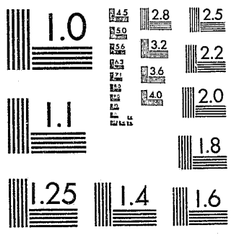

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NATIONAL EVALUATION PROGRAM

Phase II Report

Series B
Number 2

Pretrial Release: A National Evaluation of Practices and Outcomes

a publication of the **National Institute of Justice**

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NATIONAL EVALUATION PROGRAM
Phase II Report

Pretrial Release:
A National Evaluation of Practices and Outcomes

by
Mary A. Toborg

October 1981

U.S. Department of Justice
National Institute of Justice

National Institute of Justice

JAMES L. UNDERWOOD

Acting Director

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NATIONAL EVALUATION OF PRETRIAL RELEASE

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[C]riminal procedure of the Americans has only two means of action—committal and bail. . . . It is evident that a legislation of this kind is hostile to the poor man, and favorable only to the rich. The poor man has not always a security to produce, . . . and if he is obliged to wait for justice in prison, he is speedily reduced to distress.

—Alexis De Tocqueville, 1832

The purpose of the bail law. . . is to insure the presence of accused persons for trial by devices which will guarantee a maximum of certainty to society and at the same time impose a minimum of restraint upon the accused individual. . . . [T]he present system, in too many instances, neither guarantees security to society nor safeguards the rights of the accused. The system is lax with those with whom it should be stringent and stringent with those with whom it could safely be less severe.

—Arthur L. Beeley, 1927

It is clear that there is a startling amount of crime committed by persons on release awaiting trial. . . . It is not uncommon for an accused finally to be brought to trial with two, three or more charges pending. . . . Bail release [should include] the crucial element of future dangerousness based on a combination of the particular crime and past record, to deter crime-while-on-bail.

—Chief Justice Warren E. Burger, 1981

ABSTRACT

The National Evaluation of Pretrial Release focused on four broad topics:

- the release process and release outcomes;
- court appearance performance;
- pretrial criminality, as reflected in pretrial arrests and convictions for those arrests; and
- the impact of pretrial release programs.

To consider these topics, the study analyzed data on approximately 6,000 defendants from 12 jurisdictions around the country. The "delivery system" for pretrial release decisions was also assessed in each site.

The study found that 85 percent of arrested defendants secured release prior to trial; 87 percent of released defendants appeared for all required court dates; and 84 percent of released defendants were arrest-free during the pretrial period. The pretrial release programs studied had a major impact on release outcomes but little effect on court appearance or pretrial arrest rates.

Among the study's recommendations for improving pretrial release practices are:

- to identify and apprehend fugitives more effectively;
- to reduce trial delay;
- to release more defendants pending trial, particularly through citation release soon after arrest for persons charged with less serious offenses;
- to develop alternative detention facilities to reduce jail overcrowding;
- to derive less restrictive program release recommendation criteria; and
- to evaluate post-release followup activities at the individual program level.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

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We also appreciate the generous assistance of many other persons, too numerous to list individually, who helped us implement the evaluation successfully and interpret its results accurately. Any errors of fact or judgment that may appear in the study are, of course, solely our responsibility.

CHAPTER I. INTRODUCTION

For decades controversy has surrounded the nation's pretrial release practices. The appropriate handling of defendants after arrest and before court determination of guilt or innocence embodies difficult issues for the American system of criminal justice.

During the pretrial period the rights of the accused person must be balanced against the interests of the community. In the absence of any finding of guilt, defendants should retain as much freedom as possible, so that innocent persons are not harmed. At the same time the community must be assured that guilty defendants cannot evade justice. When a large group of citizens perceives an imbalance between defendants' rights and the community's concerns, controversy and public debate ensue—and sometimes changed release practices as well.

As an example, in the 1960's advocates of "bail reform" attacked the premise that defendants must post money bail to ensure the community of appearance in court. Subsequently, own recognizance release, based upon a simple promise to return for trial, became more common. Similarly, in the 1970's prominent public officials expressed concern about the efect of pretrial crime on community safety. As a result, "preventive detention" has been widely considered as a way to reduce crime on bail.

Because of the widespread interest in pretrial release practices, the National Institute of Justice sponsored a National Evaluation Program "Phase I" study of this topic.[1/] Completed by the National Center for State Courts in 1977, that review of the state of knowledge regarding pretrial release found a serious lack of basic information about release practices and outcomes.

Since the early 1960's, when the Manhattan Bail Project had demonstrated that the use of own recognizance release could be expanded, many pretrial release programs had been established throughout the country. Typically, staffs of these programs interviewed arrested defendants, identified likely candidates for different types of release, provided information to the court on defendants' backgrounds, and in some cases made release recommendations and reminded defendants of coming court appearances. However, when the National Center for State Courts surveyed 115 programs, 25 percent of them had no information on the number of defendants they had interviewed, and an even higher percentage did not know the number of defendants recommended for release without bond or the number granted such release. Moreover, only a few could provide <u>any</u> data on the rearrests of released defendants.[2/] Without reliable data many important issues about pretrial release could not be analyzed adequately.

Thus, as a result of the Phase I study, major gaps in existing knowledge about pretrial release practices and outcomes were identified. To fill these gaps and provide improved information for the public debate about pretrial release practices, the National Institute of Justice funded a "Phase II" National Evaluation of Pretrial Release, summarized in this volume. The study was concerned primarily with four broad topics:

- Release—What percentage of defendants are released pending trial? What are the most common types of release? Which defendant or case characteristics have the greatest impact on the release decision?

- Court Appearance—To what extent do released defendants appear for court? How well can failure to appear be predicted?

- Pretrial Criminality—During the pretrial period, how many defendants are rearrested; and of those, how many are convicted? What are the charges? How well can pretrial rearrest be predicted?

- Impact of Pretrial Release Programs—To what extent do pretrial release programs affect release decisions? How do the programs affect defendant behavior during the release period; for example, does notification of court dates increase appearance rates, or does supervision reduce pretrial criminality?

Decisions regarding the scope of the study included:

- to limit the analysis to adults and not to consider the special problems posed by the release of juveniles;

- to focus the evaluation on defendants processed through State and local, rather than Federal, courts;

- to analyze trial courts only and exclude release mechanisms associated with appeals of verdicts; and

- to study only pretrial release programs, rather than to include related programs providing pretrial intervention or diversion.

The detailed results of the National Evaluation of Pretrial Release appear in a three-volume final report, along with a separately bound Introduction to the study and fourteen working papers prepared during the course of the four-year project. This document summarizes the major findings, conclusions and recommendations of the evaluation. The summary has four parts: one part corresponds to each volume of the final report, and a final section presents a policy analysis of pretrial release.

PART I
RELEASE PRACTICES AND OUTCOMES: AN ANALYSIS OF EIGHT SITES

CHAPTER II. BACKGROUND

Eight jurisdictions were selected for detailed analysis of release practices and outcomes (e.g., rates of release, court appearance and pretrial criminality). These sites were Baltimore City, Maryland; Baltimore County, Maryland; Washington, D.C.; Dade County (Miami), Florida; Jefferson County (Louisville), Kentucky; Pima County (Tucson), Arizona; Santa Cruz County, California; and Santa Clara County (San Jose), California.

Sites were chosen to reflect geographic dispersion, a wide range of release types and broad eligibility for program participation (especially in terms of criminal charges). Additionally, jurisdictions were required to have enough program clients and other releasees to warrant analysis, and records had to be reasonably complete and accurate. Another key site selection criterion was the willingness of local criminal justice officials to cooperate with the study, both by making records available to the research team and by making themselves accessible for interviews.

The "delivery system" for pretrial release decisions was studied in each of the eight jurisdictions. This analysis identified the major steps in the pretrial release process and the most important organizations and individuals involved in that process. The role and specific procedures of the pretrial release program received particular attention during this part of the study, which required extensive on-site collection of information. Interviews were conducted with program staff, judges, prosecuting and defense attorneys, law enforcement officers, bondsmen, and other persons involved with pretrial release matters. Additionally, various publications dealing with release practices in each jurisdiction were reviewed.

Table 1 summarizes selected characteristics of the eight sites, including major features of the local pretrial release programs. As shown, the sites represent a wide range of pretrial release practices.

The cost estimates provided in Table 1 deserve special comment. These estimates are extremely imprecise, because of the many difficulties of determining costs, allocating them to such program activities as conducting interviews or providing supervision, and developing comparable data across sites. Nevertheless, the pattern of costs is instructive: programs with relatively small numbers of interviews (i.e., less than 5,000 interviews per year) had the highest costs per interview. Larger programs achieved economies of scale that permitted lower unit costs of operation.

In addition to the delivery system analysis of each site, a sample of defendants was studied from point of arrest to final case disposition and sentencing. Existing records were used to collect extensive data on the backgrounds of defendants, release decisions, program involvement, case outcomes, court appearances and pretrial arrests. These data were used to analyze the release process as well as the court appearance and pretrial

TABLE 1
SELECTED CHARACTERISTICS OF THE EIGHT SITES IN THE CROSS-SECTIONAL ANALYSIS

CHARACTERISTIC	BALTIMORE CITY	BALTIMORE COUNTY	WASHINGTON, D.C.	DADE COUNTY	JEFFERSON COUNTY	PIMA COUNTY	SANTA CRUZ COUNTY	SANTA CLARA COUNTY
A. Jurisdiction								
Estimated Population	850,000	700,000	675,000	1,500,000	725,000	450,000	165,000	1,200,000
Number of Arrests (1977) for Index Crimes[a]	21,000	7,000	7,400	16,000	7,600	4,350	1,050	4,550
Jail Overcrowding	Currently	Currently	In Past	In Past	In Past	Currently	In Past	Currently
Release Official(s)	Judges, Bail Commissioners	Judges, Bail Commissioners	Judges, Police	Judges	Judges	Judges, Court Commissioners, Police	Judges, Police, Sheriff	Judges, Court Commissioners, Police, Program Officials
Bail Schedule	None	None	Yes	Yes	Misdemeanors Only	Misdemeanors Only	Yes	Yes
Number of Bondsmen	15	5	3	100	0	3	3	13
B. Pretrial Release Program								
Major Eligibility Restrictions	None	Defendants Not Released at Initial Appearance Only	None	Felony Charges Only	None	Felony Charges Only	None	None
Percentage of Eligible Defendants Interviewed	85%	100%	97%	68%[b]	65%[c]	98%	36%	79%
Number of Interviews per Year	37,500	1,800	28,500	9,000	19,300	4,200	2,000	14,300
Basis of Release Recommendations	Point System	Subjective Assessment	Objective[d]	Subjective Assessment	Point System	Subjective Assessment	Subjective Assessment	Point System
Percentage of Interviewed Defendants Who Are Both Recommended For and Released on Own Recognizance	49%	12%	58%[e]	12%	53%	30%	33%	57%
Annual Expenditures (1977)[f]	$489,330	$73,300	$766,200	$104,135	$377,720	$171,500	$59,420	$426,040
Estimated Cost Per Interview[g]	$25	$34	$22	$11	$20	$42	$36	$16

[a] Index Crimes are murder, rape, robbery, aggravated assault, burglary, larceny and auto theft. Data for Pima County and Santa Cruz County are for 1976.

[b] Based on defendants seen at bond hearings; excludes persons who made bond before those hearings. More precise data were not available.

[c] Based on all defendants booked at the jail, including some persons who may have been ineligible for program consideration (e.g., juveniles or prisoners in transit). More precise data were not available.

[d] Point system for citation release recommendations; conditions recommended, if needed, for other cases.

[e] Estimate; includes all nonfinancial releases for all courts.

[f] From budgeted sums only; excludes costs budgeted by other organizations. Data for Dade, Jefferson and Santa Cruz Counties are for 1978. Amount shown for Washington, D.C., excludes Federal grant for development of computer system.

[g] Annual expenditures from budgeted sums—adjusted to include estimated costs of staff funded through other sources (e.g., CETA) and to exclude costs of supervised release—divided by the number of interviews per year..

criminality outcomes of defendants released through different mechanisms, such as own recognizance or money bail.

For each site Table 2 shows the sample size, estimated number of arrests and time period studied. Usually, the sample was randomly selected over a one-year period from all arrests except those for minor traffic offenses.[3]

Table 2 also indicates the weighted sample sizes for all sites. For certain analyses the eight individual samples were combined into one aggregate sample. This was accomplished by weighting each sample to reflect the percentage that its site's arrests represented of all arrests in the eight sites. Whenever weighted data were used, results were rounded to the nearest whole number, with each figure rounded separately. Thus, the numbers of defendants shown in subsequent tables will not always sum to the totals indicated, due to rounding.

The next three chapters discuss the release outcomes, court appearance performance and pretrial criminality, respectively, of the defendant sample. Important features of the pretrial release delivery systems in the various sites are also considered, where appropriate.

TABLE 2
SAMPLE SIZES, ESTIMATED NUMBER OF ARRESTS AND TIME PERIODS STUDIED, BY SITE

SITE	SAMPLE SIZE (unweighted)	ESTIMATED NUMBER OF ARRESTS	TIME PERIOD	WEIGHTED SAMPLE SIZE
Baltimore City	556	37,391	7/76-6/77	811
Baltimore County	419	18,528	1/77-12/77	402
Washington, D.C.	442	30,000	1/77-12/77	651
Dade County	427[a]	9,860[a]	1/78-6/78	214
Jefferson County	435	19,200	1/77-12/77	416
Pima County	409	16,534	1/77-12/77	359
Santa Cruz County	430	8,605	7/76-6/77	187
Santa Clara County	370[b]	19,389[b]	12/77-5/78	448
TOTAL	3,488	159,507		3,488

[a]Felonies only

[b]Excludes defendants released on field citations by the arresting police officer.

CHAPTER III. THE RELEASE PROCESS AND OUTCOMES

Of the 3,488 defendants in the eight-site sample, 85 percent of them secured release at some point before trial. Release rates ranged from 73 percent to 92 percent in individual sites, as shown in Table 3.

TABLE 3
RELEASE RATES BY SITE

Note: All percentages are based on the total defendant sample for each site.

Site	Detained Until Trial	Released Before Trial		
		Total Released	Released on Nonfinancial Conditions	Released on Financial Conditions
Baltimore City, MD (n=556)	13.3%	86.7%	69.3%	17.4%
Baltimore County, MD (n=419)	7.9%	92.1%	70.6%	21.5%
Washington, DC (n=442)	12.2%	87.8%	74.2%	13.6%
Dade County, FL (n=427)	15.9%	84.1%	38.3%	45.8%
Louisville, KY (n=435)	19.9%	80.1%	35.2%	44.9%
Pima County, AZ (n=409)	27.4%	72.6%	53.3%	19.3%
Santa Cruz County, CA (n=430)	10.0%	90.0%	76.0%	14.0%
Santa Clara County, CA (n=370)	14.6%	85.4%	52.8%	32.6%
Total, 8 Sites (n=3,488)	14.7%	85.3%	61.4%	23.9%

Viewed in historical perspective, these findings suggest a continuation of a trend toward higher release rates of defendants prior to trial. An analysis by Wayne Thomas of release rates in 20 cities in 1962 and 1971 found major increases over the time period: release rates for felony defendants increased from 48 percent to 67 percent and for misdemeanor defendants, from 60 percent to 72 percent. Nevertheless, at the end of the period, in 1971, only about half of the cities released as many as 70 percent of the defendants before trial.4/ In contrast, each of the eight sites listed in Table 3 had a release rate of more than 70 percent between 1976 and 1978; indeed, in all sites except one the release rates exceeded 80 percent.

Despite the increase in release rates, the detention of defendants remains a serious problem in many jurisdictions and often has contributed to jail overcrowding. Many of the defendants detained until trial were jailed for relatively long time periods: one-third of them for more than 30 days and 20 percent for more than 90 days. Additionally, defendants who secured release before trial sometimes did so only after a substantial jail term: about 3 percent of the released defendants had been jailed for 30 days or more prior to release.

The major reason for the detention of defendants was inability to post bond. Only a very small percentage of defendants were detained outright, with no possibility of release provided to them.

In general as the bond amount increased, so did the percentage of defendants detained until trial. Forty percent of the defendants with bonds of $5,000-$9,999 and 65 percent of the persons with bonds of $10,000 or more were jailed the entire pretrial period, as compared with detention rates of 25 percent for defendants with bonds of $1,001-$4,999 and 29 percent for persons with bonds of $1,000 or less.

Although bond played an important role in the detention of defendants, its impact on release was considerably less: most defendants were released without any conditions involving money. As shown earlier, in Table 3, 61.4 percent of all defendants in the sample were released on "nonfinancial" conditions (i.e., on conditions that did not involve money); for individual sites the percentage of defendants released on nonfinancial conditions ranged from 35.2 percent to 76.0 percent.

Again, these data reflect the apparent continuation of a trend documented earlier for the 1962-71 period, toward higher rates of nonfinancial release. Wayne Thomas' study of 20 cities found that rates of nonfinancial release for felony defendants increased from 5 percent in 1962 to 23 percent in 1971; for misdemeanor defendants, the increase was from 10 percent to 30 percent.5/

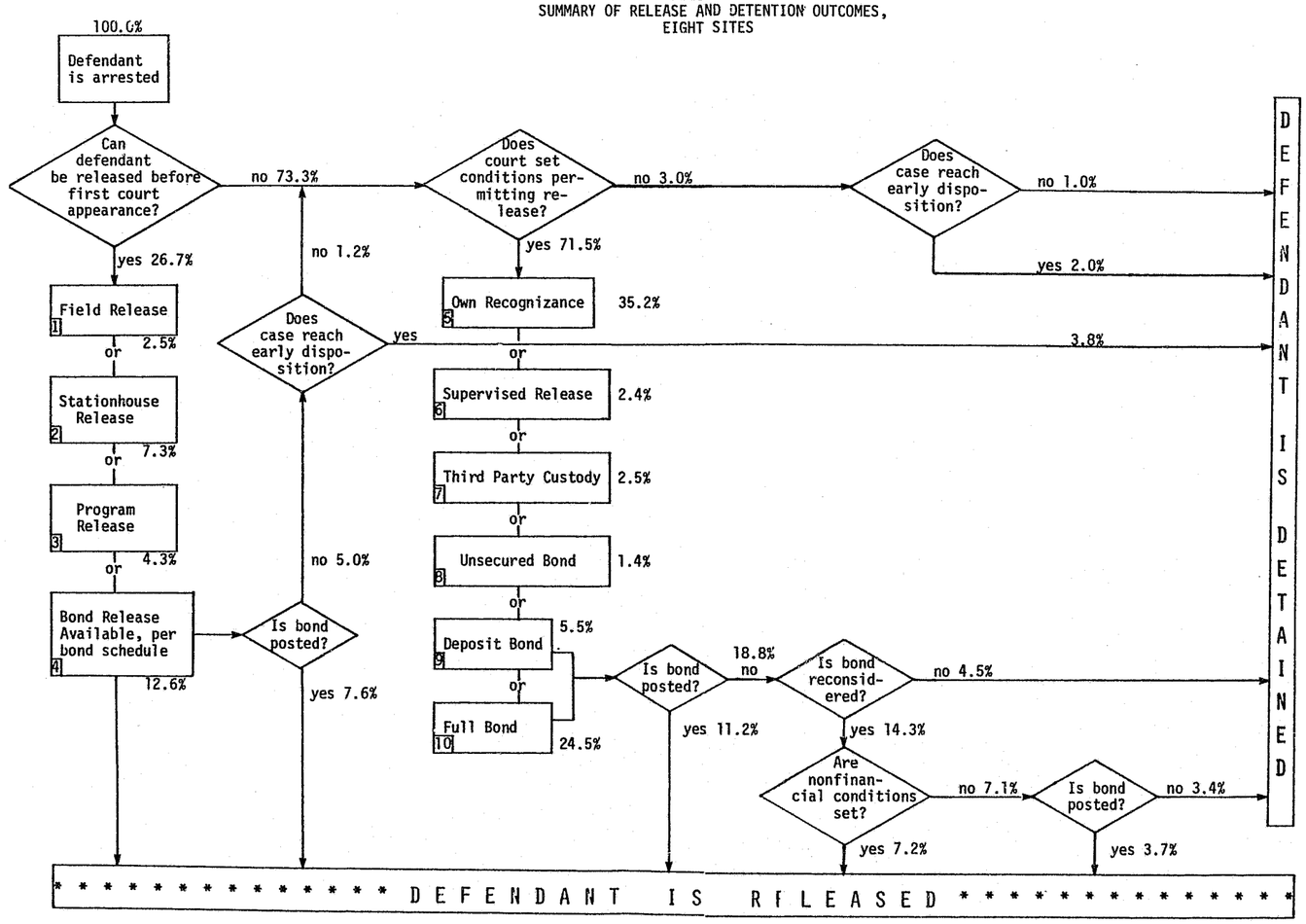
Today, a wide variety of release mechanisms are used around the country. Figure 1 shows the types of release found in the eight sites studied in detail, along with the point at which those releases occurred. After arrest there were several ways for a defendant to secure release without appearing before a judge, bail commissioner or other magistrate of the court. First, the arresting officer could make a field release of the defendant. This procedure, a form of "citation release" used for minor charges, is similar to issuing a traffic ticket and does not require taking the defendant into custody. If the person is taken to a police station or jail for booking, stationhouse release (another type of citation release) may be approved at that time, again by law enforcement officials. In Santa Clara County a similar release process operated under the authority of the local pretrial release program.

Additionally, some jurisdictions had bail schedules, listing bail amounts for various charges. Defendants in those sites could secure release at any time by posting the bond amounts shown.

Altogether, more than one-fifth of the sample obtained release prior to an appearance before a court official. Although most of these defendants were released on nonfinancial conditions, about one-third of them posted bond, based on a bail schedule.

The remaining defendants in the sample usually appeared before a judge, bail commissioner or other magistrate within a few hours. In most of the sites studied, the magistrate received information from the local pretrial release program about the defendants' community ties, criminal history and other pertinent factors.

FIGURE 1
SUMMARY OF RELEASE AND DETENTION OUTCOMES,
EIGHT SITES



A variety of release conditions were set by the magistrates in the sites studied. Own recognizance (O.R.) release was initially authorized for 35 percent of the sample. Such release usually required only a defendant's promise to appear for court. Although some jurisdictions attached other conditions to O.R. release, such as a requirement to call the pretrial release program periodically or to reside within the area until trial, defendants were rarely prosecuted for violating those conditions.

Supervised release sometimes entailed the defendant's reporting to a social service agency for treatment (for drug, alcohol or mental health problems) or employment assistance. Often, however, supervision consisted only of more frequent reporting to the pretrial release program than was required for defendants released on their own recognizance.

Under third party custody release, a third party was formally charged with responsibility for the defendant and could, if necessary, return the person to court for reconsideration of release conditions. The third party was usually a relative, social service agency or pretrial release program.

Instead of the nonfinancial release conditions discussed above, magistrates could require the posting or promise of money bond. The least restrictive financial release condition was unsecured bond: in this case the bond amount had to be paid to the court only if the defendant failed to appear. Both deposit bond and full bond required the defendant to raise money before release could be obtained. Under deposit bond a percentage (usually 10 percent) of the bail amount was posted with the court, and most of that "deposit" was returned if the defendant appeared for all court dates. Failure to appear, however, made the person who posted the deposit liable for the full face value of the bond. Deposit bond was widely used in Louisville, Kentucky, and helps explain that site's relatively low use of nonfinancial release conditions, as shown earlier in Table 3.

Full bond was usually arranged through a surety, or bondsman, who required payment of a nonrefundable fee for this service. Typically, bondsmen's fees were about ten percent of the face value of the bond. Surety bond was used in all sites studied except Louisville; because commercial bonding for profit was declared illegal by statute in the State of Kentucky in 1976, Louisville has no bondsmen.

Most jurisdictions have a formal process for reconsidering the bond amounts of defendants detained because they cannot make bail. At this reconsideration, or "bail review," any type of release may be ordered: nonfinancial release may be set; or the bond may be lowered, remain unchanged or even be raised. For the sample studied, approximately half of all defendants for whom bail was set by a magistrate had their bonds reconsidered. As a result of this reconsideration, about one-half of the defendants were released on nonfinancial conditions.

Any defendant who had a bond set but was not released at bail review could, of course, secure release prior to trial by raising the bond amount or, more commonly, the bondsman's fee. About one-fourth of the defendants whose bonds were reconsidered secured release after posting the revised bond amount, which was usually a lower sum than had been set initially.

Thus, as illustrated in Figure 1, the release process involved a variety of criminal justice officials and provided a number of release options. The process encompassed several stages at which a defendant could secure release, including arrest, booking, initial appearance in court and bail review. This process can be viewed as a sorting mechanism, which at each stage permitted additional defendants to secure release. The net result of the process was to separate defendants into two groups: released and detained. Additionally, released defendants could be divided into those who secured release on nonfinancial conditions and those for whom money was involved.

The release outcomes of defendants varied along many characteristics. Table 4 summarizes release outcomes by charge categories (Appendix A provides information on specific charges). The seriousness of charges was measured by the classifications used in the Uniform Crime Reports (UCR) of the Federal Bureau of Investigation (FBI). The most serious charges are "Part I" offenses, consisting of criminal homicide, forcible rape, robbery, aggravated assault, burglary and theft. As shown in Table 4, detained defendants were more likely than released persons to have been charged with Part I offenses: 43 percent of detained defendants were charged with Part I crimes, as compared with 35 percent of the persons released on financial conditions and 27 percent of the individuals released nonfinancially.

Although the FBI's crime categorization reflects overall charge severity, it provides little insight about specific crime groupings of interest. For example, Part I offenses include crimes against both persons and property, as do Part II offenses. To consider types of charges, the following offense categorization was used:

- crimes against persons(murder, nonnegligent manslaughter, forcible rape, robbery, aggravated assault, other assault, arson);
- economic crimes (burglary, larceny, theft, forgery, fraud, embezzlement, stolen property);
- drug crimes (distribution or possession of narcotics or marijuana);
- crimes against public morality (prostitution, sex offenses other than forcible rape or prostitution, gambling, liquor law violations, drunkenness);
- crimes against public order (weapons, driving while intoxicated, disorderly conduct, vagrancy, minor local offenses); and
- miscellaneous crimes (malicious destruction, offenses against family and children, failure to appear, violations of parole, conspiracy, possession of implements of crime, and other crimes).

As shown in Table 4, more than one-third of all detained defendants were charged with crimes against public morality or crimes against public order. When compared with released defendants, detained defendants were more likely to have been charged with crimes against persons, economic crimes, crimes against public morality and miscellaneous crimes; they were

TABLE 4
RELEASE OUTCOMES BY CHARGE

Note: Columns may not add to the totals shown, due to rounding.

Charge	Detained		Released on Financial Conditions		Released on Nonfinancial Conditions		Total Defendants	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
Part I	218	42.8%	288	34.9%	574	27.0%	1,080	31.2%
Part II	292	57.2%	537	65.1%	1,555	73.0%	2,384	68.8%
TOTAL	510	100.0%	825	100.0%	2,129	100.0%	3,464	100.0%
Crimes against persons	110	21.6%	194	23.5%	318	14.9%	622	18.0%
Economic crimes	149	29.2%	206	25.0%	552	25.9%	907	26.2%
Drug crimes	20	3.9%	99	12.0%	246	11.5%	365	10.5%
Crimes against public morality	89	17.5%	83	10.1%	188	8.8%	360	10.4%
Crimes against public order	96	18.8%	194	23.5%	723	33.9%	1,013	29.2%
Miscellaneous Crimes	46	9.0%	48	5.8%	103	4.8%	197	5.7%
TOTAL	510	100.0%	825	100.0%	2,129	100.0%	3,464	100.0%

less likely to have been charged with drug crimes and crimes against public order. When compared with persons released nonfinancially, defendants released on financial conditions were more likely to have been accused of crimes against persons and less likely to have been charged with crimes against public order; the incidence of the other charge categories was very similar for both groups.

In addition to charge differences, release outcomes varied by the defendant's prior record. Detained defendants had an average of 9.5 prior arrests, while persons released on financial conditions averaged 5.2 prior arrests and defendants released on nonfinancial conditions had 2.9 prior arrests. Comparisons based on the average number of prior convictions were similar: 4.0 for detained defendants, 2.0 for persons released on financial conditions and 1.3 for individuals released on nonfinancial conditions.

Table 5 summarizes release outcomes for three indicators of community ties: living arrangement, employment status and length of local residence. As shown, detained defendants were less likely than released defendants to have been living with spouses when arrested and were more likely to have been living alone or with unrelated persons. Detained defendants were also much more likely than released defendants to have been unemployed when arrested: 59 percent of detained defendants were unemployed, as compared with 38 percent of released defendants.

Although detained defendants differed from released persons in terms of living arrangement and employment status, the comparison of defendants released on financial versus nonfinancial conditions found the two groups remarkably similar for those indicators. Also, there were no important differences in the length of local residence for defendants with different release outcomes.

Release outcomes varied along many dimensions besides charge, prior record and community ties. To identify the most important factors associated with release outcomes, multivariate analyses were conducted. Those analyses were based on comparisons of groups of defendants. Two of the comparisons considered the net effect of the release process, through which arrested defendants were either detained or released before trial (see Figure 1) and, if released, secured release on either nonfinancial or financial conditions.

A third comparison considered the release conditions set by court officials. As Figure 1 indicated, approximately 20 percent of the defendant sample was released before the first court appearance; therefore, those defendants were excluded from the analysis of court decisions. Because court officials did not know whether defendants for whom bond was set would be able to post the bond and thus secure release, an analysis of defendants having nonfinancial, as compared with financial, release conditions set by the court differs from an analysis of defendants who secured release on nonfinancial, as compared with financial, conditions. The former analysis provides the greatest insight about the release decision-making processes of judges, bail commissioners and other magistrates, while the latter analysis permits an assessment of the results of those processes.

TABLE 5
RELEASE OUTCOMES BY SELECTED INDICATORS
OF COMMUNITY TIES

Note: Columns and rows may not add to totals shown, due to rounding.

Indicator	Detained		Released on Financial Conditions		Released on Nonfinancial Conditions		Total Defendants	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
Living Arrangement								
Lives with spouse	41	12.0%	118	23.0%	358	23.1%	517	21.5%
Lives with parent	118	34.5%	163	31.8%	528	34.0%	809	33.7%
Lives with other relative	56	16.4%	76	14.8%	239	15.4%	371	15.4%
Lives with unrelated person	67	19.6%	91	17.8%	249	16.1%	406	16.9%
Lives alone	60	17.6%	64	12.5%	177	11.4%	301	12.5%
TOTAL	341	100.0%	512	100.0%	1,551	100.0%	2,404	100.0%
Employment Status								
Employed or substitutes	181	41.1%	450	60.5%	1,229	62.0%	1,860	58.7%
Unemployed	259	58.9%	294	39.5%	754	38.0%	1,307	41.3%
TOTAL	440	100.0%	744	100.0%	1,983	100.0%	3,167	100.0%
Length of Local Residence								
Mean number of years	19.2 years		20.1 years		20.2 years		20.1 years	
Number of defendants	318		506		1,537		2,362	

The three specific comparisons related to the release process were:

- defendants detained until trial, as compared to defendants released before trial;
- defendants released on financial conditions, as compared to defendants released on nonfinancial conditions; and
- defendants for whom magistrates set financial release conditions, as compared to defendants for whom magistrates set nonfinancial release conditions.

The multivariate ("logit") analyses identified the most important variables that affected these release outcomes or decisions and also assessed the accuracy of prediction that could be accomplished with those variables. The three analyses differed considerably in their ability to predict the release outcomes or decisions accurately. The analyses of both financial/nonfinancial release outcomes and the setting of financial/nonfinancial release conditions were more successful than the release/detention prediction (or, more precisely, "retrodiction," that is, retrospective attempts at prediction with archival data).

The results of all three analyses were strikingly similar in terms of the variables that were found to have the greatest effect on release outcomes and release decisions. Program recommendations had an especially strong impact. In particular, a program recommendation of bail release was importantly associated with the detention of defendants, with their release on financial conditions when released, and with their having had financial release conditions set by court magistrates. Program recommendations for deposit bail, conditional release and denial of own recognizance release were also associated with detention or financial release, as was the lack of a release recommendation.

Other variables importantly related to release outcomes and release decisions included charges of crimes against persons, a larger number of arrest charges, involvement with the criminal justice system at the time of arrest (i.e., on probation, parole, or pretrial release for another charge) and a record of prior failure to appear. Defendants with these characteristics were more likely to have had financial release conditions set by magistrates and to have secured release, if at all, through financial mechanisms.

CHAPTER IV. COURT APPEARANCE PERFORMANCE OF RELEASED DEFENDANTS

For most defendants in most jurisdictions the legal basis of release decisions is whether the person will appear for court. Consequently, restrictions on release or the imposition of conditions that must be met to secure release can occur only if these are needed to prevent the defendant's flight.

Historically, the posting of money bail was considered necessary to insure that defendants would appear in court. The increased use of alternatives to traditional money bail, such as own recognizance release and deposit bond, raised questions about their impact on defendants' court appearance rates. Thus, the extent to which released defendants appeared for court was an important topic for consideration in the National Evaluation of Pretrial Release.

The overwhelming majority of the defendants studied appeared for court: in the eight-site sample, 87.4 percent of all released defendants appeared for every required court date. Conversely, 12.6 percent of the released defendants missed at least one court appearance.

In many ways this is a remarkable finding, particularly since failure to appear (FTA) was defined quite broadly. In general, if a defendant was required to appear in court on a certain date and did not do so, the absence was considered a failure to appear. Despite this very inclusive definition, seven-eighths of all released defendants made every court appearance required of them.

Many defendants who miss court appearances may have no intention of trying to evade justice. Instead, they have forgotten the court date, have become ill and neglected to notify the court or in some cases have been jailed on another charge.

Twenty-nine percent of the defendants who missed a court appearance returned to court of their own volition within 30 days, and an additional 16 percent returned voluntarily after that time. Approximately one-third of the defendants were returned to court as a result of an arrest, usually for another charge. Moreover, six percent of the defendants who missed court dates were tried in absentia or forfeited bail in lieu of appearance (a type of fine). Consequently, 17 percent of the defendants who failed to appear for court were still at large at the time data were collected for the National Evaluation of Pretrial Release. This is a "fugitive" rate of two percent of all released defendants.

Another aspect of the analysis of court appearance outcomes is the extent to which failure to appear disrupts court processing. Although few failures to appear were "willful," and even fewer were successful attempts to evade justice, a large percentage of missed appearances would have serious cost implications for the criminal justice system. The court appearance rates presented earlier cannot be used to consider this topic; those rates were defendant-based, that is, they reflected the percentages of defendants who missed an appearance. Because defendants may

be required to make several appearances and may miss more than one, an appearance-based measure is a better indicator of the court disruption caused by failure to appear.

Altogether, the released defendants in the sample were required to make 8,896 appearances (for all charges associated with the original arrest) and showed up for 8,361, or 94 percent, of them. Thus, only six percent of all court appearances were missed.

The overall rate of release was not systematically related to the rate of court appearance across the eight sites. The jurisdiction with the highest release rate also had one of the highest court appearance rates. The site with the lowest release rate had a court appearance rate roughly in the middle of the rate range for all sites.

Nor were there systematic differences in court appearance rates for defendants released on nonfinancial versus financial conditions across the eight sites. As shown in Table 6, the overall court appearance rate for defendants released on nonfinancial conditions was 87.8 percent and for defendants released on financial conditions, 86.4 percent. In some sites rates were higher for defendants released on nonfinancial conditions; in other sites, for persons released financially.

TABLE 6
COURT APPEARANCE RATES BY SITE
AND TYPE OF RELEASE
(FOR RELEASED DEFENDANTS ONLY)

Site	Total	For Defendants Released on Nonfinancial Conditions	For Defendants Released on Financial Conditions
Baltimore City, Md.	94.3%	95.0%	91.7%
Baltimore County, Md.	90.4%	89.3%	94.0%
Washington, D.C.	86.3%	85.1%	93.3%
Dade County, Fla.	81.6%	77.9%	84.6%
Louisville, Ky.	82.9%	86.8%	79.9%
Pima County, Ariz.	86.4%	85.2%	89.7%
Santa Cruz County, Cal.	79.5%	78.1%	86.9%
Santa Clara County, Cal	83.9%	85.9%	80.0%
Total, 8 sites	87.4%	87.8%	86.4%

The evaluation also compared defendants who appeared for all court dates with persons who missed at least one court date, to determine whether the two groups had very different characteristics. By charge category, defendants who failed to appear were more likely than other released defendants to have been charged with economic crimes and less likely to have been charged with crimes against persons or drug crimes (see Appendix A for detailed information by charge).

In terms of prior record, defendants who missed court appearances had more serious criminal records than persons who reliably showed up for court. Defendants who failed to appear had an average of 5.8 prior arrests and 2.4 prior convictions, as compared with 3.2 prior arrests and 1.3 prior convictions for defendants who always appeared for court.

There were also differences in community ties. Defendants who missed court dates were less likely than other released defendants to have been living with spouses and were more likely to have been living with unrelated persons. They were also more likely to have been unemployed: 49 percent of the defendants who failed to appear were unemployed, as compared with 37 percent of the released defendants who made all their court appearances. Additionally, defendants who missed court appearances had lived in the local area a shorter time; nevertheless, their average length of local residence was almost 19 years.

As was the case for the release outcomes discussed in the last chapter, court appearance outcomes varied along many dimensions besides charge, prior record and community ties. To identify the most important factors associated with failure to appear, multivariate analyses were conducted, similar in nature to those performed for release outcomes and decisions. The failure to appear analyses also included post-release variables, such as the type of release, type of legal representation, and number of postponements during the case.

When compared with defendants who made all court dates, persons who failed to appear were more likely:

- to have been on both probation and pretrial release for other charges when arrested;
- to have had more prior arrests;
- to have been of Hispanic ethnicity;
- to have had more charges associated with the arrest;
- to have been released on deposit bond;
- to have been represented by a public defender; and
- to have had a larger number of postponements during the trial of the case.

Additionally, defendants who failed to appear were less likely to have been charged with crimes where weapons were involved but were not found in the defendants' possession (as compared with crimes where no weapons were used, or, if used, were found in the defendants' possession).

The finding regarding the importance of Hispanic ethnicity deserves special comment. This may reflect a situation described to the evaluation team during the delivery system interviews, namely, the lack of sufficient Spanish-speaking personnel within the criminal justice system to insure

an adequate interpreter for Hispanic defendants who speak little English. Thus, it is possible that many Hispanic defendants failed to appear because they had a poor understanding of the court proceedings and requirements.

Interestingly, pretrial release program recommendations, which were very important in the analyses of release outcomes, were not important in the multivariate analysis of court appearance outcomes. Other indicators of program activities were also not significant in the court appearance outcomes analyses.

The analyses did not identify a set of characteristics that could be used to predict with reasonable accuracy the defendants who would fail to appear. This inability to develop accurate predictors reflects the difficulty of trying to predict an event that is relatively rare and experienced by persons with diverse characteristics. Only a relatively small percentage (12.6 percent) of defendants failed to appear, and those individuals did not have strikingly different characteristics from other defendants.

Although defendants who would fail to appear could not be predicted accurately, defendants who would appear for court could be identified with a high degree of accuracy. Because such a large percentage of defendants did appear for court, a prediction of appearance for all released defendants would necessarily be accurate much of the time. For the eight-site defendant sample a prediction that every released defendant would appear for all court cases would have been correct in 87.4 percent of the cases. In comparison, the multivariate analyses correctly classified 89.5 percent of the released defendants.

CHAPTER V. PRETRIAL CRIMINALITY OF RELEASED DEFENDANTS

One of the most controversial issues surrounding pretrial release practices concerns the criminality of released defendants and suggested ways of adequately protecting the public from such crimes. Chief Justice Warren Burger is among the persons who have proposed that a defendant's possible threat to the community not be overlooked in setting bail.6/ Moreover, a public opinion survey conducted in 1978 found that 37 percent of the respondents thought it was a "serious problem which occurs often" for courts to grant bail to persons previously convicted of a serious crime. This belief was shared by persons of different ethnicity, income and self-described classifications of liberal, moderate and conservative.7/

Despite widespread concern about release practices and pretrial criminality, most of the laws governing release decisions have not permitted consideration of the possible "dangerousness" of a defendant. Historically, the legal basis of release decisions has been whether the defendant will appear for court, and conditions of release (bail, supervision, etc.) have been constrained to be the least restrictive ones preventing flight. Thus, a defendant who poses a poor risk of appearing for trial can have a variety of conditions imposed to increase the likelihood of appearing, but a defendant who poses a poor risk of being crime-free during the pretrial period cannot legally be subject to similar limitations designed to reduce the probability of crime.

This situation has been questioned by many persons, and a change which often has been suggested is the legalization of "preventive detention." Such a policy, which exists in the District of Columbia and several States, would permit the detention of dangerous defendants. Opponents of preventive detention, however, note the difficulties of predicting dangerousness and stress the fact that preventive detention may violate certain Constitutional principles regarding the treatment of defendants who have been accused of crimes, but not found guilty of them. Indeed, when the District of Columbia legislation was under consideration, Senator Sam Ervin described preventive detention as "repugnant to our traditions."8/

The sharpness of the disagreement over policies concerning pretrial criminality is illustrated by the 1974 findings of a national survey of criminal justice policy-makers who were asked to rate sixteen possible goals for pretrial release. The goal, "helping to ensure that individuals who might be dangerous to the community are not granted pretrial release," was ranked second in importance by police chiefs, fifth by sheriffs, sixth by judges and eighth by county executives and district attorneys. In contrast, public defenders and program directors ranked this goal fourteenth, or third from last.9/

In the past discussions of pretrial criminality issues were hindered by lack of data. For example, a 1973 survey of 101 pretrial release programs found only 20 projects that maintained data on the rearrest rate for defendants released on own recognizance; even fewer programs (six) had information on the rearrests of bailed defendants.10/ A 1975 survey of 115 projects had similar findings: 19 programs possessed rearrest data for defendants on nonfinancial release, and only four programs had rearrest information on defendants released with financial conditions.11/

The fact that so few programs have data on pretrial criminality is partly due to the difficulty of obtaining adequate information about it. Arrest data may be protected by a variety of confidentiality provisions, making access legally difficult; police agencies may be reluctant to cooperate with the program, thus making access hard as a practical matter; and the records themselves may be incomplete, poorly organized or otherwise difficult and time-consuming to use.

Because of the lack of information on pretrial criminality and the widespread interest in the topic, an important goal of the National Evaluation of Pretrial Release was to develop data on the extent and types of crimes committed pending trial. The primary measure of "pretrial criminality" was arrests for offenses alleged to have occurred during the pretrial period. Arrests for minor traffic offenses were excluded, as were arrests for failure to appear in the initial case selected for study. Pretrial arrests that occurred outside the eight sites were included, whenever these could be identified (e.g., by checking arrest records on a Statewide basis or for neighboring jurisdictions of other States, such as the Indiana area bordering Louisville, Kentucky).

Although arrest data have been used frequently for analyses of crime, these data have serious limitations. For example, victimization studies have shown that more crimes occur than are reflected in arrest data. All crimes are not reported to the police, and even the reported crimes are not always "cleared" by arrest.

An additional drawback of arrest data is that an arrest does not reflect guilt. An arrested person may be found innocent of the offense charged; the initial charges may be reduced to lesser offenses; all charges may be dropped by the prosecutor or dismissed by the court; and so on. To overcome this limitation of arrest data, additional analysis was conducted in which only convictions (i.e., court findings of guilt or guilty pleas) for pretrial arrests were considered as pretrial crimes.

The findings of the study show that the overwhelming majority, 84 percent, of all released defendants in the eight sites had no pretrial arrests. As indicated in Table 7, the overall pretrial arrest rate was 16 percent, with rates for individual jurisdictions ranging from 7.5 percent to 22.2 percent.

TABLE 7
PRETRIAL ARREST OUTCOMES BY SITE AND TYPE OF RELEASE
(FOR RELEASED DEFENDANTS ONLY)

Site	Percentage of Released Defendants Not Rearrested Pretrial	Pretrial Arrest Rates		
		For All Released Defendants	For Defendants Released on Nonfinancial Conditions	For Defendants Released on Financial Conditions
Baltimore City, Maryland	92.5%	7.5%	6.8%	10.4%
Baltimore County, Maryland	82.9%	17.1%	15.1%	24.4%
Washington, D.C.	77.8%	22.2%	22.9%	18.3%
Dade County, Florida	82.5%	17.5%	23.8%	12.3%
Louisville, Kentucky	78.6%	21.4%	21.1%	21.6%
Pima County, Arizona	77.9%	22.1%	22.2%	19.2%
Santa Cruz County, California	90.4%	9.6%	9.3%	11.5%
Santa Clara County, California	85.4%	14.6%	11.8%	22.0%
Total, 8 Sites	84.0%	16.0%	15.3%	18.1%

Defendants released on nonfinancial conditions had a 15.3 percent rearrest rate and persons released on financial conditions, 18.1 percent. As was the case for the court appearance rates discussed in the last chapter, there were no systematic differences in pretrial arrest rates for defendants released on nonfinancial versus financial conditions across the eight sites. In some sites rates were higher for defendants released nonfinancially; in other sites, financially.

Nor were total release rates systematically related to rearrest rates. The sites with the highest rearrest rates had release rates ranging from the lowest of the eight sites to one of the highest.

Most rearrests occurred fairly early in the release period: 16 percent occurred within one week of the original arrest, 45 percent within four weeks, 66 percent within eight weeks, and 80 percent within twelve weeks. As a result, rearrests occurred more quickly than either failure to appear or the disposition of cases of released defendants.

Many defendants were rearrested repeatedly during the pretrial period. About 30 percent of the rearrested defendants were rearrested more than once, some as many as four times. On the average, each rearrested defendand had 1.4 pretrial arrests.

Assessment of pretrial criminality also requires consideration of the types of charges for which defendants were rearrested.12/ The most common rearrest category was economic crime (31 percent), followed by crimes against persons and public order (20 percent each). Information on specific charges appears in Appendix A.

A comparison of rearrest charges with the charges for the original arrest showed that rearrests were for somewhat less serious charges. Forty-three percent of the rearrests involved defendants who had been charged originally with a Part I offense, while 38 percent of the rearrests themselves were for Part I offenses. In terms of the six-category crime classification, the major difference between original and rearrest charges was that a smaller percentage of defendants were rearrested for economic crimes (31 percent of the rearrest charges, as compared with 41 percent of the original charges for rearrested defendants).

When convictions were considered, rather than arrests, the data showed that 7.8 percent of all released defendants were convicted of a pretrial arrest. Thus, about half of all pretrial arrests resulted in a conviction.13/

Analysis of the sentences imposed showed that 49 percent of the sentences stemming from pretrial arrests involved incarceration. About half of those incarcerations were for relatively less serious crimes (e.g., crimes against public morality, such as prostitution and drunkenness, and crimes against public order, such as disorderly conduct and driving while intoxicated).

Besides assessing the extent and type of pretrial arrests, the National Evaluation of Pretrial Release compared defendants who were rearrested

with those who were not. Defendants with pretrial arrests were originally charged with more serious crimes than defendants who were not rearrested: 42 percent of the rearrested group was originally charged with a Part I crime, as compared with 27 percent for other defendants. In addition, rearrested defendants had a much higher incidence of economic crimes (40 percent versus 23 percent) as their original charges and a much lower proportion of crimes against public order (19 percent versus 33 percent). Appendix A provides data on rearrests by specific charges.

Rearrested defendants also had more extensive prior records than other defendants. They averaged five prior arrests and 2.5 prior convictions, as compared with three and 1.2, respectively, for other defendants.

In terms of community ties, rearrested defendants were less likely than other released defendants to have been living with spouses and more likely to have been living with parents. They were also more likely to have been unemployed: 50 percent of the rearrested defendants were unemployed, as compared with 36 percent of the released defendants who were not rearrested.

As with the release and court appearance outcomes discussed in the preceding chapters, rearrest outcomes varied along many dimensions other than charge, prior record and community ties. To identify the most important characteristics associated with pretrial arrest, multivariate analyses were conducted. These analyses used the same procedures that had been employed for the analyses of court appearance outcomes. The results identified several differences as the most significant ones, when rearrested defendants were compared with persons not rearrested pending trial. Specifically, rearrested defendants were more likely:

- to have had more prior arrests;
- to have been charged originally with an economic crime;
- to have been charged originally with offenses in which the victims were not prior acquaintances (as compared with offenses where the victims were known or there were no victims);
- to have had bail amounts set originally between $1,001 and $1,500;
- to have been represented by a public defender;
- to have had more court appearances in the original case;
- to have failed to appear for court for the original charge;
- to have been unemployed; and
- to have been younger at the time of the original arrest.

Additionally, rearrested defendants were less likely to have had their last release option provided by a bail commissioner or to have represented themselves on legal matters at trial.

Pretrial release program recommendations, which had been important in the analyses of release outcomes and unimportant in the court appearance analyses, were not significant in the rearrest analyses. Nor were other indicators of program activities important in the multivariate analyses of pretrial arrests.

No set of variables was identified that could predict rearrest with reasonable accuracy. The situation is similar to that discussed for failure to appear for court. Because pretrial arrests were relatively rare and were scattered among defendants with diverse characteristics, accurate predictors of rearrests could not be developed. At the same time, accurate predictions about defendants who would not be rearrested could be made with relative ease, because the great majority of defendants were not rearrested pending trial.

PART II
THE IMPACT OF PRETRIAL RELEASE PROGRAMS:
A STUDY OF FOUR JURISDICTIONS

CHAPTER VI
THE NATURE OF THE ANALYSIS

An important topic considered in the National Evaluation of Pretrial Release was the impact of pretrial release programs on release, court appearance and pretrial arrest outcomes. Of particular concern were the likely outcomes, if programs did not exist.

An experimental design was chosen as the most appropriate way of studying this topic. An experimental group of defendants processed by a program was compared with a control group of defendants not processed by the program. The two groups were selected concurrently, using random assignment procedures that provided individual defendants with an equal probability of selection into either group.

Experiments were conducted in four jurisdictions: Pima County (Tucson), Arizona; Baltimore City, Maryland; Lincoln, Nebraska; and Jefferson County (Beaumont-Port Arthur), Texas. The Baltimore City and Beaumont-Port Arthur experiments covered both felony and misdemeanor charges; Lincoln was limited to defendants charged with misdemeanors. In Tucson separate experiments were implemented at the felony and misdemeanor levels.

To avoid denial of service to defendants, the experiments involved the expansion of program operations to reach persons not previously processed. As a result of this temporary expansion, funded by the National Institute of Justice, programs were able to select a control group without decreasing the number of defendants processed.

The nature of the expansion and the scope of defendants included in the experiment varied across sites. In Lincoln the days and hours of program operation were increased. All misdemeanor defendants eligible for program processing during the time period of the experiment were randomly assigned to experimental and control groups.

In Beaumont-Port Arthur the program expanded its staff to increase its interviewing capability. Initially, the program expanded its hours of operation as well, but this was later found unnecessary. As in Lincoln, all defendants eligible for program processing during the time period of the experiment were included in the study.

The Tucson situation was different. Before the experiment the program had attempted to process all felony defendants but had been unable to provide full services to persons brought to court late. Consequently, this "late arrests" group was used for the experimental analysis. Rather than a haphazard approach to processing these defendants—with the result that some persons received full services while others obtained

partial services—the late arrestees were divided into an experimental group that received full program services (i.e., interview, verification and release recommendation) and a control group that was interviewed only. All felony defendants except the late arrests group were processed normally.

Because Arizona State law requires judges making release decisions for felony defendants to obtain and consider certain specified information, the interview data were presented to the judges for all defendants, including those in the control group. This was considered an accurate reflection of the conditions that would exist in the absence of the program. Thus, the experiment tested the impact of verified information and program recommendations on release outcomes.

At the misdemeanor level in Tucson a new program was established to implement the experiment. A misdemeanor program had operated for several years, ending approximately one year before the experimental program began. Some of the same staff, including the director, were hired for the experiment, which covered all defendants booked on misdemeanor charges (except very minor ones) under the jurisdiction of the City Courts of Tucson.

In Baltimore City, where the pretrial release program interviewed virtually all defendants soon after arrest, there was no "overflow" group of defendants not interviewed. However, many defendants had point scores too low to qualify for an own recognizance (O.R.) release recommendation. Consequently, this group was used for an experimental test of the impact of expanded eligibility for O.R. release recommendations. Release decisions continued to be made by bail commissioners and judges.

Defendants with low point scores were randomly split into two groups: one group automatically received O.R. release recommendations, and the other group was processed normally. Thus, the Baltimore experiment—unlike the others—tested the impact of a change in program operations, rather than the impact of the program as a whole.

A defendant with a low point score could be excluded from the experiment for several reasons. These were:

- having a charge too serious for an automatic O.R. release recommendation;[14]
- awaiting trial on another charge, transfer to another jurisdiction or probation/parole revocation review;
- having serious psychiatric problems;
- not residing within the State;
- having a prior record of "flagrant" failure to appear (defined as two failures to appear with guilty dispositions or four FTA charges within the last two and one-half years); or
- lacking a verified address.

Thus, although each of the experiments involved the expansion of program activities, the nature of that expansion differed. Additionally, in Lincoln, Beaumont-Port Arthur and for Tucson misdemeanors, the experimental procedures were applied to all defendants eligible for program processing; while for Tucson felony and Baltimore City defendants, only part of the defendant population was affected. Because both Baltimore City and Tucson were also included in the eight-site analysis (discussed earlier, in Part I), the characteristics of the defendants in the experiments could be compared with those of the appropriate group of arrestees for the jurisdiction as a whole. As expected, the "late arrests" studied in Tucson had characteristics very similar to those of all felony arrestees, and the defendants included in the Baltimore experiment had weaker community ties than all arrestees.

The experiments themselves can be considered in two parts: one assessing program impact on release outcomes; and the second, on the court appearance and pretrial criminality rates of released defendants. In general, the experimental procedures required the program's staffmembers to interview an expanded group of arrestees, who were randomly assigned to either the experimental or control group.

For the experimental group, the program followed its normal processing procedures—typically, a release recommendation was prepared, based on verified interview information, and this recommendation was presented to a judge. For the control group, the program in most experiments did not present a recommendation or the interview to the judge (as discussed earlier, procedures differed for the Tucson felony and Baltimore City experiments).

Thus, for the control group, judges made their release decisions in the absence of program information, while for the experimental group judges had access to program information. Consequently, a comparison of the release decisions made for the experimental and control groups permitted analysis of the program's impact on:

- rate of release, that is, the extent to which defendants secured release at any point prior to final adjudication of their cases;
- speed of release, that is, the time that elapsed between arrest and release;
- type of release, that is, the extent to which defendants were released on nonfinancial, as opposed to financial, conditions; and
- equity of release, that is, the extent to which release outcomes (rate, speed and type of release) were similar for defendants of different ethnicity and of different employment status.

The analysis of program impact on the court appearance and pretrial arrest rates of released defendants was complicated by the fact that the

released defendants in the experimental and control groups for any given site would not necessarily be comparable. This was because the program's involvement with release decisions for the experimental group might result in the release of defendants with very different characteristics than the persons who secured release in the control group. If released defendants were not comparable for the two groups, then any differences in their outcomes after release might be due to other factors than program impact.

To avoid such a difficulty, a second random assignment procedure was developed. After the release decision had been made, released defendants would be assigned to groups that either received program followup or did not. A comparison of court appearance and pretrial arrest outcomes for the two groups would then reflect the impact of program followup activities.

This second random assignment procedure was successfully implemented for two of the experiments: those involving Tucson misdemeanor defendants and Baltimore City arrestees. The procedure could not be used for Tucson felony defendants or Lincoln arrestees, because those programs did not provide routine followup throughout the pretrial period. Additionally, local acceptance of a second random assignment could not be obtained in the Beaumont-Port Arthur area.

For Tucson misdemeanors all released defendants were randomly assigned to two groups: one received program followup, and the other did not. The followup consisted of the program's notifying defendants of coming court dates. Notification was accomplished by mail or telephone, in English or Spanish.

In Baltimore all defendants with low point scores who secured release on own recognizance (O.R.) were included in the experimental analysis of post-release followup services. Thus, the post-release analysis covered defendants who had been excluded from the earlier experimental test of release impact (because of charge, residence or other reasons), if they were released on O.R.

Because the Baltimore program had for several years provided some followup contact for all defendants released on O.R., it was not possible to have a control group that received no followup. The program staff thought such a control group would represent a substantial service cutback.

The routine followup normally provided to defendants consisted of monitoring telephone calls from them once a week. During these calls the defendant would be reminded of coming court dates and encouraged to comply with release conditions and "to stay out of trouble." For the minimum followup in the control group, weekly calls continued to be monitored by the program, but little was said to the defendant: the call was acknowledged and the defendant's address verified, but the defendant was not reminded of court dates or other release requirements.

For the experimental group, defendants were screened to see if they needed any special services and, if so, were referred to the appropriate

unit of the supervised release program. Drug abuse, alcohol, and mental health services were available through referral to community-based treatment programs. Additionally, a few defendants were eligible for a diversion program providing employment services.

If experimental group defendants did not need special services, they were referred to the program's "surveillance" unit. These defendants were required, at a minimum, to call the program twice a week. During these calls program staff reminded them of coming court dates and encouraged them to comply with release conditions. Some defendants were also required to report to the program in person on a periodic basis.

Thus, the experimental test of post-release followup in Baltimore City compared the impact of monitoring weekly calls from defendants in a rather perfunctory manner with the effect of more intensive followup. This more intensive followup consisted at least of two calls a week during which defendants were counseled to appear for court and stay out of trouble and often included referral to service programs as well.

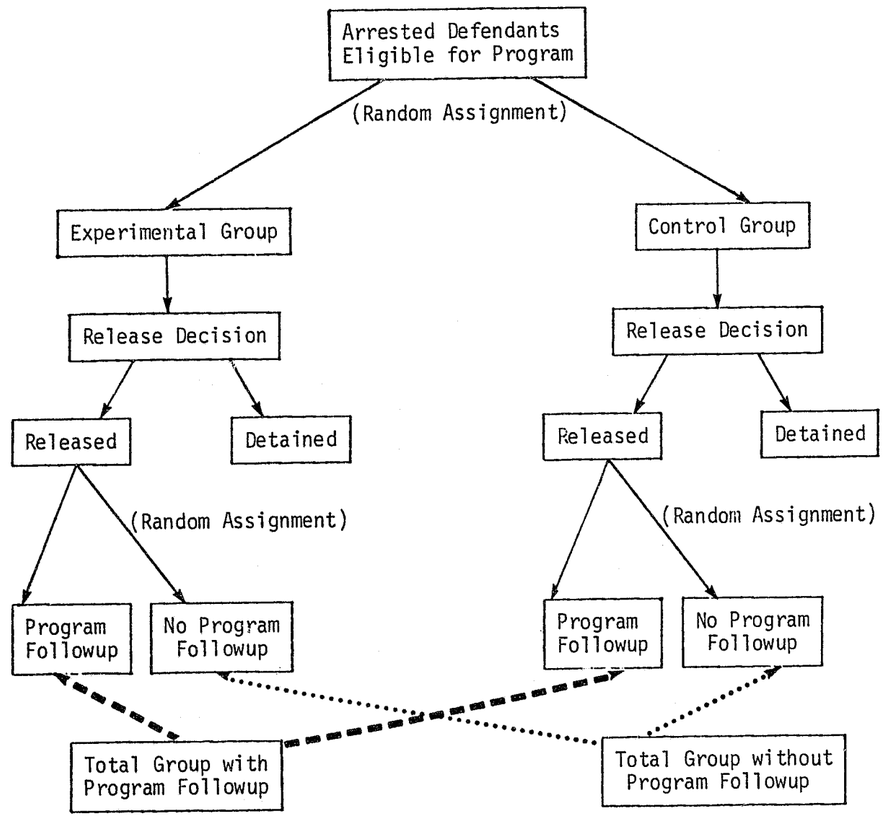
Figure 2 summarizes the experimental approach. As shown, separate analyses were conducted of program impact on release decisions (accomplished by random provision of program information to the judges making those decisions) and on defendant outcomes after release (accomplished, where possible, by random provision of program followup to released defendants).

Table 8 shows the number of defendants who participated in the experiments at each site, as well as the time periods over which the experimental and control groups were developed. As indicated, the experiments involved 1,570 defendants in the four sites. Both Tucson and Baltimore City had relatively large numbers of defendants (719 and 528, respectively), while the Beaumont-Port Arthur and Lincoln experiments were smaller in scope (193 and 130, respectively).

Table 8
SIZE AND DATES OF EXPERIMENTS

Site	Number of Defendants	Time Period of Experiment
Two-Stage Random Assignment:		
Tucson Misdemeanors	424	Nov. 1978 - Jan. 1979
Baltimore City	528	May 1979 - Aug. 1979
One-Stage Random Assignment:		
Beaumont-Port Arthur	193	Sept. 1978 - Mar. 1979
Lincoln	130	Dec. 1978 - Aug. 1979
Tucson Felonies	295	Nov. 1978 - Mar. 1979
TOTAL	1,570	

FIGURE 2
SUMMARY OF EXPERIMENTAL APPROACH WITH TWO-STAGE RANDOM ASSIGNMENT



The first experiment began in Beaumont-Port Arthur in September 1978 and the last, in Baltimore City in May 1979. The selection of the experimental and control groups required from three to nine months to complete and ended in August 1979.

In each site the backgrounds of the experimental and control group defendants were compared for three major types of characteristics:

- community ties, including family ties, residence and employment, because these factors often form much of the basis for programs' release recommendations;
- criminality, including current charge (because this may be an important determinant of release eligibility and has commonly been used to determine bond amounts, e.g., in bond schedules) and prior criminal record (because this may be associated with both the release outcome and subsequent criminality of released defendants); and
- demographic characteristics of the defendants, such as age, ethnicity and sex.

These comparisons showed that the experimental procedures had resulted in the selection of experimental and control groups having similar characteristics in all sites except Beaumont-Port Arthur. In that jurisdiction the two groups differed along six of the nineteen background characteristics for which they were compared. Consequently, for that site it was impossible to determine conclusively whether differences between experimental and control group outcomes were due to program impact or other factors, although statistical techniques were used to assess the likely effects of each.

The next three chapters discuss the experimental analyses. First, program impact on release outcomes (i.e., rate, speed, type and equity of release) is considered, followed by analysis of program impact on the court appearance and pretrial criminality rates of released defendants. Finally, results of a brief cost-effectiveness analysis are presented.

CHAPTER VII
PROGRAM IMPACT ON RELEASE OUTCOMES

Program impact on release outcomes was analyzed by comparing the experimental and control groups' outcomes for rate, speed, type and equity of release. Differences between the two groups' outcomes were considered significant whenever statistical tests indicated that such differences would have occurred by chance no more often than five times out of one hundred.

Three experiments showed positive program impact on the overall rate of release. In Baltimore City, where the impact of changed program procedures was tested, 144 out of 148 (97 percent) experimental defendants were released prior to trial, as compared with 145 out of 158 (92 percent) controls. 15/ The high rate of release for both groups is somewhat surprising, because all defendants in the experimental study had low point scores. The release rate is probably due to the limited charge eligibility for inclusion in the experiment.

In Lincoln 77% of the experimental group was released, as compared with 47% of the control group. The low release rate for the control group is partly explained by the fact that many of the misdemeanor defendants' cases were settled at the first court appearance; these defendants, technically, were detained until trial. In the experimental group more defendants were released before the first court appearance, as a result of program intervention.

In Beaumont-Port Arthur 86 percent of the experimental group and 57 percent of the control group secured release pending trial. However, because the experimental and control groups were not equivalent, it could not be conclusively determined whether the apparent program impact was real or due to differences in defendant characteristics. When limited statistical controls were exercised, the program appeared to have a positive effect on the release of most defendants. However, the impact of program processing had not been able to override the adverse effect on release of (1) a longer prior record, (2) employment as a laborer, or (3) very low education. Defendants with any of those three characteristics had similar release outcomes in both the experimental and control groups; other defendants fared better when processed by the program.

In Tucson, at both the felony and misdemeanor levels, rates of release were similar for the experimental and control groups. For felony defendants the release rates were 86 percent for the experimental defendants and 85 percent for the controls. For the misdemeanor defendants the release rates were identical for both groups at 68 percent. The lower release rate for misdemeanor defendants occurred because more of them had their cases settled at the first court appearance, when release conditions would otherwise have been set.

In terms of speed of release, as indicated by the mean number of days from arrest to release, only the Baltimore experiment showed significant program impact. In that site released defendants in the experimental group secured release 0.7 days after arrest, on the average, as compared with 2.8 days for members of the control group.

Baltimore was also the only experiment with significant program impact on type of release: 91.5 percent of the released defendants in the experimental group were released on nonfinancial conditions, as compared with 72% of the released defendants in the control group. For Tucson felonies and misdemeanors and for Lincoln defendants no important differences were found in the extent of release on nonfinancial conditions for the experimental versus control groups. In Beaumont-Port Arthur type of release was not compared, because the nature of the program ensured a difference. The least restrictive type of release in that jurisdiction is three percent bond, available only through the program (own recognizance release is not authorized in the area).

The experimental analysis also included a brief consideration of program impact on the equity of release for defendants of different employment status (a proxy for income level) and ethnicity. The rate, speed and type of release were compared for employed versus unemployed defendants in the experimental group and, separately, in the control group. If the release outcomes were the same for both the experimental and control groups (i.e., either both groups showed similar release rates by employment status or both groups had the same differences, such as lower release rates for unemployed defendants), no program impact on equity was considered to have occurred.

If there was a significant difference between the experimental and control groups for release outcomes by employment status, further analysis was conducted. Because employed versus unemployed defendants in the two groups might vary for other characteristics that were the real explanation for the different release outcomes, employed versus unemployed defendants in each group were compared for 16 background characteristics. When differences were found between the experimental and control groups, the effect on release outcomes was considered. Only if this could not explain the difference in release outcomes by employment status between the experimental and control groups was program impact on equity considered to have occurred. In all such cases, the experimental group showed similar release outcomes by employment status, but the control group did not. Consequently, the program's impact on release equity was considered positive in these cases.

Based on this analysis, program operations were associated with more equitable release outcomes by employment status in two sites, Baltimore and Lincoln. In Baltimore 99 percent of the employed defendants in the control group were released, as compared with 86 percent of the unemployed defendants; this difference was a statistically significant one. For the experimental group, 99 percent of employed and 96% of unemployed defendants secured release, an insignificant difference. Findings were similar in Lincoln, although small numbers of defendants were involved in the comparisons, due to the small size of the total experiment in that site.

In Tucson, at both the felony and misdemeanor levels, unemployed defendants were detained at a significantly higher rate that employed persons in both the experimental and control groups. For felony defendants, 92 percent of employed persons in the experimental group were released, as compared with 79 percent of unemployed defendants. In the control group 94 percent of employed and 76 percent of unemployed defendants were released. At the misdemeanor level, 77 percent of the employed and 50 percent of the

unemployed defendants were released in the experimental group. Comparable percentages for the control group were 74 percent of the employed and 58 percent of the unemployed defendants.

Only the Beaumont-Port Arthur experiment showed equivalent release rates for employed and unemployed defendants in both the experimental and control groups. As discussed earlier, the lack of comparability for the experimental and control groups as a whole in Beaumont-Port Arthur reduces the level of confidence in all findings for that site.

The analysis of the equity of release outcomes by ethnicity was similar to that conducted by employment status. In this case outcomes were compared for white versus minority defendants.

In two experiments, for Tucson felonies and Baltimore defendants, the experimental groups showed no differences in the rate of release of minority versus white defendants, while in the control groups minority defendants were significantly less likely to be released. For Tucson felonies 90 percent of white and 77.5 percent of minority defendants were released in the control group, as compared with 89 percent of white and 82 percent of minority defendants in the experimental group. For Baltimore 100 percent of white and 87 percent of minority defendants were released in the control group; comparable release percentages for the experimental group were 100 percent of white and 96 percent of minority defendants.

In the remaining three experiments, Lincoln, Beaumont-Port Arthur and Tucson misdemeanors, no significant differences in release rates were found for minority versus white defendants in either the experimental or control group. Although Tucson misdemeanor release rates were higher for minority than white defendants in the experimental group, further analysis showed that this was due to the higher employment rate of minority defendants. No release differences remained by ethnicity after the effects of employment status were taken into consideration.

Table 9 summarizes program impact on release outcomes in the five experiments. As indicated, four of the five programs showed an impact on release outcomes; only the Tucson misdemeanor program did not. Three programs affected the rate of release; one, the speed of release; one, the type of release; and three, the equity of release. By site, Baltimore City (where the effect of changed program procedures was tested) showed the greatest impact, with each release outcome affected favorably. Lincoln was next (two outcomes affected) followed by Beaumont-Port Arthur (one outcome affected) and Tucson (one outcome affected in the felony study and none in the misdemeanor experiment).

TABLE 9
SUMMARY OF PROGRAM IMPACT ON RELEASE OUTCOMES

Note: + indicates positive program impact on outcomes; 0 indicates no effect.

OUTCOME	BALTIMORE CITY	LINCOLN	TUCSON FELONIES	TUCSON MISDEMEANORS	BEAUMONT-PORT ARTHUR
Rate of Release	+	+	0	0	+
Speed of Release	+	0	0	0	0
Type of Release	+	0	0	0	NA*
Equity of Release:					
By Employment Status	+	+	0	0	0
By Ethnicity	+	0	+	0	0

*Not applicable.

CHAPTER VIII
PROGRAM IMPACT ON COURT APPEARANCE AND PRETRIAL CRIMINALITY OUTCOMES

An important program impact issue concerns the outcomes of defendants after release, specifically, whether defendants processed by programs experience different rates of court appearance or pretrial criminality than control group defendants. As in the analysis of release outcomes, discussed in the preceding chapter, differences were considered significant when their likelihood of occurring by chance was no more than five times out of one hundred.

For both Tucson misdemeanors and Baltimore defendants, persons were randomly assigned after release to two groups: one received program followup and the other did not. As expected, the experimental and control groups were equivalent in each site for all of the background characteristics for which they were compared (17 characteristics in Baltimore and 18 in Tucson).

In Tucson no outcomes differences were found for the experimental group, which was notified by the program of coming court dates, as compared with the control group, which received no program notification. Court appearance rates for both groups were 88 percent.16/ Nor were there significant differences in pretrial criminality rates: six percent of the experimental and five percent of the control group was rearrested during the pretrial period. Approximately half of the pretrial arrests led to convictions; three percent of the experimental and two percent of the control group had a pretrial arrest conviction.

Results of the Baltimore experiment, which tested the impact of intensive versus minimal program supervision, were similar. Court appearance rates were 83 percent for both the experimental and control groups. Pretrial criminality rates also showed no significant differences: pretrial arrest rates were eight percent for the experimental and nine percent for the control group, and pretrial arrest conviction rates were five percent for the experimental and four percent for the control group.

There are several possible explanations for the apparent lack of impact of supervision activities. First, supervision may in fact be ineffective at reducing failure to appear or pretrial criminality rates. Second, program followup may indeed have an effect on defendant behavior during the pretrial period, but the impact of minimal supervision may be as great as the effect of more intensive supervision. (Recall that the Baltimore experiment did not include a group with no program followup; the experiment tested "minimal" versus "more intensive" supervision, rather than "some" versus "no" followup.)

A third possible explanation is that the impact of supervision, and particularly of treatment for services, may occur over a longer time span than the pretrial period. Limiting the impact analysis to the time between arrest and trial will necessarily miss any subsequent outcomes differences. Finally, supervision may need to be applied very selectively

in order to be effective. If so, comparisons of large groups in which all defendants received some followup could obscure the beneficial effects of supervision on a much smaller group of persons.17/

As discussed earlier, three of the five experiments involved a random assignment of defendants only before release decisions were made. Consequently, there was no reason to expect released defendants in the experimental and control groups to be comparable. However, analysis of 17 background characteristics for released defendants showed that these groups were comparable for Tucson felonies. In Lincoln 16 of the 18 background characteristics analyzed were comparable, and statistical controls were exercised for the remaining two characteristics to assess their impact on post-release defendant outcomes. In Beaumont-Port Arthur, as expected because of the lack of comparability for all experimental and control group defendants, released defendants in the experimental and control groups had very different characteristics (six of the nineteen characteristics compared showed significant differences between released experimental and control group defendants).

In general for Tucson felony, Lincoln and Beaumont-Port Arthur defendants, there were no differences in the court appearance or pretrial criminality outcomes of the experimental and control groups. Indeed, as shown in Table 10, the only difference was a lower rate of pretrial arrest conviction for the experimental group in Beaumont-Port Arthur. As discussed earlier, the lack of comparability between the experimental and control groups in Beaumont-Port Arthur makes it impossible to determine whether the pretrial arrest conviction difference was due to program impact or to the differences in defendant characteristics.

Thus, in none of these three sites were program operations associated with worse court appearance or pretrial criminality outcomes than when the programs did not function. This occurred even though two of the three sites had significantly higher release rates for experimental group defendants. Consequently, the release of additional defendants did not lead to increased disruption of court operations (through lower court appearance rates) or to greater harm to community safety (through higher pretrial criminality rates).

The same rate of failure to appear or pretrial arrest does, of course, result in a larger absolute number of cases, when the total number of released defendants increases. However, it should be stressed that the additional defendants released in the experimental groups posed no greater relative risks than the smaller numbers of defendants released in the control groups.

Table 11 summarizes program impact on court appearance and pretrial criminality outcomes for the five experiments. As shown, for the two experiments where random assignment occurred after release, so that the impact of program followup activities could be tested, no differences in outcomes were found between the experimental and control groups. However, these tests of followup impact were quite limited in scope. In

TABLE 10
COURT APPEARANCE AND PRETRIAL CRIMINALITY RATES
FOR TUCSON FELONY, LINCOLN, AND BEAUMONT-PORT ARTHUR DEFENDANTS
(RELEASED DEFENDANTS ONLY)

SITE AND OUTCOME	EXPERIMENTAL GROUP	CONTROL GROUP
Tucson (Felonies):		
Court appearance	90%	92%
Pretrial arrest	10%	12%
Pretrial arrest conviction	6%	7%
Lincoln:		
Court appearance	90%	82%
Pretrial arrest	10%	12%
Pretrial arrest conviction	7%	6%
Beaumont-Port Arthur:		
Court appearance	86%	80%
Pretrial arrest	5%	14%
Pretrial arrest conviction	3%	14%

Note: The only significant difference was for pretrial arrest convictions in Beaumont-Port Arthur, where the lack of comparability between the experimental and control groups precludes attributing the pretrial arrest conviction difference to program impact.

TABLE 11
SUMMARY OF PROGRAM IMPACT ON COURT APPEARANCE
AND PRETRIAL CRIMINALITY OUTCOMES

Note: + indicates positive program impact on outcomes; 0 indicates no effect.

OUTCOME	PROGRAMS WITH POST-RELEASE RANDOM ASSIGNMENT		PROGRAMS WITHOUT POST-RELEASE RANDOM ASSIGNMENT		
	Tucson Misdemeanors	Baltimore City	Tucson Felonies	Lincoln	Beaumont-Port Arthur
Court Appearance	0	0	0	0	0
Pretrial Rearrest	0	0	0	0	0
Pretrial Rearrest Conviction	0	0	0	0	+

one situation (Tucson misdemeanors), mail/telephone notification of coming court dates was tested, and in the other site (Baltimore City) the impact of minimal versus more intensive supervision was analyzed. Thus, these findings cannot be considered conclusive regarding the impact of supervision on defendant behavior after release.

In the three experiments where random assignment occurred only before release, there were in general no differences in outcomes between the experimental and control groups. Although two of three sites had higher release rates for experimental group defendants, these defendants did not have higher rates of failure to appear or pretrial criminality.

CHAPTER IX
COST-EFFECTIVENESS OF PROGRAMS

The experiments provided an excellent opportunity for analyzing the cost-effectiveness of pretrial release programs, because defendant outcomes in the absence of program activities could be compared with outcomes when programs operated. Consequently, a major difficulty with past cost-effectiveness analyses of pretrial release programs could be avoided, namely, the assumption that defendants not released initially would have been detained the entire pretrial period.18/ In fact it is likely that some of these defendants would have secured release eventually, for example, by posting bail. For the experimental sites the extent of detention could be compared directly for the experimental and control groups; assumptions about eventual release outcomes were unnecessary.

The analysis considered the cost-effectiveness of pretrial release programs, rather than the cost-effectiveness of pretrial release practices.19/ These may differ, because many persons (e.g., judges, attorneys, bondsmen) besides program staff affect release outcomes.

Additionally, cost-effectiveness was studied from the viewpoint of the criminal justice system (CJS), not that of defendants, the public at large or another group. Thus, costs were included in the analysis only if the CJS incurred them; similarly, benefits were counted only when the CJS accrued them.

Many problems were encountered in the development of the cost-effectiveness analysis. A major difficulty was the relatively poor cost data available locally. Because records were usually not maintained in ways that facilitated retrieval of precise cost data, the cost estimates developed were often very rough ones. Also, although marginal cost estimates were sought, these could not always be obtained.

Consequently, for a variety of reasons, the following cost-effectiveness analysis should be considered suggestive, rather than definitive. It is intended to provide additional perspective on the possible impact of pretrial release programs.

Four broad categories of costs were considered: (1) detention; (2) failure to appear; (3) pretrial arrest; and (4) program costs. Detention costs were based on marginal jail costs and included all detention by all defendants. Thus, detention costs for defendants who were eventually released were counted, as well as jail costs for persons detained the entire pretrial period.

Failure to appear (FTA) costs included costs associated with the initial occurrence of FTA, as well as any additional costs incurred because of the FTA when defendants returned to court. Similarly, the costs of pretrial arrest had several components: apprehension, booking, program costs if defendants were processed again, detention (if any), court processing costs and sentencing costs.

Program costs, where applicable (i.e., usually only for the experimental group), reflected average costs, rather than the marginal costs

used in other cases. This was because the experimental procedures were typically designed to study the impact of a program, as compared with its absence. If there were no program, there would be neither fixed nor variable costs, so average cost estimates were appropriate.

Program cost-effectiveness was assessed by estimating and summing the costs in these four broad categories for the experimental and control groups, statistically adjusted to compensate for the different sizes of the groups. The group with the lowest costs was then judged to reflect the most cost-effective mode of operation (i.e., either with or without a pretrial release program).

The programs that were most cost-effective for the criminal justice system were not necessarily those that showed the greatest impact on defendant outcomes. Indeed, the most cost-effective program operated at the Tucson felony level, where program impact on defendant outcomes had been quite limited (of all outcomes studied, only the equity of release by ethnicity had shown positive program impact). Nevertheless, total experimental group costs were only 38 percent of control group costs for the Tucson felony experiment.

Both the Beaumont-Port Arthur program and Baltimore's changed release criteria (first random assignment analysis) were also cost-effective to the criminal justice system. Baltimore's post-release followup (second random assignment analysis) as well as the Lincoln and Tucson misdemeanor programs showed higher costs for the experimental than control groups.

In general the more cost-effective programs processed felony level defendants (though not necessarily exclusively) and had minimal followup of defendants after release. Neither of the programs that processed only misdemeanor defendants was cost-effective, based on the analysis conducted. Nor was Baltimore's more intensive supervision cost-effective, when compared with the minimal supervision received by the control group.

When the relative contributions of various types of costs to total costs were considered, failure to appear was the least costly category in most sites. Indeed, in one jurisdiction (Beaumont-Port Arthur) failure to appear generated revenue, because of the amount of bond forfeitures collected in that site.

Although actual failure to appear (FTA) costs were relatively low, potential FTA costs are quite high. In the jurisdictions studied, prosecution for FTA was rare; during the experiments it occurred only in Baltimore City. Had prosecution of FTA been more common, costs would have been substantially higher, because of the high costs for court processing of the charge. Moreover, if harsh sentences were imposed for FTA, costs would be even higher. Thus, the decision not to prosecute routinely for failure to appear seems a cost-effective one for the criminal justice system.

Pretrial arrest costs were relatively high, especially for the experiments that included felony-level defendants (Tucson, Beaumont-Port Arthur and Baltimore City). These costs were largely due to the sentencing costs for defendants convicted of pretrial arrests and, in particular, to the costs of incarceration for those persons sentenced to prison.

Another aspect of the analysis of sentencing costs is the way those costs differed for the experimental versus control groups in the three experiments that included felony defendants. In each case, the sentences were much more severe in the control group than in the experimental group. In particular, the extent of incarceration in the control group was much greater than in the experimental group. This suggests that the operations of the pretrial release programs may have led to less harsh sentences for felony defendants convicted of pretrial arrests, even though program operations did not affect the overall rate of rearrests.

The manner in which this impact occurred is not known. Programs may have served as advocates for rearrested defendants and helped ameliorate sentences by providing information about defendants' circumstances that would not otherwise have been available. It is also possible that the mere existence of more complete information about defendants had a positive effect on sentences, without the need for programs to serve as advocates. Alternatively, programs may have affected defendant behavior so that less serious crimes were committed, with the difference in sentencing severity reflecting this fact. Finally, it is possible that these findings, based on a relatively small number of sentenced defendants in the sites studied, would not be replicated if additional jurisdictions were analyzed or if a larger number of defendants were studied in the same sites. Thus, the findings suggest a topic that may deserve greater attention in future studies, covering larger numbers of defendants.

As noted earlier, the two programs that handled only misdemeanors were not found cost-effective. This occurred for two major reasons. First, most defendants charged with misdemeanors were released relatively quickly in both the experimental and control groups. Thus, there was little opportunity for program operations to generate savings in detention costs. Second, the rearrested defendants were charged with relatively minor offenses for which punishments were not severe. Thus, programs had little potential for accruing savings in pretrial arrest and sentencing costs. Because failure to appear costs were universally low, the misdemeanor-level programs were unable to generate savings that could offset their costs of operations.20/

In addition to the lack of cost-effectiveness for misdemeanor programs, the post-release followup activities studied were not found cost-effective. Neither of the two experimental tests of program followup showed a positive effect on costs. In both cases the activities were relatively expensive to implement and did not lead to reduced costs in other categories. The combined effect of a misdemeanor-level program and post-release followup probably accounts for the very high costs found for the Tucson misdemeanor program vis-a-vis the control group.

In conclusion, the limitations of the cost-effectiveness analysis should be emphasized again. The cost estimates were often imprecise, because of the difficulties of obtaining more suitable data. Moreover, the analysis was conducted from the viewpoint of the criminal justice system. Had a different basis been selected for the analysis, other costs and savings might reasonably have been included, such as costs

incurred by defendants (e.g., bail payments, job losses or family strife caused by detention), benefits accruing to released defendants (e.g., improved ability to assist in the preparation of a defense), increased welfare costs, or pretrial crime costs not borne by the criminal justice system (e.g., costs to the victim, increased "fear of crime" by the general public or costs of private sector security expenses stemming from such increased fear).

Finally, it should be stressed that costs, however they are defined and calculated, are not the only considerations appropriate for analysis of pretrial release programs. Cost-effectiveness analysis should be viewed merely as providing an additional perspective about program impact on the release process and its outcomes.

PART III. PRETRIAL RELEASE WITHOUT FORMAL PROGRAMS

CHAPTER X. PRETRIAL RELEASE WITHOUT FORMAL PROGRAMS

Most of the analyses of the National Evaluation of Pretrial Release are based on jurisdictions where formal programs exist. However, a limited study of areas without such programs was undertaken, to gain insight about pretrial release practices under those circumstances.

Starting in the early 1960's, many jurisdictions made increasing use of own recognizance release and other types of nonfinancial release. This development was often accompanied by the establishment of pretrial release programs. However, given the widespread adoption of changed release practices within a relatively short time period, certain sites might have endorsed these changes without pretrial release programs. Indeed, Wayne Thomas' study of 20 jurisdictions found that in 1971 some cities without programs had nonfinancial release rates comparable to those of cities with programs. 21/

Analysis of a Site That Never Had a Program

To develop increased understanding about release practices in the absence of any program influences, a case study was conducted of one jurisdiction that had never had a program. Richmond, Virginia, was selected for analysis partly because it was one of the few large cities that had never had a program. 22/ Also, it is near two large cities (Baltimore City, Md., and Washington, D.C.) included in other parts of the National Evaluation of Pretrial Release. Thus, the results of the Richmond analysis could be compared with findings for nearby jurisdictions having programs.

The analysis of pretrial release in Richmond was conducted in a manner similar to that for the eight sites discussed in Part I. For a random sample of about 400 defendants arrested between July 1976 and June 1977, data were collected on background characteristics, current charge, type of release, court appearance, pretrial criminality and case disposition. Additionally, as in the case of the other sites studied, local criminal justice system officials were interviewed about pretrial release practices.

Richmond had a lower rate of release (59 percent) and a lower rate of nonfinancial release (33 percent, including unsecured bond) than any of the eight sites. It also experienced higher rates of court appearance (97 percent) and lower rates of pretrial arrest (2 percent).

Richmond had much less extensive data than the other sites. It was particularly difficult to obtain comprehensive information on arrests. Thus, some of the apparent outcomes differences between Richmond and the eight sites may in fact be due to differences in record-keeping.

In Richmond defendants with more serious charges and longer prior records were more likely to be detained. Also, if released, defendants with more serious charges were more likely to be released on bond. The one community ties variable for which reasonably complete data were available (local residence status) did not affect release outcomes.

The Richmond findings were compared with those of Washington, D.C., and Baltimore City, Maryland, two nearby jurisdictions included in the eight-site analysis, and with those of Louisville, Kentucky, the jurisdiction from the eight-site analysis with defendant characteristics most similar to Richmond's. More serious charges and longer prior records were found importantly related to detention and financial release in those three jurisdictions, but community ties factors also affected release outcomes within them (e.g., defendants who were not local residents were more likely to be detained). This suggests that "reform" jurisdictions may not so much have replaced their reliance on traditional considerations (i.e., charge and prior record) as they have expanded the range of factors considered.

Analysis of Sites Where Programs Ended

Some analysts have suggested that long-term program operations are unnecessary. Rather, programs may be required, if at all, only to acclimate judges to various release possibilities. After judges have reduced their reliance on money bond and begun using a wider range of release alternatives, they might continue this behavior, regardless of whether a pretrial release program exists. On the other hand, others have argued that if a program were disbanded, judges might revert to the release practices that prevailed before the program began. 23/

This topic was studied through a brief analysis of release practices before, during and after program operations. Eighteen programs were identified that had ceased operations at some point. Information was obtained on twelve of them: Cincinnati and Cleveland, Ohio; Tucson, Arizona; Oakland and West Covina, California; Bucks County, Pennsylvania; Milwaukee, Wisconsin; Las Vegas, Nevada; Chicago, Illinois; New Haven, Connecticut; Manhattan, Kansas; and Lake County, Indiana. Most of this information was acquired from telephone interviews with persons who had been involved with the program (e.g., former directors or judges) and from available program reports and previous research analyses. Additionally, site visits were made to two jurisdictions (West Covina and Tucson) to obtain more detailed information.

The nature, cost and extensiveness of these programs varied widely. There were no special operating characteristics to distinguish them from programs that continued to function.

In terms of release rates, four sites had data for time periods other than those when programs operated. These very limited data indicated that—for whatever reason—release rates increased after programs started

and continued to increase while they were in operation. After the programs ended, release rates stabilized at the program level; although the rates did not increase further, they also did not decline.

Court appearance rates were available for only three of the programs studied, and in only one site did these data cover more than one time period. In that site (Tucson) court appearance rates were lower for own recognizance releases after the program's demise than during the program's existence. In all three sites (Chicago and Cleveland were the other two) data were available to compare court appearance rates for defendants processed by the program with those of other defendants. In each site the court appearance rate was higher for the program defendants; however, data were not available on other characteristics that might have affected the groups' court appearance rates.

No site studied had pretrial arrest data across time periods. Only two programs had information on rearrests for defendants processed by the program versus other defendants. In one case (Chicago), there was no significant difference between the pretrial arrest rates. In the other site (Cleveland), the program group had a lower rearrest rate, but the program was so limited in scope that this result may have been due to "creaming" the "safe" defendants.

Because of the very limited information available about defunct programs, one was selected for detailed analysis: Milwaukee, Wisconsin. The program there had operated as part of the Sheriff's Department and processed felony defendants.

The Milwaukee analysis of defendant outcomes was patterned after the analyses of the eight sites discussed in Part I. Random samples of approximately 150 felony defendants each were selected for three one-year time periods: calendar year 1972, before the program began; calendar year 1975, roughly the peak of the program's period of operations; and July 1977-June 1978, after the program ended. The data collected for individual defendants were essentially the same as those collected for the eight-site analyses discussed earlier.

There was no significant change in the rate of release when the "before program" period was compared with the "during program" period or when the "during program" period was compared with the "after program" period. However, there was a significant decline in the overall release rate when the pre-program and post-program periods were compared. This indicates that the jurisdiction experienced declining release rates over time but suggests that this did not result from program operations.

The major difference across the various time periods was in the type of release. Because Milwaukee did not use own recognizance release for felony defendants, unsecured bond and deposit bond were considered the least restrictive types of release. The data indicated that significantly fewer defendants were released in these ways while the program operated than either before or after its existence. Over the entire time period studied, however, there was a significant increase in release on unsecured and deposit bond.

There were no significant differences in the court appearance or pretrial criminality rates across the time periods. Thus, the periods when more defendants were released and/or when they were released on less severe conditions were not periods when defendants' post-release misconduct rates increased. This suggests, as have the other analyses, that higher release rates can be attained with no offsetting increases in failure to appear or pretrial criminality rates.

Program impact over time was also considered for the Tucson, Arizona, misdemeanor program. This analysis was based on the cross-sectional data (discussed in Part I), which covered a time period spanning the demise of the program, and the experimental data (discussed in Part II), obtained when the program was resumed. Thus, program impact during and after the operation of an initial misdemeanor program could be studied, as well as the subsequent impact of a revised program.

Release rates were significantly higher for the latest time period (the "new program" period) than for the earliest period (the "old program" period). The data suggest that this was due more to a trend in the jurisdiction toward higher release rates for defendants charged with misdemeanors than to the impact of the program (e.g., release rates did not decline sharply during the "no program" period).

The only other significant difference across time periods was the sharply lower pretrial criminality rate in the "new program" period, as compared to the "no program" period. However, it was difficult to attribute this to the operations of the program, because the program made little effort to affect pretrial criminality rates. Moreover, the experimental analysis found no significant differences in pretrial criminality rates for the "new program" group, as compared with a randomly selected control group. Thus, this difference, too, seemed to relect a change in the jurisdiction, rather than program impact.

Concluding Remarks about Sites without Programs

The analyses of sites without programs were hindered by a lack of data. When jurisdictions were studied for time periods before, during and after program operations, much more information was available when programs functioned than when they did not. Additionally, data were much less complete for Richmond, where a program had never existed, than for the various program jurisdictions that had been studied. Thus, if more complete information about defendants' backgrounds is desirable, programs certainly help meet this objective.

PART IV. POLICY ANALYSIS

CHAPTER XI. POLICY ANALYSIS

Court Appearance

The overwhelming majority (almost 90 percent) of released defendants made every court appearance required of them. Additionally, of those who failed to appear, many returned to court of their own volition, and others came back as a result of an arrest for another charge. Fugitives (that is, persons who had not returned to court by the time of the study's data collection) comprised 2 percent of released defendants.

In most cases failure to appear (FTA) was not very costly to the criminal justice system. Relatively little court time was lost due to FTA, and most defendants were returned to court through low-cost mechanisms.

Widespread prosecution of FTA as a separate charge could be quite costly to the criminal justice system, due to the high court costs that would be incurred. Thus, rather than routinely prosecuting FTA, most jurisdictions have accepted as tolerable the levels of court disruption and inconvenience it causes. This is probably a wise course of action, given that most failures to appear do not seem to be willful attempts to abscond.

At the same time, there is a small group of defendants who are successfully evading justice. More systematic efforts are needed to identify and track these fugitives, so that they can be returned to court.

Most of the jurisdictions studied responded in the same way to all failures to appear. Typically, a bench warrant was issued by the court, and law enforcement officials attempted to serve these warrants as time and resources permitted. An alternate approach of targeting followup efforts on defendants who had not returned to court within a specified time period (for example, 90 days)—especially if their charges were serious ones—could help reduce the fugitive rate. Such efforts to make it more difficult to evade justice successfully seem particularly needed and more desirable than "tougher" responses to all failures to appear, as has sometimes been proposed.

► Recommendation 1: Courts should implement systematic followup procedures to identify fugitives (i.e., defendants who have not returned to court after a certain period, such as 90 days), and law enforcement agencies should make special efforts to apprehend these individuals. No person should be permitted to evade justice without efforts by the jurisdiction to return the individual to court.

► Recommendation 2: Routine prosecution for failure to appear, or similar actions to punish all defendants who fail to appear, should not be undertaken. Many defendants who fail to appear do not act as if they are willfully trying to evade justice; indeed,

they often return to court of their own volition within a short time. Widespread prosecution for failure to appear in such cases would be very costly to the criminal justice system and unlikely to produce significant benefits.

Pretrial Criminality

Perhaps no topic concerning pretrial release is as controversial as that of pretrial criminality. In recent years a variety of alternatives have been suggested for reducing "crime on bail"; these include speedier trials, preventive detention of "dangerous" defendants and harsher punishment for "career criminals."

A first consideration regarding pretrial criminality is the magnitude of the problem. This is difficult to assess accurately, because of data limitations. In this study pretrial arrests were used as the primary measure of pretrial criminality, even though all crimes do not result in arrests and all arrests do not reflect guilt.

Approximately one out of six defendants in the eight-site sample were rearrested during the pretrial period. Almost one-third of these persons were rearrested more than once, some as many as four times, before their original cases were settled.

Rearrests were for somewhat less serious charges than the original arrests. Nevertheless, almost two-fifths of the rearrests were for Part I crimes, the crimes considered most serious by the FBI. By type of crime, one-fifth of the rearrested defendants were charged with crimes against persons and almost one-third with economic crimes. Thus, although the extent of pretrial criminality cannot be measured precisely, there is sufficient evidence that the problem's magnitude merits concern.

Proposed alternatives for reducing pretrial arrests can be assessed with the data from the National Evaluation of Pretrial Release. One widely supported proposal is to hold speedier trials. However, this is unlikely to cause major reductions in pretrial arrest rates unless trials are held much more quickly than the 60- to 90- day periods commonly discussed. Two-thirds of all pretrial arrests occurred within 60 days of the original arrest; indeed, almost one-half the pretrial arrests occurred within 30 days.

However, the pretrial arrest rate reductions that could be achieved through speedier trials would be greater for more serious (Part I) crimes and crimes against persons. For example, more than half the pretrial arrests for robbery could have been avoided if trials had been held within eight weeks of release. If trials had occurred within four weeks, more than two-thirds of the pretrial arrests for robbery and more than three-fourths of the burglary rearrests could have been avoided.

Because of wide variation across sites in terms of the speed with which rearrests occurred, the potential impact of speedier trials on pretrial arrest rates is much greater for some jurisdictions than others. In Washington, D.C., almost half of all pretrial arrests occurred more

than eight weeks after release. In contrast, the comparable percentage for Santa Cruz County was 16 percent. Consequently, the impact of speedy trial requirements on pretrial arrest rates will differ across jurisdictions, as well as by the specific time periods imposed.

Another approach that has been recommended for reducing pretrial arrest rates is to permit the preventive detention of defendants who are considered likely to commit crimes during the pretrial release period. Unfortunately, no consistently reliable way of accurately identifying such defendants has yet been developed. Past studies have not been notably successful in their ability to predict pretrial arrests. Nor are the findings from the National Evaluation of Pretrial Release more promising.

The best prediction technique for pretrial arrests developed as part of the National Evaluation of Pretrial Release would, if followed for the sample of released defendants studied, have reduced the pretrial arrest rate by one-sixth. However, to achieve this reduction would have required the detention of almost as many defendants who were not rearrested pretrial as persons who were. Thus, there would have been a substantial (30 percent) increase in detention, with its attendant costs for both the criminal justice system and defendants, but only a modest decline in the pretrial arrest rate.

Moreover, it is highly possible that even the modest reduction estimated for the pretrial arrest rate exceeds the likely reduction that would occur if the prediction approach were used in the future. Typically, prediction techniques derived for one defendant sample over one time period are less effective when applied to other samples or other time periods. Additionally, predictions of pretrial arrests are based only on data for released defendants. Thus, there is no way to judge the accuracy of such predictions, if they were to be applied to detained defendants as well.

Many opponents of preventive detention base their opposition partly on the inability to develop accurate predictors of pretrial arrest. However, one should remember that the same prediction difficulties apply to development of accurate indicators of future failure to appear.

Preventive detention has also been opposed because it permits persons to be jailed for actions they might take but perhaps never would take. Again, the same criticism applies to detention because of flight risk.

Finally, preventive detention has been opposed as unconstitutional and as an unwarranted break with the legal tradition that bases pretrial detention on flight considerations alone. This issue may be at least partly resolved through a court case that involves the constitutionality of the preventive detention statute for the District of Columbia. Although that statute was upheld by the D.C. Court of Appeals in a 1981 decision,[24] the case is expected to be reviewed by the Supreme Court.

Advocates of preventive detention often view it as a way of reducing pretrial arrest rates. However, as discussed earlier, estimated crime reductions based on the prediction analyses of the National Evaluation of Pretrial Release would appear to be rather modest. Additionally, if preventive detention were permitted, judges might merely substitute its use

for the setting of high money bond, which often results in detention. If such substitution were widespread, preventive detention would have little effect on either pretrial arrest rates or detention.25/ The basis of detention would change, but not its extent.

It is quite likely that if preventive detention were more widely adopted, the impact would be much less than either the advocates or the opponents of such action anticipate. Pretrial arrest rates are unlikely to be reduced drastically. Also, the extent to which detention might increase is questionable, for several reasons, including: sub rosa preventive detention may exist now, through the setting of high money bail; detention facilities are often overcrowded, which produces reluctance to increase jail populations; detention itself is costly; and preventive detention legislation may include procedural safeguards to limit the detention that can occur. Thus, the magnitude of the controversy over preventive detention, and the intensity of the debate about it, may far exceed its potential impact on either pretrial arrest or detention rates.

Although preventive detention is unlikely to affect pretrial arrest rates significantly, its authorization would permit judges to insure the detention until trial of certain defendants. At present this option is rarely available to judges: although the setting of extremely high money bail often results in detention until trial, the possibility of release still exists. Consequently, some persons have advocated the authorization of preventive detention as a means of expanding the range of choices available to judges making release decisions. In this case preventive detention would presumably be used only rarely and thus would not be viewed as a crime control measure.

Further analysis of the likely effect of preventive detention, especially for "dangerous" defendants, is an important area for future research. Because several States permit consideration of "dangerousness" when release decisions are made, the impact of such legislation could be studied. Of particular interest would be the extent to which the dangerousness provisions were used, the conditions under which they were used, changes in detention and pretrial arrest rates, and judicial opinions about the efficacy and utility of the legislation.

In addition to speedy trials and preventive detention, a variety of policies have been proposed to reduce pretrial criminality. These include:

- imposing consecutive, rather than concurrent, sentences for pretrial crimes, so that no one can commit "two (or more) crimes for the price of one";
- providing supervision during the pretrial period for defendants thought to pose high rearrest risks; and
- changing the court calendaring of cases, so that cases involving defendants considered high rearrest risks would be tried relatively quickly.

Another possibility for reducing pretrial crime is to reduce the extent of multiple rearrests during the pretrial period. If all rearrested

defendants in the eight-site sample had been rearrested only once, rather than an average of 1.4 times each, total pretrial arrests would have declined by 29 percent. One proposal for reducing multiple pretrial arrests is to revoke a defendant's release at the time of the first rearrest. This could be implemented (subject to certain procedural limitations, such as a finding of probable cause) by including "no rearrest" as an initial release condition and revoking the release for violation of that condition. Various ways that multiple rearrests might be reduced are now being explored in a research study sponsored by the National Institute of Justice.26/ The impact of such alternatives as improved mechanisms for providing rearrest information to releasing magistrates and harsher court responses to pretrial arrests will be considered.

In summary, in terms of reducing pretrial arrest rates, the findings of the National Evaluation of Pretrial Release suggest that speedier trials would have a more substantial impact than could be attained by application of rearrest prediction criteria to all defendants. While use of the best predictors of future criminality would have reduced the pretrial arrest rate by 16 percent, trials within 12 weeks of arrest would have resulted in a 20 percent decrease and trials within 8 weeks, a 34 percent decline. Even if trials had been held within four weeks of arrest, however, the pretrial arrest rate would have declined by only slightly more than half. Forty-five percent of the rearrested defendants were rearrested within four weeks. Indeed, one-sixth of all rearrested defendants were rearrested within one week.

The findings of the National Evaluation of Pretrial Release and other studies suggest that major reductions in pretrial arrest rates will require several types of actions. No single solution—whether preventive detention, speedier trials, elimination of multiple arrests or another approach—is likely by itself to reduce pretrial arrest rates dramatically.

Moreover, reductions in pretrial arrests may not result in reductions in total arrests. Whether this occurs depends on the dispositions of the original cases and the sentences imposed upon guilty defendants. Only about one-half of all arrests studied resulted in findings of guilt. Also, many guilty defendants were given suspended sentences, placed on probation or otherwise permitted to remain in the community. The extent to which such persons may continue to engage in criminality is illustrated by the fact that 16 percent of the defendants in the eight-site sample were on probation or parole at the time of the arrest selected for study.

➤Recommendation 3: Jurisdictions should continue their efforts to promote speedier trials. However, trials will have to occur much more rapidly than has commonly been proposed, if pretrial arrest rates are to be reduced substantially. Trials within 60 days would have decreased the pretrial arrest rate in the sites studied by an estimated one-third, while trials within 30 days would have resulted in about a 55 percent decline.

➤Recommendation 4: Action should be taken to reduce the extent to which defendants are rearrested repeatedly during the pretrial period. Such efforts might include improvements in the mechanisms for identifying defendants with pending charges,

so that this information could be brought to the court's attention; provisions to accelerate the processing of cases for defendants with pretrial arrests; and revocation of release for defendants rearrested during the pretrial period.

➤Recommendation 5: Jurisdictions should adopt a multi-faceted approach to the reduction of pretrial criminality. Of the various policy changes now under consideration, no single proposal is likely by itself to reduce pretrial criminality significantly. In addition to speedier trials and efforts to reduce multiple pretrial arrests, jurisdictions should consider consecutive, rather than concurrent, sentences for persons convicted of pretrial crimes and changed court calendaring of cases, so that cases involving defendants thought to pose high rearrest risks would be tried relatively quickly.

➤Recommendation 6: Because of the great interest in preventive detention, especially for "dangerous" defendants, the experiences of jurisdictions that have authorized preventive detention should be studied. Of particular importance is the extent to which the "dangerousness" provisions have been used and the resulting impact on pretrial arrest and detention rates.

Release and Detention

Available evidence strongly suggests that more defendants could be released pending trial and that rates of failure to appear and pretrial criminality would not increase substantially, if at all. This statement is supported by a number of findings. For example, the National Evaluation of Pretrial Release found no relationship between rates of release and rates of failure to appear or pretrial arrest for the individual sites studied. The jurisdictions with the highest release rates did not have the highest rates of failure to appear or pretrial arrest. Nor did the sites with the lowest release rates consistently have the lowest failure to appear and pretrial arrest rates. Moreover, a brief analysis of jurisdictions where pretrial release programs had ended also found no direct correspondence between release rates and failure to appear or pretrial arrest rates.

In addition, in the experimental analysis of four jurisdictions, three sites had higher release rates for the experimental than for the control group. Despite higher release rates, the experimental group in these sites had failure to appear and pretrial arrest rates that were no different from those of the control group.

These various findings suggest that release rates can be increased without offsetting increases in failure to appear and pretrial arrest rates. Consequently, it may be possible to alleviate jail overcrowding, reduce jail costs and extend pretrial liberty to additional defendants without significantly increasing the disruption of court processing (as reflected in failure to appear rates) or decreasing the level of community safety (as shown by pretrial arrest rates).

In terms of types of release, large differences were found across sites. For example, the two California jurisdictions studied made the

most extensive use of citation release, both field release by the arresting officer and stationhouse release shortly after booking. Of the other sites in the study, only Tucson and Washington, D.C., used citation release more than occasionally.

Because citation release occurs quickly, its expanded use has great potential for reductions in detention. Citation release is particularly appropriate for relatively minor offenses, especially charges that are rarely punished by incarceration upon conviction. Feeney has suggested that many persons charged with petty theft, shoplifting, simple assault and disorderly conduct could be released on citations, many through field release. He has also proposed stationhouse release for defendants charged with drunkenness.27/

In most cases detained defendants had a release option available to them. Their detention resulted from an inability to raise the amount of bond set, rather than from a judicial determination that they should not be released. Many of the detainees were charged with relatively minor offenses: more than one-third of all defendants detained until trial were charged with crimes against public morality (such as prostitution and drunkenness) or crimes against public order (such as disorderly conduct and driving while intoxicated).

Some defendants initially detained eventually made bail and secured release. In the eight-site defendant sample three percent of the persons released before trial were jailed for 30 days or more before they secured release. An additional four percent of the released defendants were detained between seven and thirty days. Because such defendants were ultimately considered safe release risks, one may question whether their detention was necessary. If such detention were necessary immediately after arrest, one may question whether these defendants should have been permitted to secure release at a later date.

The ambiguous relationship between financial release decisions and ultimate release outcomes has caused some persons to recommend abolishing the money bond system. Then, all detention would be specifically ordered by the court and could be ended only by court action. This, for example, is the position expressed by the National Association of Pretrial Services Agencies (NAPSA) in its "standards" for pretrial release.28/

The abolition of money bond might, however, present difficulties that have not been fully explored. For example, the use of bail schedules provides a way for many defendants to secure release soon after arrest. Although such persons might be released without the need for any payment under a system without money bond, the defendants involved might prefer a simple payment and immediate release to a possible delay associated with screening for nonfinancial release. There may also be little advantage to the criminal justice system in requiring a more elaborate determination of release eligibility, especially if the charges are relatively minor ones for which release is usually authorized.

The great durability of the money bond system, particularly in view of the many changes in pretrial release practices within the past 20 years, suggests that the use of money bond may provide advantages that have not been fully considered. It is also possible, of course, that money bond

continues to be used mainly from the force of habit. The role of money bond in the pretrial process is currently receiving further analysis in a study focused on the role of bail bondsmen.29/ This research should provide additional insight not only about the role of money bond but also about the implications for the criminal justice system, if it were to be eliminated.

Rather than abolishing money bail, some persons have suggested replacing surety bond with deposit bond. The National Evaluation of Pretrial Release included only one site where deposit bond was used extensively. Criminal justice system officials in that site were very satisfied with its adoption and implementation. However, such an assessment of one site is not an adequate basis for evaluating the general utility of deposit bond.

A study is now in progress of the impact of recent California legislation authorizing deposit bond for persons charged with misdemeanors.30/ As this study is completed, it should provide considerable information about the impact of deposit bond within California.

Another aspect of the consideration of the bond system concerns the reasons for setting high money bond, which is likely to result in detention. Although risk of flight is the sole legal basis for setting release conditions for most defendants in most jurisdictions, high money bail may sometimes be required because of concerns about the risk of danger to the community.31/ Such use of high money bail could achieve sub rosa preventive detention. This has apparently occurred even in jurisdictions where preventive detention has been authorized by law. For example, one explanation for the relatively low use of the preventive detention statute in Washington, D.C., is that detention can be achieved more easily through high money bail than the more cumbersome procedures required by the preventive detention legislation.32/

Besides the possible use of detention (and high money bail) because of flight or rearrest concerns, its use has been suggested as reflecting the imposition of pretrial punishment. Although by law punishment cannot be imposed until there has been a finding of guilt, in practice pretrial incarceration (or payment of a "bail fine") may be the only "punishment" received by many persons eventually found guilty. The use of suspended sentences and probation at the time of adjudication makes pretrial incarceration of defendants, in Packer's words, "not only a useful reminder that crime does not pay but also the only such reminder they are likely to get."33/ A related concept is that a "taste of jail," presumably provided as quickly as possible after the alleged offense, will serve as a deterrent to any criminal activities that a defendant might consider in the future.34/

If detention is in fact used for these reasons, this would help explain the lack of a finding of strong relationships between factors associated with release decisions and those associated with failure to appear and rearrest. Strong relationships would not be expected if release decisions were based on other concerns than flight and rearrest risks.

Another topic that deserves mention is the evidence suggesting that the effects of detention may extend beyond the pretrial period. Although the impact of detention on subsequent outcomes was not a topic addressed in the National Evaluation of Pretrial Release, other studies have concluded that detention alone may have an adverse effect on subsequent case dispositions and/or the severity of the sentences imposed on guilty defendants.35/ If so, then decisions made quite early in criminal processing and often quite quickly as well, may have long-lasting effects on defendants.

➤Recommendation 7: Jurisdictions should seek ways to release more defendants pending trial. Available evidence suggests that higher release rates can be achieved without increases in rates of failure to appear or pretrial rearrest.

➤Recommendation 8: Greater use should be made of citation release, both field release by arresting officers and stationhouse release after booking. Such releases are particularly appropriate for defendants charged with relatively minor offenses and/or offenses for which incarceration sentences are rarely imposed. Because law enforcement officers have been traditionally more concerned about apprehending defendants than releasing them, the most effective way to increase citation releases may be to extend release authority to the pretrial release program.

➤Recommendation 9: Further analysis should be conducted to determine the effects, if any, of detention on subsequent case outcomes and sentences.

Detention Facilities

Detained defendants are often housed in overcrowded and/or outmoded facilities. Indeed, defendants jailed pending trial often face living conditions that are much worse than those of prison inmates, who have been convicted of crimes.

Because of the high cost of jail construction, widespread improvements may be unlikely as long as jail populations remain at current levels. Ways of reducing the detention population, through earlier releases and expanded release eligibility, are discussed elsewhere. It is also important to consider whether living conditions for persons who are detained can be improved without the need for costly jail construction or renovation.

There is no inherent reason why jails must always be used as detention facilities. Halfway houses or other facilities with more amenities than jails could also be used, much as the incarceration of sentenced offenders occurs in both maximum and minimum security institutions. Moreover, part-time confinement (e.g., on weekends or at night only) might be used to a greater extent. In addition, such alternatives to jail as "house arrest" might be explored. Defendants who absconded or otherwise violated release conditions might then be confined in traditional jails, along with persons charged with the most serious crimes or posing the highest release risks.

A related consideration concerns persons who do not belong in jail, and whom jail staffs are poorly equipped to handle, but who remain there because more appropriate institutions cannot or will not accept them. For example, jail staff in some sites reported during delivery system interviews that they were forced to house persons with mental health, drug, alcohol and similar problems, because placements at social service agencies could not be arranged. In addition to the lack of adequate treatment for such defendants, their presence made administration of the jail more difficult. The National Evaluation of Pretrial Release did not include an analysis of the extent of this problem. However, the trend toward "deinstitutionalization" of persons with mental health problems and general cutbacks in social services have probably exacerbated the problem in recent years. If so, this suggests that "savings" in social services expenditures may be at least partly offset by increased criminal justice system costs.

➤Recommendation 10: Jurisdictions should examine the possible use of alternative detention facilities, such as halfway houses or similar places with more amenities and lower custodial costs than jails. Jurisdictions should also consider the increased use of part-time confinement (e.g., on weekends or at night only).

➤Recommendation 11: Jurisdictions should study the extent to which jails now house persons with problems that cannot be handled adequately by jail staffs. The possibility of alternative placements for individuals with mental health, drug, alcohol or similar difficulties should be explored.

➤Recommendation 12: The National Institute of Justice (NIJ) should study the utility and feasibility of alternative detention facilities and approaches. Such options as detention in facilities similar to halfway houses, part-time custody and house arrest should be considered. Information on the experiences of other countries with such approaches could be especially useful. In addition, the use of alternative detention facilities and approaches should be considered as a candidate for a "field test" under NIJ's program for testing the efficacy of possible changes in criminal justice processes.

Equity of Release

The multivariate analyses of release outcomes, conducted for the eight-site sample, found no evidence of release biases based on ethnicity, sex or employment status (used as a rough measure of income). However, the experimental analyses undertaken in four jurisdictions suggest that biases based on employment status and ethnicity may exist when pretrial release programs do not operate. In three of the four sites, greater release equity was found for the experimental groups, where full program services were provided, than for the control groups.

Aside from consideration of whether release biases by ethnicity or income exist, and of program impact on equitable release outcomes, there is a broader issue concerning release equity. Specifically, given the inability to isolate accurate predictors of failure to appear or rearrest, one must ask whether any process that results in the detention of certain

defendants and the release of others can be considered "equitable." Certainly, no mechanism for screening defendants has as yet been validated as one that accurately identifies only defendants who would be highly likely to violate release conditions, if they were released.

Another approach to the concept of release equity is based on reducing the disparity in the release conditions set for defendants having similar characteristics. Such an approach makes no claims about the equity of the release criteria themselves but simply seeks to have them applied systematically. Research now underway in Philadelphia is testing the impact of "bail guidelines," similar in concept to "sentencing guidelines," on reducing disparity in the setting of release conditions.36/ When conpleted, this analysis should provide additional insight about the impact of this approach to increasing the equity of the release process.

Program Operations and Impact

Program Impact on Release Outcomes

In four of the five experiments (excluding only the Tucson misdemeanor experiment), program operations had a positive effect on release outcomes: more defendants were released; defendants were released more quickly or on less restrictive conditions; and/or release outcomes by ethnicity and employment status were more equitable for defendants who received full program processing. The multivariate analyses of eight cross-sectional sites also found strong program impact on release outcomes. In particular the type of program recommendation was one of the most important factors affecting release/detention outcomes, nonfinancial/financial release outcomes, and nonfinancial/financial release decisions.

If a program recommended own recognizance release, that was likely to be both the decision of the court and the release outcome of the defendant. On the other hand, if a program recommended financial release, or made no recommendation, financial release options were likely to be set. As a result, 44 percent of the defendants for whom bail was recommended were detained until trial, as were 28 percent of the defendants who received no release recommendation after program interview. In comparison, the detention rate for all arrested defendants was 15 percent.

The effect of the lack of a program recommendation on release outcomes deserves special comment, because programs often describe this as a "neutral" action, one that might be taken due to lack of verification or for a similarly "neutral" reason. However, the lack of a recommendation is evidently not perceived by the court as a neutral action; rather it is strongly associated with the setting of financial release conditions and, due to defendants' inabilities to meet those conditions, with higher-than-average detention rates. (Recall that the multivariate analyses included consideration of other factors that might explain this outcome.)

Given these findings, programs cannot view their operations only as neutral, information-gathering activities. Instead, they must recognize the way the information they provide is used by the court. In particular they must be aware that their recommendations are often likely to determine release outcomes.

➤Recommendation 13: Programs should revise their release recommendation policies, so that specific recommendations are made for all interviewed defendants. Such action is needed because the lack of a recommendation does not have the effect of a "neutral" action; rather, it is highly likely to result in the setting of financial release conditions.

Program Impact on Court Appearance and Rearrest

Although strong program impact on release outcomes was found, there was little evidence that program followup after release affected either failure to appear or pretrial arrest rates. In the multivariate analyses, none of the indicators of program followup (e.g., frequency, mode or length of contact) had a major effect on defendants' post-release outcomes.

Moreover, two experimental tests of program followup after release found no impact from these activities. However, the experimental tests were quite limited in scope. In one the effect of program notification of court dates was tested for defendants charged with misdemeanors; and in the other, the impact of more intensive supervision was compared with the results of minimal supervision.

Other research has had mixed findings concerning the impact of program followup after release. A study of supervised release in the District of Columbia found that supervision affected court appearance rates favorably but had no impact on pretrial criminality.37/ An analysis of supervised defendants in Philadelphia found that they had lower failure to appear rates, and were no more likely to be rearrested pretrial, than defendants in the comparison groups studied.38/ Supervised defendants in Monroe County, New York, were also found to have slightly lower failure to appear rates than unsupervised defendants.39/ Finally, a study of notification of coming court dates for New York City defendants found that notified defendants had lower failure to appear rates than defendants who were not notified.40/

The effect of supervision is now being studied in more detail through a field test, sponsored by the National Institute of Justice in three sites. An evaluation, conducted by the National Council on Crime and Delinquency, should provide a more detailed analysis of the impact of supervision.41/

➤Recommendation 14: Programs should evaluate their post-release followup activities to determine whether these are effective. The admittedly limited analyses conducted in the present study suggest that followup activities in some jurisdictions may have little impact on failure-to-appear or rearrest rates but be quite expensive to conduct.

Cost Effectiveness of Programs

In terms of programs' cost-effectiveness, comprehensive analysis could not be undertaken, due to the relatively poor cost data available and resource constraints precluding detailed development of such data. Nevertheless, the rough cost-effectiveness estimates prepared suggest

several areas that warrant further consideration. First, the most cost-effective programs for the criminal justice system processed felony defendants, though not necessarily exclusively. This suggests that programs that handle only misdemeanor charges might increase their cost-effectiveness by expanding their operations to felony charges as well.

Another important factor affecting cost-effectiveness was the extent of supervision provided: less supervision was more cost-effective, for the sites studied. Although these findings cannot be considered definitive regarding the impact of supervision, they are consistent with other study findings and suggest that programs providing supervision should carefully evaluate its impact and costs.

Finally, incarceration costs—both detention costs and sentences of incarceration for persons convicted of pretrial arrests—contributed significantly to total costs. This suggests that jurisdictions interested in cutting costs should give careful consideration to whether incarceration could be reduced, without incurring offsetting losses in the quality of justice.

➤Recommendation 15: Programs that currently process only defendants charged with misdemeanors should consider expanding their operations to the felony level. Study findings suggest that such expansion would increase a program's cost-effectiveness to the criminal justice system.

Release Recommendation Criteria of Programs

Concerning program's release recommendation criteria, the study findings suggest that these criteria could safely be less restrictive. One experimental analysis tested the impact of less restrictive criteria by extending own recognizance release recommendation eligibility to selected defendants who lacked sufficient points to secure such a recommendation under normal program procedures. This experiment had the strongest impact on release outcomes of any conducted: more defendants were released; more were released nonfinancially; release was secured more quickly; and release outcomes showed greater equity by ethnicity and employment status. Despite the fact that many more defendants were released, the rates of failure to appear and pretrial arrest for the experimental group were no different than those for the control group.

Additional support for less restrictive program criteria is provided by the fact that judges release more defendants on own recognizance than programs recommend. This occurs even though program recommendations are strongly related to release outcomes, as discussed earlier. The net effect of judges' overriding program recommendations is that less restrictive criteria for own recognizance release are used. Because this now occurs, programs may wish to revise their recommendation criteria accordingly.

Concerning specific changes in the criteria used, whether point systems or completely subjective recommendation schemes, the evaluation findings have little to suggest. Because reliable, accurate predictors of failure to appear or rearrest were not found, the analysis did not provide a sound empirical basis for revising program recommendation

criteria or their relative weights. The study findings suggest that existing criteria can be "validated" only to a limited extent: while defendants who meet these criteria can be shown to pose reasonable levels of release risk, defendants who fail to meet the criteria cannot be shown to pose excessive risks. Indeed, the findings of the Baltimore experiment suggest that many of the defendants who fail to meet the current criteria for own recognizance release recommendations could, in fact, safely be released on own recognizance.

Given these findings, the best approach for programs to implement may be to focus on identifying ways to make the criteria less restrictive in general, rather than to expend great effort on revising the specific elements included in the criteria or their relative weights. Thus, cut-off points (whether derived objectively or subjectively) for own recognizance release recommendations could be lowered, or eligibility could be extended to groups of defendants now excluded (e.g., persons lacking full verification of interview information or excluded because of charge, residence or other reasons).

Extensions of release recommendation eligibility could be adopted on a trial basis, with defendant outcomes monitored to determine impact. If, as the evaluation data for Baltimore suggest, increases in own recognizance release recommendations for selected groups of defendants resulted in higher release rates without increases in failure to appear or rearrest rates, the program could continue the changed procedures. If different outcomes occurred, due perhaps to special local circumstances, the changes could be abandoned at the end of the test period.

Such tests of the effect of changed recommendation criteria appear particularly needed in view of the findings from a recent survey conducted by the Pretrial Services Resource Center. When asked whether they had made any changes in their "approach to determining release eligibility since the program began, based on research with program data," almost half of the responding programs stated that they had not. Moreover, about half of the programs using point systems "indicated that they had adapted their procedures from another program, making some changes to fit local needs." Only eleven programs reported that their own research had affected the development of their point systems.42/ Thus, programs' release recommendation procedures were often developed in a somewhat haphazard manner and have continued to be used without reassessment or change. This, too, suggests that programs should consider whether revised procedures might better suit current local needs.

An innovative approach to release recommendation criteria was adopted in Washington, D.C., in July 1980. The program there now rates defendants separately in terms of risk of flight and risk of rearrest and, for higher risk defendants in either category, develops a plan for reducing the level of risk through court-imposed conditions of release. Additionally, a specific release recommendation and/or release plan is developed for each defendant; the agency has abolished its earlier practice of making no recommendation in certain cases. Moreover, no recommendations or release conditions are proposed that involve financial considerations.

The Washington, D.C., experience with the revised recommendation system is being evaluated, with final publication of the results scheduled

for 1983.43/ When completed, this study should provide considerable information about the likely impact of this approach to developing program release recommendations.

A final comment concerning screening defendants and developing release recommendations should be made. While laws governing release practices typically state a presumption of release, actual release practices do not embody such a presumption. Rather, in most cases, defendants are detained until they can be shown to be "good" risks. The arrested population is often subjected to several "screenings," with the defendants who are considered the best release risks released most quickly.

An alternative approach would be to assume that all defendants should be released unless they could be shown to be poor risks. Only the defendants judged to be poor risks at one screening stage would be held until the next stage. Consequently, program recommendation criteria would be designed to identify poor, rather than good, risks. Thus, for example, point systems would be structured to flag poor release risks rather than their current focus on identifying defendants who have sufficiently good backgrounds to qualify for release recommendations.

Although no jurisdiction has replaced screening for safety with screening for risk, a case decided in 1980 by the California Supreme Court requires the prosecution to show that a defendant should not be released on own recognizance and to produce the defendant's criminal record relevant to prior court appearances and assessment of flight risk.44/ The impact of this decision on actual release practices in California has not been evaluated.

➤Recommendation 16: Programs' release recommendation criteria should be less restrictive than at present. Study findings suggest that such changes could increase the number of defendants released pending trial without increases in failure to appear or pretrial rearrest rates.

Resource Allocation

A topic that deserves further study is whether resource allocation can be improved in the pretrial release system by targeting more attention on cases that pose greater risks, with a corresponding lessening of effort expended on lower risk cases.45/ Currently, in many jurisdictions, arrested defendants receive virtually identical pretrial release processing, regardless of the charges against them or the extent of the release risk they are thought to pose. After booking, arrestees are interviewed in the same way by program staff, who employ comparable verification procedures for all defendants prior to presenting release-related information to the court. As a result, many pretrial resources are used for full processing of relatively minor charges and/or defendants who are obviously good release risks. This raises the possibility that screening mechanisms could be introduced—similar to medical "triage," where patients are quickly divided into groups needing immediate care and those whose conditions are less serious—so that "easy cases" would be handled immediately, with "tougher cases" set aside for further processing.

For example, a brief interview form could be used to sort defendants, with low release risks and/or persons with minor charges released immediately, either by law enforcement officials or by the pretrial release program. Thus, only cases of greater risk and/or with more serious charges would come before the court. More time would be available to consider these cases, due to the reduced workload resulting from the prior release of many defendants.

For the defendants who go to court, the program could provide more detailed, verified information, perhaps similar to that commonly provided now for all interviewed defendants. For certain cases, even this information might not be considered adequate by the court. In such cases, rather than making an immediate release decision, it may be preferable to require the program to conduct a more thorough evaluation of the defendant's release risk.

Additionally, after the court's release decisions have been made, programs could conduct followup to identify defendants who did not secure release and consider whether changed release conditions should be recommended to the court. This may be particularly important for defendants who received no recommendation from the program. Study findings suggest that such persons are more likely to have financial release conditons set, and thus to be detained—even though the reason for the lack of a recommendation may have been the lack of verification, rather than information suggesting the defendant would be a poor release risk.

There may also, of course, be other situations in which further consideration of a defendant's circumstances would show that the expected release risk was minimal and, therefore, that reconsideration of release conditions should be recommended. Moreover, the program might be able to develop special supervision procedures or other arrangements for certain defendants that would reduce their risk sufficiently to permit release. Finally, if alternative detention facilities are available (as recommended earlier), the program could determine whether a less restrictive custodial arrangement might be appropriate.

For those defendants who are detained as poor release risks, their cases could be given priority in court calendaring. This procedure is currently used in certain jurisdictions, to minimize the length of pretrial detention. Expedited trials might also be held for released defendants who were considered higher risks than other persons.

Such reallocations of resources might reduce unnecessary hardship on defendants, particularly those whose situations warrant little or no detention. At the same time, changed procedures could permit the criminal justice system to target its efforts more effectively on persons who merit greater attention. Moreover, such targeting of resources could generate savings for local governments, which are often financially strapped now and under considerable pressure to provide services at lower costs.

➤Recommendation 17: Jurisdictions should consider ways that resources might be allocated more effectively, so that greater effort could be expended on more difficult release decisions, by reducing the attention given to easier cases. This would require multi-stage release screening mechanisms, with more detailed information developed at each stage.

➤Recommendation 18: Programs should screen the detained population after release decisions have been made and, when appropriate, recommend reconsideration of release conditions to the court.

➤Recommendation 19: Expedited trials should be considered for detained defendants and for released defendants thought to pose higher-than-average release risks.

➤Recommendation 20: The National Institute of Justice should consider implementing a "field test" of improved resource allocation in the pretrial release process. Participating jurisdictions would test ways that lower risk defendants might be processed more quickly, so that greater attention could be given to other defendants.

Variations by Site

The National Evaluation of Pretrial Release found wide variations by site—in release practices; in defendant outcomes; in factors related to those outcomes; and in the ability to predict release, failure to appear or pretrial arrest successfully. Consequently, while the findings suggest general trends and national patterns, these may not be fully applicable to any individual jurisdiction.

Because of the site variation found, jurisdictions should evaluate their own pretrial release systems. The National Evaluation of Pretrial Release suggests broad areas where changes might be made effectively and also provides methodological approaches to analyses of such areas, but its findings must be reviewed mindful of specific local contexts.[46]

➤Recommendation 21: Due to the great variation across jurisdictions, individual communities and programs should evaluate their pretrial release practices. The impact of less restrictive release criteria and of followup activities after release deserve particular attention. Both topics are especially suited for study through experimental designs and short-term tests of the impact of changed procedures.

➤Recommendation 22: The National Institute of Justice should publish a handbook that could be used by local jurisdictions to assess their pretrial release practices. Much of the developmental work for such a handbook has already been accomplished, as part of the National Evaluation of Pretrial Release and through various materials prepared by the Pretrial Services Resource Center and other organizations. The compilation and organization of such materials into an evaluation handbook could greatly assist any jurisdiction that would like to examine its current pretrial release practices and to consider changes in them.

NOTES

1. Wayne H. Thomas, Jr., et al., National Evaluation Program Phase I Summary Report: Pretrial Release Programs (Washington, D.C.: U.S. Department of Justice, Law Enforcement Assistance Administration, April 1977).

2. Ibid., p. 84.

3. Because Dade County and Santa Clara County lacked complete records for a one-year period, a shorter time span was studied. Additionally, the Dade County sample consisted only of felony defendants, and the Santa Clara County sample excluded defendants released through field citations by arresting officers.

4. Wayne H. Thomas, Jr., Bail Reform in America (Berkeley, Cal.: University of California Press, 1976), pp. 37-38, 65-66. Although Thomas' analysis and the National Evaluation of Pretrial Release covered different sites, both sets of sites could be expected to reflect major national trends reasonably well.

5. Ibid., pp. 39, 72.

6. Warren E. Burger, "Annual Report to the American Bar Association," February 8, 1981.

7. Yankelovich, Skelly and White, Inc., The Public Image of Courts: A National Survey of the General Public, Judges, Lawyers and Community Leaders, Volume I, May 1978, pp. 184-7.

8. As quoted in Thomas, Bail Reform in America, op. cit., p. 230. Also see Sam J. Ervin Jr., "Foreword: Preventive Detention—A Step Backward for Criminal Justice," Harvard Civil Rights—Civil Liberties Law Review, Volume 6 (1971), pp. 297-8, for a critique of the preventive detention legislation for the District of Columbia.

9. Russell V. Stover and John A. Martin, "Results of a Questionnaire Survey Regarding Pretrial Release and Diversion Programs," in National Center for State Courts, Policymakers' Views Regarding Issues in the Operation and Evaluation of Pretrial Release and Diversion Programs: Findings From a Questionnaire Survey (Denver, Colorado: National Center for State Courts, 1975), p. 25.

10. Hank Goldman, Devra Bloom and Carolyn Worrell, The Pretrial Release Program: Working Papers (Washington, D.C.: U.S. Office of Economic Opportunity, 1973).

11. Thomas, National Evaluation Program, op. cit., p. 83.

12. All of the analyses by charge considered only the most serious charge for arrests involving more than one charge.

13. This is probably an understatement of the "true" extent of guilt, because only convictions for the pretrial arrest charges were considered. However, both the original and rearrest charges may have been handled jointly in a plea bargain, resulting in dismissal of the rearrest charge in exchange for a guilty plea on the original charge.

14. A list of 34 eligible charges was developed. These were attempted false pretense; attempted larceny; attempted storehouse breaking; censor board violations, obscenity laws; common assault without resisting arrest, deadly weapon, breaking and entering or malicious destruction as companion charges; daytime burglary without weapon; disorderly conduct; disorderly intoxication (not chronic alcoholic); disturbing the peace; failure to pay (court, food bill, taxi, wages); false pretense, under $500 (per check); false report; gambling; impersonating an officer; indecent exposure; interfering; larceny under $500; larceny after trust under $500; lottery, bookmaking; malicious destruction under $500; pandering; possession of heroin, barbiturates, amphetamines, other drugs; possession of marijuana; prostitution, soliciting, disorderly house (not narcotics violation); receiving stolen goods under $500; rogue and vagabond; shoplifting under $500; storehouse breaking; tampering; telephone misuse (excluding second time against same person); theft under $500; trespassing (not a repeat on same establishment); unlawful acts related to minors; and welfare fraud.

15. This was judged a "real" difference, even though the .06 statistical significance level slightly exceeded the .05 cutoff.

16. For an analysis of the impact of notification with different results, see Marian Gewirtz, "Brooklyn PTSA Notification Experiment," New York City Pretrial Services Agency Research Department, December 1976. In that study, "the 'notified' group consistently showed lower FTA rates than the 'non-notified' group during the ten weeks examined" (p. 3).

17. An evaluation now in progress, sponsored by the National Institute of Justice and conducted by the National Council on Crime and Delinquency, will provide greater insight about the impact of supervision during the pretrial period. The study involves experimental analyses of supervision's impact in Miami, Fla.; Milwaukee, Wisc.; and Portland, Ore. For more information see Test Design: Supervised Pretrial Release (Washington, D.C.: U.S. Department of Justice, National Institute of Justice, February 1980).

18. See the discussion in Thomas, National Evaluation Program, op. cit., p. 46.

19. See Stuart Nagel, Paul Wice and Marian Neef, Too Much or Too Little Policy: The Example of Pretrial Release, Sage Professional Papers in Administrative and Policy Studies, Volume 4, Series No. 03-037 (Beverly Hills and London: Sage Publications, 1977) for an approach to the analysis of the cost-effectiveness of overall release policies.

20. This analysis suggests that misdemeanor-level programs might be cost-effective in jurisdictions where defendants charged with misdemeanors were detained for long periods of time or where they were rearrested for charges carrying severe penalties.

21. Thomas, Bail Reform in America, op. cit., pp. 151-154.

22. Since the time Richmond was studied, a small program at the misdemeanor level has begun.

23. Thomas, et al., National Evaluation Program, op. cit., pp. 34-37.

24. U.S. v. Edwards, decided by the District of Columbia Court of Appeals, May 8, 1981.

25. It is true, of course, that high money bond offers the possibility of release, even if it is never secured, while preventive detention does not.

26. Martin D. Sorin, "Judicial Responses to Multiple Pretrial Arrests," grant awarded by the National Institute of Justice, U.S. Department of Justice, February 26, 1981.

27. Floyd F. Feeney, "Citation in Lieu of Arrest: The New California Law," Vanderbilt Law Review, Volume 25, No. 2 (March 1972), pp. 379-380.

28. National Association of Pretrial Services Agencies, Performance Standards and Goals for Pretrial Release and Diversion: Pretrial Release (Washington, D.C.: National Association of Pretrial Services Agencies, July 1978), pp. 25-28.

29. Mary A. Toborg, "Analysis of the Role of the Bail Bondsman," grant awarded by the National Institute of Justice, U.S. Department of Justice, August 22, 1980.

30. "Bail Reform Evaluation," sponsored by Office of Criminal Justice Planning, State of California. The study is being conducted by the National Council on Crime and Delinquency.

31. William M. Landes, "Legality and Reality: Some Evidence on Criminal Procedure," Journal of Legal Studies, Volume III (June 1974), pp. 325-327, suggests this possibility.

32. Nan C. Bases and William F. McDonald, Preventive Detention in the District of Columbia: The First Ten Months (Washington, D.C.: Georgetown Institute of Criminal Law and Procedure and the Vera Institute of Justice, March 1972).

33. Herbert L. Packer, The Limits of the Criminal Sanction (Stanford, California: Stanford University Press, 1968), p. 212.

34. Forrest Dill, Bail and Bail Reform: A Sociological Study, unpublished Ph.D. Dissertation, University of California at Berkeley, 1972, p. 50.

35. See, for example, Anne Rankin, "Pretrial Detention and Ultimate Freedom: A Statistical Study," New York University Law Review, Volume 39 (1964), pp. 631-655, and John S. Goldkamp, Two Classes of Accused: A Study of Bail and Detention in American Justice (Cambridge, Massachusetts: Ballinger Publishing Company, 1979).

36. John S. Goldkamp, Michael R. Gottfredson and Dewaine L. Gedny, Jr., "Bail After Bail Reform: The Feasibility of a Guidelines Approach," Pretrial Services Annual Journal, Volume III (1980), pp. 3-19.

37. District of Columbia Bail Agency, How Does Pretrial Supervision Affect Pretrial Performance? (Washington, D.C.: D.C. Bail Agency, May 1978).

38. Herbert Miller, et al., Evaluation of Conditional Release Program (Washington, D.C.: Institute for Criminal Law and Procedure, Georgetown University Law Center, 1976).

39. Stochastic Systems Research Corporation, Evaluation of Monroe County Pretrial Release, Inc. (Rochester, N.Y.: Stochastic Systems Research Corporation, 1972).

40. See note 16, above.

41. See note 17, above.

42. Donald E. Pryor and D. Alan Henry, Pretrial Issues, No. 2, "Pretrial Practices: A Preliminary Look at the Data" (Washington, D.C.: Pretrial Services Resource Center, April 1980), pp. 19-20.

43. "Research on Recommendation Policies Affecting the Pretrial Release Decision That Separate the Issues of Appearance and Safety," sponsored by District of Columbia Pretrial Services Agency.

44. Van Atta v. Scott.

45. Although defendants who will fail to appear or be rearrested pending trial cannot be isolated ("predicted") with certainty when release decisions are made, characteristics can be identified that are associated with higher-than-average risks of failure to appear or rearrest. For example, as the following table shows, defendants in the eight-site sample who were on probation, parole or pretrial release for another charge at the time of the arrest selected for study were more than twice as likely to be rearrested pending trial as defendants with no criminal justice involvement when arrested. However, most (72.5%) of the defendants on probation, parole or pretrial release for another charge when arrested were not rearrested pending trial. Thus, one must distinguish between accurate prediction of defendants who would fail to appear or be rearrested—which could not be accomplished—and identification of characteristics associated with higher risks of failure to appear or pretrial arrest.

(note continued on next page)

Criminal Justice Status When Arrested	Rearrested Pretrial		Not Rearrested Pretrial		TOTAL Defendants	
	Number	Percent	Number	Percent	Number	Percent
On probation, parole or pretrial release for another charge	153	27.5%	403	72.5%	556	100.0%
No criminal justice involvement	275	13.3%	1,797	86.7%	2,072	100.0%
TOTAL	428	16.3%	2,200	83.7%	2,628	100.0%

46. A major local evaluation now in progress is "Pretrial Release Services in New York State," sponsored by the Division of Criminal Justice Services, State of New York. The evaluation is being conducted by the Center for Governmental Research, Inc., Rochester, New York.

APPENDIX A

RELEASE, COURT APPEARANCE AND PRETRIAL ARREST OUTCOMES BY SPECIFIC CHARGES

LIST OF TABLES

TABLE A-1
RELEASE OUTCOMES BY SPECIFIC CHARGES
(EIGHT-SITE ANALYSIS)

Note: Columns and rows may not add to totals shown, due to rounding.

Charge	Detained		Released on Financial Conditions		Released on Nonfinancial Conditions		Total Defendants	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
Murder, manslaughter	12	48.0%	8	32.0%	5	20.0%	26	100.0%
Forcible rape	7	29.2	6	25.0	11	45.8	24	100.0
Robbery	45	37.5	39	32.5	36	30.0	120	100.0
Aggravated assault	23	12.8	66	36.7	91	50.6	179	100.0
Burglary	47	20.5	54	23.5	128	55.9	229	100.0
Larceny, theft	61	14.3	97	22.7	270	63.1	429	100.0
Auto theft	23	31.5	17	23.3	33	45.2	73	100.0
Simple assault	22	8.2	73	27.1	174	64.7	269	100.0
Arson	0	0.0	3	100.0	0	0.0	3	100.0
Forgery, counterfeiting	3	7.5	9	22.5	28	70.0	40	100.0
Fraud	8	10.4	18	23.4	51	66.2	76	100.0
Embezzlement	0	0.0	0	0.0	6	100.0	6	100.0
Stolen property	7	13.0	10	18.5	37	68.5	54	100.0
Malicious destruction	5	9.1	6	10.9	44	80.0	54	100.0
Weapons	11	8.2	34	25.4	89	66.4	134	100.0
Prostitution, vice	8	8.2	23	23.7	66	68.0	97	100.0
Sex offenses other than forcible rape or prostitution	4	15.4	4	15.4	18	69.2	26	100.0
Narcotics distribution	4	7.4	20	37.0	30	55.6	55	100.0
Gambling	0	0.0	12	26.7	33	73.3	45	100.0
Offenses against family and children	1	8.3	7	58.3	4	33.3	12	100.0
Driving while intoxicated	23	4.2	112	20.5	411	75.3	545	100.0
Liquor law violations	10	15.6	11	17.2	43	67.2	64	100.0
Drunkenness	67	52.3	32	25.0	29	22.7	128	100.0
Disorderly conduct	39	16.8	34	14.7	159	68.5	232	100.0
Vagrancy	14	37.8	9	24.3	14	37.8	37	100.0
Failure to appear	7	36.8	8	42.1	4	21.1	19	100.0
Narcotics possession	12	10.3	37	31.9	67	57.8	116	100.0
Marijuana distribution	2	6.1	12	36.4	19	57.6	32	100.0
Marijuana possession	2	1.2	30	18.5	130	80.2	163	100.0
Minor local offenses	10	15.2	6	9.1	50	75.8	66	100.0
Violation of parole	6	42.9	7	50.0	1	7.1	14	100.0
Other offenses	28	28.9	20	20.6	49	50.5	97	100.0
TOTAL	510	14.7%	825	23.9%	2,129	61.4%	3,464	100.0%

TABLE A-2
RELEASE CONDITIONS SET BY MAGISTRATES,
BY SPECIFIC CHARGES (EIGHT-SITE ANALYSIS)

Note: Columns and rows may not add to totals shown, due to rounding.

Charge	Financial Release Conditions Set		Nonfinancial Release Conditions Set		Total Defendants with Release Conditions Set	
	Number	Percent	Number	Percent	Number	Percent
Murder, manslaughter	12	70.5%	5	29.5%	15	100.0%
Forcible rape	10	48.3	11	51.7	21	100.0
Robbery	73	67.3	36	32.7	109	100.0
Aggravated assault	73	44.9	89	55.1	162	100.0
Burglary	77	38.7	122	61.3	200	100.0
Larceny, theft	113	34.2	218	65.8	331	100.0
Auto theft	26	45.5	31	54.5	57	100.0
Simple assault	74	31.8	160	68.2	234	100.0
Arson	3	100.0	0	0.0	3	100.0
Forgery, counterfeiting	9	26.9	24	73.1	32	100.0
Fraud	17	25.4	50	74.6	67	100.0
Embezzlement	0	0.0	3	100.0	3	100.0
Stolen property	9	21.5	34	78.5	43	100.0
Malicious destruction	9	18.3	39	81.7	47	100.0
Weapons	25	28.0	64	72.0	89	100.0
Prostitution, vice	23	34.1	45	65.9	68	100.0
Sex offenses other than forcible rape or prostitution	6	27.3	16	72.7	22	100.0
Narcotics distribution	16	34.9	30	65.1	46	100.0
Gambling	4	10.6	33	89.4	37	100.0
Offenses against family and children	4	49.0	4	51.0	8	100.0
Driving while intoxicated	44	21.4	160	78.6	204	100.0
Liquor law violations	8	19.3	32	80.7	40	100.0
Drunkenness	39	62.4	24	37.6	63	100.0
Disorderly conduct	47	24.2	149	75.8	196	100.0
Vagrancy	13	51.4	13	48.6	26	100.0
Failure to appear	14	75.6	4	24.4	18	100.0
Narcotics possession	22	29.4	54	70.6	76	100.0
Marijuana distribution	9	32.8	18	67.2	27	100.0
Marijuana possession	16	12.7	106	87.3	122	100.0
Minor local offenses	6	18.5	28	81.5	35	100.0
Violation of parole	13	91.4	1	8.6	14	100.0
Other offenses	23	36.5	40	63.5	63	100.0
TOTAL	837	33.8%	1,642	66.2%	2,479	100.0%

TABLE A-3
FAILURE TO APPEAR BY SPECIFIC CHARGES
(EIGHT-SITE ANALYSIS)

Note: Columns and rows may not add to totals shown, due to rounding.

Charge	Failed to Appear		Appeared for Trial		Total Released Defendants	
	Number	Percent	Number	Percent	Number	Percent
Murder, manslaughter	1	7.1%	13	92.9%	14	100.0%
Forcible rape	5	28.9	12	71.1	17	100.0
Robbery	9	11.9	66	88.1	75	100.0
Aggravated assault	6	3.8	151	96.2	157	100.0
Burglary	31	17.2	151	82.8	182	100.0
Larceny, theft	66	18.1	301	81.9	368	100.0
Auto theft	9	17.8	41	82.2	50	100.0
Simple assault	31	12.6	216	87.4	247	100.0
Arson	0	0.0	3	100.0	3	100.0
Forgery, counterfeiting	8	20.8	29	79.2	36	100.0
Fraud	14	20.9	54	79.1	68	100.0
Embezzlement	0	0.0	6	100.0	6	100.0
Stolen property	6	11.8	42	88.2	47	100.0
Malicious destruction	6	12.7	43	87.3	49	100.0
Weapons	18	14.6	105	85.4	123	100.0
Prostitution, vice	25	27.9	64	72.1	89	100.0
Sex offenses other than forcible rape or prostitution	2	8.9	20	91.1	22	100.0
Narcotics distribution	1	2.0	49	98.0	50	100.0
Gambling	1	1.1	44	98.9	45	100.0
Offenses against family and children	1	11.1	10	88.9	11	100.0
Driving while intoxicated	64	12.2	459	87.8	523	100.0
Liquor law violations	6	11.4	47	88.6	54	100.0
Drunkenness	5	8.3	56	91.7	61	100.0
Disorderly conduct	15	7.8	177	92.2	192	100.0
Vagrancy	1	6.1	22	93.9	23	100.0
Failure to appear	0	0.0	12	100.0	12	100.0
Narcotics possession	14	13.6	90	86.4	104	100.0
Marijuana distribution	0	0.0	31	100.0	31	100.0
Marijuana possession	13	7.8	148	92.2	161	100.0
Minor local offenses	9	15.3	48	84.7	56	100.0
Violation of parole	0	0.0	9	100.0	9	100.0
Other offenses	8	11.4	62	88.6	70	100.0
TOTAL	374	12.6%	2,580	87.4%	2,954	100.0%

TABLE A-4
PRETRIAL REARREST BY SPECIFIC CHARGES
(EIGHT-SITE ANALYSIS)

Note: Columns and rows may not add to totals shown, due to rounding.

Original Charge	Rearrested Pretrial		Not Rearrested Pretrial		Total Released Defendants	
	Number	Percent	Number	Percent	Number	Percent
Murder, manslaughter	3	21.4%	11	78.6%	14	100.0%
Forcible rape	3	16.0	14	84.0	17	100.0
Robbery	13	16.9	62	83.1	75	100.0
Aggravated assault	26	16.7	131	83.3	157	100.0
Burglary	50	27.6	132	72.4	182	100.0
Larceny, theft	89	24.1	279	75.9	368	100.0
Auto theft	15	29.0	36	71.0	50	100.0
Simple assault	40	16.2	207	83.8	247	100.0
Arson	1	30.4	2	69.6	3	100.0
Forgery, counterfeiting	12	33.8	24	66.2	36	100.0
Fraud	12	18.3	56	81.7	68	100.0
Embezzlement	0	0.0	6	100.0	6	100.0
Stolen property	10	21.9	37	78.1	47	100.0
Malicious destruction	9	18.4	40	81.6	49	100.0
Weapons	18	14.4	105	85.6	123	100.0
Prostitution, vice	31	35.1	58	64.9	89	100.0
Sex offenses other than forcible rape or prostitution	0	0.0	22	100.0	22	100.0
Narcotics distribution	5	9.3	46	90.7	50	100.0
Gambling	6	13.3	39	86.7	45	100.0
Offenses against family and children	0	0.0	11	100.0	11	100.0
Driving while intoxicated	54	10.2	469	89.8	523	100.0
Liquor law violations	4	6.8	50	93.2	54	100.0
Drunkenness	7	11.9	54	88.1	61	100.0
Disorderly conduct	14	7.2	178	92.8	192	100.0
Vagrancy	3	13.6	20	86.4	23	100.0
Failure to appear	0	0.0	12	100.0	12	100.0
Narcotics possession	17	16.5	87	83.5	104	100.0
Marijuana distribution	4	14.1	26	85.9	31	100.0
Marijuana possession	10	6.0	151	94.0	161	100.0
Minor local offenses	2	4.1	54	95.9	56	100.0
Violation of parole	0	0.0	9	100.0	9	100.0
Other offenses	12	17.4	57	82.6	70	100.0
TOTAL	470	16.0%	2,484	84.0%	2,954	100.0%

TABLE A-5
PRETRIAL REARREST CONVICTIONS BY SPECIFIC CHARGES
(EIGHT-SITE ANALYSIS)

Note: Columns and rows may not add to totals shown, due to rounding.

Original Charge	Convicted of Pretrial Rearrest		Not Rearrested or Not Convicted if Rearrested		Total Released Defendants	
	Number	Percent	Number	Percent	Number	Percent
Murder, manslaughter	0	0.0%	14	100.0%	14	100.0%
Forcible rape	1	8.7	15	91.3	17	100.0
Robbery	8	10.3	67	89.7	75	100.0
Aggravated assault	10	6.7	146	93.3	157	100.0
Burglary	26	14.5	156	85.5	182	100.0
Larceny, theft	43	11.6	325	88.4	368	100.0
Auto theft	7	13.7	43	86.3	50	100.0
Simple assault	17	7.0	230	93.0	247	100.0
Arson	1	30.4	2	69.6	3	100.0
Forgery, counterfeiting	6	16.8	30	83.2	36	100.0
Fraud	4	5.7	64	94.3	68	100.0
Embezzlement	0	0.0	6	100.0	6	100.0
Stolen property	4	9.3	43	90.7	47	100.0
Malicious destruction	3	5.9	47	94.1	49	100.0
Weapons	4	3.6	118	96.4	123	100.0
Prostitution, vice	15	16.5	75	83.5	89	100.0
Sex offenses other than forcible rape or prostitution	0	0.0	22	100.0	22	100.0
Narcotics distribution	2	4.7	48	95.3	50	100.0
Gambling	4	9.8	41	90.2	45	100.0
Offenses against family and children	0	0.0	11	100.0	11	100.0
Driving while intoxicated	29	5.6	494	94.4	523	100.0
Liquor law violations	3	6.0	50	94.0	54	100.0
Drunkenness	5	8.2	56	91.8	61	100.0
Disorderly conduct	6	3.2	186	96.8	192	100.0
Vagrancy	3	13.6	20	86.4	23	100.0
Failure to appear	0	0.0	12	100.0	12	100.0
Narcotics possession	11	10.9	93	89.1	104	100.0
Marijuana distribution	4	14.1	26	85.9	31	100.0
Marijuana possession	4	2.4	157	97.6	161	100.0
Minor local offenses	2	4.1	54	95.9	56	100.0
Violation of parole	0	0.0	9	100.0	9	100.0
Other offenses	5	7.1	65	92.9	70	100.0
TOTAL	230	7.8%	2,724	92.2%	2,954	100.0%

APPENDIX B

GLOSSARY

GLOSSARY

Note: For definitions of common criminal justice system terms not listed below, see SEARCH Group, Inc., Dictionary of Criminal Justice Data Terminology, (Washington, D.C.: U.S. Department of Justice, Law Enforcement Assistance Administration, 1976).

Appearance—See "Court Appearance."

Arraignment—The court appearance in which a defendant is informed of the charges which have been brought.

Arrest—Taking a person into custody by authority of law, for the purpose of charging the individual with a criminal offense.

Bail Commissioner—A magistrate authorized to make release decisions. Bail commissioners exist only in certain jurisdictions.

Bench Warrant—A document issued by a judicial officer directing that a person who has failed to obey an order or notice to appear be brought before the court.

Bond Forfeiture—The loss of a bond posted to guarantee a defendant's appearance for required court proceedings. Such forfeiture may be ordered by the court when the defendant fails to appear.

Bond Schedule—A list showing bond amounts for specified offenses. A defendant charged with one of these offenses can secure release by posting the amount indicated (either personally or through a third party, such as a bonding agent).

Bonding Agent—A person or company which posts the bond required for a defendant to secure release. A commercial bonding agent receives a fee from the defendant for this service; the fee is usually about 10% of the face value of the bond. See also "release on bond."

Bondsman—See "Bonding Agent."

Booking—An administrative action, by law enforcement officials, which records an arrest and identifies the person, place, time, arresting authority and reason for the arrest.

Case Disposition—The final judicial decision which terminates a criminal proceeding by a judgment of conviction or acquittal, or a dismissal of the case.

Cash Bond—See "Release on Bond."

Charge—A formal allegation that a specific person has committed a specific offense.

Citation Release—Either field release or stationhouse release.

Community Ties—Links with the local jurisdiction, as shown by length of residence in the area, the number of relatives in the area, extent of family support, nature of employment and similar factors.

Conditional Release—Release of an accused person, who has been taken into custody, upon a promise by the accused to abide by certain rules and to appear in court as required for criminal proceedings. Common conditions of release are to stay away from complaining witnesses, to reside in a certain area and to refrain from unlawful behavior.

Conviction—A judgment of a court that the defendant is guilty of the offense charged.

Court Appearance—The act of coming into a court and submitting to the authority of that court. As used in this study, a "real" scheduled court appearance is one in which (1) the defendant had to appear (i.e., not just the attorney); (2) the defendant did appear; and (3) court proceedings other than simply a postponement occurred.

Crimes Against Persons—As used in this study, crimes against persons consist of murder, non-negligent manslaughter, forcible rape, robbery, aggravated assault, other assaults and arson.

Crimes Against Public Morality—As used in this study, crimes against public morality consist of prostitution, sex offenses other than forcible rape or prostitution, gambling, liquor law violations and drunkenness.

Crimes Against Public Order—As used in this study, crimes against public order consist of weapons offenses, driving while intoxicated, disorderly conduct, vagrancy and minor local offenses.

Deposit Bond—See "Release on Bond."

Detention—Incarceration of an accused person before trial.

Disposition—See "Case Disposition."

Drug Crimes—As used in this study, drug crimes consist of distribution or possession of narcotics or marijuana.

Economic Crimes—As used in this study, economic crimes consist of burglary, larceny, theft, forgery, fraud, embezzlement and stolen property.

Failure-To-Appear (FTA)—The act of not showing up for a required court proceeding. Measures of failure-to-appear are usually either defendant-based (e.g., the number of defendants who miss a court appearance) or appearance-based (e.g., the number of court appearances which are missed). Sometimes estimates of "willful" FTA are also derived; such estimates exclude failures to appear which occur because of forgetfulness, sickness or similar reasons.

Field Release—Release of an accused person by a law enforcement officer at the time of arrest, without taking the accused into custody, upon a promise to appear in court as required for criminal proceedings. Also called "summons" release.

Financial Release—The release of an accused person, when the release is conditioned in some way upon the posting of money or the promise to pay a certain sum if required court appearances are not made. Includes release on bond, unsecured bond and deposit bond.

Forfeiture—See "Bond Forfeiture."

Fugitive—A person who failed to appear for required court proceedings and was not subsequently returned to court (either voluntarily or involuntarily).

Index Crimes—See "UCR Offense Classifications."

Initial Appearance—The first appearance of a defendant in the court which has jurisdiction over the case. Sometimes called a preliminary hearing.

Nonfinancial Release—The release of an accused person, when the release is in no way conditioned upon the posting or promise of money. Includes release on own recognizance, release to third party, conditional release, supervised release, citation release and stationhouse release, as long as these types of release are not coupled with the posting or promise of money.

Offense—An act committed in violation of a law forbidding it.

Own Recognizance (OR)—See "Release on Own Recognizance."

Parole—The status of an offender conditionally released from prison prior to the expiration of the person's sentence and placed under the supervision of a parole agency.

Part I Crimes—See "UCR Offense Classifications."

Part II Crimes—See "UCR Offense Classifications."

Personal Recognizance—See "Release on Own Recognizance."

Plea Bargaining—The exchange of prosecutorial or judicial concessions, commonly a lesser charge, the dismissal of other pending charges, or a recommendation by the prosecutor for a reduced sentence, in return for a plea of guilty.

Point System—A rating scheme in which points are assigned for various factors (e.g., residence, employment, prior record). A defendant must receive a certain minimum score to be eligible for an own recognizance release recommendation from a pretrial release program.

Postponement—The deferring of court proceedings until a later date.

Preliminary Hearing—See "Initial Appearance."

Pretrial Criminality—An unlawful act committed while awaiting trial for another alleged offense. In this study pretrial criminality is measured by (1) arrests for new offenses allegedly committed during the pretrial period and (2) convictions for these arrests. The term "pretrial criminal" is used only for defendants who were convicted as a result of a pretrial arrest.

Pretrial Release Program—An organization which facilitates decisions about the release of defendants during the time between arrest and disposition of the case. Usually, such programs interview defendants about their community ties, verify the information provided, and present this information to a judicial officer who makes the release decision. Programs may also notify released defendants of coming court appearances and offer other follow-up services during the release period.

Preventive Detention—Incarceration of an accused person before trial in order to avert crimes which the person is considered likely to commit if released.

Probable Cause—A set of facts and circumstances which would induce a reasonably intelligent and prudent individual to believe that an accused person had committed a specific crime.

Probation—The conditional freedom granted by a judicial officer to a convicted offender, as long as the person meets certain conditions of behavior.

Release on Bail—See "Release on Bond."

Release on Bond—The release of an accused person who has been taken into custody, upon a promise to pay a certain sum of money or property if the person fails to appear in court as required.

If no money or property is required to be deposited in advance, this is an "unsecured bond" or "unsecured appearance bond."

If money or property is required to be deposited in advance, and is deposited by a third party (such as a bonding agent) rather than by the defendant, this is a "surety bond." A commercial bonding agent charges a fee (usually around 10% of the face value of the bond) for serving as a surety; this fee is not refunded if the accused person appears in court as required. Bonding agents also often require collateral for all or part of the remaining bond amount.

If only a percentage of the bond amount must be deposited in advance, with most of that deposit returned if the accused person appears in court as required, this is a "deposit bond" or "percentage bond" (sometimes called a "cash bond").

Release on Own Recognizance (ROR or OR)—Release of an accused person who has been taken into custody, upon a promise to appear in court as required for criminal proceedings. This study distinguishes between "Program OR," in which the release was recommended by the program, and "Judges' OR," in which the person was released against or in the absence of a program recommendation.

Release to Third Party—Release of an accused person who has been taken into custody to a third party who promises to return the accused to court for criminal proceedings.

Sentence—The penalty imposed by a court upon a convicted person, or the court decision to suspend imposition or execution of the penalty.

Sheriff's OR—See "Stationhouse Release."

Stationhouse Release—Release of an accused person by a law enforcement officer after the booking process has been completed, upon a promise to appear in court as required for criminal proceedings. Sometimes called "Sheriff's OR."

Supervised Release—Release of an accused person, who has been taken into custody, upon a promise by the accused to report periodically to pretrial release program staff, court officials or staff of another organization. The extent of supervision varies widely; "little" supervision is much like conditional release (where a condition is periodic reporting, e.g., through a weekly telephone call) and "extensive" supervision is similar to third party custody.

Surety Bond—See "Release on Bond."

Suspended Sentence—The court decision postponing the execution of a sentence that has been pronounced by the court. When the court suspends a sentence, it retains jurisdiction over the person and may later execute the sentence.

Third Party Custody—See "Release to Third Party."

Trial—The examination of issues of fact and law in a case, beginning when the jury has been selected in a jury trial, or when the first witness is sworn, or the first evidence introduced in a court trial, and concluding when a verdict is reached or the case is dismissed.

UCR Offense Classifications—Crime categories used in the Federal Bureau of Investigation's Uniform Crime Reporting program. Part I Offenses are those crimes which are the most likely to be reported, which occur with sufficient frequency to provide an adequate basis for comparison, and which are serious crimes by nature or volume. Part I offenses are criminal homicide, forcible rape, robbery, aggravated assault, burglary, larceny-theft and motor vehicle theft. Index crimes consist of all Part I crimes except negligent manslaughter (a type of criminal homicide). Part II Offenses are those crimes that do not meet the Part I criteria of seriousness or frequency.

Unsecured Appearance Bond—See "Release on Bond."

APPENDIX C

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