROBBERY AND B&E CASES, 1974-75

	Percent Change in Average Sentence Severity Score (Index A) with a Change in a Given Factor[a]			
	Robbery		B&E Offenses	
Illegitimate Factor	1974	1975	1974	1975
Ethnicity				
Black	NS	+28	+22	+14
Other	NS	+51	+51	NS
Pretrial custody status				
Jail	NS	+40	+26	+31
On bail or O.R.	NS	NS	NS	NS
Defended by private attorney				
Defendant-retained	NS	+67	+26	NS
Court-appointed	NS	NS	(b)	(b)
Convicted at trial				
Court	NS	(b)	(b)	-13[c]
Jury	NS	(b)	(b)	

NOTE: NS = not significant; + = sentence severity score increased; - = score decreased.

[a]Entries reported are those for which the regression equation was statistically significant at a 95 percent level of confidence and the regression coefficient on a given variable was statistically significant at a 50 percent level of confidence.

[b]Result not reported because of extremely low frequency of event.

[c]Calculated by comparing the average sentence for defendants convicted at trial with the average for a set of defendants entering pleas who matched those tried in the important case and background characteristics.

- The effect of choosing trial over plea was, when significant at all, to reduce the sentence, not to increase it, as the common view would hold. But our sample of trials was too small to generate a very reliable analysis.

HOW THE COURT SYSTEM TREATS DEFENDANTS WITH PRIOR CRIMINAL RECORDS

Because of a growing interest in the issue of the habitual offender or career criminal, we present here, for convenience, data on the dispositional and sentence outcomes for defendants who differed in terms of the seriousness of their prior records, even though such a distinction is not an aspect of the evenhandedness issue. The prior record categories into which defendants are grouped are those established by the California Bureau of Criminal Statistics (see Sec. IV for a more complete definition).

Dispositional Measures

First it must be emphasized that any prior record effects on dispositions revealed in Table 7.20 are not "pure"; i.e., there may well be disproportionate numbers of defendants in a given prior record category who are also members of other categories that have been shown to influence dispositions (such as nature of original charges). The data presented in Table 7.20 seem to display no particular pattern. For robbery defendants with minor records there was a marked increase over time in the plea rate and the conviction rate; but for other offense-record combinations there were no strong period-to-period changes registered. Defendants in the heavier prior record cases (major plus prison as compared with none plus minor) did have lower straight plea rates in 1975 robbery cases and in 1974 B&E cases, but no other important differences emerged in dispositional outcomes within periods and offenses. It appears, then, that Dade County, in the period under analysis, took no special notice of prior criminal record in disposing of adult felony offenders once they reached circuit court. It may well be, however, that a larger fraction of persons with relatively light prior records were screened out at the charging decision; on this issue we have no data.

Sentence Outcomes

Finally, we attempt to isolate the effect of prior record on sentence severity, holding all other specified factors constant. Table 7.21 gives estimates of the effect of each criminal record category on Sentence Severity Index A scores when compared with an otherwise identical situation in which the defendant had no prior record. In general, for B&E offenses, prior record had the expected effect; it increased the sentence, other things being equal; and the more serious the record, the greater the increase. For robberies, the explanation for 1974 did not meet our statistical standard; in 1975, prior record appeared to *reduce* the sentence meted out. There may have been mitigating circumstances associated with these crimes that we were unable to identify in our data.

Together these findings seem to suggest that it is only in the case of B&E sentences that prior record manifested a consistent pattern. Whether the lack of pattern for dispositions and the anomolous results for robbery sentences are the consequences of absence of policy or of technical considerations that have affected our analysis is a matter for further research.

DELAY

Speediness of justice is a valued objective of the system. We will therefore attempt to illuminate the factors responsible for the attainment of speedy justice or the converse in Dade County in 1974. The time cutoff for our 1975 sample of cases occurred too early in that year for our figures on delay to be meaningful for that period. An early cutoff meant that we substantially oversampled short cases, as Fig. 7.3 reveals. Figure 7.3 compares the mean delay (elapsed time from arrest to final disposition) for sampled cases originating in the same month in the two data collection years and shows that although cases originating very early in the two data

Table 7.20

The Relationship between Prior Record and Dispositional Measures in Dade County Robbery and B&E Cases, 1974-75

Dispositional Measure	Percent of All Defendants with a Given Prior Record Category															
	Robbery								B&E Offenses							
	1974				1975				1974				1975			
	None	Minor	Major	Prison	None	Minor	Major	Prison	None	Minor	Major	Prison	None	Minor	Major	Prison
Not convicted																
Pretrial dismissal	9	11	0	0	0	4	7	0	0	13	0	33	0	14	0	0
Nol-pros and diversion	9	3	18	0	3	8	7	0	9	7	5	0	0	0	5	0
Trial acquittal, dismissal, hung jury	27	11	0	43	3	0	20	0	5	0	0	0	0	5	0	0
Total	45	25	18	43	6	12	34	0	14	20	6	33	0	19	5	0
Convicted																
Straight plea	9	54	55	43	60	56	33	40	48	33	78	50	61	50	60	75
Received plea bargain (reduced charges and/or counts)	27	21	27	14	30	28	33	60	24	33	16	17	35	27	35	12
Gross plea rate (pleas ÷ N)	36	75	82	57	90	84	66	100	72	66	94	67	96	77	95	87
At trial	19	0	0	0	4	4	0	0	14	13	0	0	4	4	0	13
All convictions (conv. ÷ N)	55	75	82	57	94	88	66	100	86	79	94	67	100	81	95	100
All dispositions (N)	100(11)	100(28)	100(11)	100(7)	100(30)	100(25)	100(15)	100(5)	100(21)	100(15)	100(18)	100(6)	100(23)	100(22)	100(20)	100(8)
Trial conviction rate (trial convictions ÷ trials)	40	0	--	0	50	100	0	--	75	0	--	--	100	50	0	100

Table 7.21

THE INDEPENDENT EFFECT OF PRIOR CRIMINAL RECORD ON SENTENCE SEVERITY IN DADE COUNTY ROBBERY AND B&E CASES, 1974-75

Prior Criminal Record[b]	Percent Change in Sentence Severity Score (Index A) Associated with a Given Category of Prior Record[a]			
	Robbery		B&E Offenses	
	1974	1975	1974	1975
Minor	NS	-37	+30	+27
Major	NS	-16	+65	+42
Prison	NS	-25	+65	+69

[a]Entries are only for variables in equations that were statistically significant at the 95 percent level and in which the regression coefficient on that variable was statistically significant at the 50 percent level; entry NS (not significant) otherwise.

[b]Measured against no prior record.

collection periods (December 1973 and December 1974) had similar elapsed times, the delay measure in later cases showed wide differences.

In Table 7.22 three kinds of delay measures are presented: the median number of days between major events in a case, the minimum number of days that elapsed between major events for the slowest 10 percent of cases, and the fraction of cases that exceeded the 180-day arrest-to-trial rule obtaining in the Dade Circuit Court. The table shows delays in our 1974 sample cases in terms of elapsed times, classified by offense type. Dismissal includes nol-pros and diversion; final disposition is the point at which a case exits from the circuit court, i.e., dismissal, diversion, etc., or sentencing.

Whether measured from arrest or from arraignment, the time to dismissal was the shortest of the median elapsed times for all felonies and for robberies, but the longest for B&E offenses. Why this should be is unclear unless the legal proceedings necessary to obtain a dismissal, nol-pros, or diversion are more lengthy in B&E cases. Somewhat surprisingly, elapsed times to trial were seldom the longest of the elapsed times to major events—i.e., dismissal, etc., plea, or final disposition in the exemplary offenses—although they generally were in the sample of all felony cases.

It is interesting to note that the slowest 10 percent of the cases tended to run two to three times longer in elapsed times than did the average case. The typical robbery cases ran longer than the average for all offenses (when measured by the time from either arrest or arraignment to final disposition) and the typical B&E case is more speedy, although it is not known why nondwelling B&E cases should have taken longer to dispose than dwelling B&E cases. Elapsed time for conviction to sentence was quite uniform across offenses at four days but dropped to one day for nondwelling B&E offenses.

The Florida supreme court has established a rule that sets the maximum elapsed time from arrest to trial at no more than 180 days. It is interesting to observe how

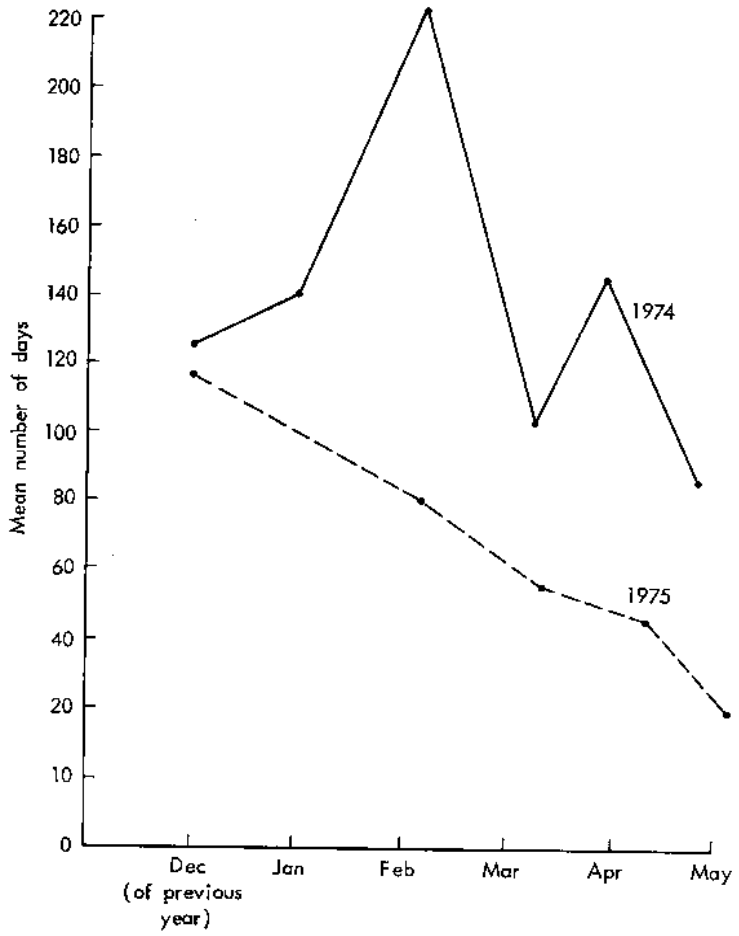


Fig. 7.3—Average case elapsed time (arrest to final disposition) for samples of felony cases originating in selecting months in Dade County, 1974-75

often this standard is met in those cases that went to trial and also for cases with other types of dispositions. Table 7.22 presents such indicators. Looking first at trials, we see that for all offense classes other than robberies (in which a third of the cases extend beyond the 180-day criterion) the objective was being fairly well attained. For other types of dispositions, the record was not as impressive, except in dwelling B&E offenses. In the other offenses, from one-eighth to one-half of dismissals took longer than 180 days, and for elapsed time, from arrest to guilty plea, there was little improvement. As a summary measure of delay, elapsed time from arrest to final disposition, which aggregates all disposition types, one-sixth to about one-fourth of all cases extended beyond the 180-day limit set for trial cases. Why robbery cases should have extended beyond 180 days so often is not obvious. It may be that more serious cases take longer to process because defense attorneys, in the attempt

Table 7.22

Measures of Elapsed Time in Dade County Felony Cases, 1974

Type of Disposition	All Felonies	Robbery[a]	B&E Offenses[a] Dwelling	B&E Offenses[a] Nondwelling
	Median Number of Days			
From arrest to--				
Dismissal	88	119	116	188
Guilty plea	109	123	76	95
Trial	131	119	109	72
Final disposition	109	124	90	103
	Minimum Number of Days for Longest 10 Percent of Cases			
From arrest to--				
Dismissal	214	--	--	--
Guilty plea	299	--	--	--
Trial	246	--	--	--
Final disposition	270	--	--	--
	Percent of Cases Exceeding 180-Day Standard			
From arrest to--				
Dismissal	12	23	0	50
Guilty plea	17	23	3	17
Trial	8	33	0	0
All cases[b]	15	23	2	18
	Median Number of Days			
From arraignment to--				
Dismissal	58	100	89	171
Guilty plea	67	91	47	57
Trial	106	103	91	45
Final disposition	83	99	62	74
	Minimum Number of Days for Longest 10 Percent of Cases			
From arraignment to--				
Dismissal	187	--	--	--
Guilty plea	237	--	--	--
Trial	198	--	--	--
Final disposition	205	--	--	--
	Median Number of Days			
From conviction to--				
Sentencing	4	4	4	1

[a]Entries for minimum number of days for longest 10 percent of cases are omitted because of small size of sample.

[b]Excluding elapsed time between conviction and sentencing.

to avoid a long sentence for their clients, request more continuances. Dwelling B&E cases showed a better record in meeting speediness standards than did all felonies; for nondwelling B&E's, the record was mixed.

Continuances are both a manifestation of delay and a cause of it. In Table 7.23 we present data on continuances granted in trials and in noncontested cases as estimated from a 1974 sample of all felony cases. We can see fairly clearly from these figures that continuances were much more common in trials (92 percent versus 67 percent); that the average trial case was continued more often (4.3 times versus 2.3 times); but that the average continuance lasted the same number of days (42). The net result, however, is that continuance contributed half again as many days to the delay in trial cases as in noncontested cases. The Dade County Circuit Court records usually do not identify the movant for a continuance that has been granted, and so our attempt to attribute responsibility for continuances resulted in rather unenlightening findings.

We had hypothesized that several factors might influence the delay experienced in a particular case when measured by the elapsed time from arraignment to final disposition. Table 7.24 shows independent estimates of the direction and size of the influence of a number of causative factors expressed as change in the mean elapsed time for all felonies and for robberies in 1974. (Other factors, used as controls, also contributed to the explanation of elapsed times. The full specifications of the regression equations are given in App. E.) We had considered that pretrial custody status would have an influence that depended on whether the defendant was in jail or on pretrial release, because those in jail would be anxious to reach a disposition, which

Table 7.23

CONTINUANCE MEASURES IN DADE COUNTY, 1974
(Based on sample of all felony cases)

For uncontested cases	
Number of cases continued ÷ all cases (%)	67
Number of continuances ÷ all cases	2.3
Number of days continued ÷ number of continuances	42
Number of days continued ÷ all cases	97
Percent of total number of continuances attributed to--	
Defense	9
Prosecution	7
Court and other, including unidentified cases[a]	84
(N)	(86)
For contested cases	
Number of cases continued ÷ all cases (%)	92
Number of continuances ÷ all cases	4.3
Number of days continued ÷ number of continuances	42
Number of days continued ÷ all cases	149
Percent of total number of continuances attributed to--	
Defense	12
Prosecution	5
Court and other, including unidentified cases[a]	83
(N)	(12)

[a]Attributed to court alone, defense and prosecution together, and unidentified attribution. In Dade County records, the majority of continuances granted cannot be attributed among types of movants.

Table 7.24

THE INDEPENDENT EFFECT OF HYPOTHESIZED INFLUENCES ON ELAPSED TIME FROM ARRAIGNMENT TO FINAL DISPOSITION FOR ALL FELONIES AND FOR ROBBERIES IN DADE COUNTY, 1974

Influence on Elapsed Time	Percent of Change in Average Elapsed Time	
	All Felonies	Robbery
Pretrial custody status		
Jail	NS	+39
Bail or O.R.	NS	+97
Defended by private attorney	NS	NS
Proxy for court calendar crowding	+7	+5
Type of disposition		
Dismissed	-18	-30
Plea bargained	-33	-42
Tried	+54	+32
R^2	(.37)	(.52)

NOTE: NS = not significant.

might well reduce total jail time; on the other hand, those on bail or O.R. might appreciate further delay, which could create problems for the prosecution because of witness availability. But both categories of pretrial custody status turned out to have nonsignificant effects for felonies in general and both increased elapsed times in robberies, but bail-and-O.R. status had a larger effect. Type of defense counsel was not a significant influence for either robberies or all offenses, although we had speculated that private attorneys may have asked for continuances more frequently or that the public defender may have had more familiarity with the system. Our time-trend variable, a proxy for the build-up in the case backlog, had a small but very consistent effect in lengthening case times in both offense groups. Type of disposition had the expected effects, confirming the finding reported from Table 7.22. Dismissals and charge and count bargains shortened elapsed times (when measured against straight pleas), whereas trials (court and jury combined) lengthened elapsed times.

In summary:

- Delay tended to be shortest for cases where dismissal occurred and longest for cases going to trial, but the pattern was not consistent across offense categories.
- The slowest 10 percent of cases took two to three times as long as the typical case.
- Robbery cases experienced relatively long delays and B&E cases relatively short ones when compared with all felonies.
- Dade County met its 180-day arrest-to-trial standard fairly consistently except in robbery, although other types of dispositions quite often extended beyond the 180-day criterion, again especially in robbery cases.

- Continuances had a substantial influence in lengthening trial cases, as compared with noncontested cases.
- The size of the case backlog tended to exert a consistent but small influence on length of delay in the typical case; other hypothesized factors, such as pretrial custody status and type of defense counsel, exhibited no very consistent influence on delay.

USE OF VICTIMS, OTHER WITNESSES, AND JURORS

The only data available from the circuit court records pertaining to the use of victims, witnesses, and other jurors was on the number of witnesses called per case. The remaining data on number of appearances per lay participant and time spent by the lay participant in various activities were obtained from responses to mail surveys of these three classes of participants. The responses, collected in the fall of 1975, rely on the memories of those who participated in cases in the spring of 1975 and thus must be viewed as rough estimates of what actually occurred.

Table 7.25 presents statistics on the use of victims and witnesses, about 90 percent of whom appear to have cooperated with the courts when asked to do so. Victims and other witnesses spent between two and three hours at the courthouse on an average appearance. But since the average victim appeared 2.2 times, and since there was an average of one victim per case, the number of appearances per victim per disposition also averaged 2.2. Multiplying (i.e., 2.2 times 2.1) we get an estimate of 4.6 victim hours expended per disposition in Dade County. In comparison, the average other witness appeared four times per case and, since the average case had 1.4 other witnesses, our estimate of the appearance per witness per disposition is 5.6, and the average number of hours expended per witness per disposition is 14 (i.e., 5.6 times 2.5).

In Table 7.26, we record survey response-based estimates of juror time expenditures. Jurors, we see, tended to spend a substantial fraction of their time waiting in the jury room. Although the survey responses showed some juror time that was devoted to civil cases, we believe this result to stem from response errors of the jurors surveyed since, in Dade County, separate jury pools are maintained for criminal and civil cases. The remaining time was spent in voir dire and trials in criminal cases, but neither activity absorbed as much time as the essentially idle waiting time spent in the jury room or elsewhere in the courthouse. (The sum of the average percentages exceeds 100 because jurors failed to allocate their time properly by duration across types of activities; we feel, however, that the averages are probably good approximations of the proportional pattern of juror activities.)

* * *

NOTE: During our data collection period in Dade County, the circuit court was in the process of collecting information on the duration and frequency of the activities engaged in by judges as part of a weighted caseload analysis. Access to this information would have permitted us to comment on the efficiency with which judicial time was used in the system; unfortunately we were unable, after repeated attempts, to gain such access.

Table 7.25

MEASURES OF THE USE OF VICTIMS AND WITNESSES IN DADE COUNTY, SPRING 1975

Type of Response from Lay Participants	Victims	Other Witnesses[a]
Cooperativeness		
Cooperative (%)	90	88
Not cooperative (%)	6	10
Not asked (%)	4	2
Total responses (N)	100 (101)	100 (100)
Average number of appearances by lay participants	2.2	4.0
Average number of appearances by lay participants per disposition	2.2[b]	5.6[c]
Duration of appearances by lay participants		
Less than one hour (%)	10	6
One to two hours (%)	45	44
Three hours (all morning or afternoon) (%)	43	41
Six hours (all day) (%)	2	9
Total responses (N)	100 (88)	100 (79)
Average duration of appearance (hr)	2.1	2.5

[a] 91 percent of the witnesses surveyed appeared for the prosecution.

[b] Assumes one victim per disposition × 2.2 appearances per victim.

[c] Assumes 1.4 witnesses called per disposition × 4.0 appearances per witness.

Table 7.26

MEASURES OF THE USE OF JUROR TIME IN DADE COUNTY, 1975
(Percent of time spent)

Activity	Percent of Jurors Responding					Average Percent of Time Spent
	None	Less Than 25	25–49	50–74	75–100	
Waiting in jury room (N = 160)	1	23	31	26	19	47
Jury selection: criminal cases (N = 161)	3	60	27	5	4	25
In trial: criminal cases (N = 154)	9	29	30	20	12	38

GAUGING OVERALL PERFORMANCE: SUMMARY AND COMMENTS

We will now attempt to synthesize what we have learned about the performance of the judicial, prosecutorial, and defense agencies that deal with adult felony defendants in Dade County. We will state the most important findings and inferences, followed by a brief discussion noting changes in performance measures over time, across offenses, or along other interesting dimensions. Necessary qualifications to our findings, occasioned by inadequacy of sampling, measurement ambiguity, or problems in statistical analysis, are given where appropriate.

I. *A substantial fraction of suspects arrested for the exemplary offenses were not subjected to felony proceedings in circuit court.*

Rationale: The proportion of police bookings rejected, which averaged about one third (and occurred primarily as a result of the suspect not being held to answer by the magistrate and secondarily as the result of "no action" and "no information" decisions by the state attorney) remained fairly substantial and constant across time periods (1974 and 1975) and exemplary offenses (B&E offenses and robbery).

Qualifications: (1) The available records did not enable us to ascertain the cases in which the defendant was arraigned in circuit court on charges of a lower felony degree than those on which he was booked by the police. (2) The data on reasons why the case was rejected either by the magistrate or the state attorney were very limited.

II. *Charging accuracy appears to have improved somewhat between 1974 and 1975.*

Rationale: For dwelling B&E offenses, but not appreciably for other B&E offenses or robbery, the no-conviction rate fell and the rate of conviction on all charges as filed rose. The frequency of pretrial dismissal and conviction on lesser charges than the most serious filed also declined for B&E defendants, but again the decrease was not impressive.

Qualifications: (1) The nature of dispositions also depends on reasons other than the accuracy of the charge. (2) The time truncation in our data collection for B&E cases in 1975 may have biased our sample of cases toward pleas of guilty and away from dismissal dispositions, which took longer. Since a large fraction of pleas in Dade County were to all original charges, these biases may account for some or all of the apparent improvement.

III. *The performance measures we were able to construct could not adequately capture the nature or impact of guilty plea negotiations.*

Rationale: Most plea negotiations in Dade County take the form of sentence agreements, but these are not recorded in case folders. Reductions in charges and counts were relatively uncommon (particularly for the more serious offenses) and had unimpressive effects on dispositions and on sentences when they did occur. Lack of appropriate data made it impossible to capture the impact of sentence agreements (relative to straight pleas) on sentence severity imposed. However, resistance to charge and count bargaining in robbery cases *may* be responsible for the longer elapsed times experienced for those cases.

Qualification: The dispositional bias resulting from the time truncation in the second data collection period (i.e., fewer trials and (B&E) dismissals, more pleas) undermines the reliability of any year-to-year findings relative to this issue.

IV. *Illegitimate factors play some role in explaining variation in sentencing; legitimate factors have larger and more consistent impacts on sentence outcomes. Little year-to-year change is observable.*

Rationale: Sentence severity (as scored by indices that conflate various categories of sentence) was generally associated with the seriousness of the offense and a similar pattern was applied to both data collection periods, although sentence severity on second and third degree B&E convictions did increase somewhat over time. When other legitimate factors were introduced—age of defendant, his prior criminal record, terms of bargains struck—an appreciable share of sentence variance was "explained." Illegitimate factors, such as ethnicity, pretrial custody status, type of defense counsel, and crowded correctional facilities, sometimes affected sentences, but the effects were either small or were inconsistent across offense/year pairs.

Method of Conviction: The fact of conviction at trial, holding constant other characteristics of the case and defendant, did not add to the severity of the average sentence when compared with those convicted by guilty plea.

Qualifications: (1) The ubiquity of unrecorded sentence agreements made it impossible to truly isolate the impact of the various factors. (2) Trials were such rare events in our sample that generalizations concerning their effects are subject to substantial error.

V. *Dispositional outcomes were affected by some illegitimate factors.*

Rationale: Though ethnicity had little association with type of disposition, robbery defendants who failed to obtain pretrial release were more likely to be convicted on all charges, especially in the first period studied. Representation by private counsel (court-appointed and defendant-retained) had no consistent effect on sentence outcomes. The conviction rate for B&E defendants represented by the public defender rose in the second period.

Qualifications: (1) Estimates of effects are based on cross-tabulations that cannot adequately capture the independent influence of a particular factor. (2) The effect of any of the factors on the screening of felony suspects is unknown.

VI. *A defendant's prior criminal record had little impact on how his case was disposed, but often did tend to affect the severity of his sentence.*

Rationale: No clear pattern of dispositional outcomes by prior record emerged from cross-tabulations of the data. Persons convicted of B&E offenses received heavier sentences according to the seriousness of their past behavior, but convicted robbers experienced no such penalty.

Qualifications: As above under finding V.

VII. *Median elapsed times (arraignment to final disposition) were affected by the nature of the offense and the type of disposition and by the size of the felony case backlog. The typical felony case lasted 109 days. The Master Calendar Experiment began too recently to permit an evaluation of its impact on delay.*

Rationale: As to case delay in 1974, median elapsed times were relatively longer for robbery than for all felonies and shorter for B&E. Generally, dismissals occurred fairly quickly, especially when compared with cases going to trial. An increase in the case backlog added to elapsed times for all felonies and for robberies; pretrial custody status affected robbery elapsed times. The 180-day standard for arrest-to-trial was usually met except for robberies, but other types of dispositions (e.g., dismissals and guilty pleas) frequently exceeded this period, especially in robbery cases.

Qualification: All of these results apply to the 1974 data collection period only. Sample problems made it infeasible to ascertain whether these same relationships extended into 1974 or to attempt to assess the impact of the Master Calendar Experiment.

VIII. *Based on their mail survey responses, victims, other witnesses, and jurors devoted considerable time to assisting felony proceedings. Juror idleness seemed excessive.*

Rationale: Victims and other witnesses spent between two and three hours at the courts on an average appearance. Given estimates of the numbers of each type of participant per case and the number of times each appeared, we can conclude that about 5 hours of the victim's time and about 14 hours of the time of other witnesses were devoted to the typical case. Jurors reported that they spent more time waiting in the jury room than they spent in either jury selection or case deliberations.

Qualification: Responses are based on memory and often do not properly account for all the time a lay participant spent. No data were available on the use of judicial time.

IX. *Problems of data availability make it impossible to render an overall evaluation of performance. For most issue areas, trends over time cannot be ascertained.*

Rationale: Screening appears to be sensitive and charging accuracy may have improved somewhat. Illegitimate factors do not seem to play a major role in dispositions or sentences. The systemic impact of plea negotiations cannot be ascertained. Delays, however, are long, for which the growing case backlog must bear a considerable responsibility; efficiency in the use of lay participants might well be augmentable.

VIII. THE ROLE OF CRIMINAL CASE AUDITING IN PERFORMANCE MEASUREMENT

Marvin Lavin, Raymond Sinetar, and Gerald Uelmen

BACKGROUND

Case Auditing Characterized

In the context of this performance measures study, the term *case auditing* designates a procedure in which an experienced criminal justice practitioner or a team of such individuals closely examines the records of a selected sample of cases that have entered the court system being analyzed and have undergone (or are undergoing) disposition. The auditors may be internal personnel—i.e., participants in the regular operations of the system, with or without substantial management or policymaking responsibilities; or they may be external and more or less independent in their relationship to the system. How the sample of cases should be chosen for auditing would properly depend on the purposes of this review. For example, the appropriate sampling might be fully random over a period of system activities, or be random within a stratified or clustered design, or even be confined to (and possibly exhaust) a specified class of cases—for example, those with multiple defendants, filed within a stated period, and not reaching trial within 270 days from date of initial arrest.

Whatever the designated purposes of the audit, those conducting it would seek

- To assemble the full documentation on the selected cases.
- To assess the state of the evidence at various points during the proceedings.
- To discern departures from normally prevailing policies of the agencies involved.
- To comprehend the reasons why events occurred as they did.

For completeness, case auditing might include interviews with some of the participants in the selected cases, not only with some of the practitioners but also possibly some of the lay persons who were involved. Indeed, where the auditors are external to the system and are likely to lack firsthand knowledge of local rules and policy, they may be compelled to interview senior system officials to obtain a proper foundation for their work. Normally, materials pertaining to each of the selected cases would be drawn from various case files, including those of the police, the prosecutor's office, the court, the public defender's office, the probation authorities, etc. While the coverage of these several sources tends to overlap on some aspects of a case, the appearance of disparities among them on the same item could be especially revealing to an auditor.

Case auditing might be conducted at regular intervals, say, quarterly or semiannually, serving to continually monitor the operations of the court system or a component agency; for this purpose it may stand alone (especially if the system is a small one) or be an adjunct to other management information devices. Alternative-

ly, case auditing could be an occasional activity within the jurisdiction, to illuminate the causes of, and remedies for, operational problems.

Case Auditing Related to Statistical Performance Measures

We are interested in the case auditing approach for at least three reasons:

1. As a performance assessment system in itself: Some court systems, particularly smaller ones, may want to install case audit procedures as an alternative to statistical performance measures for evaluating operations and programs.
2. As substantiation to the statistical approach: Case auditing can provide another perspective into the processes of a court system and thus be used as a check on the insights derived from statistical performance measures.
3. As a source of new hypotheses on how court systems work: By delving behind quantitative data, case auditing of even a relatively small sample of cases may reveal explanations not made apparent by statistical performance measures.

For this discussion, let us assume that a court system employs statistical performance measures. It may not possess a formal information system, but nevertheless it systematically collects some data on various aspects of its operation and uses them to provide quantitative indices of performance. These measures may be few in number and gross in texture—for example, a complaint rejection rate, a felony case filing rate, a plea of guilty rate, a trial rate, a trial conviction rate, an average time between arrest and trial, and possibly others. Or they may be numerous and richly detailed, as typified by a measure such as proportion of jury trials producing convictions, where the defendant was a black, represented by a public defender, and held in custody throughout the proceeding. But whatever the extent of the performance measures that the court system presently employs and the uses it makes of them, an appropriate question to raise is, *What value would case auditing have in this context?* Answers to this question should reflect the essential distinction between the two information approaches. On the one hand, statistical performance measures purport to capture a relatively limited amount of information about each of a relatively large body of events; by enlarging the set of such measures, one gains completeness in describing system performance. By contrast, case auditing offers a deeply detailed view of how a relatively small sample of cases is processed by the system from entry to disposition and why events turned out as they did.

To begin with, one value of the case auditing approach is that it goes behind numerical rates to uncover true causes of events. It can help establish the degree of confidence that may be placed on inferences from a set of performance measures about a population of cases that has been processed by the system. If the audit findings from a sample of cases are consistent with the statistical inferences concerning the parent population, then confidence in the latter would be enhanced. For example, suppose that one observed the following magnitudes in a given set of measures, relative to an earlier period or perhaps by comparison with those in a closely similar jurisdiction:

Complaint rejection rate—lower.
Pretrial dismissal rate—higher.

Guilty plea rate with charges as originally filed—lower.
Trial rate with charges as originally filed—lower.
Trial conviction rate on charges as originally filed—lower.
Guilty plea rate with lesser charges—higher.

One inference that might be drawn from the magnitudes of these measures is that the prosecutor's office has increased its propensity to "overcharge." Confidence in this inference, as against alternative explanations, would be enhanced by a showing that cases were indeed being filed at levels of charge severity and multiplicity not sufficiently supported by the evidence available at time of screening. Within the limitations of sample size, case auditing could provide such a showing; or, alternatively, help to discredit the inference.

Another way of expressing this potential value of case audits conducted as an adjunct to the use of statistical performance measures is to say that they are means of increasing the *reliability* of the entire performance information system. To this end, auditors should scrutinize the sample cases as if prompted by the question, *What significant details of performance are being concealed by the statistical nature of the calculated measures?*

One shortcoming of statistical performance measures is that in some circumstances the inferences desired from them are ambiguous. To illustrate, consider the difficult problem of assessing the accuracy of prosecutorial charging—that is, on the basis of information available at the time of complaint screening, does the prosecutor file those charges for which the available admissible evidence will support a conviction? This question may be illuminated, if imperfectly, by collecting data and calculating performance measures to show what happens to defendants in terms of the original charges and the ultimate disposition. One asks: What is the bind-over rate? What is the overall pretrial dismissal rate? At what rate are cases disposed of as a result of a plea to lesser charges? With what relative frequency are defendants tried on charges less severe than those originally filed? On the average, how large a reduction in sentence severity occurs when cases are disposed of by pleas of guilty to reduced charges? At what rate do trial convictions result on charges less serious than those originally filed? And so on. The more extensive the set of relevant measures, the less ambiguous could be inferences concerning prosecutorial charging accuracy. Yet, the outcome of each event in a criminal proceeding is a product of many factors, only one of which might be charging inaccuracy. Pretrial dismissals occur for various reasons other than inaccurate charging. Plea bargains reflect many considerations in addition to the possibility of inaccurate charging. In a practical sense, some degree of ambiguity is unavoidable in assessing the accuracy of prosecutorial charging by means of statistical performance measures alone. Case auditing affords a means of reducing the imprecision of such inferences; its impact will depend on the scope and frequency of the auditing efforts.

Case auditing, in other words, can aid in locating the "missing variables" which help explain the outcome in question. And a major advantage of the auditing approach is that such elusive variables may come to light in the very process of close-range personal observation of the workings of the system.

Many events in a criminal proceeding are the outcome of the legitimate exercise of prosecutorial or judicial discretion. For example, ought prosecution be deferred in favor of diverting the defendant into a rehabilitation program? Do "the interests

of justice" justify filing no charges against the suspect? How much expert assistance at public expense should be provided the indigent defendant in preparing his defense? Should sentencing be suspended in favor of unsupervised probation? Inevitably, questions are raised about possible abuses of discretion. Sometimes these issues are properly presented to higher courts in the course of appeals. Often they are not and cannot be so reviewed as in the instance of prosecutorial discretion to reject a complaint. Statistical performance measures may serve to give a gross indication of the presence of abuse when they attest to an anomalously high frequency of a particular action, but case audits (combined with appropriate interviews) would then be necessary to expose the problem more fully.

Our concluding comment on the value of case auditing in relation to the use of statistical performance measures touches on what may have the richest potential of all. Case audits, even of modest dimensions, can reveal overlooked ways of interpreting the calculated magnitudes of performance measures. That is, case audits can produce insights about criminal proceedings which escape an unimaginative consideration of statistical performance measures.[1] And case audits may suggest new hypotheses about what is going on in the court system to be tested, by, among other ways, inspecting the past and current magnitudes of the statistical measures.

Practitioner Viewpoints on the Role of Case Auditing

As one phase of our study of statistical performance measures, we conducted a set of interviews aimed at eliciting the views and attitudes of criminal justice practitioners (judges, court administrators, defense counsel, prosecutors, and academicians) toward the value and use of performance measures.[2] Thirty-three professionals were queried in the course of 26 interviews, which ranged from an hour and a half to a full day. The interviews were conducted in more than a dozen jurisdictions located in major metropolitan areas throughout the country. Case auditing was one of many topics on which these practitioners expressed their opinions.

Current professional views about the relative roles of case auditing and statistical performance measures seemed strongly divided. Some of our interviewees felt that reliable monitoring of the performance of a court system required case auditing. To them, this role was dominant. Others were not so extreme, but if compelled to a choice between case auditing and statistical performance measures, would tend to opt for the former as their information source. Still others put a relatively higher value on statistical measures, but chiefly as a means of internal management control. Others advocated a broader role for statistical measures, in which they included the assessment of progress toward the external goals of the court system and the comparison of one jurisdiction's system with that of another. Generally, they expressed no opposition to case audits per se; their divergence of views related to how much and how regularly case auditing should be performed, given its costliness and the alternatives available in other information sources.

[1] For example, as discussed in Sec. VI, an experiment to limit the use of plea bargaining in one of our pilot demonstrations resulted in a much higher rate of conviction at original charge level; case auditing revealed that improved preparation of cases by the local police was, along with increased prosecutorial zeal, contributing to the desired outcome.

[2] This survey is described in detail in Sec. III.

CASE AUDITING IN THIS STUDY

Scope and Design

Prompted by our appreciation of the relevance of case audits to the use of performance measures, as discussed above, and by comments made in the interviews, we included a modest case auditing effort in the demonstration phase of this study. Case audits were conducted in the circuit court systems of both Multnomah County, Oregon, and Dade County, Florida. The auditing team was composed of two Rand consultants—one a highly experienced prosecutor and supervisor from the Office of the District Attorney, Los Angeles County (the largest, and generally regarded as one of the most professional in the country) and the other a professor of criminal law and a state-certified specialist in the field, who had substantial experience as both a criminal defense counsel and a federal prosecutor.

The scope of this undertaking was dictated mainly by the resource limitations on our study. Its design derived from our specific interests, as described below, and from the constraints on scope.

The auditing team was able to devote three to four working days in each of the two demonstration jurisdictions. Given this limitation, it was estimated that the appropriate sample size at each location would be roughly 20 to 25 cases.[3]

The samples, as finally selected, comprised 20 cases in Multnomah County and 23 cases in Dade County, drawn at random (subject to a sampling guide described below) from the much larger collection of cases subjected to statistical analysis during the demonstration phase of our study. However, the case auditing effort was conducted and assessed before preliminary notions as to what the statistical analysis would show were available. In a real sense, therefore, this limited case audit effort can be viewed as an independent check of *some* of the inferences that we subsequently drew from our central statistical approach to measuring performance.

Our case auditing effort focused primarily on plea bargaining. The small samples necessarily limited the potential of the audits to illuminate prosecutorial screening and sentencing variations. The audits proved even less useful for examining evenhandedness and case processing efficiency, the other selected issue areas. In Multnomah County we sought to assess the effects of an important no plea negotiation experiment involving three types of offenses (namely, dwelling burglaries, armed robberies, and receiving stolen property), under Portland's High Impact Anti-Crime Program, supported by LEAA funding. We were prompted to tailor the Multnomah case audit sample to reflect this special interest (which extended to our statistical performance analysis as well). To select our samples we used the following guides:

- Only cases whose disposition was by pretrial pleas of guilty would be chosen.
- Approximately one-half of the cases would be sampled from a period of months preceding the initiation of the district attorney's No Plea Negotiation Experiment; one-half of the cases, from a period following its initiation. The effect of defendants' characteristics would be controlled by match-

[3] It should be noted that the sample sizes used to calculate statistical performance measures averaged over 600 cases in each jurisdiction.

ing (insofar as possible) each sampled case in the first period with one in the second period, the matching being in terms of the age and prior record of the defendants, the two most accessible items of personal information.

- Approximately one-half of the cases from each time period would be chosen to contain at least one count of Dwelling Burglary I as the most serious charged offense, this being one of the Impact crime types subjected to the No Plea Negotiation Experiment; for purposes of comparison (i.e., as a "control"), one-half would be cases of Nondwelling Burglary I, which were not Impact offenses and were therefore processed differently.

The planned Multnomah County case audit sample is shown in Table 8.1.

The Dade County case audit sample was in large measure shaped by the earlier Multnomah design because we wanted to enhance interjurisdictional consistency and thus make comparison easier. Again the sample was divided between two periods of time—namely, before and after the initiation of a master calendar experiment in the circuit court. This basis for a division in time was convenient and definite, but otherwise had little significance to the case auditing activity, which involved too few cases to shed light on the calendaring experiment. Once again we concentrated on burglary offenses, notwithstanding differences between Oregon and Florida statutes in the definitions of these offenses and their penalties. Matching of the defendant's age and prior record in paired cases from the two time periods was also attempted here. Table 8.2 shows the planned Dade County case audit sample.

Findings from the Multnomah County Case Audit

Data drawn from the files of the 20 cases audited by the Rand team in Multnomah County are summarized in Table 8.3, and observations and inferences made by the auditors are given below, broadly grouped by issue area. It is to be remembered, however, that the primary focus of this auditing activity was on plea bargaining, with the sample designed accordingly.

Prosecutorial Screening. *Nondwelling Burglaries.* The sample contained nine *nondwelling burglary cases*, four from the first (1973) period and five from the second (1974). All of these cases were strong, with the suspect being apprehended at the scene, generally as the result of a police response to a silent alarm. Furthermore, legally admissible confessions were obtained from five of the nine defendants. There appeared to be a form of overcharging in the screening of nondwelling burglaries in both time periods; that is, they were routinely charged as Burglary I whenever any type of implement was used that could be characterized as a "burglary tool," even if it was only a screwdriver. Five of these nine cases originally charged as Burglary I were disposed of by a plea to a reduced charge. In only two cases were multiple charges filed originally; in both, conviction was on a single charge. The screening of these cases was performed by the specialized Intake Unit of the district attorney's office, located separately in a police facility. The unit's filing policy was not standardized, but was purported to be "somewhere between a prima facie case and beyond a reasonable doubt."

Dwelling Burglaries. Taken as a whole, the 11 *dwelling burglary cases* in the audit sample—six in the first period prior to the initiation of the No Plea Negotiation Experiment and the formation of the Impact Unit in the district attorney's

Table 8.1

PLANNED CASE AUDIT SAMPLE FOR MULTNOMAH COUNTY, OREGON
(Burglaries[a] with dispositions only by plea of guilty)

Case No.	Dwelling	Period[b]	Defendant's Age	Defendant's Prior Record[c]
1	Yes	1	20-24	None
2	Yes	1	24+	Prison
3	Yes	1	24+	Minor
4	Yes	1	24+	Major
5	Yes	1	24+	Major
6	Yes	1	20—	None
7[d]	Yes	2	20—	None
8[d]	Yes	2	20-24	Minor
9[d]	Yes	2	20-24	Major
10[d]	Yes	2	24+	Major
11	Yes	2	20—	None
12	No	1	24+	Prison
13	No	2	20-24	None
14	No	1	20-24	None
15	No	1	24+	Major
16	No	2	24+	Prison
17	No	2	20—	None
18	No	2	20-24	None
19	No	2	24+	Prison
20	No	1	24+	Prison

[a]Entering and remaining unlawfully in a dwelling with intent to commit a crime in the dwelling is burglary in the first degree, a Class A felony; if the building involved is not a dwelling and the perpetrator is not armed either with a burglar's tool or a deadly weapon and does not cause or attempt to cause physical injury, the offense is burglary in the second degree, a Class C felony (ORS 164.225).

[b]Period 1 comprises the first 11 months in 1973; Period 2, the first 11 months in 1974.

[c]The four categories of prior record are defined in Sec. IV. Definitions are based on the work of the California Bureau of Criminal Statistics.

[d]Impact cases.

Table 8.2

PLANNED CASE AUDIT SAMPLE FOR DADE COUNTY, FLORIDA
(Burglaries[a] with dispositions only by plea of guilty)

Case No.	Dwelling	Period[b]	Defendant's Age	Defendant's Prior Record[c]
1	Yes	1	20—	None
2	Yes	1	20—	Major
3	Yes	1	20-24	None
4	Yes	1	20—	Prison
5	Yes	1	24+	Major
6	Yes	2	20—	None
7	Yes	2	20—	Major
8	Yes	2	20-24	Minor
9	Yes	2	20-24	Prison
10	Yes	2	20-24	Major
11	Yes	1	20-24	Minor
12	Yes	1	24+	Prison
13	No[d]	2	20—	None
14	No[d]	1	20—	None
15	No[d]	2	20-24	None
16	No[d]	1	24+	Prison
17	No[e]	2	24+	Major
18	No[d]	2	20—	None
19	No[e]	2	24+	Prison
20	No[d]	2	20-24	None
21	No[d]	2	20-24	Prison
22	No[d]	1	20-24	Prison
23	No[d]	2	20—	None

[a]Breaking and entering a dwelling house, when armed with a dangerous weapon or if an occupant is assaulted, is a felony of the first degree; without such arming or assault, it is a second-degree felony (SS 810.01).

[b]Period 1 spans 6 months from December 1973 through May 1974; Period 2, 6 months from December 1974 through May 1975.

[c]The four categories of prior record are defined in Sec. IV. Definitions are based on the work of the California Bureau of Criminal Statistics.

[d]Refers to the offense defined in SS 810.02, breaking and entering a building other than a dwelling (or a ship or vessel) with intent to commit a felony--a burglary offense that is a second-degree felony.

[e]Refers to SS 810.05, breaking and entering any building (or ship, vessel, aircraft, or railroad car) with intent to commit a misdemeanor--a third-degree felony.

Table 8.3

MULTNOMAH COUNTY CASE AUDIT INFORMATION

Case No.	Defendant's Background: Age	Prior Record	Custody Status	Type of Counsel[a]	Days (Arrest to Sentence)	Original Charge[b]	Convicted Charge	Sentence	Strength of Case: Sentencing Judge	Caught at Scene	Admission	Affirmative Defense	Physical Evidence	Severity of Offense: Weapon	Theft	Victim Present	Mode of Entry	Number of Co-Defendants	PSR[c]
1st Period, Dwelling																			
1	20	No	O.R.	PD	33	B-1	Theft-1	2 yr-prb $500 fine	1	Yes	No	No	Poss pty	No	Yes, stereo	No	?	0	No
2	25	Prison	Jail	PD	113	B-1 Theft-1	Theft-1	1 yr-jail 5 yr-prb	1	No 1 EW[d]	No (Denial)	No	Poss pty	No	Yes, TV	No	?	0	Yes
3	26	Minor	O.R.	DR	170	B-1	B-2	18 mo-prb $85 fine	2	Yes	Yes	No	Poss pty	No	Yes, food	No	?	2	No
4	29	Major	Jail	PD	149	B-1 CAID Hars	B-1	5 yr-prb	1	Yes	Yes	No	Poss pty	No	Yes, purse, clothes	Yes	?	0	Yes
5	32	Major	Jail	PD	56	B-1	B-1	5 yr-prb $200 fine	3	No	No	No	Poss pty	No	Yes, TV	No	?	0	Yes
6	19	No	O.R.	PD	23	B-1 Theft-1	Attempted B-2	18 mo-prb $250 fine	2	No	Yes	No	Poss pty	No	Yes, TV	?	Forced	1	No
2nd Period, Dwelling (Impact Offenses)																			
7	18	No	O.R.	PD	205	B-1 Escape	B-1	5 yr-prb 1 yr-jail	4	No	Yes	No	No	No	Yes, rifles	No	Window	1	Yes
8	23	Minor	O.R.	CA	137	B-1	B-1	10 yr-prison	5	Yes EWs	No	No	Poss pty	No	Yes, $4, purse	No	Back door	0	Yes
9	24	Major	Jail	CA	89	B-1	B-1	1 yr-jail 5 yr-prb $216 fine	4	No	No	No	FP[e] & poss pty	No	Yes, checks, stereo	No	?	0	Yes
10	34	Major	Jail	CA	107	B-1 B-2	B-1	5 yr-prb	3	No	No (Denial)	No	Poss pty	No	Yes	No	?	0	Yes
11	18	No	O.R.	PD	96	B-1	B-1	5 yr-prb 1 yr-jail	3	Yes	?	No	No	No	Yes, liquor	Yes	Back door	0	Yes
1st Period, Nondwelling																			
12	32	Prison	Jail	PD	34	B-1	B-2	3 yr-prison	6	Yes	Yes	No	No	Knife	No	No	Roof	0	No
13	21	No	O.R.	CA	58	B-1	B-2	3 yr-prb	2	Yes	?	No	Tool	No	No	No	Door	1	No
14	21	No	O.R.	PD	71	B-1 Mschf	Crim. Mschf-2	3 yr-prb	4	Yes	No	Yes	No	No	No	No	Forced	0	No
15	33	Major	Jail	PD	87	B-1	B-1	10 yr-prison	3	Yes	Yes	No	No	No	Yes, money	No	Picked lock	0	Yes

(continued)

Table 8.3—continued

Case No.	Defendant's Background: Age	Prior Record	Custody Status	Type of Counsel[a]	Days (Arrest to Sentence)	Original Charge[b]	Convicted Charge	Sentence	Strength of Case: Sentencing Judge	Caught at Scene	Admission	Affirmative Defense	Physical Evidence	Weapon	Severity of Offense: Theft	Victim Present	Mode of Entry	Number of Co-Defendants	PSR[c]
2nd Period, Nondwelling																			
16	60	Prison	Jail	PD	97	B-1	B-2	3 yr-prb	7	Yes	No	No	Pry tools	No	Attempted	No	Forced	0	No
17	19	No	O.R.	CA	74	B-1	B-2	3 yr-prb $100 fine	8	Yes	Yes	No	No	Gun in car	Yes, TV	No	Back door	2	Yes
18	21	No	O.R.	PD	165	B-1	Attempted B-1	5 yr-prb $505 fine	8	Yes	Yes	No	FP	No	Yes, food	No	Roof hole	4	Yes
19	28	Prison	Jail O.R.	PD	175 (350)	B-1	B-1	5 yr-prb	3	Yes	No	No	Master key	No	Yes, drugs	No	Key	1	Yes
20	35	Prison	Jail	PD	147	B-1 Ex-convict w/gun	B-1	15 yr-prison	4	Yes	Yes	No	Tools	Gun	Attempted, money	No	Window	0	Yes

[a]PD = public defender; DR = defendant-retained private attorney; CA = court-appointed private attorney.

[b]CAID = criminal activity in drugs; Hars = harassment (Class B misdemeanor); Mschf = criminal mischief (Class A misdemeanor if in the 2nd degree).

[c]PSR = pre-sentence report.

[d]EW = eye witness.

[e]FP = fingerprints.

CASE NOTES: Case 5 - Federal charges pending (defendant received 5-yr commitment to federal program); defendant has drug problem.
Case 7 - Escape was from juvenile facility; defendant has extensive juvenile record; implicated by co-defendant.
Case 8 - DA agreed to take no position on sentence.
Case 9 - Defendant has drug problem.
Case 10 - B-1 charge based on residence, B-2 on dentist's office; defendant has 15-yr drug problem.
Case 11 - DA agreed to take no position on sentence; probation suspended after 3 mo with 3-yr sentence then imposed.
Case 12 - Escapee at time of offense.
Case 13 - School building burglarized.
Case 14 - Entered office of rubber co. for alleged purpose of using phone; DA agreed not to oppose probation.
Case 15 - Had served five years in San Quentin.
Case 17 - Burglarized golf-course clubhouse.
Case 18 - Arrested while on O.R.; stolen shotgun in car; theft charge dismissed (bargain).
Case 19 - Skipped after arrest; failure-to-appear charge dismissed (bargain); drug problem; accomplice sentenced to 7 yr after trial.
Case 20 - Two additional burglary charges dismissed.

office, and five subsequent cases—appeared to be strong. Two cases from each period involved arrests at the scene, with admissions coming from the two such 1973 cases. Case strength derived generally (nine of eleven cases) from the existence of physical evidence, usually stolen property found in the defendant's possession, coupled with his being linked to the scene by eyewitnesses or prior acquaintance with the victim. But in only one case was a positive fingerprint identification made. Taken separately for the two periods, the case audit revealed no significant difference in the strength of the residential burglary cases for the two periods.

All 11 cases were charged as Burglary I offenses. Four of the six 1973 cases were disposed of on reduced charges. The five 1974 cases, consonant with Impact policy, were disposed of by a plea to the original charge of Burglary I.

The sample presented no indication of an increase in the use of multiple charges in the second period. In three of the six 1973 cases, charges in addition to Burglary I were included. In two of the 1974 (Impact) cases, additional charges were filed, but these arose from separate incidents.

Impact cases are filed by the Impact Unit, with the filing deputy retaining responsibility for the prosecution of the case. Filing standards for these cases appear to be both tougher and more explicit than for cases that enter by way of the Intake Unit. The audit as a whole, but not the particular cases audited, revealed no evidence that residential burglaries, if weak, were charged as some offense other than Burglary I to shunt them from the Impact Unit.

Plea Bargaining. We shall discuss in succession three types of plea bargains—namely, charge reduction, dismissal of unrelated charges, and sentence concessions—first for the nondwelling burglary cases in both periods and then for the dwelling burglary cases, both non-Impact and Impact.

Nondwelling Burglaries. In the nine nondwelling burglary cases audited from the 1973 and 1974 periods, only three defendants pled guilty to the original Burglary I charge, which carries a 20-year maximum, and all three had prior prison records. These results suggest that the office of the district attorney's bargaining posture was to employ charge reduction as a means of "giving a break" to some defendants, but not those with heavy prior records. In the six cases in which charges were reduced, the audit sample reveals no pattern that explains the depth of concession. The substantial reduction in one case clearly derived from its weakness; that is, given the defendant's affirmative defense wherein his claim of necessity was strongly corroborated, the misdemeanor charge of Criminal Mischief II was more accurate than Burglary I, but the remaining cases present scant clues as to the determination of the pled charge.

In three of the nine nondwelling burglaries, the plea was accompanied by the dismissal of unrelated charges. Only one nondwelling burglary case among the nine could be said to reflect a sentencing concession.

Dwelling Burglaries—1973. Turning next to dwelling burglary cases from the first (1973) period before the No Plea Negotiation Experiment, we find that four of the six were disposed of by pleas to reduced charges, but no rationale for observed disparity was evidenced by the audit information. But the Impact cases from 1974 reflect an entirely different situation. Consistent with the avowed policy of handling these dwelling burglaries, none of the five defendants charged with Burglary I were offered reduced charges; all pled to the charge as originally filed.

The pattern observed above for nondwelling burglaries concerning concessions in unrelated cases does appear in two dwelling burglary cases drawn from the first

period. The remaining 1973 dwelling burglary cases, all of which contained charge reductions, involved no dismissal of unrelated charges. Finally, none of the 1973 dwelling burglaries presented any indications of sentence concessions.

Dwelling Burglaries—1974. We have already noted that none of the five Impact cases (1974 residential burglaries) revealed charge reductions. However, two of these five pleas of guilty to an original Burglary I charge were accompanied by dismissal of unrelated charges. Two of the cases that did not involve dismissal of unrelated charges were accompanied by a specific agreement by the prosecutor to take no position on sentencing.

Interpretation. In brief, what did the Rand case audit effort show about plea bargaining in Multnomah County for burglary offenses in the time periods addressed? To begin with, we observed that the different types of prosecutorial concessions tended to be mutually exclusive—either a charge reduction, a dismissal of unrelated charges, or a sentence concession might be given, but rarely two of them in the same case. For the non-Impact cases, the most common concession was a charge reduction, but we perceived no consistent policy governing the depth of reduction. While the policies for dealing with the Impact cases have eliminated charge reductions as plea inducements, such cases may often involve the other two types of concessions. And our background interviews indicated that still another type of concession—namely, not to proceed with unrelated charges that had not yet been filed—was not uncommon, even in Impact cases.[4]

The case audit revealed other useful and interesting information. We observed, for example, that there was an effort to dispose of cases at the pretrial conference; this meeting provided the usual context for plea bargaining. The court took no part in the pretrial conference. Furthermore, there was little "judge-shopping" in connection with pleas of guilty. In the face of a strong judicial policy against it, few defense attorneys risk court disapproval by the use of such tactics. There were a number of inducements for defendants to plead guilty. The district attorney's office had a reputation of willingness to go to trial, despite the contrary indication of a low overall trial rate. If a case was tried and the defendant convicted, the prosecutor would not be constrained in making sentence recommendations. And at least some judges were thought to be more severe in sentencing convicted defendants who have insisted on going to trial.

Sentencing Variation. A sample of only 20 cases is, of itself, not a sound basis for inferences that observed sentencing disparities are or are not justified. Nonetheless, we discuss briefly what we observed in this area and comment on possible explanatory circumstances in the Multnomah context.

The 15 non-Impact cases, both dwelling and nondwelling burglaries drawn from both periods, reflect an appearance of leniency. Only four defendants were incarcerated; their common denominator was that all had previously served prison sentences. The one with the heaviest prior record drew a 15-year sentence. This factor seemed more influential than the gravity of the charge to which the defendant pled.

The other 11 defendants in the non-Impact cases were given straight probation, for periods ranging from 18 months to five years, with the length of the probationary period being roughly proportional to the defendant's prior record. Many of the sentences appeared unduly lenient to our auditors.

[4] We found no reference to this practice in the audited files.

As a whole, the sentences meted out in the five Impact cases were much more severe than for the other group, with incarceration being imposed in four of the cases, even in two cases for 18-year-olds with no prior adult record. The one prison sentence imposed in an Impact case, a 10-year period of incarceration, seemed unusually harsh for the facts of the case.

One cannot attribute the greater severity of sentencing in the Impact cases to the fact that these defendants pled guilty to Burglary I. Rather, it appears that the identification of these cases as "Impact" had some effect on the court's sentencing decisions. Other considerations are that a more extensive pre-sentence report was prepared for Impact cases than for routine cases, that nearly half of the non-Impact cases audited had no pre-sentence report prepared, and that the prosecutors were said to have been more actively concerned about sentencing in the Impact cases even though in two of the Impact cases the prosecutor expressly agreed to take no position on sentencing as part of the plea agreement. The limited information that the case audits revealed about how the defendant's pretrial custody status affected his treatment and disposition seemed consistent with the conventional wisdom; in this small sample we found no evidence that the type of defense counsel influenced case outcomes.

Findings from the Dade County Case Audit

Twenty-three cases whose disposition was by plea of guilty were audited in Dade County. Information pertaining to these cases is summarized in Table 8.4. The comments and inferences by the two auditors are presented within the framework of the issues.

In Sec. V we discussed the unusual nature of complaint screening in Dade County. Observation of these procedures led us to expect that the cases bound over to circuit court for felony prosecution would include many relatively weak ones. Yet the 23 audited burglary cases, both dwelling and nondwelling, were strong and did not demonstrate inadequacy of screening. In nine of them (nearly 40 percent of the sample), positive fingerprint identifications linked the defendant to the scene of the burglary. Confessions were given in six cases, only two of which contained the fingerprint identifications. The defendant was caught at the scene in six of the 12 dwelling cases and in eight of the nondwelling cases. Only three cases of the sample had neither apprehension at the scene, nor a confession, nor a positive fingerprint identification. It appears, then, that this sample of audited cases—because it was designed primarily to illuminate plea bargaining—is poorly suited to reveal deficiencies in screening.

Some *inconsistency* of charging is shown by the nondwelling cases of the audited sample. Six of these 11 cases were filed as multiple-count cases. Yet the same evidentiary basis appeared, more or less, in most of the other nondwelling cases charged only as single-count burglaries. The dwelling burglaries, by contrast, presented a consistent pattern of charging. The cases that were filed with multiple counts almost invariably involved a charge of grand larceny, seemingly justified by the evidence. Absent such evidence, the case was filed as a single-count burglary.

In sum, the audited cases do not reflect significant prosecutorial screening effort. Ill-fitted as our particular sample may be to illuminate this issue area, the result is, nonetheless, consistent with the policy of the state attorney's office (and with a

Table 8.4

Dade County Case Audit Information

Case No.	Race	Defendant's Background: Age	Prior Record	Custody Status	Type of Counsel[a]	Days (Arrest to Sentence)	Original Charge	Conviction Charge	Sentence	Strength of Case: Sentencing Judge	Caught at Scene	Admission	FP[b]	Gun	Severity of Offense: Theft	Victim Present	Mode of Entry	Number of Co-Defendants	PSR[c]
1st Period, Dwelling																			
1	B	17	No	O.R	PD	86	B/E Dw[d] Gr Lrc	Same	5 yr-prb	1	No	Yes	No	Yes	Yes, guns	No	Window	1	Yes
2	B	19	Major	Jail	PD	51	B/E Dw (4 cts) Gr Lrc (4 cts)	B/E Dw (1 ct)	12 yr-prison	1	No	No	Yes	No	Yes, TV, $$, stereo	No	Window	0	No
3	B	21	No	O.R.	PD	113	B/E Dw	Tres-pass	24 days jail	2	Yes	No	Yes	No	No	No	Forced	0	No
4	B	19	Prison	O.R.	PD	215	B/E Dw Roving	Same	10 yr-prb	3	Yes	Yes	No	No	No	Yes	?	0	No
5	W	58	Major	Jail	PD	?	B/E Dw Gr Lrc	Same	1 yr-jail	4	No	No	Yes	No	Yes, valu-ables	No	Door	0	No
11	PR	20	Minor	Bail	PD	93	B/E Dw	Same	5 yr-prb	4	Yes	No	Yes	No	No	No	?	2	No
12	B	38	Prison	Jail	PD	43	B/E Dw	Same	2 yr-prison	5	Yes	No	No	No	No	Yes	?	0	No
2nd Period, Dwelling																			
6	B	16	No	Jail	PD	?	B/E Dw Att GL	Same	5 yr-prison	5	Yes	No	No	No	Yes, TV	No	Back door	2	No
7	B	18	Major	Bail	PD	31	B/E Dw Obs Jus	Same	5 yr-prison	6	Yes	No	No	No	No	No	Ladder	1	No
8	B	22	Minor	Jail	PD	91	B/E Dw Gr Lrc	Same	1 yr-jail	7	No	No	No	No	Yes, TV	No	Window	1	No
9	PR	21	Prison	Jail	PD	?	B/E Dw Gr Lrc	Same	10 yr-prison	7	No	No	Yes	Yes	Yes, guns, $	No	Back door	0	No
10	Cuban	21	Major	Bail	DR	95	B/E Dw	Same	3 yr-prison	4	No	No	No	No	No	No	Back door	2	No
1st Period, Nondwelling																			
14	W	17	No	Bail	DR	?	B/E Bld Mal Dest	Same	3 yr-prb	7	No	Yes	Yes	No	Yes	No	Window	1	No
16	W	34	Prison	Bail	PD	129	B/E Bld	Same	5 yr-prison	2	Yes	No	No	No	No	No	Roof	0	No
22	W	21	Prison	O.R.	DR	144	B/E Bld Gr Lrc	Same	3 yr-prison	1	No	No	No	No	Yes, drugs	No	Window	0	No

(continued)

Table 8.4—continued

Case No.	Race	Defendant's Background: Age	Prior Record	Custody Status	Type of Counsel[a]	Days (Arrest to Sentence)	Original Charge	Conviction Charge	Sentence	Sentencing Judge	Strength of Case: Caught at Scene	Admission	FP[b]	Gun	Severity of Offense: Theft	Victim Present	Mode of Entry	Number of Co-Defendants	PSR[c]
2nd Period, Nondwelling																			
13	B	18	No	Jail	PD	186	B/E Bld	Same	2 yr-prb	8	Yes	No	Yes	No	No	No	Window	1	No
15	B	22	No	O.R.	PD	92	B/E Bld Gr Lrc Mal Dest A & B	All but A & B	3 yr-prb	5	Yes	No	No	No	Yes, $, food, cigs	No	Door	1	No (wvd)
17	W	59	Major	Jail	PD	67	B/E Bld	B/E w/ intent misdm.[d]	3 yr-prison	9	Yes	No	No	No	No	No	Window	0	No
18	W	19	No	Jail	PD	79	B/E Bld Gr Lrc Bg Tools	Same	18 mo-prb	10 (master cal.)	Yes	Yes	No	No	Yes, drugs	No	A/C vent	1	No (wvd)
19	Cuban	36	Prison	Bail	PD	132	B/E Bld	B/E w/ intent misdm.[e]	6-12 mo-jail	8	Yes	Yes	Yes	No	Yes, paint	No	Window	1	Yes
20	B	24	No	O.R.	PD	165	B/E Bld Gr Lrc	B/E Bld	6 mo-jail 2 yr-prb	8	Yes	No	No	No	Yes, $$	No	Door	0	Yes
21	B	23	Prison	Bail	PD	?	B/E Bld Gr Lrc Loiter	Same	3 1/2-yr prison	11	No	No	Yes	No	Yes, clothes	No	?	0	No
23	W	19	No	O.R.	PD	71	B/E Bld	Same	5 yr-prb	7	Yes	Yes	No	No	Yes, drugs	No	Window	1	No

[a] PD = public defender; DR = defendant-retained private attorney.

[b] FP = fingerprints.

[c] PSR = pre-sentence report.

[d] See footnote a, Table 8.2

[e] See footnote d, Table 8.2.

CASE NOTES:
Case 1 - Codefendant not apprehended.
Case 2 - Conc. to another B/E Dwelling case; probation revoked.
Case 3 - Found guilty on stipulated facts; skipped; rearrested.
Case 4 - Sentence consec. to another B/E Dwelling and CL case with 2 yr prison and 2 yr probation; skipped.
Case 5 - Same defendant as Case 17.
Case 7 - Conc. to two prior cases; probation revoked; co-defendant acquitted.
Case 8 - Identified by two neighbors. Consec. to another B/E case.
Case 9 - Consec. to another B/E Dwelling case.
Case 10 - Codefendant who testified received 2 yr probation.
Case 11 - Defendant shot fleeing scene; sentence consec. to another.
Case 12 - Defendant served 3 yr in prison on prior B/Es.
Case 13 - Probation later revoked; sentenced to 1 yr jail.
Case 14 - Conc. to another B/E of theater, same night.
Case 15 - Codefendant assaulted food store manager.
Case 16 - Three prison priors in three states for burglary.
Case 17 - Same defendant as Case 5.
Case 18 - Used crowbar; both defendants "cooperated" after arrest.
Case 21 - Conc. with three other burglary cases.
Case 22 - Doctor's office; conc. to attempted B/E Bld case.
Case 23 - Doctor's office; defendant waited in car while codefendant entered; codefendant received one yr jail sentence.

long-standing tradition in Dade County) that the court have the primary responsibility to exercise the screening function.

Plea Bargaining. As a preliminary to the discussion of the plea negotiation aspects of the audited burglary cases, we note the posture of the principal involved agencies toward such bargaining. The state attorney is a vociferous public critic of plea bargaining. He has recently stated that negotiated pleas are not in the best interest of either the community or the individual, that his policy is not to permit a plea agreement without the approval of the victim and the police, and never to invade the court's province by bargaining over sentences.[5] The position of the public defender is said to be that his office will not enter a plea of guilty without advance assurance of what sentence would be meted out.[6] The position of the bench may be inferred from the observation that sentence bargains commonly are struck in the trial judge's chambers. The upshot of this melange of policy appears to be, judging from the case audit, that all types of plea bargaining are infrequent save one—sentence bargaining, which is commonplace.

In only two of the audited cases was the plea to a level of charge less than that originally charged. While the auditing team concluded that the great majority of cases disposed of on a plea of guilty involved a sentence assurance or agreement, this cannot readily be shown by the audit case sample since the case files do not normally contain a record of such agreements, if any. Also, the case files usually lacked a pre-sentence investigation report.[7]

It was evident to the auditing team that sentence bargains struck in Dade County would be strongly shaped by the concern of both sentencing judges and the state attorney's office for the views of the victim and the police in individual cases, and by crowding in correctional facilities as well. Other sources of discrepancies in sentences are discussed below.

Sentencing Variation. Collectively, the sentences in the audited cases turned out to be quite severe, with incarceration often imposed. Of the 11 defendants convicted of dwelling burglaries, six received prison sentences and two were given jail terms of one year. Three young defendants were given straight probation, but in one of these instances, the term of probation was consecutive to a prison sentence in an unrelated case. The average duration of the prison sentences was roughly six years in length.

The defendants convicted of nondwelling burglaries also did not receive patently lenient treatment. Four were given prison terms, averaging nearly four years in length. Jail terms were imposed on two. The remaining five were given straight probation, but they were all young first-offenders.

Eleven different judges imposed sentences in the audited cases. Their sentencing patterns seem quite similar, with the defendant's prior record appearing to be a salient consideration. On the face of the audited cases, the effect of sentence agreements in burglary plea-of-guilty cases appears to be diminished by a pervasive inclination toward severity of treatment by judges in all cases except those most deserving of leniency.

[5] Crim. L. Rptr. 2438 (Aug. 27, 1975).

[6] Private communication.

[7] Florida law provides a right to a pre-sentence investigation only when the defendant is younger than 18 or has no prior record. Apparently this investigation is frequently waived even when these requirements are met.

Case Processing Efficiency; Evenhandedness. The size and nature of the case sample audited precluded useful inferences about the Master Calendar Experiment or other case processing issues.

Further, we were unable to perceive any pattern of events in the audited cases that could be related to the race, type of counsel, or custody status associated with the defendants.

RECAPITULATION

Strongly divided views about the relative roles of case auditing and statistical performance measurement, expressed by the experienced criminal justice practitioners interviewed during an early phase of this study, led us to conduct auditing on a modest scale in the two demonstration jurisdictions, Multnomah County and Dade County. We sought to investigate several of the claimed benefits of case auditing used in conjunction with the statistical analysis of court system performance and, in particular, with the calculation of performance measures. Could case auditing enhance confidence in inferences drawn from the statistical analysis of the court system records? Would it be a source of findings not revealed by the statistical treatment of relatively large volumes of operational data from the system? These were general questions at issue.

Since our resources for case auditing were sharply constrained, it was necessary to focus the scope rather narrowly. The sample of cases numbered 20 in Multnomah County and 23 in Dade County, all involving burglary as the principal charge and all disposed of by a plea of guilty. Two principal factors embodied in the design of the case sample were *time* (i.e., two different periods of operation) and *type of burglary* (i.e., occurring in a dwelling or a nonresidential building). Within each jurisdiction, the case samples were roughly balanced in terms of the most accessible defendant characteristics, age, and prior record. Our particular interest in shaping the Multnomah case audit sample was to further illuminate a limited No Plea Negotiation Experiment undertaken in that jurisdiction; the design of the parallel Dade case audit sample was then largely dictated by our wish to make interjurisdictional comparisons, if appropriate. The Rand case audit team was composed of two seasoned practitioners.

The Multnomah case audit developed a picture that was consonant with our information, both statistical and qualitative, from other sources and with findings based thereon.

Much the same could be said for the Dade case audit. But the generally poorer state of the case files, coupled with the diffuse nature of case screening and the practice of concealing plea agreements,[8] if any, in that jurisdiction made case auditing generally less fruitful than in Multnomah. Differences in screening, plea bargaining, and sentencing between the two time periods and between types of burglaries perceived by means of the case audit activity corresponded to differences revealed by the statistical approach. The cases that we audited produced no significant new insights, for example, about how practice relates to policy in this jurisdiction. But

[8] That is, the case files in Dade County show only instances of agreements to reduce the number or level of charges; sentence concessions or agreements to drop charges in unrelated cases simply are not recorded.

this result might be attributed to the character of the selected sample rather than to the auditing process. Perhaps if the sample had been deliberately designed to encompass cases of marginal strength,[9] the audit might have exposed forces at work which were unlikely to be revealed by statistical analysis. What our experience does underscore is the relevance of an elementary guideline, namely: One needs accurate, full, and fairly detailed knowledge about the nature of a court system if he is to make sound and comprehensive inferences about its performance on the basis of statistical analysis of operational data. If he has that knowledge, case auditing is unlikely to produce surprises. If he lacks it, case auditing could be instructive.

CONCLUDING REMARKS

Case auditing is an interesting method of generating system knowledge for analysts who can then go about constructing statistical performance measures. This suggests that audits be scheduled early in any exercise to develop the performance measures auditing approach. Case auditing has more direct benefits as well, as we believe our experience has taught us:

1. It can provide information about important but nonquantifiable variables which, although impossible to integrate into a statistical estimation equation, must inform the interpretation of statistical estimates.
2. It can be the source of *new* hypotheses about how the system works, which then can be tested on the basis of data collected in a statistical performance measures enterprise.
3. It can give assurance that the causal sequence uncovered in a statistical exercise accords with the perceptions concerning cause-and-effect of people actually on the scene.
4. It has more validity, on its face, to the average practitioner; that is, it seems more natural and less mysteriously technical. Thus, when there is concurrence between audit and performance measure results, the conclusions of the statistical enterprise will be more acceptable to the interested professional reader as well as to the lay person.

This last point is *not* to argue that case audits should be used primarily to confirm or validate results derived from well-executed statistical analyses. The audit samples are inevitably so small that any discrepancy in findings must be considered inconclusive. The best source of technical confirmation for the statistical results is a *better* sample than the original, or a *better* formulation of the hypothesis, or *better* measurement of variables, none of which a case audit can guarantee.

Taken together, the two audits intensify our appreciation of the dramatic differences between the two jurisdictions in felony proceedings. In fact, interjurisdictional comparisons in the absence of case audits seem to us rather risky. And the findings emphasize that comparisons between these two jurisdictions are best made in terms of output measures (e.g., incarceration rate, diversion rate, cost per disposition, etc.) that show impacts on the community, rather than in terms of internal performance measures (e.g., rate of dismissal at preliminary hearings).

[9] The cases were selected randomly and, as it turned out, all were judged by the audit team to be strong or fairly strong cases.

Although we generally believe that case audits would best be conducted in the initial phases of a project to measure court performances, there is one factor that operates in the other direction. Sudden departures from trends or normal patterns will often show up in the statistical indicators. When that happens, on-the-scene investigation by experienced practitioners can reveal the reason, and this ferreting activity can be successfully combined with a case audit.

Case audits are by their nature much more expensive per case included than the data collection required to develop performance measures. That is obvious; they require all of the information that one has to collect for performance measures plus more. So as the sample size for the case audit expands, it begins to approach that typically utilized for generating statistical performance measures, but of course at much greater cost.[10] Obviously, case audit samples must be much smaller. We trade off comprehensiveness of information against depth of information, or extensity for intensity. The question becomes: Is that more intensive information worth the extra cost? Our experience indicates that the costs are worth paying if a modest, well-designed audit undertaking is performed early in the course of events, by experienced and thoughtful practitioners who consciously seek ways to improve the subsequent development of statistical performance measures.

[10] We should also observe the existence of trends toward PROMIS-type information systems, which not only facilitate the use of statistical performance measures but also provide a depth of detail that resembles case auditing results in many respects. For example, such systems typically collect and process a number of information elements on case strength. Consequently, they can show how case strength tends to explain observed events in the criminal proceeding, not only as a statistical effect in a large number of cases but also as separate observations pertaining to individual cases (as does a case audit). PROMIS-type systems may thus be regarded as a bridge between the two (polar) performance measure approaches discussed above.