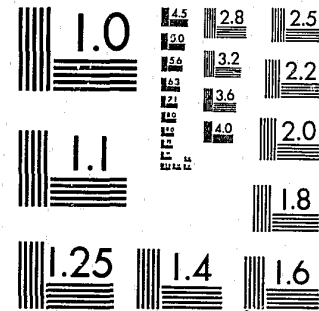


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PRETRIAL RELEASE: A NATIONAL EVALUATION
OF PRACTICES AND OUTCOMES

VOLUME II
The Impact of Pretrial Release Programs:
A Study of Four Jurisdictions

August 1981

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INTRODUCTORY NOTE

Pretrial Release: A National Evaluation of Practices and Outcomes presents the results of a national study. An *Introduction* provides a brief history of pretrial release practices and describes the overall evaluation. The detailed findings, conclusions and recommendations of the study appear in three volumes:

- *Release Practices and Outcomes: An Analysis of Eight Sites* analyzes the ways that defendants secure release pending trial as well as the extent and correlates of pretrial criminality and failure-to-appear.
- *The Impact of Pretrial Release Programs: A Study of Four Jurisdictions* examines the extent to which program activities result in different release outcomes or changed defendant behavior during the pretrial period.
- *Pretrial Release Without Formal Programs* considers the nature of release decision-making in selected jurisdictions that lack pretrial release programs, because such programs either were never established or lost their funding.

Each of these volumes is self-contained and can be read singly or in conjunction with other volumes. The *Introduction* provides background material pertinent to all of them.

A summary of the evaluation is also available. Entitled *Summary and Policy Analysis*, it provides a nontechnical discussion of the key features, findings and recommendations of the overall research effort.

Additionally, fourteen working papers have been prepared. Twelve of the working papers discuss the pretrial release practices in the individual jurisdictions studied; the remaining papers present detailed analyses of defendant outcomes for the two pilot test sites. Important findings from the various working papers have been included in the relevant volumes of the study.

ACKNOWLEDGEMENTS

A study of this scope could not have been completed without the assistance and support of a great many persons. We would especially like to thank the many officials at the National Institute of Justice who helped us implement the study, in particular, project monitors Dr. Bernard A. Gropper and former Institute staffmembers Dr. Richard T. Barnes and Mr. Michael Mulkey. Many helpful comments were also received from Mr. W. Robert Burkhart, Ms. Cheryl Martorana, Ms. Deborah Viets, Dr. Phyllis Jo Baunch, Dr. Lawrence Bennett, Mr. Joel Garner, former Director Blair Ewing and others at the Institute. Among officials at the Law Enforcement Assistance Administration, former Administrator James M. H. Gregg and Mr. Dennis Murphy deserve special mention for their assistance during the research project.

The Pretrial Services Resource Center provided invaluable information about current developments in the pretrial field, related research in progress and the present status of specific pretrial release programs. Their assistance saved us many hours of effort and greatly facilitated our work. Special thanks go to Ms. Madeleine Crohn, Dr. Donald Pryor, Mr. D. Alan Henry, Ms. Ann Jacobs, Ms. Nancy Waggner, Ms. Audrey Barrett and former Center associate, Dr. Michael Kirby for their helpfulness.

Directors and staff of the pretrial release programs studied, and criminal justice system officials in those sites, gave unstintingly of their time to help us understand the nature of local pretrial release practices and to enable us to collect necessary evaluative data. We would particularly like to thank the directors of the programs that participated in the study: Mr. Richard Motsay, former Director, and Mr. John Camou, present Director, Pretrial Release Services Division, Supreme Bench, Baltimore City, Md.; Judge William T. Evans, Chief Administrative Judge, District Court of Maryland, Towson (Baltimore County), Md.; Mr. Bruce D. Beaudin, Director, Pretrial Services Agency, Washington, D.C.; Ms. Cheryl Johnson, former Director, Pretrial Release Program, Dade County, Fla.; Mr. Robert A. Foote, Esq., Special Projects Administrator, Pretrial Jail Overcrowding Project, Dade County, Fla.; Ms. Cheryl Welch, Director, Pretrial Intervention, Dade County, Fla.; Mr. Raymond Weis, Director of Pretrial Services, Louisville, Ky.; Mr. Horace Cunningham, former Director, and Mr. George Corneveaux, Jr., present Director, Correctional Volunteer Center, Pima County, Ariz.; Ms. Cherie Mason, former Director, and Ms. Connie Carter, present Director, Pretrial Services Division, Municipal Court, Santa Cruz County, Calif.; Mr. Ronald J. Obert, Director, Office of Pretrial Services, Santa Clara County, Calif.; Ms. Mariclare Thomas, former Director, and Mr. Randolph Scott, present Director, Pretrial Release Program, Lincoln Corrections Division, Lincoln, Neb.; and Mr. Russell Ortego, Director, Pretrial Information Program, Jefferson County, Texas.

In the jurisdictions without pretrial release programs, we are particularly grateful to Chief Judge Michael Sullivan and Mr. George Mueller, former Director, Bail Evaluation Program, Milwaukee, Wisconsin; and Judge Jose R. Davila, Jr., Richmond General District Court, Richmond,

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Virginia. Additionally, special insight about the Kentucky Statewide system was provided by Mr. William Davis, Director, Administrative Office of the Courts, State of Kentucky; Mr. Steven F. Wheeler and Mr. John C. Hendricks, Co-Directors, Pretrial Services Agency, State of Kentucky; and Mr. Lawrence Henderlight, Pretrial Services Officer, Corbin, Ky.

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.

HIGHLIGHTS

Background

This volume of the National Evaluation of Pretrial Release considers issues related to program impact. These include:

- What impact do pretrial release programs have on the rates of release of defendants awaiting trial? On the types of release (e.g., own recognizance, deposit bond, money bail)? On the speed with which release is secured?
- What impact do pretrial release programs have on the "equity" of the release process (i.e., are certain types of defendants, such as poor individuals or members of minority groups, more likely to secure release when formal programs exist)?
- Do programs result in the release of defendants with a significantly different level of risk than defendants released in the absence of program activities?
- What is the extent of criminality for pretrial releasees who receive program followup, as compared with released defendants who do not?
- What are the failure-to-appear rates for pretrial releasees who receive program followup versus those who do not?
- What costs and benefits are associated with program operations?

An experimental design was chosen as the most appropriate way of studying these issues. Defendants processed by a program were 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 expanded program operations to reach persons not previously processed (e.g., due to budget constraints that limited a program's hours of operation). 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.

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In general the experimental procedures worked as follows. The program's staffmembers interviewed 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 sites did not present a recommendation or the interview to the judge.* 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:

- the overall rates of release of defendants;
- the speed with which release was obtained;
- the types of release secured by defendants (e.g., own recognizance, deposit bond, money bail); and
- the "equity" of the release process.

After the release decisions had been made, two issues were of major concern:

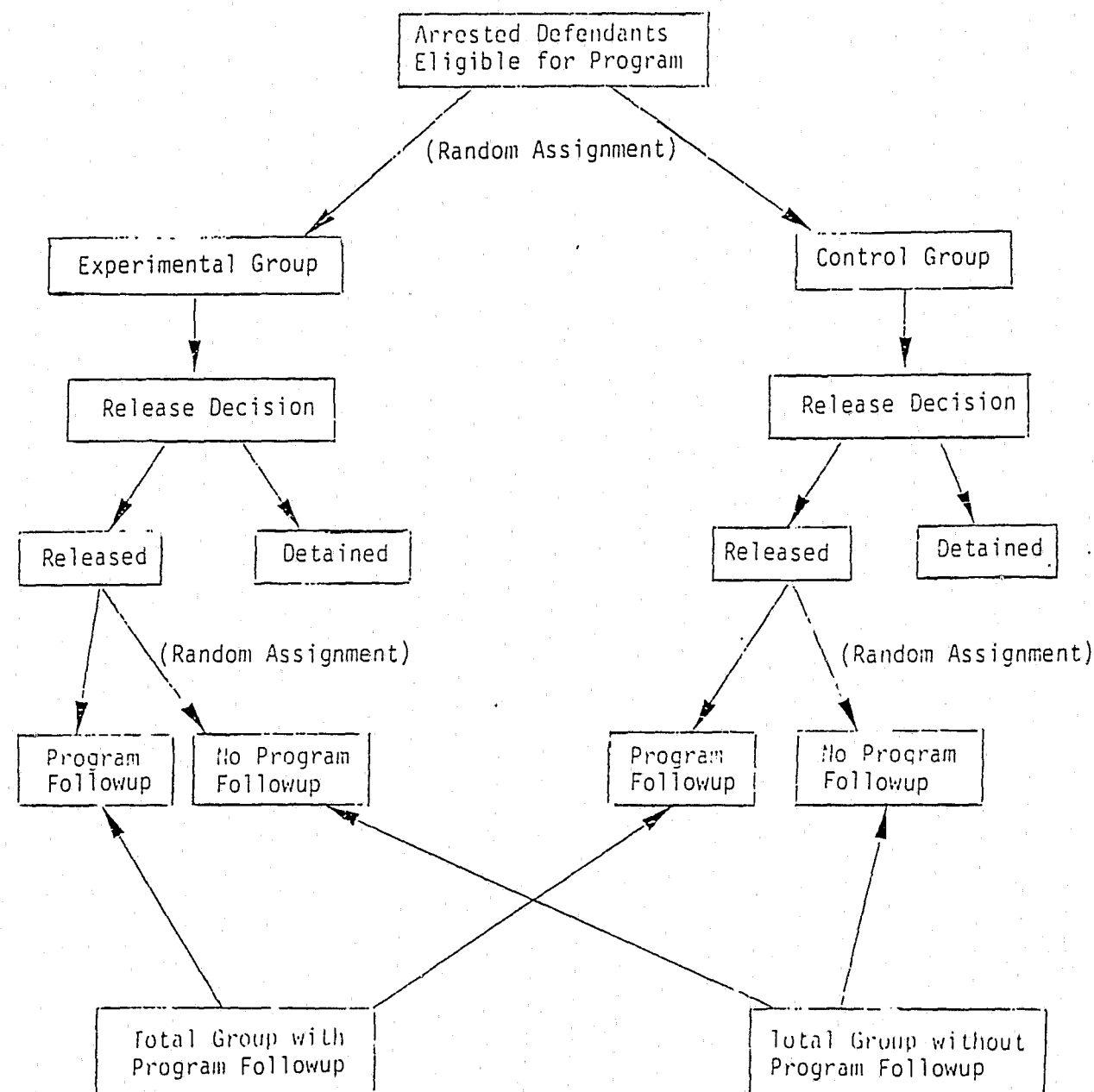
- differences in failure-to-appear (FTA) rates for defendants in the experimental versus control groups; and
- differences in pretrial criminality rates for these groups, as reflected in rates of pretrial rearrests and of convictions for those rearrests.

These comparisons of post-release outcomes considered whether program activities resulted in the release of defendants who posed a different level of risk in terms of missing court appearances or committing criminal acts during the pretrial period.

Because the experimental and control groups were selected before release decisions were made, there was no reason to expect groups of released defendants to have comparable characteristics. As a result, differences in defendant outcomes after release might be due to factors other than program impact. To permit better analysis of program impact on post-release outcomes (i.e., failure to appear and pretrial criminality), we implemented a second random assignment procedure in two of the jurisdictions. After the release decision had been made, released defendants in those sites were assigned to groups that either received program followup or did not (see Figure 1). Defendant outcomes were then compared

* Somewhat different experimental procedures were used in Baltimore City and for felony defendants in Tucson.

FIGURE 1
SUMMARY OF EXPERIMENTAL APPROACH WITH
TWO-STAGE RANDOM ASSIGNMENT



for these groups to assess the impact of program followup activities. Thus, 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 by random provision of program followup to released defendants).

In some cases the experimental procedures described above had to be modified to meet local constraints. The procedures used at each site are described in detail in the relevant chapters of this volume. Briefly, the major modifications were:

- For Tucson felony defendants, State law required certain information to be provided to the judge making a release decision. Consequently, this information from defendant interviews was provided to judges for all defendants, including those in the control group. Defendants in the experimental group received "full program services," including verification of information and presentation of release recommendations to the judge.
- In Baltimore City, where the program interviewed all arrested defendants, the impact of a program modification was tested. That modification consisted of extending eligibility for an own recognizance release recommendation to selected defendants who did not have sufficient "points" to receive such a recommendation automatically. While defendants in the control group were processed routinely, defendants in the experimental group received an automatic own recognizance release recommendation. Thus, the Baltimore City experiment tested the impact of extending a program "service" to a group of defendants who would otherwise have been excluded, because of low point scores.
- In Baltimore City a second random assignment (after release) tested the impact of relatively intensive followup services, as compared with minimal post-release supervision. The experimental group was screened for referral to services, if needed, and was required to call the program at least twice per week. During these calls, defendants were reminded of court dates and cautioned "to stay out of trouble." Control group defendants called the program once a week and the program merely acknowledged the calls. Thus, the post-release experiment tested the impact of "more" versus "less" supervision; it did not test the impact of "some" versus "no" followup.

Altogether, 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).

The experimental procedures resulted in the selection of groups of defendants having similar characteristics in all sites except Beaumont-Port Arthur. In that jurisdiction, the experimental and control groups

differed along six of the 19 background characteristics for which they were compared.

Program Impact on Release Outcomes

We analyzed program impact on four types of release outcomes:

- 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.

As shown in Table 1, three programs had a positive impact on the overall rate of release of defendants: Baltimore City, where the impact of changed program procedures was tested; Lincoln; and Beaumont-Port Arthur. Because the experimental and control groups were not equivalent in Beaumont-Port Arthur, we cannot determine conclusively whether the apparent program impact was real or due to differences in defendant characteristics. When we exercised statistical controls, it appeared that the program had a positive effect on the release of most defendants. However, the impact of program processing was not able to override the adverse effect on release of (1) a longer prior record, (2) employment as a laborer, or (3) 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.

TABLE 1
SUMMARY OF PROGRAM IMPACT ON RELEASE OUTCOMES

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

OUTCOME	TUCSON FELONIES	TUCSON MISDEMEANORS	BALTIMORE CITY	LINCOLN	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 Ethnicity	+	0	+	0	0
By Employment Status	0	0	+	+	0

*Not applicable, because the nature of the program ensured a difference: defendants could be released on 3% bond only if they were in the experimental group, except in unusual cases. (There is no own recognizance release in the jurisdiction, so 3% bond is the least restrictive type of release.)

In terms of speed of release, only the Baltimore City experiment showed positive program impact. This was also the case for type of release: in Baltimore City more of the defendants processed by the program were released on nonfinancial conditions.

Program operations affected the equity of the release process in three jurisdictions: Tucson, Baltimore City and Lincoln. In two sites (Tucson and Baltimore City), the program 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.* In the remaining three experiments there were no differences in release rates for minority versus white defendants in either the program or control group.**

For two sites (Baltimore City and Lincoln), program operations were associated with more equitable release of unemployed defendants than occurred in the control group. In Tucson (at both the felony and misdemeanor levels), unemployed defendants were detained at a significantly higher rate than employed persons in both the program and control groups. Only one experiment showed equivalent release rates for unemployed and employed defendants in both the program and control groups; that outcome occurred in Beaumont-Port Arthur, where, because of the lack of group comparability discussed earlier, we have somewhat limited confidence in the findings.

In summary, four of the five programs studied experimentally 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 measure affected favorably. Lincoln was next (two measures affected), followed by Beaumont-Port Arthur (one measure affected) and Tucson (one measure for the felony study and none for the misdemeanor experiment).

Program Impact on Failure To Appear and Pretrial Criminality

Table 2 summarizes program impact on post-release outcomes, specifically, failure to appear, pretrial rearrest, and pretrial rearrest conviction rates. For the three experiments (Tucson felonies, Lincoln and Beaumont-Port Arthur) where only a pre-release random assignment occurred, there was one instance of significant outcomes difference between program and control groups: in Beaumont-Port Arthur released program group defendants had lower rates of rearrest conviction than control

* No other factors were identified that might account for this difference (e.g., the other characteristics of white versus minority defendants in the two groups were quite similar).

** Although Tucson misdemeanor release rates were significantly higher for minority defendants in the program group, this was due to their higher employment rate.

group defendants. Thus, in none of these three sites were program operations associated with worse failure to appear or pretrial criminality outcomes than when the programs did not function. This occurred even

TABLE 2
SUMMARY OF PROGRAM IMPACT ON FAILURE TO APPEAR AND
PRETRIAL CRIMINALITY

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

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

though two of the three sites had significantly higher release rates for program group defendants. Consequently, the release of additional defendants did not lead to increased disruption of court operations (through higher failure to appear rates) or to greater harm to community safety (through higher pretrial criminality rates).

For the two experiments where random assignment occurred after release, so that the impact of program followup activities could be tested, no differences were found between the program and control groups for either failure to appear or pretrial criminality. Although this suggests that program followup after release has no impact on defendant behavior, one must remember that the tests of followup impact were quite limited in scope. In 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, our findings cannot be considered conclusive regarding the impact of supervision on defendant behavior after release.

Cost-Effectiveness of Programs

We undertook a cost-effectiveness analysis, because of the great interest in this topic. We considered cost-effectiveness issues from the viewpoint of the criminal justice system (CJS), rather than that of defendants, the public at large or another group. Thus, costs were included in the analysis only if the CJS incurred them and benefits, only when the CJS accrued them. Thus, our analysis excludes many possible costs and benefits that might reasonably be included in an analysis conducted from a different perspective (e.g., "saved" defendants' wages, decreased welfare costs for defendants' families or costs incurred by victims of pretrial crime).

We considered four broad categories of costs:

- detention;
- failure to appear;
- pretrial arrest; and
- 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 costs incurred when defendants returned to court. Similarly, the costs of pretrial arrest had several components: apprehension, booking, program costs if defendants were re-processed, detention (if any), court processing costs and sentencing costs.

The cost estimates derived for this analysis were often very rough ones, because of the many difficulties involved in obtaining accurate cost data across jurisdictions. Thus, the cost-effectiveness findings should be viewed as suggestive, rather than conclusive.

Program cost-effectiveness, measured by comparing costs to the criminal justice system for the program and control groups, varied widely. The most cost-effective programs, at least based on our limited analysis, were not necessarily those that showed the greatest impact on defendant outcomes.

In general, the more cost-effective programs processed felony level defendants (though not necessarily exclusively) and provided minimal followup after release. The fact that misdemeanor-only programs were not found cost-effective stemmed from two main reasons. First, most defendants charged with misdemeanors were released relatively quickly in both the program and control groups. Thus, there was little opportunity for program operations to generate savings in detention costs. Second, the rearrested defendants were rearrested on relatively minor charges 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.

At the felony level the more serious nature of the cases led to higher bonds and the potential for much longer periods of detention. Additionally, rearrested defendants were rearrested on more serious charges, having more severe penalties. Thus, felony-level programs—by affecting detention and pretrial arrest costs—were able to generate savings that could offset their costs.

In addition to the lack of cost-effectiveness for misdemeanor programs, post-release followup activities were not cost-effective. Neither of the

two experimental tests of program followup activities showed a positive effect on defendant outcomes. In both cases the activities were relatively expensive to implement and did not lead to reduced costs in other categories.

When the relative contributions of various types of costs to total costs were considered (see Table 3), failure to appear was the least costly category in most sites. In one jurisdiction (Beaumont-Port Arthur) failure to appear actually generated revenue, because of the amount of bond forfeitures collected in that site.

TABLE 3
PERCENTAGE OF TOTAL COSTS, BY MAJOR COST CATEGORIES

Note: Percentages may not add to 100% due to rounding.

COST CATEGORY	TUCSON FELONY		BEAUMONT- PORT ARTHUR		BALTIMORE CITY		LINCOLN		TUCSON MISDEMEANOR	
	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group
Detention	47%	18%	29%	37%	3%	11%	42%	67%	16%	76%
Failure to Appear	1%	1%	-3%	-15%	10%	11%	2%	5%	2%	6%
Pretrial Arrest	33%	81%	58%	79%	33%	70%	15%	28%	8%	18%
Program	19%	1%	16%	0%	55%	9%	41%	0%	74%	0%
TOTAL	100%	100%	100%	100%	100%	100%	100%	100%	100%	100%

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 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. For the Tucson felony control group, 78% of the total pretrial arrest costs were due to incarceration costs (for six defendants, sentenced to a total of more than 25 years in prison). Comparable percentages for Beaumont-Port Arthur and Baltimore City were 90% and 40%, respectively.

Another aspect of the analysis of sentencing costs is the way those costs differed for the program 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 program group. In particular, the extent of incarceration in the control group was much greater than in

the program group. This suggests that the operations of pretrial release programs may lead 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 occurs is not known. Programs may serve as advocates for rearrested defendants and help ameliorate sentences by providing information about defendants' circumstances that would not otherwise be available. It is also possible that the mere existence of more complete information about defendants has a positive effect on sentences, without the need for programs to serve as advocates. Alternatively, programs may affect defendant behavior so that less serious crimes are committed, with the difference in sentencing severity reflecting this fact. Finally, it is possible that our findings for three sites, having a relatively small number of sentenced defendants, would not be replicated if additional jurisdictions were studied or if a larger number of defendants were analyzed in the same sites.

Conclusions

The experimental analysis of program impact leads to the following conclusions:

- The pretrial release programs studied had a positive effect on the release of defendants pending trial. Program operations were associated with higher release rates, higher rates of non-financial release, speedier release and/or greater equity of release in four of the five experiments conducted.
- The improvements in release outcomes were not offset by higher rates of failure to appear or pretrial criminality for defendants processed by the program.
- There is no evidence from our study that more intensive post-release followup has a positive effect on defendants' failure to appear or pretrial criminality rates. However, our experimental tests of followup activities were quite limited in scope, and the findings may not be broadly applicable to many jurisdictions.
- There may be great potential for programs to affect release outcomes, without increasing failure to appear or pretrial criminality rates, through the use of less restrictive release recommendation criteria. In the one jurisdiction where we tested the effect of relaxing the program's point system criteria, every release outcome measure was affected positively, and rates of failure to appear and pretrial criminality were no higher than in the control group.
- Unemployed defendants had more difficulty securing release than employed defendants. This suggests that the bail reform movement's goal of eliminating release inequities associated with income level has yet to be achieved.

- Release inequities associated with ethnicity were less common than those associated with employment status. Moreover, program operations tended to eliminate any release inequities associated with ethnicity.
- Programs that included defendants charged with felonies seemed to be more cost-effective than programs that processed only misdemeanors.
- Failure to appear was not very costly to the criminal justice system relative to other major categories of costs. Indeed, one jurisdiction made a profit on failure to appear as a result of the large amount of bond forfeitures collected in that site.
- Failure to appear could become very costly to the criminal justice system, if prosecution for failure to appear became more widespread.
- Pretrial arrest was relatively costly for the experiments that included defendants charged with felonies. These costs were largely due to the sentences imposed on defendants convicted of pretrial arrests, especially when the sentences involved incarceration.
- Pretrial arrest costs were much higher for the control group than the program group in the three experiments that included defendants charged with felonies. This difference was largely due to much harsher sentences for the defendants convicted of pretrial arrests in the control group vis-a-vis the program group. The reasons for this difference could not be ascertained.

Several conclusions apply to the research tasks, including:

- It is possible, but difficult, to implement experimental designs testing the impact of pretrial release program activities.
- Although many programs were concerned about the possibility of a lawsuit based on "denial of service" and refused to participate in experiments for that reason, no legal challenges arose during any of the five experiments. This suggests that program fears about possible lawsuits may be much greater than the actual likelihood that such lawsuits will occur. If so, special efforts may be needed to allay program fears, if additional experimental studies are contemplated.
- The experimental analyses resulted in findings and conclusions about program impact that could not have been developed through other types of analyses.
- Data needed for a completely accurate, comprehensive analysis of program cost-effectiveness were not available locally and probably could have been developed only at considerable expense. Any future efforts to conduct cost-effectiveness analyses of programs should be cognizant of this limitation.

Recommendations

The following recommendations are based on the findings and conclusions from the experimental analyses:

- Pretrial release programs should continue to be supported, particularly those that process felony defendants, with their greater potential for cost savings due to program operations.
- Programs should scrutinize their post-release followup activities to determine whether they are effective. Our findings suggest that such activities may have little impact on defendant behavior but be quite expensive to conduct.
- Programs should give careful consideration to adopting less restrictive release recommendation criteria. The findings from the Baltimore City experiment suggest that such action might increase program impact on release outcomes, while not increasing failure to appear or pretrial criminality rates.
- Jurisdictions seeking ways to cut criminal justice system costs should consider whether incarceration can be decreased, either pretrial detention or post-adjudication imprisonment. Such costs were quite large in some of the sites studied. Also, if the jurisdiction prosecutes defendants for failure to appear, it should consider whether this practice merits continuation, given its rather high costs.
- Additional analysis should be conducted of the equity of release by employment or income status. In particular, programs should study whether their operations perpetuate release inequities for poor defendants.
- Additional analysis should be conducted to determine program impact on sentencing costs for defendants convicted of pretrial arrests and to ascertain the underlying reasons for such impact.
- Because of the wide variation found in the types and levels of program impact, other jurisdictions would be well advised to evaluate their programs and determine their specific effects.
- Programs should use experimental designs to test the effect of possible changes in program operations (e.g., providing different levels of supervision or implementing less restrictive release recommendation criteria). Such tests of program variations would not raise "denial of services" concerns and could provide great insight about the program activities that are most effective within a specific jurisdiction.

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I. INTRODUCTION

This volume considers the impact of pretrial release programs on release decisions, failure to appear and pretrial criminality. An experimental design was chosen as the most appropriate way of studying these issues. This design compared a group of defendants who were processed by a program with an equivalent group of defendants who were 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.

The experimental component of the national pretrial release evaluation was the most difficult to implement. Although there were many reasons for this, as discussed in Appendix A, a major one was programs' concern that defendants in the control group would be "denied service." We tried several ways of dealing with this concern (see Appendix A for details) before resolving the issue. The solution adopted, which permitted us to implement experimental procedures in four jurisdictions, involved expansion of program operations to reach populations not previously processed (e.g., due to budget constraints, limiting the program's hours of operation). This temporary expansion of program operations was supported by research funds the National Institute of Justice provided, in order to facilitate the experimental analyses deemed crucial for an adequate assessment of program impact.¹ Because the programs had an "overflow" situation before the

¹Because existing programs were being expanded, temporarily, the costs to the Federal government were relatively modest. A special grant of about \$75,000 supported the program expansion costs for all of the sites. Participating jurisdictions often provided staff support, office space, etc., at no charge, so total costs would be somewhat higher.

experiments began, their expanded operations permitted them to select a "program group" and a "control group" without decreasing the number of defendants processed.²

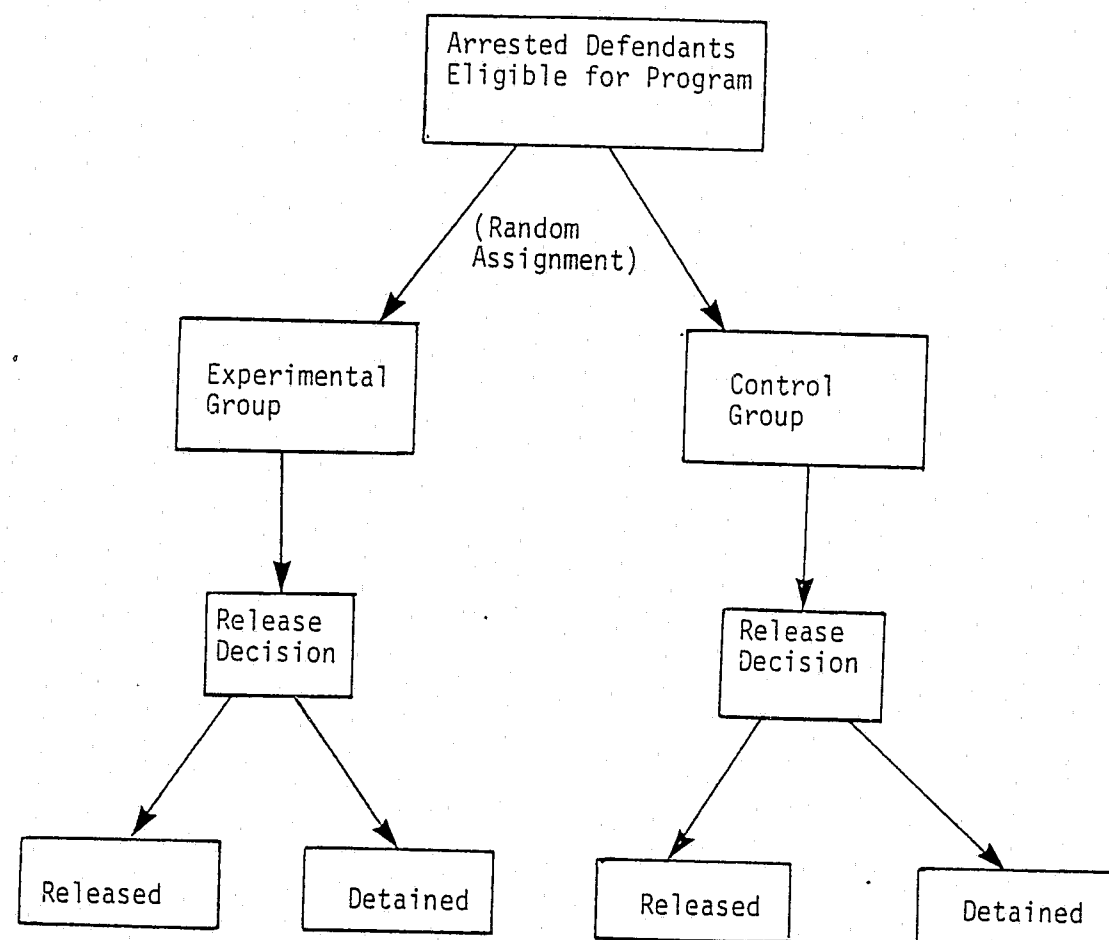
The experimental design is shown in Figure 1.1. The program's staff members interviewed an expanded group of arrestees, who were randomly assigned to either the experimental or control group. For the experimental group, the program followed its normal processing procedures—typically, a release recommendation was prepared, based on verified interview information, and this recommendation was presented to a judge. For the control group, the program did not present a recommendation or the interview to the judge.³ 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 those decisions. Several types of possible program effects were of particular interest, including impact on:

- the overall rates of release of defendants;
- the speed with which release was obtained;
- the types of release secured by defendants (e.g., own recognizance, deposit bond, money bail);
- bond amounts; and
- the "equity" of the release process (e.g., whether certain types

²A similar approach for identifying an "overflow" group on a random basis had been successfully used in an evaluation of the Court Employment Program, a diversion program in New York City. See Vera Institute of Justice, "Court Employment Project Evaluation, Research Design and Implementation: A Preliminary Report," December 1977.

³Somewhat different experimental procedures were used in Baltimore City and for felony defendants in Tucson, as discussed later.

FIGURE 1.1
SUMMARY OF EXPERIMENTAL APPROACH



of defendants, such as poor individuals or members of minority groups, were more likely to secure release when formal programs existed).

After the release decisions had been made, two issues were of major concern:

- differences in failure-to-appear (FTA) rates for defendants in the experimental versus control groups; and
- differences in pretrial criminality rates for these groups, as reflected in rates of pretrial rearrests and of convictions for those rearrests.

These comparisons of post-release outcomes considered whether program activities resulted in the release of defendants who posed a different level of risk in terms of missing court appearances or committing criminal acts during the pretrial period.

An additional topic for study through the experiments was the cost-effectiveness of programs (see Appendix B for a discussion of the cost-effectiveness analysis procedures). The experiments compared outcomes when programs operated with outcomes when they did not and thus provided almost ideal conditions for cost-effectiveness analysis.

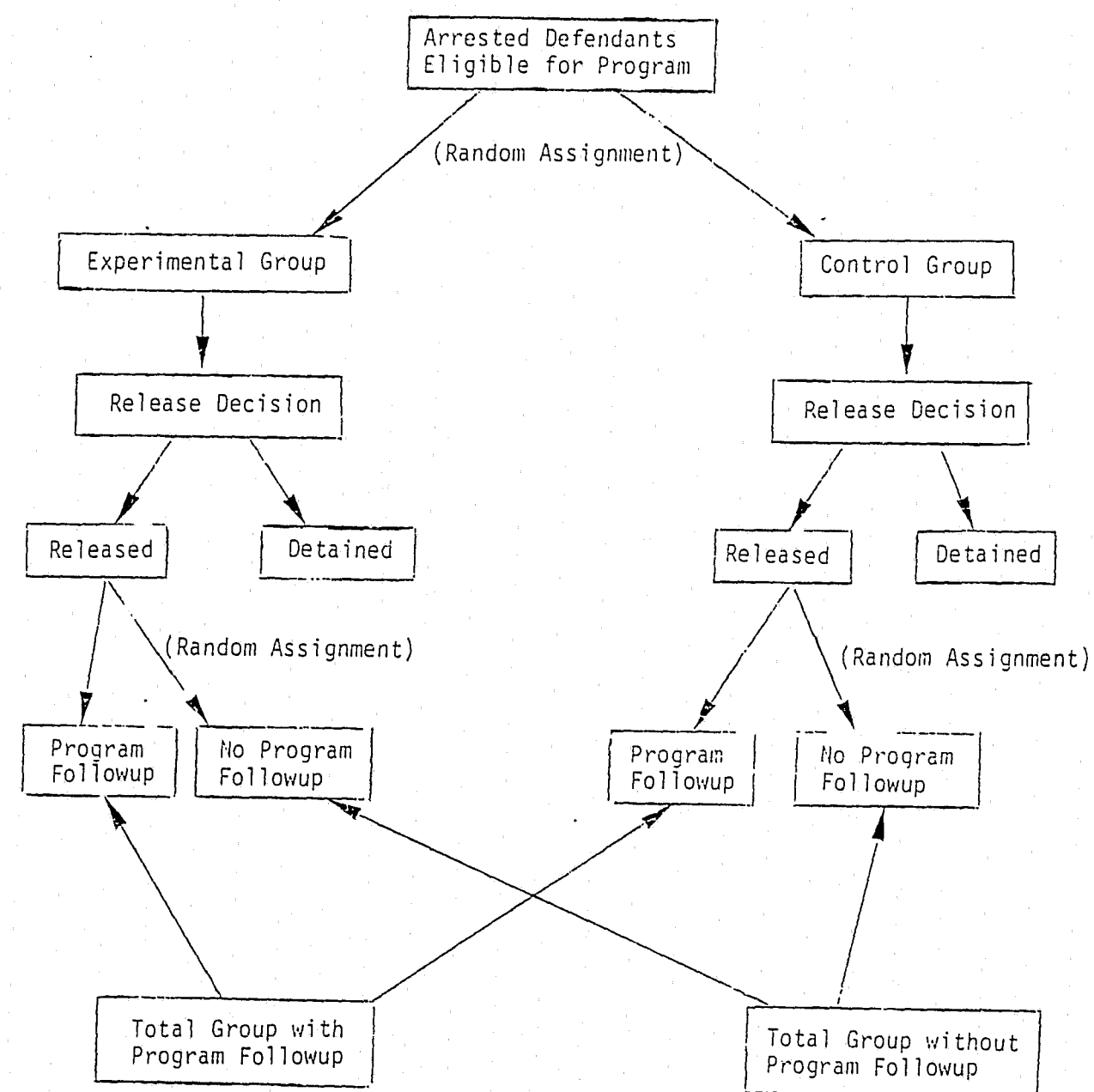
A limitation of the procedures shown in Figure 1.1 is that the experimental and control groups would be comparable only before release decisions were made. There was no reason to expect groups of released defendants to be comparable, and analysis of their outcome differences alone might be misleading. For example, a reasonable outcome of the experiment might have been for the experimental group to have both higher release rates and worse outcomes, in terms of FTA and pretrial criminality. Considering the post-release outcomes alone would have provided an inaccurate assessment of the overall situation.

To avoid such a difficulty, we tried to implement a second random assignment, as shown in Figure 1.2. After the release decision had been made, released defendants were assigned to groups that either received program followup or did not. Defendant outcomes (e.g., failure-to-appear and pretrial criminality rates) were then compared for these groups to assess the impact of program followup activities. Thus, separate analyses were conducted of program impact on release decisions (accomplished by random provision of program information to the judges making these decisions) and on defendant outcomes after release (accomplished by random provision of program followup to released defendants).

Although more powerful than the one-stage random assignment of Figure 1.1, the experimental design shown in Figure 1.2 was more cumbersome to implement. Despite this, the design was successfully used in two of the jurisdictions studied.

In most cases, the random assignment procedures were based on the defendant's date of birth. The 366 days of the year were randomly placed on two lists. One list of birthdates was designated for the experimental group; and the other, for the control group. A program staff person could then easily determine which group a defendant was in, by consulting the birthdates' lists. The same procedure was used for the second random assignment; the birthdates list for each post-release group (i.e., the program followup group and the group without followup) consisted of one-half the birthdates from the two earlier lists. Thus, roughly half the group receiving program followup would initially have been in the experimental group (established before the release decisions were made) and the other half, in the control group.

FIGURE 1.2
SUMMARY OF EXPERIMENTAL APPROACH WITH
TWO-STAGE RANDOM ASSIGNMENT



This random assignment was relatively easy to implement, because the date of birth was usually routinely collected on defendants. Additionally, because birthdates could be easily checked from other sources, we thought their use would discourage program staff from trying to manipulate the experimental and control groups in any way (e.g., placing a well-liked defendant who belonged in the control group into the program group, if the interviewer thought that might help the person).

The director of the Lincoln experiment was uncomfortable with birthdates as a random assignment mechanism, because a defendant arrested more than once during the study period would always be in the same group. Thus, we used a different randomization procedure in Lincoln. Because we anticipated that it would be about six months before the experiment acquired sufficient cases to study, due to the low volume of defendants, we randomly assigned each day over a six-month period to be either a "research" day or a "program" day. On program days all defendants were placed in the experimental group. On research days, all defendants became part of the control group. When the experiment was extended for an additional three months, we generated another list of program and research days for that period.

Table 1.1 presents summary information on the implementation of the experiments, which were conducted in four jurisdictions: Beaumont-Port Arthur, Texas; Tucson, Arizona; Lincoln, Nebraska; and Baltimore City, Maryland. Two of these sites (Tucson and Baltimore City) were included in the cross-sectional analyses presented in Volume I. Consequently, we were able to compare the experimental groups' characteristics and outcomes with the earlier findings for the jurisdictions as a whole.

TABLE 1.1
SUMMARY OF IMPLEMENTATION OF EXPERIMENTS

ITEM	BEAUMONT- PORT ARTHUR, TEXAS	TUCSON, ARIZONA MISDEMEANORS	TUCSON, ARIZONA FELONIES	LINCOLN, NEBRASKA	BALTIMORE CITY, MARYLAND
STARTING DATE OF EXPERIMENT	SEPT. 1978	NOV. 1978	NOV. 1978	DEC. 1978	MAY 1979
DATE RANDOM ASSIGNMENT ENDED	MARCH 1979	JAN. 1979	MARCH 1979	AUG. 1979	AUG. 1979
NUMBER OF RANDOM ASSIGN- MENTS PER DEFENDANT	ONE	TWO	ONE	ONE	TWO
TOTAL NUMBER OF DEFEN- DANTS IN EXPERIMENT	193	424	295	130	528

Although only misdemeanor defendants were studied in Lincoln, both felony and misdemeanor defendants were included in the experiments at the other three sites. In Tucson separate experiments were conducted for the misdemeanor and felony levels, because of the structure of the courts and the pretrial release program.

The experimental procedures began in the first site (Beaumont-Port Arthur) in September 1978 and in the last site (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 two jurisdictions a second random assignment was implemented, as shown in Figure 1.2, as well as the first (see Figure 1.1). Altogether, the experiments involved 1,593 defendants in the four sites. Both Tucson and Baltimore City had relatively large numbers of defendants, while the Beaumont-Port Arthur and Lincoln experiments were smaller in scope.

After the groups had been selected, we waited several months before collecting followup data on the defendants. This was done so that as many cases as possible would have reached a final disposition, and thus data on pretrial performance would be reasonably complete. Consequently, followup data were collected in the fall and winter of 1979-80, with analysis conducted shortly thereafter. Appendix C describes the methodology used for the study and includes the various data collection forms.

In addition to analyses of defendant outcomes, we studied the delivery systems through which release decisions were made. These delivery system analyses were similar to those conducted for the sites included in the cross-sectional analyses of Volume I. Particular attention was given, however, to whether criminal justice system officials changed their behavior during the course of the experiment and, if so, the likelihood

that such changes would continue if the programs were to end.

The next five chapters of this volume discuss each of the individual experiments, including:

- the nature of the pretrial release program;
- the details of the experiment conducted;
- the outcomes of the experiment; and
- the findings concerning program cost-effectiveness.

Following these discussions, a final chapter of the volume summarizes the major findings of the experimental analyses and presents conclusions and recommendations derived from them.

II. THE TUCSON FELONY EXPERIMENT

A. Background

The Correctional Volunteer Center in Tucson has been in operation since 1972. At the time of our cross-sectional analysis (see Volume I), the program was processing felony defendants only.⁴ Although the program functioned around the clock and attempted to process all felony arrestees, it could not provide full program services to defendants brought to court late. This "late arrests" group constituted the "overflow" group for the experimental analysis. Rather than using a haphazard approach to processing these defendants—with the result that some persons received full services while others received partial services—the late arrestees were divided at the time of interview into an experimental group that received full program services and a control group that was interviewed only.

Because Arizona State law requires judges making release decisions to obtain and consider certain specified information about defendants, the interview data was 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, because in that event the judges would presumably comply with the State law by obtaining the required information (which is very similar to the program interview) through some means. For the experimental group, though not for the control group, the program also verified the information and presented release recommendations to the court. Thus, the experiment tests the impact

⁴For more information on the program, see Kristina Peterson, Delivery System Analysis of Pima County (Tucson), Arizona, Working Paper No. 6, March 1979.

of verified information and program recommendations on the release process.

Although the experiment was limited to the "late arrests" group, local criminal justice and program officials thought these defendants would be reasonably representative of felony arrestees as a whole. No systematic biases were thought to be associated with time of arrest for felony charges. (Driving while intoxicated, an offense much more likely to occur at night than during the day, is a misdemeanor charge.)

In this experiment defendants were randomly assigned before release only; no second assignment occurred after release.⁵ Hence, the experiment used the design of Figure 1.1, rather than that of Figure 1.2. The written procedures prepared to guide program staff in the implementation of the experiment appear in Appendix D.

B. Outcomes of Experiment

A first consideration regarding the experiment is whether the random assignment procedures led to equivalent experimental and control groups. We considered group comparability 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.

⁵The program itself has never emphasized routine followup services for all released defendants. Instead, it provides intensive followup for a specifically selected group of supervised defendants. There was no "overflow" among these defendants.

Many indicators could have been used to assess comparability in each of these areas. The indicators selected were chosen because of their general use by pretrial release programs to assess release risk, findings from existing research that indicate that certain of these indicators may be associated with failure to appear or pretrial criminality, and ability to acquire the information within the operational constraints of the experiments. Nineteen specific indicators were selected, as shown in Table 2.1. Besides indicators for the three major categories listed earlier, we also considered the educational backgrounds of defendants.

For the Tucson felony experiment data were available for more than half the defendants in each group for 17 out of the 19 indicators (see Table 2.1). The two groups were comparable for each of these 17 characteristics, as Table 2.2 shows (Appendix E provides the detailed data).

Table 2.3 indicates the experimental outcomes for rate, speed and type of release. There were no statistically significant differences (.05 level) between the two groups for any of these outcome measures. Approximately 85% of the defendants in each group secured release at some point prior to trial. Of these defendants, 81%-85% were released on nonfinancial, as compared to financial, conditions. The mean number of days from arrest to release was 3.9 days in one group and 6.6 days in the other (see details in Appendix E).

Because some defendants who were not released had their cases settled at arraignment and thus were detained for a very short time (usually less than one day), we compared the rates of release for the two groups with release redefined to include defendants detained for one day or less. There were no statistically significant differences between the two groups on this basis either. Nor were there important differences in the mean

TABLE 2.1
PERCENTAGES OF CASES IN EXPERIMENTAL AND CONTROL GROUPS
WITH DATA FOR SELECTED CHARACTERISTICS

CHARACTERISTIC	EXPERIMENTAL GROUP (n=130)	CONTROL GROUP (n=165)
<u>Community Ties</u>		
Local residence status	98%	96%
Years of local residence	79%	84%
Months at present address	92%	95%
Marital status	92%	93%
Family support	92%	96%
With whom defendant lives	94%	92%
Employment status	98%	98%
Income level (employed defendants only)	81%	72%
Public assistance	9%	8%
Occupation	55%	61%
<u>Criminality</u>		
Current charge	99%	100%
Number of prior arrests	100% ^a	100% ^a
Number of prior convictions	100% ^a	100% ^a
Criminal justice system status at time of arrest	56%	39%
Age at first adult arrest	60%	51%
<u>Demographic Characteristics</u>		
Age at arrest	100%	100%
Ethnicity	99%	98%
Sex	100%	99%
<u>Other</u>		
Education	72%	79%
^a Includes cases with missing information.		

TABLE 2.2
SUMMARY OF COMPARABILITY OF EXPERIMENTAL
AND CONTROL GROUPS
(n=295)

CHARACTERISTIC	SIGNIFICANCE LEVEL
<u>Community Ties</u>	
Local residence status	N.S.*
Years of local residence	N.S.
Months at present address	N.S.
Marital status	N.S.
Family support	N.S.
With whom defendant lives	N.S.
Employment status	N.S.
Income level (employed defendants only)	N.S.
Occupation	N.S.
<u>Criminality</u>	
Current charge	N.S.
Number of prior arrests	N.S.
Number of prior convictions	N.S.
Age at first adult arrest	N.S.
<u>Demographic Characteristics</u>	
Age at arrest	N.S.
Ethnicity	N.S.
Sex	N.S.
<u>Other</u>	
Education	N.S.
* Indicates the two groups were not significantly different (.05 statistical level).	

TABLE 2.3
RATE, SPEED AND TYPE OF RELEASE FOR
EXPERIMENTAL AND CONTROL GROUPS

OUTCOME	EXPERIMENTAL GROUP (n=130)		CONTROL GROUP (n=165)	
	Number	Percent	Number	Percent
<u>Rate of Release</u>				
Defendants released	112	86%	140	85%
Defendants <u>not</u> released	18	14%	25	15%
TOTAL	130	100%	165	100%
<u>Speed of Release</u>				
Mean number of days from arrest to release ^{a/}	6.6		3.9	
<u>Type of Release</u>				
Nonfinancial	94	85%	108	81%
Financial	17	15%	25	19%
TOTAL	111	100%	133	100%
^a n=107 for experimental group and n=128 for control group.				

number of days of detention (for both detained defendants and those who eventually secured release) in the two groups (see the later discussion of cost-effectiveness for details).

Although we thought there might be a difference in the bond amounts for the two groups, no such difference was found. Bond amounts were somewhat lower in the experimental group, but the difference was not statistically significant (see data in Appendix E).

Another consideration regarding possible program impact on release outcomes concerns the "equity" of release, that is, whether program operations seem to result in release outcomes that are fairer than they would be otherwise. To analyze this issue, we considered release decisions by ethnicity and by employment status (a proxy for income level) for the experimental group and, separately, for the control group. Results are shown in Tables 2.4 and 2.5 and summarized in Table 2.6. Both the experimental and control groups showed release outcome differences by employment status. Unemployed defendants were less likely than employed defendants to secure release: 79% of the unemployed defendants in the experimental group were released, as compared with 92% of the employed defendants; in the control group, 76% of the unemployed defendants and 94% of those with jobs were released.

For type of release, approximately the same percentage of unemployed as employed defendants were released on nonfinancial, as compared to financial, conditions. Unemployed defendants in the experimental group, though not those in the control group, were detained longer than employed defendants prior to obtaining release (an average of 10 days, versus 3.4 days).

Analysis of failure to appear (FTA) and pretrial criminality rates by employment status suggests that there may be a reasonable basis for the disproportionate detention of unemployed defendants, if the likelihood of pretrial criminality is considered a legitimate basis for detention. Unemployed defendants in the control group (though not in the experimental group) who secured release were much more likely than employed defendants to be rearrested during the pretrial period: 22% of unemployed defendants were rearrested, versus 7% of employed defendants. Unemployed defendants were also much more likely to be convicted for a pretrial arrest:

TABLE 2.4
EQUITY OF RELEASE FOR EXPERIMENTAL GROUP
(n=112)

OUTCOME	ETHNICITY				EMPLOYMENT STATUS			
	WHITE		MINORITY		EMPLOYED OR SUBSTITUTES		UNEMPLOYED	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
<u>Rate of Release (all defendants)^a</u>								
Defendants released	70	89%	42	82%	60	92%	50	79%
Defendants <u>not</u> released	9	11%	9	18%	5	8%	13	21%
TOTAL	79	100%	51	100%	65	100%	63	100%
<u>Speed of Release (released defendants)</u>								
Mean number of days from arrest to release ^b	6.5 days		6.7 days		3.4 days		10.0 days	
<u>Type of Release (for released defendants)</u>								
Nonfinancial	57	83%	37	88%	52	87%	40	82%
Financial	12	17%	5	12%	8	13%	9	18%
TOTAL	69	100%	42	100%	60	100%	49	100%
^a Significant at the .06 level for employment status; not significant for ethnicity.								
^b n=66 for white; n=40 for minority; n=58 for employed; n=47 for unemployed. Significant at the .03 level for employment status; not significant for ethnicity.								

TABLE 2.5
EQUITY OF RELEASE FOR CONTROL GROUP
(n=140)

OUTCOME	ETHNICITY				EMPLOYMENT STATUS			
	WHITE		MINORITY		EMPLOYED OR SUBSTITUTES		UNEMPLOYED	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
<u>Rate of Release (all defendants)^a</u>								
Defendants released	85	90%	55	77.5%	88	94%	51	76%
Defendants <u>not</u> released	9	10%	16	22.5%	6	6%	16	24%
TOTAL	94	100%	71	100.0%	94	100%	67	100%
<u>Speed of Release (released defendants)</u>								
Mean number of days from arrest to release ^{b/}	3.9 days		3.8 days		3.6 days		4.6 days	
<u>Type of Release (for released defendants)</u>								
Nonfinancial	64	80%	44	83%	69	82%	38	79%
Financial	16	20%	9	17%	15	18%	10	21%
TOTAL	80	100%	53	100%	84	100%	48	100%
^a Significant at the .04 level for ethnicity and at the .003 level for employment status.								
^b n=79 for white; n=49 for minority; n=85 for employed; n=42 for unemployed.								

TABLE 2.6
SUMMARY OF EQUITY OF RELEASE FOR
EXPERIMENTAL AND CONTROL GROUPS

OUTCOME	EXPERIMENTAL GROUP (n=112)		CONTROL GROUP (n=140)	
	Ethnicity	Employment Status	Ethnicity	Employment Status
Rate of Release	N.S.	.06	.04	.003
Speed of Release	N.S.	.03	N.S.	N.S.
Type of Release	N.S.	N.S.	N.S.	N.S.

14% of the unemployed (released) defendants were convicted, versus 3% of the employed (released) defendants. There were no differences in FTA rates by employment status.

The analysis presented above is, of course, a limited one, because it is based on the experiences of defendants who secured release. The apparent bias against unemployed defendants resulted in their greater detention. Thus, the analysis of post-release outcomes of employed versus unemployed defendants assumes that the detained defendants would have had outcomes similar to those of released defendants, had the detained defendants been released as well. While this is not an unreasonable assumption, it was not one whose validity we could test.

Although release outcomes differences appeared in both the experimental and control groups by employment status, only the control group showed a difference on the basis of ethnicity. Minority defendants in the control group were less likely than white defendants to be released: 77.5% of the minority defendants were released, versus 90% of the white defendants (comparable percentages for the experimental group were 82% and 89%, a statistically insignificant difference). Unlike the situation with employed versus unemployed defendants, there were no differences in the failure to appear or pretrial criminality rates of released defendants having different ethnicity that might help explain the differential detention rates. To assess whether the apparent lack of bias in the experimental group as compared to the control group might be explained by different characteristics of minority versus white defendants in the two groups, we compared minority versus white defendants in each group in terms of the 16 characteristics listed earlier, in Table 2.2. Results are shown in Table 2.7. As illustrated,

TABLE 2.7
COMPARABILITY OF WHITE VERSUS MINORITY
DEFENDANTS IN EXPERIMENTAL AND
CONTROL GROUPS

CHARACTERISTIC	EXPERIMENTAL GROUP (n=112)	CONTROL GROUP (n=140)
<u>Community Ties</u>		
Local residence status	N.S.	N.S.
Years of local residence	.0000	.03
Months at present address	.03	.003
Marital status	N.S.	N.S.
Family support	N.S.	N.S.
With whom defendant lives	N.S.	N.S.
Employment status	N.S.	N.S.
Income level (employed defendants only)	N.S.	N.S.
Occupation	N.S.	N.S.
<u>Criminality</u>		
Current charge	.06	N.S.
Number of prior arrests	.003	.008
Number of prior convictions	.004	.0003
Age at first adult arrest	N.S.	N.S.
<u>Demographic Characteristics</u>		
Age at arrest	N.S.	N.S.
Sex	N.S.	N.S.
<u>Other</u>		
Education	N.S.	N.S.

the comparison of minority versus white defendants shows virtually identical differences within both the experimental and the control group. In each case the groups are different in terms of years of local residence (minorities were local residents for a longer period of time), months at present address (minorities had lived there longer), and number of prior arrests and convictions (minorities had more).

There was one marginally significant difference (.06 level) for the experimental but not the control group: minorities in the experimental group were more likely than whites to have been charged with crimes against persons and less likely than whites to have been charged with economic crimes. This difference should presumably have made minorities in the experimental group somewhat more likely to be detained, when compared with minorities in the control group—the reverse of the actual situation.

On the basis of the comparisons shown in Table 2.7, it does not appear that the disproportionate detention of minority defendants in the control, as compared to the experimental, group can be explained by different characteristics of white versus minority defendants in the two groups. Because post-release outcomes of white versus minority defendants also fail to explain the differential detention rates, we are left with the possibility that program operations in some way influenced release decisions to avoid a bias in terms of ethnicity that would otherwise have existed.

Besides program impact on release decisions, we were interested in studying program effects on the pretrial behavior of released defendants. As shown in Table 2.8, a comparison of the characteristics of released defendants found no statistically significant differences (at the .05

TABLE 2.8
SUMMARY OF COMPARABILITY OF RELEASED DEFENDANTS
IN THE EXPERIMENTAL AND CONTROL GROUPS
(n=252)

CHARACTERISTIC	SIGNIFICANCE LEVEL
<u>Community Ties</u>	
Local residence status	N.S.
Years of local residence	N.S.
Months at present address	N.S.
Marital status	N.S.
Family support	N.S.
With whom defendant lives	N.S.
Employment status	N.S.
Income level (employed defendants only)	N.S.
Occupation	N.S.
<u>Criminality</u>	
Current charge	N.S.
Number of prior arrests	N.S.
Number of prior convictions	N.S.
Age at first adult arrest	N.S.
<u>Demographic Characteristics</u>	
Age at arrest	N.S.
Ethnicity	N.S.
Sex	N.S.
<u>Other</u>	
Education	N.S.

level) between the experimental and control groups (see Appendix E for details). Table 2.9 compares the released defendants in the two groups in terms of rates of failure to appear, pretrial rearrest and conviction for pretrial rearrest. There were no statistically significant differences between the two groups. Rates of FTA were 8%-10%; of pretrial rearrest, 10%-12%; and of conviction for pretrial rearrest 6%-7%.

Another topic of interest is the effect that particular program recommendations have on release decisions and post-release defendant behavior. Data related to this issue appear in Table 2.10. As shown, a program recommendation for own recognizance (O.R.) release had a major impact on the likelihood both that a defendant would be released (96.5% of defendants with O.R. recommendations were released, as compared with 78% of defendants with other recommendations) and that, if released, the type of release would be nonfinancial (94.5% of released defendants who had O.R. recommendations were released on nonfinancial conditions, as compared with 75% of released defendants with other recommendations).

The type of program recommendation was not associated with different rates of failure to appear or pretrial criminality for those defendants who secured release. This suggests that program recommendation criteria are not good predictors of pretrial misconduct, since released defendants who were considered worse risks performed no worse than other defendants. It should be noted, however, that the rating system was associated with higher detention rates for defendants considered poorer risks; a comparison of released defendants cannot assess whether those detained would have had worse outcomes than those released. Nevertheless, if the program recommendations were in fact accurate assessments of probable defendant behavior after release, then this accuracy should be reflected

TABLE 2.9
FAILURE TO APPEAR AND PRETRIAL CRIMINALITY
FOR EXPERIMENTAL AND CONTROL GROUPS

OUTCOME	EXPERIMENTAL GROUP (n=112)		CONTROL GROUP (n=140)	
	Number	Percent	Number	Percent
<u>Failure to Appear (FTA)</u>				
Defendants who FTA	11	10%	11	8%
Defendants who do <u>not</u> FTA	101	90%	129	92%
TOTAL released defendants	112	100%	140	100%
<u>Pretrial Criminality</u>				
Defendants with rearrests	11	10%	17	12%
Defendants <u>without</u> rearrests	101	90%	123	88%
TOTAL released defendants	112	100%	140	100%
Defendants with rearrest convictions	7	6%	10	7%
Defendants <u>without</u> rearrest convictions	105	94%	130	93%
TOTAL released defendants	112	100%	140	100%

TABLE 2.10
OUTCOMES BY PROGRAM RECOMMENDATION
(EXPERIMENTAL GROUP ONLY)

OUTCOME	OWN RECOGNIZANCE (n=57)		OTHER (n=73)	
	Number	Percent	Number	Percent
<u>Rate of Release^a</u>				
Defendants released	55	96.5%	57	78%
Defendants <u>not</u> released	2	3.5%	16	22%
TOTAL	57	100.0%	73	100%
<u>Type of Release^b</u>				
Nonfinancial	52	94.5%	42	75%
Financial	3	5.5%	14	25%
TOTAL	55	100.0%	56	100%
<u>Failure to Appear (FTA)</u>				
Defendants who FTA	6	11%	5	9%
Defendants who do <u>not</u> FTA	49	89%	52	91%
TOTAL released defendants	55	100%	57	100%
<u>Pretrial Criminality</u>				
Defendants with rearrests	7	13%	4	7%
Defendants <u>without</u> rearrests	48	87%	53	93%
TOTAL released defendants	55	100%	57	100%
Defendants with rearrest convictions	5	9%	2	3.5%
Defendants <u>without</u> rearrest convictions	50	91%	55	96.5%
TOTAL released defendants	55	100%	57	100.0%

^aSignificant at the .006 level.

^bSignificant at the .005 level.

when only released defendants are considered. Such accuracy was not found.

Although the findings regarding program recommendation impact are not encouraging with regard to the program's ability to identify defendants who are likely to fail to appear or to be rearrested, it should be noted that the higher release rates associated with an O.R. recommendation did not result in higher rates of pretrial misconduct. Thus, the release of more defendants, and on nonfinancial conditions, did not lead to any increased disruption of court operations or decreased levels of public safety.

C. Comparison With Cross-Sectional Findings

Because Tucson was included in the cross-sectional study (see Volume I), it is possible to compare the experimental findings with those of the earlier analysis. One concern in such a comparison is whether the characteristics of the defendants in the experimental study were roughly comparable to those of defendants in the cross-sectional study. Although some differences might be expected because of the different time periods involved (the experimental study covered the period November 1978—March 1979 and the cross sectional analysis, calendar year 1977), many differences would suggest that the experimental group was a special sub-section of Tucson arrestees rather than representative of the entire population served by the felony program. Because the experiment included only "late arrests," the possibility of such a bias existed, even though local program and other criminal justice system officials thought it was unlikely.

When the felony defendants in the cross-sectional and experimental studies were compared, they were comparable for most characteristics. However, in the cross-sectional study there were fewer defendants who

supported a nuclear family. Additionally, defendants in the cross-sectional study had longer prior records than the experimental defendants. Finally, the groups differed when a six-way charge categorization was considered, although they were comparable when only a Part I/Part II charge classification was used. Defendants in the cross-sectional study were more likely to have been arrested for drug crimes as well as crimes against public order and persons; they were less likely to have been arrested for economic crimes, crimes against public morality or other crimes. The fact that drug crimes tend to follow an epidemic curve suggests that the charge differences are due more to differences across the time periods studied than to an unrepresentative experimental sample.

Table 2.11 compares the outcomes for the felony defendants in the cross-sectional and experimental studies. The major difference is the sharply higher rates of pretrial rearrest and of conviction for pretrial rearrests during the time period of the cross-sectional study.

A related concern was whether criminal justice system officials in the Tucson area would change their normal procedures as a result of awareness that an experiment was underway. To assess this possibility, we conducted followup interviews with selected criminal justice system officials after the experiment ended. Most of the questions asked them sought their perceptions of changes that other CJS officials might have made during the experimental period. A short summary of the findings appears in Appendix F.

Of particular interest was the possible impact of the new determinate sentencing legislation on failure to appear rates. There had been some speculation among local CJS officials that the prospect of harsher sentences

TABLE 2.11
COMPARISON OF OUTCOMES FOR
FELONY DEFENDANTS IN CROSS-SECTIONAL
AND EXPERIMENTAL STUDIES

OUTCOME	CROSS-SECTIONAL STUDY GROUP (n=175)	EXPERIMENTAL GROUP (n=130)	CONTROL GROUP (n=165)
Rate of Release	82%	86%	85%
Speed of Release	3.7 days	6.6 days	3.9 days
Rate of Nonfinancial Release	80%	85%	81%
Failure-to-Appear Rate	10%	10%	8%
Pretrial Rearrest Rate	27%	10%	12%
Pretrial Conviction For Rearrest Rate	20%	6%	7%

might encourage more defendants to flee. This did not occur, in part because during the period of the experiment the prosecutor rarely invoked the new legal provisions available.

We did not identify any concerted efforts to undermine the experiment; indeed some of the magistrates reported that they were unaware that a felony-level experiment was in progress. Some magistrates did, however, say that they attempted to obtain information from defendants about their backgrounds, when no such information had been provided to them, and to verify any unverified information (e.g., by questioning the defendant about related topics or questioning any of the defendant's friends or relatives who were in court that day). Also, the program director thought that the public defenders had attempted to verify unverified information and had engaged in special efforts to secure release for defendants in the control group. These actions were made possible, according to the program director, because of the low volume of defendants in the control group each day and could not have been undertaken for all arrestees. Finally, the program director thought that the felony program had had a "halo" effect on the control group, so that overall release decisions were more lenient than they would have been had the program not existed at all.

Any of these reported activities would, if widespread, have served to reduce the outcomes differences between the experimental and control groups. Although it does not seem that these activities were in fact widespread, the possibility that they were responsible for the general lack of outcomes differences between the experimental and control groups cannot be completely discounted.

D. Cost-Effectiveness of Program

It is difficult to provide an accurate assessment of the cost-

effectiveness of any pretrial release program, because of the difficulties in deciding which of many possible costs and benefits to include in the analysis and the further problems posed by trying to develop reasonable monetary estimates of those costs and benefits. Despite these difficulties, we decided—as discussed earlier—to try to develop at least rough estimates of program cost-effectiveness, because of the great interest in this topic.

At one level the Tucson felony program's cost-effectiveness during the experimental period could be assessed by noting that its major apparent effect on outcomes, as demonstrated in the data presented earlier, was to increase the equity of the release decisions made for defendants of different ethnicity. Thus, to judge the program as cost-effective would require that the increase in the equity of release be valued at a sum at least as great as the cost of program operations. Because reasonable people will differ on the value that should be placed on equity of release, this approach to cost-effectiveness estimation is inconclusive.

A second approach is the one documented in detail in Appendix B. It focuses on the costs the criminal justice system incurs when a program operates, as compared with when it does not. The least-cost situation is then considered the most desirable. Besides the costs of program operations, the costs of detention, failure to appear and pretrial criminality are estimated.

Table 2.12 shows the various costs for the program (experimental) group in the Tucson felony experiment. Because Appendix B presents the detailed basis for the unit cost estimates shown, we will not discuss them here. Most of the items shown in Table 2.12 are self-explanatory. Consequently, we will make only a few brief comments about a few of the items where the basis of the calculations may not be apparent immediately.

TABLE 2.12
COST SUMMARY, PROGRAM GROUP,
TUCSON FELONY EXPERIMENT
(n=130)

ITEM	COST
<u>Detention</u>	
2,031 days @ \$10 per day	\$20,310.00
<u>Failure To Appear (FTA)</u>	
• 14 occurrences (none at trial)	
—Court and County Attorney costs, 14 cases @ \$6.22	87.08
—Public Defender costs, 12 cases @ 35c court costs plus \$1.84 location costs	26.28
—Bench warrant issued, 9 cases @ \$1.87	16.83
—Bench warrant service, 1 attempt @ \$63.70	63.70
• 9 returns to court	
—Court and County Attorney costs, 9 cases @ \$24.15	217.35
—Public Defender costs, 7 cases @ \$10.58	74.06
—Bail hearings, 1 @ \$31.40	31.40
—No prosecution of FTA; no detention resulting from FTA; no bond forfeitures ordered	
Subtotal, FTA costs	\$516.70
<u>Pretrial Arrest (PTA)</u>	
• Apprehension, 17 cases @ \$16.00	\$272.00
• Booking, 17 cases @ \$8.79	149.43
• Program costs	
—3 interviews @ \$24.72	74.16
—1 supervised release @ \$176.62	176.62
• No detention resulting from PTA	
• Court processing costs	
—Detailed in Table 2.13	5,880.00
—No jury costs	
• Sentencing costs	
—Pre-Sentence Investigations: 3 convicted defendants, Superior Court; weighted, 1.5 @ \$106	159.00
6 convicted defendants, City Court; weighted, 3.67 @ \$50	183.50
—Probation Supervision: 2 defendants, Superior Court, 5 years @ \$242 per year	1,210.00
—Incarceration: 1 defendant, 12 months @ \$300 per month	3,600.00
—Less fines for 5 defendants (assumed paid)	-429.00
Subtotal, PTA costs	\$11,275.71
<u>Other</u>	
• Fees from deposit bonds	-230.00
<u>Program</u>	
• Interview and verification, 130 defendants @ \$24.72	3,213.60
• Tracking, 94 defendants @ \$53.96	5,072.24
• No supervised releasees	
Subtotal, Program costs	\$8,285.84
TOTAL COSTS	\$40,158.25

TABLE 2.13
COURT PROCESSING COSTS FOR PRETRIAL ARRESTS,
PROGRAM GROUP, TUCSON FELONY EXPERIMENT

COURT	OUTCOME	ATTORNEY	TOTAL NUMBER OF CASES	NUMBER OF CASES ADJUDICATED ON SAME DAY AS:		COST
				ORIGINAL ARREST	OTHER PTA	
Justice	Dismissal	Public Defender	1	0	0	1 @ \$91 = \$ 91
City	Dismissal	Court-Appointed	1	0	1	½ @ \$25 = 12.50
City	Plea	Court-Appointed	5	0	1	4½ @ \$33 = 148.50
City	Convicted by Judge	Court-Appointed	1	0	0	1 @ \$128 = 128
Superior	Dismissal	Public Defender	4	2	0	3 @ \$496 = 1,488
Superior	Plea	Public Defender	2	1	0	1½ @ \$1,488 = 2,232
Superior	Plea	Private	1	0	0	1 @ \$1,020 = 1,020
Superior	Dismissal	Court-Appointed	1	0	0	1 @ \$590 = 590
Superior	Dismissal	Private	1	1	0	½ @ \$340 = 170
						TOTAL COST = \$5,880.00

Detention includes all detention by all defendants in the program group. This includes any detention of defendants who were eventually released, as well as the time spent in jail by persons detained until the final dispositions of their cases.

Failure to appear (FTA) costs include costs associated with the initial occurrence of FTA, as well as costs incurred when defendants returned to court. Similarly, the costs of pretrial arrest (PTA) had several components: apprehension, booking, program costs if defendants were re-processed, detention (if any), court processing costs and sentencing costs. In the calculation of court processing costs, we usually assumed that the defendant's type of legal representation was the same for the pretrial arrest as for the original arrest.⁶ Additionally, as Table 2.13 indicates, when cases were settled on the same day, court processing costs were distributed among the pretrial arrest(s) and original case. For example, if one pretrial arrest and the original case were settled on the same day, one-half the processing costs were allocated to the pretrial arrest. Similarly, if two pretrial arrests and the original case were adjudicated jointly, two-thirds the processing costs were attributed to the pretrial rearrests.

The same types of allocations were made for sentencing costs. For example, if a defendant was sentenced to imprisonment of 36 months on the original charge and an additional 36 months on the pretrial rearrest, with the sentences to run concurrently, then 18 months of incarceration costs were allocated to the pretrial rearrest. The same approach was used to allocate probation terms and costs of pre-sentence investigations as well.

⁶ The one exception was when a defendant had a public defender for an original arrest tried in Superior Court, but was rearrested for a charge under the jurisdiction of the City Court, where public defenders are not used. In those cases, court-appointed attorneys were assumed to be the type of representation.

As shown in Table 2.12, total costs associated with the program group were about \$40,000, with approximately half of this sum due to detention costs, followed by pretrial arrest and program costs. Failure to appear costs were comparatively low, totaling less than \$1,000; this is largely due to the fact that responses to FTA were not very costly ones for the criminal justice system to implement (e.g., FTA was not prosecuted, nor did detention result from it). The pretrial arrest costs were largely due to the court processing costs involved and to the sentences imposed on defendants convicted of pretrial arrest charges. More than one-half of the total pretrial arrest cost was due to court processing expenses and about one-third was accounted for by the costs of incarcerating one defendant for one year.

Tables 2.14 and 2.15 summarize the costs for the control group. These costs were estimated in the same way as for the program group. One item which may need further explanation is the inclusion of interview costs (the last entry in Table 2.14). This was done because Arizona law requires that certain information be provided to the magistrate making release decisions. Thus, even if there were no program, the costs of interviewing defendants to acquire this information would still be incurred by the criminal justice system.

As indicated in Table 2.14, costs for the control group were about \$145,000. Approximately 80% of these costs were due to the costs of pretrial arrest, with most of those costs due in turn to the incarceration of 6 defendants for a combined total of more than 25 years. Detention was the next most costly item, accounting for about 17% of total costs. Interview costs were a little over 1%; FTA costs were again the smallest of the four major cost categories, totaling less than \$1,000.

TABLE 2.14
COST SUMMARY, CONTROL GROUP,
TUCSON FELONY EXPERIMENT
(n=165)

ITEM	COST
<u>Detention</u>	
2,518 days @ \$10 per day	\$ 25,180.00
<u>Failure To Appear (FTA)</u>	
• 14 occurrences (none at trial)	
—Court and County Attorney costs, 14 cases @ \$6.22	87.08
—Public Defender costs, 12 cases @ 35¢ court costs plus \$1.84 location costs	26.28
—Bench warrant issued, 9 cases @ \$1.87	16.83
—Bench warrant served, 2 cases @ \$63.70 for service and \$9.00 for booking	145.40
• 8 returns to court	
—Court and County Attorney costs, 8 cases @ \$24.15	193.20
—Public Defender costs, 7 cases @ \$10.58	74.06
—Bail hearings, 3 @ \$31.40	94.20
—No prosecution of FTA; no detention resulting from FTA; no bond forfeitures ordered	
Subtotal, FTA costs	\$637.05
<u>Pretrial Arrest (PTA)</u>	
• Apprehension, 28 cases @ \$16	\$ 448.00
• Booking, 28 cases @ \$8.79	246.12
• Program Costs	
—7 interviews (no verification) @ \$12.36	86.52
• Detention resulting from PTA, 554 days @ \$10	5,540.00
• Court processing costs	
—Detailed in Table 2.15	19,023.25
—No jury costs	
• Sentencing costs	
—Pre-Sentence Investigations: 8 convicted defendants, Superior Court; weighted, 5 @ \$106	530.00
7 convicted defendants, City and Justice Courts; weighted, 2.5 @ \$4	10.00
—Probation supervision: 1 defendant, Superior Court, 4 years @ \$242 per year	968.00
—Incarceration: 6 defendants, 303 months* @ \$300 per month	90,900.00
—Less fines for 7 defendants (assumed paid)	—551.00
Subtotal, PTA costs	\$117,200.89
<u>Other</u>	
• Fees from deposit bonds	-415.00
<u>Interviews</u>	
• 165 defendants @ \$12.36	2,039.40
TOTAL COSTS	\$144,642.34
* Includes one defendant sentenced to 126 months.	

TABLE 2.15
COURT PROCESSING COSTS FOR PRETRIAL ARRESTS,
CONTROL GROUP, TUCSON FELONY EXPERIMENT

COURT	OUTCOME	ATTORNEY	TOTAL NUMBER OF CASES	NUMBER OF CASES ADJUDICATED ON SAME DAY AS		COST
				ORIGINAL ARREST	OTHER PTA	
Justice.	Dismissal	Public Defender	3*	1*	2*	3/4 @ \$91 = \$68.25
Justice	Plea	Public Defender	1	1	0	1/2 @ \$272 = \$136
City	Dismissal	Court-Appointed	2	0	0	2 @ \$25 = \$50
City	Plea	Court-Appointed	6	0	2	5 @ \$33 = \$165
Superior	Dismissal	Public Defender	7	0	0	7 @ \$496 = \$3,472
Superior	Plea	Public Defender	8	2	0	7 @ \$1,488 = \$10,416
Superior	Acquitted by Judge	Public Defender	1	0	0	1 @ \$4,716 = \$ 4,716
						TOTAL COST = \$19,023.25
* Same defendant; original charge and PTA's were adjudicated on same day, so 3/4 of processing cost was allocated to PTA.						

The cost data presented above for the program and control groups cannot be directly compared for two reasons. First, there are more defendants in the control than in the program group; this alone should cause control group costs to be higher. Second, the pretrial arrest costs for the two groups were based on the number of arrests actually experienced by them. This number was larger for the control group than for the program group. However, statistical testing of this difference found it insignificant. Thus, the pretrial arrest costs are somewhat inflated for the control group. Similar considerations apply to failure to appear and detention, although the cost differences are not so great in these instances.

Table 2.16 presents the results of weighting the cost data first to account for different group sizes and then to reflect equivalence in terms of extent of detention, failure to appear and pretrial arrest. As shown, costs for the control group still exceed those for the program group by a margin of almost 3-to-1. The difference is due primarily to the costs of pretrial arrest in the two groups, with the control group receiving more severe sentences, with greater associated costs for the criminal justice system. Because of the small number of cases with convictions in each group (9 cases, involving 7 defendants, in the program group; and 15 cases, involving 10 defendants, in the control group), it is possible that this difference is not a real one and that a repetition of the experiment would find the cost situations reversed. Only a few defendants would need to switch groups to change the pretrial cost results.

If the finding is in fact valid, there are two possible explanations for it. First, even though the program does not have a statistically significant impact on the pretrial arrest rate, as shown in Table 2.9,

TABLE 2.16
COMPARISON OF COSTS FOR PROGRAM AND CONTROL GROUPS,
TUCSON FELONY EXPERIMENT

COST CATEGORY	PROGRAM GROUP		CONTROL GROUP	
	Quantity	Cost	Quantity	Cost
<u>Unweighted</u>				
Detention	2,031 days	\$ 20,310	2,518 days	\$ 25,180
Failure to Appear	14 cases	516	14 cases	637
Pretrial Arrest	17 cases	11,276	28 cases	117,201
Other	N.A.	- 230	N.A.	- 415
Program	N.A.	8,286	N.A.	2,039
TOTAL		\$ 40,158		\$144,642
NUMBER OF DEFENDANTS	130		165	
<u>Weighted To Reflect Equal Group Sizes</u>				
Detention	2,579 days	\$ 25,794	2,518 days	\$ 25,180
Failure to Appear	18 cases	657	14 cases	637
Pretrial Arrest	22 cases	14,321	28 cases	117,201
Other	N.A.	- 292	N.A.	- 415
Program	N.A.	10,523	N.A.	2,039
TOTAL		\$ 51,003		\$144,642
NUMBER OF DEFENDANTS	165		165	
<u>Weighted To Reflect Equal Detention, FTA and PTA</u>				
Detention	2,579 days	\$ 25,794	2,579 days	\$ 25,784
Failure to Appear	18 cases	657	18 cases	822
Pretrial Arrest	28 cases	18,188	28 cases	117,201
Other	N.A.	- 292	N.A.	- 415
Program	N.A.	10,523	N.A.	2,039
TOTAL		\$ 54,870		\$145,431
NUMBER OF DEFENDANTS	165		165	

it may influence the release process in such a way that defendants who commit less serious offenses (as measured by the severity of the punishments imposed for those offenses) are released.⁷ For example, the program may select for own recognizance release recommendations defendants who pose lesser risks of committing serious offenses. Alternatively, the program may affect the way the criminal justice system responds to pre-trial arrests, so that defendants processed by the program are treated less harshly than other defendants.

We cannot determine from the available data which of these possible explanations is more likely to be correct. If it is the former, then it suggests that the program is having a major impact on the nature, though not the extent, of pretrial criminality. If the latter is the better explanation, it suggests that the lack of program processing might have an inequitable effect on the sentencing of defendants convicted of pre-trial arrests. In either case the finding, if real, has important implications. For this reason the topic may deserve further study.

⁷Note that this occurs even though there is no difference in the overall charge distribution for released defendants in the program versus control groups, as shown in Table 2.8. Additionally, remember that the experiment tested the impact of program verification and recommendation on outcomes; information on defendants' backgrounds was provided to the court for both the program and control groups.

III. THE TUCSON MISDEMEANOR EXPERIMENT

A. Background

A pretrial release program operated at the misdemeanor level in Tucson for about two-and-one-half years (from January 1975 to July 1977) before local budgetary problems led to its demise (see the "defunct program" analyses in Volume III for additional information).⁸ Some of the staff members, including the director, of the former program were still in contact with the Correctional Volunteer Center (CVC) at the time we were discussing experimental possibilities for the area.

Although we had not wished to implement our experimental analyses by establishing new programs, because of the special difficulties usually faced by new programs and the possibility that these difficulties might bias the findings, Tucson offered a unique situation: a misdemeanor level program could be re-established, a little over a year after its demise, using some of the same staff as before. Additionally, the program could operate under the CVC's umbrella, so that the problems of developing program infrastructure (e.g., finding office space, developing needed relationships, etc.) could be avoided. Thus, the potential drawbacks of establishing a new program seemed offset in the specific Tucson situation.

The misdemeanor experiment involved the random assignment of defendants both before and after release decisions were made. All defendants under the jurisdiction of the City Court of Tucson, except persons charged with such relatively minor offenses as jaywalking and trespassing, were

⁸See also Kristina Peterson, Delivery System Analysis of Pima County (Tucson), Arizona, Working Paper No. 6, March 1979.

interviewed by program staff. For defendants in the experimental group, the staff verified the interview information and prepared a release recommendation for the court. For control group defendants, no information was given to the court by program staff; the completed interviews for these persons were simply sent to us for inclusion in the data base.

After release decisions were made, all released defendants were again randomly assigned to two groups: one that received program followup and one that did not. The followup consisted of the program's notifying the defendants of coming court dates. This notification was accomplished by mail or telephone, in English or Spanish.

Appendix D includes the instructions provided to program staff to assist them in implementing the experiment. The next section presents the results of that implementation.

B. Outcomes of Experiment

Tables 3.1 and 3.2 consider the comparability of the experimental and control groups. As shown in Table 3.1, data were available for more than half the defendants in each group for 18 of the 19 characteristics listed; data were incomplete only for public assistance status. Table 3.2 shows that the two groups were comparable for each of these 18 characteristics (see detailed data in Appendix E).

Release outcome data are presented in Table 3.3. As was the case for the felony study, there were no statistically significant differences between the experimental and control groups in terms of rate, speed or type of release.⁹ Additionally, the distribution of bond amounts did not differ significantly for the two groups (see data in Appendix E). Nor did the two groups differ in terms of rates of release when detention of one day or less was included with the released, rather than the detained, defendant group.

Tables 3.4, 3.5, and 3.6 consider the equity of release by ethnicity and employment status for the two groups. Again, as in the felony study, unemployed defendants in both groups were much less likely to secure release. In the experimental group, 50% of the unemployed defendants were released, as compared with 77% of the employed defendants; in the control group, the percentages were 58% and 74%, respectively. Unlike the felony study, where unemployed released defendants in the control group were found more likely to be rearrested pretrial, there were no such differences in the

⁹Unlike the felony study, no information on defendants' backgrounds was presented to magistrates for defendants in the control group.

TABLE 3.1
PERCENTAGES OF CASES IN EXPERIMENTAL AND CONTROL
GROUPS WITH DATA FOR SELECTED CHARACTERISTICS

CHARACTERISTIC	EXPERIMENTAL GROUP (n=224)	CONTROL GROUP (n=200)
<u>Community Ties</u>		
Local residence status	100%	100%
Years of local residence	84%	81%
Months at present address	92%	89%
Marital status	94%	96%
Family support	95%	92%
With whom defendant lives	99%	97%
Employment status	99%	99%
Income level (employed defendants only)	87%	86%
Public assistance	48%	46%
Occupation	81%	85%
<u>Criminality</u>		
Current charge	100%	100%
Number of prior arrests	100% ^a	100% ^a
Number of prior convictions	100% ^a	100% ^a
Criminal justice system status at time of arrest	64%	62%
Age at first adult arrest	65%	61%
<u>Demographic Characteristics</u>		
Age at arrest	100%	100%
Ethnicity	94%	96%
Sex	100%	100%
<u>Other</u>		
Education	83%	81%
^a Includes cases with missing information.		

TABLE 3.2
SUMMARY OF COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS
(n=424)

CHARACTERISTIC	SIGNIFICANCE LEVEL
<u>Community Ties</u>	
Local residence status	N.S.*
Years of local residence	N.S.
Months at present address	N.S.
Marital status	N.S.
Family support	N.S.
With whom defendant lives	N.S.
Employment status	N.S.
Income level (employed defendants only)	N.S.
Occupation	N.S.
<u>Criminality</u>	
Current charge	N.S.
Number of prior arrests	N.S.
Number of prior convictions	N.S.
Criminal justice system status at time of arrest	N.S.
Age at first adult arrest	N.S.
<u>Demographic Characteristics</u>	
Age at arrest	N.S.
Ethnicity	N.S.
Sex	N.S.
<u>Other</u>	
Education	N.S.
*Indicates the two groups were not significantly different (.05 statistical level.	

TABLE 3.3
RATE, SPEED AND TYPE OF RELEASE FOR
EXPERIMENTAL AND CONTROL GROUPS

OUTCOME	EXPERIMENTAL GROUP (n=224)		CONTROL GROUP (n=200)	
	Number	Percent	Number	Percent
<u>Rate of Release</u>				
Defendants released	152	68%	136	68%
Defendants <u>not</u> released	72	32%	64	32%
TOTAL	224	100%	200	100%
<u>Speed of Release</u>				
Mean number of days from arrest to release <u>d</u> /	0.64		0.67	
<u>Type of Release</u>				
Nonfinancial	106	70%	99	73%
Financial	46	30%	37	27%
TOTAL	152	100%	136	100%
^a n=139 for experimental group and n=125 for control group				

TABLE 3.4
EQUITY OF RELEASE FOR EXPERIMENTAL GROUP
(n=224)

OUTCOME	ETHNICITY				EMPLOYMENT STATUS			
	WHITE		MINORITY		EMPLOYED OR SUBSTITUTES		UNEMPLOYED	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
<u>Rate of Release (all defendants)</u> ^a								
Defendants released	64	59%	88	76%	113	77%	38	50%
Defendants <u>not</u> released	44	41%	28	24%	33	23%	38	50%
TOTAL	108	100%	116	100%	146	100%	76	100%
<u>Speed of Release (released defendants)</u>								
Mean number of days from arrest to release ^b	0.5 days		2.1 days		0.5 days		1.0 days	
<u>Type of Release (for released defendants)</u>								
Nonfinancial	50	78%	56	64%	79	70%	27	71%
Financial	14	22%	32	36%	34	30%	11	29%
TOTAL	64	100%	88	100%	113	100%	38	100%
^a Significant at the .01 level for ethnicity and at the .0001 level for employment status. ^b n=60 for white, n=80 for minority, n=102 for employed, n=36 for unemployed.								

TABLE 3.5
EQUITY OF RELEASE FOR CONTROL GROUP
(n=200)

OUTCOME	ETHNICITY				EMPLOYMENT STATUS			
	WHITE		MINORITY		EMPLOYED OR SUBSTITUTES		UNEMPLOYED	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
<u>Rate of Release (all defendants)</u> ^a								
Defendants released	72	66%	64	71%	96	74%	40	58%
Defendants <u>not</u> released	38	34%	26	29%	33	26%	29	42%
TOTAL	110	100%	90	100%	129	100%	69	100%
<u>Speed of Release (released defendants)</u>								
Mean number of days from arrest to release ^b	0.6 days		0.8 days		0.5 days		1.1 days	
<u>Type of Release (for released defendants)</u>								
Nonfinancial	53	74%	46	72%	72	75%	27	68%
Financial	19	26%	18	28%	24	25%	13	32%
TOTAL	72	100%	64	100%	96	100%	40	100%
^a Significant at the .03 level for employment status; not significant for ethnicity.								
^b n=67 for white, n=58 for minority, n=87 for employed, n=38 for unemployed.								

TABLE 3.6
SUMMARY OF EQUITY OF RELEASE FOR
EXPERIMENTAL AND CONTROL GROUPS

OUTCOME	EXPERIMENTAL GROUP (n=224)		CONTROL GROUP (n=200)	
	Ethnicity	Employment Status	Ethnicity	Employment Status
Rate of Release	.01	.0001	N.S.	.03
Speed of Release	N.S.	N.S.	N.S.	N.S.
Type of Release	N.S.	N.S.	N.S.	N.S.

misdemeanor study in post-release outcomes for unemployed versus employed defendants who secured release.

The equity analysis for misdemeanors also shows a difference by ethnicity. Minority defendants in the experimental group were much more likely than white defendants to secure release. To assess whether this difference could reasonably be attributed to program impact, we analyzed the characteristics of white versus minority defendants in the experimental group, as compared with the control group. Results appear in Table 3.7. As indicated, there are six characteristics along which white versus minority defendants differ significantly in either the experimental or control group but not in both. Minority defendants in the control group had more prior arrests and convictions than white defendants; in the experimental group, prior records of minority and white defendants were comparable. Minority defendants in the experimental group, though not in the control group, were more likely than white defendants to be local residents, to be married, to have the occupation of a laborer and to be employed.

Because of the many differences between the experimental and control groups when minority and white defendants are compared, it is possible that the higher release rate for minority defendants in the experimental group is due to these differences, rather than to program impact. Indeed, since unemployed defendants were much less likely to be released, the fact that more minority than white defendants were employed in the experimental group (but not in the control group) could itself account for the release rate difference. This seems to be the case, because when we controlled for employment status, there was no statistically

TABLE 3.7
COMPARABILITY OF WHITE VERSUS MINORITY
DEFENDANTS IN EXPERIMENTAL AND CONTROL GROUPS

CHARACTERISTIC	EXPERIMENTAL GROUP (n=224)	CONTROL GROUP (n=200)
<u>Community Ties</u>		
Local residence status	.004	N.S.
Years of local residence	.003	.0000
Months at present address	.006	.002
Marital status	.02	N.S.
Family support	.0002	.0002
With whom defendant lives	.0002	.002
Employment status	.0005	N.S.
Income level (employed defendants only)	N.S.	N.S.
Occupation	.02	N.S.
<u>Criminality</u>		
Current charge	N.S.	N.S.
Number of prior arrests	N.S.	.0006
Number of prior convictions	N.S.	.0001
Criminal justice system status at time of arrest	N.S.	N.S.
Age at first adult arrest	N.S.	N.S.
<u>Demographic Characteristics</u>		
Age at arrest	N.S.	N.S.
Sex	N.S.	N.S.
<u>Other</u>		
Education	.005	.02

significant difference in release rates by ethnicity.¹⁰

Table 3.8 shows the rate and type of release for defendants in the experimental group who received own recognizance (O.R.) versus other recommendations. As in the felony study, program recommendations had a strong effect on release outcomes: 76.5% of the defendants with an O.R. recommendation secured release, as compared with 55% of other defendants. Moreover, of the defendants with an O.R. recommendation, 80% of those released were released on nonfinancial conditions; the comparable percentage for defendants lacking an O.R. recommendation was 49%.¹¹

As discussed in Section A, the Tucson misdemeanor experiment included a second random assignment of defendants after release: one group received program notification of coming court dates, and the other group did not. Table 3.9 shows that the two groups of defendants were comparable for all 18 of the characteristics previously considered (see Appendix E for details). Table 3.10 presents the failure to appear and pretrial criminality outcomes for the two groups. There were no statistically significant differences in outcomes between the groups.¹² Thus, it appears that the misdemeanor program's notification activities had no effect on the rate at which defendants failed to appear or were rearrested during the pretrial period.

¹⁰ However, the significance level for employed defendants was .09, reasonably close to our .05 cutoff point. The level for unemployed defendants, on the other hand, was .48.

¹¹ Failure to appear and pretrial criminality outcomes are not compared by program recommendation, because of the post-release assignment of defendants to groups that either received program followup or did not.

¹² We also separately analyzed the outcomes for the experimental and control groups of the first random assignment. In each case the defendants notified were comparable, and their failure to appear and pretrial criminality rates were not significantly different.

TABLE 3.8
RELEASE OUTCOMES BY PROGRAM RECOMMENDATION
(EXPERIMENTAL GROUP ONLY)

OUTCOME	OWN RECOGNIZANCE (n=132)		OTHER (n=92)	
	Number	Percent	Number	Percent
<u>Rate of Release</u> ^a				
Defendants released	101	76.5%	51	55%
Defendants <u>not</u> released	31	23.5%	41	45%
TOTAL	132	100.0%	92	100%
<u>Type of Release</u> ^b				
Nonfinancial	81	80%	25	49%
Financial	20	20%	26	51%
TOTAL	101	100%	51	100%
^a Significant at the .002 level.				
^b Significant at the .001 level.				

TABLE 3.9
SUMMARY OF COMPARABILITY OF
EXPERIMENTAL AND CONTROL GROUPS FOR NOTIFICATION ANALYSIS
(n=287)

CHARACTERISTIC	SIGNIFICANCE LEVEL
<u>Community Ties</u>	
Local residence status	N.S.
Years of local residence	N.S.
Months at present address	N.S.
Marital status	N.S.
Family support	N.S.
With whom defendant lives	N.S.
Employment status	N.S.
Income level (employed defendants only)	N.S.
Occupation	N.S.
<u>Criminality</u>	
Current charge	N.S.
Number of prior arrests	N.S.
Number of prior convictions	N.S.
Criminal justice system status at time of arrest	N.S.
Age at first adult arrest	N.S.
<u>Demographic Characteristics</u>	
Age at arrest	N.S.
Ethnicity	N.S.
Sex	N.S.
<u>Other</u>	
Education	N.S.

CONTINUED

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TABLE 3.10
FAILURE TO APPEAR AND PRETRIAL CRIMINALITY
FOR EXPERIMENTAL AND CONTROL GROUPS

OUTCOME	EXPERIMENTAL GROUP (n=160)		CONTROL GROUP (n=127)	
	Number	Percent	Number	Percent
<u>Failure to Appear (FTA)</u>				
Defendants who FTA	19	12%	15	12%
Defendants who do <u>not</u> FTA	141	88%	112	88%
TOTAL released defendants	160	100%	127	100%
<u>Pretrial Criminality</u>				
Defendants with rearrests	9	6%	6	5%
Defendants <u>without</u> rearrests	151	94%	121	95%
TOTAL released defendants	160	100%	127	100%
Defendants with rearrest convictions	5	3%	3	2%
Defendants <u>without</u> rearrest convictions	155	97%	124	98%
TOTAL released defendants	160	100%	127	100%

C. Comparison With Cross-Sectional Findings

The experimental findings for Tucson misdemeanors can be compared with those of the cross-sectional study, covering a period about two years earlier. Because Tucson misdemeanor outcomes are discussed in greater detail in Volume III, only a brief comparison is presented in this chapter.

Misdemeanor defendants in the experimental study differed in three respects from those of the cross-sectional study. First, they were arrested for somewhat less serious crimes: fewer of them were charged with Part I crimes; also, fewer were charged with economic crimes and more with crimes against public order. Second, experimental defendants had less extensive prior records. However, they were more likely to have been involved with the criminal justice system at the time of the arrest studied.

Table 3.11 compares the outcomes for the misdemeanor defendants in the cross-sectional and experimental studies. As was the case for felony defendants, the largest difference is the sharply lower rate of pretrial criminality (as measured by either rearrests or convictions for them) during the experimental period. The rate of release also increased over time, and the mean number of days required to secure release declined.

As in the felony study, we were interested in whether local officials behaved differently during the experimental period than they would have ordinarily. Appendix F discusses the results of our followup interviews on this topic. Although there do not appear to have been major efforts to undermine the experiment, it is possible that some "contamination" occurred. For example, the director of the misdemeanor study

TABLE 3.11
COMPARISON OF OUTCOMES FOR MISDEMEANOR DEFENDANTS
IN CROSS-SECTIONAL AND EXPERIMENTAL STUDIES

Outcome	Cross-Sectional Study Group (n = 191)	Experimental Group (n = 224)	Control Group (n = 200)
Rate of Release	59%	68%	68%
Speed of Release	1.18 days	0.64 days	0.67 days
Rate of Nonfinancial Release	67%	70%	73%
Failure to Appear Rate	15%	12%	12%
Pretrial Rearrest Rate	20%	6%	5%
Pretrial Conviction for Rearrest Rate	11%	3%	2%

reported to us that magistrates would sometimes ask program interviewers for information on control group defendants and that the interviewers would provide it. The director did not know how often this occurred; nor do we, since we were not monitoring the study on-site. Thus, it is possible that the lack of outcomes differences for the experimental versus control groups stems partly from departures from the study design.

D. Cost-Effectiveness of Program

At one level the Tucson misdemeanor program's cost-effectiveness can be judged by noting that it had no statistically significant impact on either release outcomes or defendant misconduct after release and concluding that its costs were not offset by any improvements in the outcomes studied. Thus, to consider the program cost-effective would require a judgment that its operations led to intangible benefits that were deemed worth the program costs.

Another way to consider cost-effectiveness is to analyze the costs to the criminal justice system of having the program operate versus not operate. Tables 3.12—3.15 summarize the cost data for the program (experimental) and control groups. Cost estimation details appear in Appendix B.

For the program group total costs were about \$24,000, with approximately three-fourths of the costs attributed to program operations (see Table 3.12). Of the remaining cost categories, detention was the most expensive, followed by pretrial arrest. As with the felony program, failure to appear was the least costly category. Total costs for the control group were about \$3,400, largely accounted for by detention costs (see Table 3.14).

TABLE 3.12
COST SUMMARY, PROGRAM GROUP,
TUCSON MISDEMEANOR EXPERIMENT

ITEM	COST
<u>Detention</u>	
389 days @ \$10 per day	\$3,890.00
<u>Failure To Appear (FTA)</u>	
• 20 occurrences	
—Court and City Attorney costs, 20 cases @ 66¢	13.20
—Bench warrant issued, 18 cases @ 50¢	9.00
—Bench warrant served, 6 cases @ \$18.20 (\$9.20 for service and \$9.00 for booking)	109.20
• 10 returns to court	
—Court and City Attorney costs, 5 voluntary returns @ \$6.43	32.15
—Court and City Attorney costs, 5 returns through arrests @ \$7.04	35.20
—Detention resulting from FTA, 24 days @ \$10 per day	240.00
—No prosecution of FTA; no execution of bond forfeitures	
Subtotal, FTA costs	\$ 438.75
<u>Pretrial Arrest (PTA)</u>	
• Apprehension, 12 cases @ \$16	192.00
• Booking, 12 cases @ \$8.79	105.48
• Program costs	
—2 interviews @ \$30.20	60.40
• Detention resulting from PTA, 78 days @ \$10 per day	780.00
• Court processing costs	
—Detailed in Table 3.13	692.33
—No jury costs	
• Sentencing costs	
—Pre-sentence investigations, 5 convicted defendants; weighted, 4.5 @ \$50	225.00
—Probation supervision, 4 defendants, 5 years @ \$4 per year	20.00
—Incarceration, 4 defendants, 2.5 months @ \$300	750.00
—Less fines for 3 defendants (assumed paid)	-1,009.00
Subtotal, PTA costs	\$1,516.21
<u>Program</u>	
• Interview, 224 defendants @ \$30.20	6,764.80
• Notification, 160 defendants @ \$68.75	11,000.00
Subtotal, Program costs	\$17,764.80
TOTAL COSTS	\$23,909.76

TABLE 3.13
COURT PROCESSING COSTS FOR PRETRIAL ARRESTS,
PROGRAM GROUP, TUCSON MISDEMEANOR EXPERIMENT

COURT	OUTCOME	ATTORNEY	TOTAL NUMBER OF CASES	NUMBER OF ADJUDICATED CASES SETTLED ON SAME DAY AS:		COST
				ORIGINAL ARREST	OTHER PTA	
Justice.	Dismissal	Public Defender	1	0	0	1 @ \$91 = \$91.00
Justice	Plea	Public Defender	1	0	0	1 @ \$272 = \$272.00
City	Dismissal	Other than Public Defender	4	3	1	2 1/3 @ \$25 = \$58.33
City	Plea	Other than Public Defender	4	0	1	3 1/3 @ \$33 = \$110.00
City	Open *	Other than Public Defender	2	0	0	2 @ \$80.50 = \$161.00
						TOTAL COSTS = \$692.33
*Costs of open cases were estimated as one-half the cost of a plea plus one-half the cost of a court trial.						

TABLE 3.14
COST SUMMARY, CONTROL GROUP,
TUCSON MISDEMEANOR EXPERIMENT

Item	Cost
<u>Detention</u>	
266 days @ \$10 per day	\$2,660.00
<u>Failure To Appear</u>	
• 20 occurrences	
—Court and City Attorney costs, 20 cases @ 66¢	13.20
—Bench warrant issued, 18 cases @ 50¢	9.00
—Bench warrant served, 8 cases @ \$18.20 (\$9.20 for service and \$9.00 for booking)	145.60
—Bench warrant service attempted unsuccessfully, 1 case @ \$9.20	9.20
—Less bond forfeiture executed, 1 case @ \$135.00	-135.00
• 13 returns to court	
—Court and City Attorney costs, 7 voluntary returns @ \$6.43	45.01
—Court and City Attorney costs, 6 returns through arrest @ \$7.04	42.24
—Detention resulting from FTA, 11 days @ \$10 per day	110.00
• 2 prosecutions of FTA; 2 cases @ \$8.00	16.00
Subtotal, FTA costs	\$ 255.25
<u>Pretrial Arrest (PTA)</u>	
• Apprehension, 6 cases @ \$16.00	\$ 96.00
• Booking, 6 cases @ \$8.79	52.74
• Detention resulting from PTA, 1 day @ \$10 per day	10.00
• Court processing costs	
—Detailed in Table 3.15	269.00
—No jury costs	
• Sentencing costs	
—Pre-Sentence Investigations, 3 convicted defendants @ \$50	150.00
—Probation supervision, 1 defendant, 1 year @ \$4 per year	4.00
—Incarceration, 1 defendant, 20 days @ \$10 per day	200.00
—Less fines for 3 defendants (assumed paid)	-303.00
Subtotal, PTA costs	\$ 478.74
TOTAL COSTS	\$3,393.99

TABLE 3.15
COURT PROCESSING COSTS FOR PRETRIAL ARRESTS,
CONTROL GROUP, TUCSON MISDEMEANOR EXPERIMENT

NOTE: All pretrial arrests were adjudicated in City Court

Outcome	Total Number of Cases	Number of Cases Adjudicated on Same Day As:		Cost
		Original Arrest	Other Pretrial Arrest	
Dismissal	3	0	0	3 @ \$ 25 = \$ 75
Plea	2	0	0	2 @ \$ 33 = \$ 66
Convicted by Judge	1	0	0	1 @ \$128 = \$128
				TOTAL COST= \$269

As in the case of the felony-level analysis presented in the last chapter, the control group costs cannot be compared directly with the program group costs, because (1) the groups are of different size, and (2) the costs of detention, failure to appear and pretrial rearrest should be weighted to reflect the lack of significant differences between the two groups. Table 3.16 presents the weighted cost comparisons. Because the Tucson misdemeanor experiment was a two-stage test, analyzing release and post-release impact separately, the number of defendants to whom the cost categories apply varies. For detention costs all studied defendants are used, while failure to appear and pretrial arrest costs apply only to the defendants who participated in the notification analysis.

As shown in Table 3.16, costs for the program group are significantly higher than for the control group after the necessary weighting has been accomplished. There are no savings in detention, failure to appear or pretrial arrest expenses to offset the costs of the program. Based on this analysis, neither the pre-release nor post-release program activities were cost-effective.

TABLE 3.16
COMPARISON OF COSTS FOR PROGRAM AND CONTROL GROUPS,
TUCSON MISDEMEANOR EXPERIMENT

Cost Category	Program Group		Control Group	
	Quantity	Cost	Quantity	Cost
<u>Unweighted</u>				
Detention	389 days, 224 defts	\$3,890	266 days, 200 defts	\$2,660
Failure to appear	20 FTA, 160 defts	439	20 FTA, 127 defts	255
Pretrial arrest	12 PTA, 160 defts	1,816	6 PTA, 127 defts	479
Program	N.A.	17,765	N.A.	0
TOTAL		\$23,910		\$3,394
<u>Weighted To Reflect Equal Group Sizes</u>				
Detention	389 days, 224 defts	3,890	298 days, 224 defts	\$2,980
Failure to Appear	20 FTA, 160 defts	439	25 FTA, 160 defts	321
Pretrial arrest	12 PTA, 160 defts	1,816	8 PTA, 160 defts	604
Program	N.A.	17,765	N.A.	0
TOTAL		\$23,910		\$3,905
<u>Weighted To Reflect Equal Detention, FTA & PTA</u>				
Detention	389 days, 224 defts	3,890	389 days, 224 defts	\$3,890
Failure to appear	25 FTA, 160 defts	549	25 FTA, 160 defts	321
Pretrial arrest	12 PTA, 160 defts	1,816	12 PTA, 160 defts	906
Program	N.A.	17,765	N.A.	0
TOTAL		24,020		5,117

IV. THE BALTIMORE CITY EXPERIMENT

A. Background

The Baltimore City pretrial release program interviews virtually all defendants soon after arrest. This is accomplished through around-the-clock, decentralized operations; program staff work in nine districts as well as the agency's downtown headquarters.¹³

As in many major metropolitan areas that have adopted pretrial release reforms, Baltimore had no "overflow" group of defendants who were not interviewed. However, there was a sizeable group of defendants whose point scores were too low for them to receive an own recognizance (O.R.) release recommendation. Consequently, an experiment was designed, using this group of defendants and testing the impact of a program recommendation for O.R. release on subsequent outcomes. This experiment also provided insight about the effects of relaxing the current point system criteria, a topic of special interest to Baltimore officials.

Thus, the Baltimore City experiment differed somewhat from the others conducted. Defendants with low point scores (i.e., less than the six points usually required for an O.R. release recommendation) were split into two groups: one group automatically received O.R. release recommendations and the other group was processed normally.¹⁴ Although defendants with less than six points are technically not eligible for O.R. release recommendations, interviewers are not bound by the point scores

¹³For more information on program operations, see Nathan I. Silver, Delivery System Analysis of Baltimore City, Maryland, Working Paper No. 1, July 1978.

¹⁴All defendants with six or more points were processed as usual; they were not part of the experiment.

and may recommend O.R. release if they consider a defendant a good risk. Thus, it was possible for members of the control group to receive O.R. release recommendations.

In this experiment the major program service whose impact was being tested was the automatic provision of a release recommendation to certain defendants. Under normal program conditions such automatic recommendations were limited to defendants having at least six points (although in unusual circumstances interviewers can override this requirement). We were testing the effect of expanding this program "service" to additional defendants, who were presumably a "higher risk" group.

We could not use the entire group of defendants having fewer than six points for the study. Figure 4.1 shows the list of acceptable charges for study defendants; other charges were considered too serious for inclusion in the experimental test. Defendants could be excluded for other reasons as well (see Figure 4.1). These exclusions were:

- defendants detained for transfer to another jurisdiction, for probation revocation review, etc.;
- defendants awaiting trial on another charge;
- defendants with serious psychiatric problems;
- out-of-State residents;
- defendants with a prior record of "flagrant" failure to appear (defined as two failures to appear with guilty dispositions or four FTA charges since January 1, 1977); and
- defendants who lacked a verified address.

The lack of a verified address caused the exclusion of many defendants, because of the program's inability to locate a reference or other source of verification before the release decisions were made. The other major reason for excluding defendants was charge. The remaining exclusionary factors were used much less often.

FIGURE 4.1
ACCEPTABLE CHARGES AND OTHER REASONS FOR EXCLUSION,
BALTIMORE RELEASE EXPERIMENT

LIST OF ACCEPTABLE CHARGES

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 (Exclude second time against same person)
THEFT—UNDER \$500
TRESPASSING (not a repeat on same establishment)
UNLAWFUL ACTS—RELATED TO MINORS
WELFARE FRAUD

LIST OF EXCLUSIONS (BESIDES CHARGE)

DETAINERS
PRETRIAL REARRESTS
PSYCHIATRIC CASES
OUT-OF-STATE RESIDENTS
UNVERIFIED ADDRESS
FTA RECORD ONLY IF FLAGRANT VIOLATION (2 FTA'S WITH GUILTY
DISPOSITIONS OR 4 FTA CHARGES SINCE 1/1/77)

In addition to the analysis of impact on release, the Baltimore experiment included an experimental analysis of the impact of post-release followup services on defendant behavior (see Figure 1.2). Defendants from the release experiment who were released on own recognizance were told to report to the program's downtown headquarters.¹⁵ There, they were divided into two groups: one group received minimum program followup and the other group, more intensive followup. Because the 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 that such a control group would represent a substantial service cutback. Also, because at the time of sentencing judges often asked about a defendant's call-in record, the program staff members were concerned that a control group without call-in requirements might receive harsher sentences than otherwise.

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

¹⁵The only exceptions were defendants charged with relatively minor offenses, such as disorderly conduct, whose cases were likely to be settled the following day. The release period for these defendants would be too short to implement followup procedures.

supervised release program. Drug abuse, alcohol, and mental health services were available through referral to community-based treatment programs. Additionally, some 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.

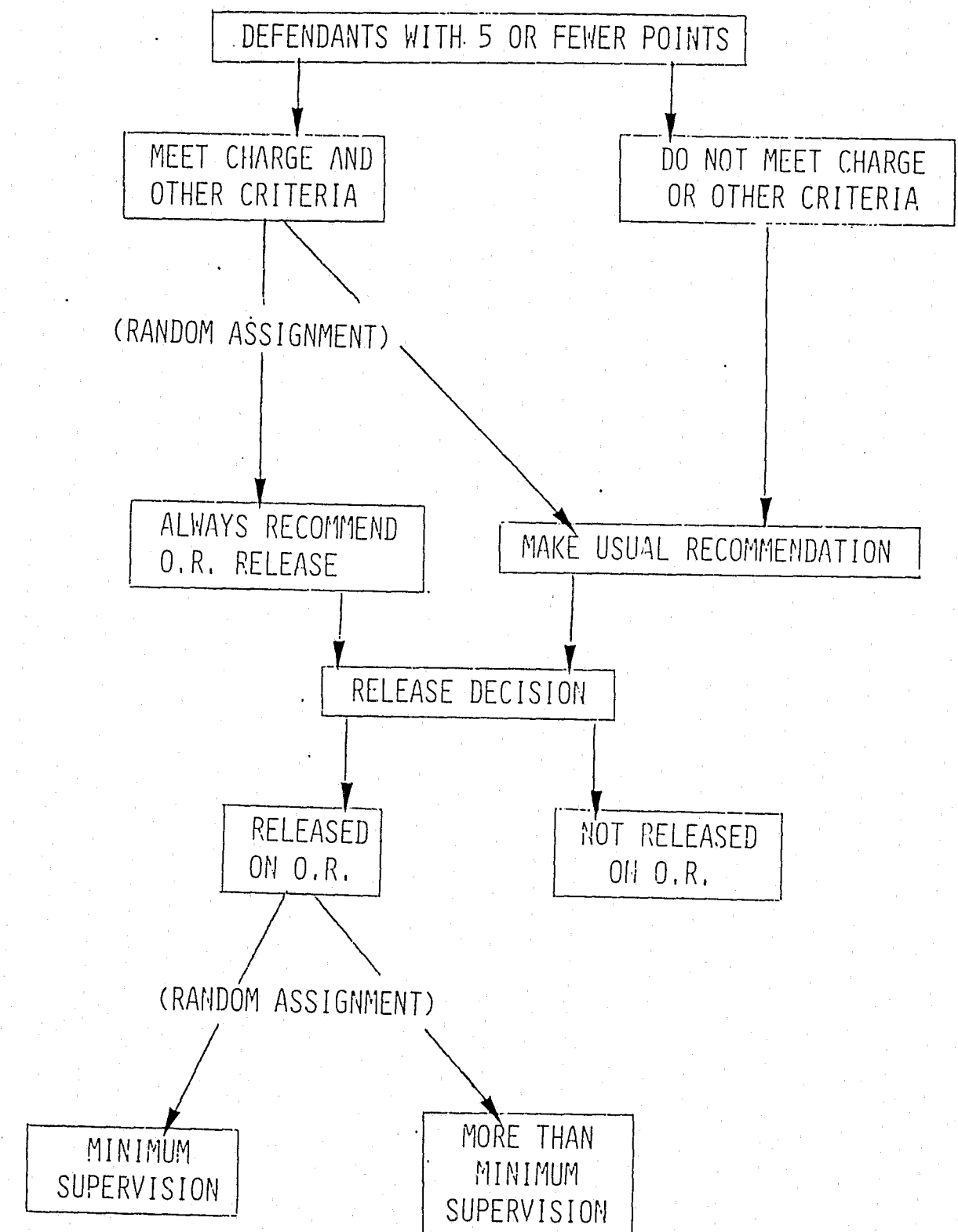
Thus, the experimental test of post-release followup compared the impact of monitoring weekly calls from defendants in a rather perfunctory manner with the effect of more intensive followup. This more intensive followup consisted at least of two calls a week during which defendants were counseled to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

Because defendants with fewer than six points who were excluded from the release experiment might be released on own recognizance by the bail commissioners, it was possible to increase the size of the followup experiment by including these persons. This was accomplished by directing the interviewers to refer virtually every defendant release on O.R. with fewer than six points to the program's downtown headquarters.¹⁶

The approach for the Baltimore experiment is shown in Figure 4.2. As illustrated there and discussed earlier, the experimental procedures were more complex than in the other sites studied. Not only was a two-stage random assignment procedure implemented but a supplemental group of defendants was also included in the followup stage of the experiment,

¹⁶ Only defendants whose cases were likely to be settled the following day were excluded, as discussed earlier.

FIGURE 4.2
SUMMARY OF APPROACH FOR BALTIMORE EXPERIMENT



though they were excluded from the release stage of the study. Additionally, the characteristics of the jurisdiction made experimental implementation more difficult than elsewhere. The program is a large one, with approximately 35 interviewers working three different shifts to provide around-the-clock operations. Moreover, the program staff work in ten different physical locations. Under these conditions it was difficult to get the experimental procedures implemented in the same way by all interviewers.

Largely as a result of an unusually high level of cooperation by program staff, we were eventually able to work out all implementation difficulties and ambiguities. We also took several special monitoring steps that had not been necessary in less complex sites. First, because the program's downtown headquarters receives all completed interview forms on the weekday following the interview, we were able to review all cases with fewer than six points to see if they had been handled correctly (e.g., that defendants had been excluded only for appropriate reasons, that experimental group defendants had always received O.R. release recommendations, and that defendants released on O.R. had been referred to the downtown office). When errors were identified, these were reported to the shift supervisors, who discussed them with the individual interviewers. In this way misunderstandings about study procedures were quickly identified and corrected.

When interviewers failed to refer defendants released on O.R. to the downtown office, or when referred defendants failed to report, efforts were made to contact those persons in the intensive followup

group. In most cases these efforts were successful; the defendants reported, and were referred to the appropriate supervision unit.

Figure 4.3 shows the form used to monitor the implementation of the experiment. One of these forms was completed for each defendant having fewer than five points. When errors were identified, the form and a copy of the interview were channeled to the appropriate staff person to followup on the problem and, if possible, correct it.

Besides the daily followup conducted in Baltimore on the experimental implementation, we also reviewed copies of the interview forms in our Washington offices on a weekly basis. This served as an additional check to insure that study procedures were implemented uniformly and accurately.

FIGURE 4.3
FOLLOWUP FORM: IMPLEMENTATION OF EXPERIMENTAL
PROCEDURES IN BALTIMORE

Defendant Name _____ Birthdate _____
Investigator _____ Arrest Date _____
Eligible for study? ☐ Yes ☐ No Charge _____
If yes: ☐ Program Group (YELLOW LIST) or ☐ Non-Program Group
☐ Supervision (BLUE LIST) or ☐ Minimum Intervention

Recommended for OR? ☐ Yes ☐ No * Released on OR? ☐ Yes ☐ No
*
Correct action? ☐ Yes ☐ No * Referred to Office? ☐ Yes ☐ No
*
* Correct Action? ☐ Yes ☐ No
*

Is follow-up with investigator needed? ☐ Yes ☐ No
Did follow-up occur? ☐ Yes ☐ No

Did defendant come to office? ☐ Yes ☐ No
Is follow-up needed to get defendant to office? ☐ Yes ☐ No (e.g., if
defendant is in
minimum inter-
vention group)
Was defendant properly assigned to "supervision" or "minimum intervention"
group? ☐ Yes ☐ No If not, why not (e.g., court-ordered treatment
for defendant in minimum intervention group)? _____

Is follow-up needed with supervision unit? ☐ Yes ☐ No
Did follow-up occur? ☐ Yes ☐ No
Follow-up actions taken to get defendant to office: _____

Comments: _____

B. Outcomes of Experiment

Tables 4.1 and 4.2 summarize information on the comparability of the experimental and control groups (for the first random assignment, testing program impact on release outcomes). The groups were comparable for all 17 characteristics for which data were available on more than half the defendants in each group (see detailed data in Appendix E).

As shown in Table 4.3, there were sharp differences between the groups for each release outcome:

- A total of 97% of the experimental group defendants secured release, versus 92% of control group defendants.
- The average time required to gain release was 0.7 days in the experimental group and 2.8 days in the control group (see Appendix E for details).
- Many more of the released defendants were released on nonfinancial conditions in the experimental group (91.5%, as compared with 72%).

Tables 4.4-4.6 indicate that program operations also affected the equity of release by ethnicity and employment status. In both cases release rate differences were found in the control group, but not in the experimental group: white defendants in the control group were much more likely to secure release, as were employed defendants. These differences were not explained by differences in the release risks posed by white versus minority or employed versus unemployed defendants. Based on analysis of released defendants, minority defendants in the experimental group (though not the control group) were somewhat more likely to be rearrested during the pretrial period; however, minority defendants in either group were no more likely than white defendants to be convicted of pretrial arrests or to fail to appear. There were no failure to appear or pretrial criminality rate differences for employed versus unemployed defendants released in either group.

TABLE 4.1
PERCENTAGES OF CASES IN EXPERIMENTAL AND CONTROL
GROUPS WITH DATA FOR SELECTED CHARACTERISTICS

CHARACTERISTIC	EXPERIMENTAL GROUP (n=148)	CONTROL GROUP (n=158)
<u>Community Ties</u>		
Local residence status	99%	99%
Years of local residence	95%	94%
Months at present address	97%	99%
Marital status	99%	100%
Family support	100%	99%
With whom defendant lives	97%	98%
Employment status	100%	100%
Income level (employed defendants only)	82%	87%
Public assistance	33%	27%
Occupation	91%	86%
<u>Criminality</u>		
Current charge	99%	100%
Number of prior arrests	100% ^a	100% ^a
Number of prior convictions	100% ^a	100% ^a
Criminal justice system status at time of arrest	45%	39%
Age at first adult arrest	71%	77%
<u>Demographic Characteristics</u>		
Age at arrest	100%	100%
Ethnicity	100%	100%
Sex	100%	100%
<u>Other</u>		
Education	97%	96%
^a Includes cases with missing information.		

TABLE 4.2
SUMMARY OF COMPARABILITY OF EXPERIMENTAL
AND CONTROL GROUPS
(n=306)

CHARACTERISTIC	SIGNIFICANCE LEVEL
<u>Community Ties</u>	
Local residence status	N.S.*
Years of local residence	N.S.
Months at present address	N.S.
Marital status	N.S.
Family support	N.S.
With whom defendant lives	N.S.
Employment status	N.S.
Income level (employed defendants only)	N.S.
Occupation	N.S.
<u>Criminality</u>	
Current charge	N.S.
Number of prior arrests	N.S.
Number of prior convictions	N.S.
Age at first adult arrest	N.S.
<u>Demographic Characteristics</u>	
Age at arrest	N.S.
Ethnicity	N.S.
Sex	N.S.
<u>Other</u>	
Education	N.S.
* Indicates the two groups were not significantly different (.05 statistical level).	

TABLE 4.3
RATE, SPEED AND TYPE OF RELEASE FOR
EXPERIMENTAL AND CONTROL GROUPS

OUTCOME	EXPERIMENTAL GROUP (n=148)		CONTROL GROUP (n=158)	
	Number	Percent	Number	Percent
<u>Rate of Release</u> ^{a/}				
Defendants released	144	97%	145	92%
Defendants <u>not</u> released	4	3%	13	8%
TOTAL	148	100%	158	100%
<u>Speed of Release</u> ^{b/}				
Mean number of days from arrest to release ^{c/}	0.7		2.8	
<u>Type of Release</u> ^{d/}				
Nonfinancial	130	91.5%	105	72%
Financial	12	8.5%	40	28%
TOTAL	142	100.0%	145	100%
^a Significant at the .06 level. ^b Significant at the .05 level. ^c n=138 for experimental group and n=139 for control group. ^d Significant at the .0001 level.				

TABLE 4.4
EQUITY OF RELEASE FOR EXPERIMENTAL GROUP
(n=148)

OUTCOME	ETHNICITY				EMPLOYMENT STATUS			
	WHITE		MINORITY		EMPLOYED OR SUBSTITUTES		UNEMPLOYED	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
<u>Rate of Release (all defendants)</u>								
Defendants released	56	100%	88	96%	69	99%	75	96%
Defendants <u>not</u> released	0	0%	4	4%	1	1%	3	4%
TOTAL	56	100%	92	100%	70	100%	78	100%
<u>Speed of Release (released defendants)</u>								
Mean number of days from arrest to release ^a	0.7		0.7		0.6		0.8	
<u>Type of Release (for released defendants)</u>								
Nonfinancial	52	93%	78	91%	63	91%	67	92%
Financial	4	7%	8	9%	6	9%	6	8%
TOTAL	56	100%	86	100%	69	100%	73	100%
^a n=55 for white; n=83 for minority; n=66 for employed; n=72 for unemployed								

TABLE 4.5
EQUITY OF RELEASE FOR CONTROL GROUP
(n=158)

OUTCOME	ETHNICITY				EMPLOYMENT STATUS			
	WHITE		MINORITY		EMPLOYED OR SUBSTITUTES		UNEMPLOYED	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
<u>Rate of Release (all defendants)</u> ^a								
Defendants released	60	100%	85	87%	70	99%	75	86%
Defendants <u>not</u> released	0	0	13	13%	1	1%	12	14%
TOTAL	60	100%	98	100%	71	100%	87	100%
<u>Speed of Release (released defendants)</u>								
Mean number of days from arrest to release ^b	2.5		3.0		3.4		2.2	
<u>Type of Release (for released defendants)</u>								
Nonfinancial	46	77%	59	69%	51	73%	54	72%
Financial	14	23%	26	31%	19	27%	21	28%
TOTAL	60	100%	85	100%	70	100%	75	100%
^a Significant at the .01 level for both ethnicity and employment status. ^b n=57 for white; n=82 for minority; n=67 for employed; n=72 for unemployed.								

TABLE 4.6
SUMMARY OF EQUITY OF RELEASE FOR
EXPERIMENTAL AND CONTROL GROUPS

OUTCOME	EXPERIMENTAL GROUP (n=148)		CONTROL GROUP (n=158)	
	Ethnicity	Employment Status	Ethnicity	Employment Status
Rate of Release	N.S.	N.S.	.01	.01
Speed of Release	N.S.	N.S.	N.S.	N.S.
Type of Release	N.S.	N.S.	N.S.	N.S.

To consider whether the release rate differences might be due to varied defendant characteristics, rather than to program impact, we compared minority versus white defendants and employed versus unemployed defendants in the experimental and control groups for each of the 17 characteristics listed in Table 4.2. The results appear in Table 4.7. As indicated, the comparison of white versus minority defendants showed similar results in both the experimental and control groups for most characteristics. However, in the control group, minority defendants had resided in the area for a significantly longer period of time than white defendants. Also, in the experimental group, minority defendants were more likely to have been charged with economic crimes and less likely to have been charged with drug crimes, when compared with white defendants. Both of these differences should presumably have worked to the advantage of minority defendants in the control group—the reverse of what was found. Thus, differences in defendant characteristics do not appear to explain the apparent bias in release rates by ethnicity for control group defendants.

For employed versus unemployed defendants, the major difference found was by age in the experimental group: unemployed defendants were both older at the time of arrest and had been older at the time of their first adult arrest. Because age differences might affect release outcomes for the experimental group, we analyzed release rates by employment status while controlling for age. Defendants were grouped into two categories for this analysis: (1) 25 years of age or younger and (2) 26 years of age or older. Neither group showed a significant difference in release outcomes by employment status. Thus, age differences between employed and unemployed defendants in the experimental group do not account for the overall finding of greater release equity by employment

TABLE 4.7
COMPARABILITY OF WHITE VERSUS MINORITY AND
OF EMPLOYED VERSUS UNEMPLOYED DEFENDANTS
IN EXPERIMENTAL AND CONTROL GROUPS

CHARACTERISTICS	White Versus Minority Defendants		Employed Versus Unemployed Defendants	
	Experimental Group (n=148)	Control Group (n=158)	Experimental Group (n=148)	Control Group (n=158)
<u>Community Ties</u>				
Local residence status	NS	NS	NS	NS
Years of local residence	NS	.03	NS	NS
Months at present address	NS	NS	NS	NS
Marital status	NS	NS	NS	NS
Family support	NS	NS	NS	NS
With whom defendant lives	NS	NS	NS	NS
Employment status	NS	NS	NA	NA
Income level (employed defendants only)	NS	NS	NA	NA
Occupation	NS	NS	NS	NS
<u>Criminality</u>				
Current charge	.03	NS	NS	NS
Number of prior arrests	.003	.01	NS	NS
Number of prior convictions	.007	.02	NS	NS
Age at first adult arrest	NS	NS	.005	NS
<u>Demographic Characteristics</u>				
Age at arrest	NS	NS	.01	NS
Ethnicity	NA	NA	NS	NS
Sex	NS	NS	NS	NS
<u>Other</u>				
Education	NS	NS	NS	.02

status for the experimental group, when compared with the control group. Thus, the greater equity of release found for the experimental group cannot be explained by differences in defendant characteristics for the groups and must instead be considered an effect of program operations.

Table 4.8 summarizes the comparability of released defendants in the experimental and control groups structured for the analysis of post-release outcomes. As discussed in the preceding section, these groups were composed from all defendants released on own recognizance (O.R.) who had five or fewer points and who did not have their cases settled within one court day of release. Thus, the groups included many of the released defendants whose cases had been used in the first random assignment procedures as well as additional defendants whose cases had been excluded from those procedures (e.g., due to lack of a verified address or to the seriousness of the charge) but were nevertheless released on O.R. As shown in Table 4.8, the released defendants in the experimental and control groups were comparable for all of the characteristics considered (see Appendix E for details).

Table 4.9 presents the failure to appear and pretrial criminality rates for the experimental and control groups. As shown, there were no significant differences between the groups for these rates. Thus, the group receiving minimal supervision performed as well as the group receiving more intensive supervision.¹⁷ Because 28 of the defendants assigned to the more intensive supervision group did not report to the

¹⁷ We also separately analyzed the defendants who had participated in the first random assignment and those who had not. The results were the same. In each case the experimental and control groups were comparable, and there were no significant differences between them for failure to appear and pretrial criminality rates. Moreover, we compared the supervision outcomes separately for the experimental and control groups of the first random assignment. In each case those defendants with minimal versus more intensive supervision were comparable, and their failure to appear and pretrial criminality rates were not significantly different.

TABLE 4.8
SUMMARY OF COMPARABILITY OF RELEASED DEFENDANTS
IN THE EXPERIMENTAL AND CONTROL GROUPS
(n=323)

CHARACTERISTIC	SIGNIFICANCE LEVEL
<u>Community Ties</u>	
Local residence status	N.S.
Years of local residence	N.S.
Months at present address	N.S.
Marital status	N.S.
Family support	N.S.
With whom defendant lives	N.S.
Employment status	N.S.
Income level (employed defendants only)	N.S.
Occupation	N.S.
<u>Criminality</u>	
Current charge	N.S.
Number of prior arrests	N.S.
Number of prior convictions	N.S.
Age at first adult arrest	N.S.
<u>Demographic Characteristics</u>	
Age at arrest	N.S.
Ethnicity	N.S.
Sex	N.S.
<u>Other</u>	
Education	N.S.

TABLE 4.9
FAILURE TO APPEAR AND PRETRIAL CRIMINALITY
FOR EXPERIMENTAL AND CONTROL GROUPS

OUTCOME	EXPERIMENTAL GROUP (n=171)		CONTROL GROUP (n=152)	
	Number	Percent	Number	Percent
<u>Failure to Appear (FTA)</u>				
Defendants who FTA	29	17%	26	17%
Defendants who do <u>not</u> FTA	142	83%	126	83%
TOTAL released defendants	171	100%	152	100%
<u>Pretrial Criminality</u>				
Defendants with rearrests	13	8%	14	9%
Defendants <u>without</u> rearrests	158	92%	138	91%
TOTAL released defendants	171	100%	152	100%
Defendants with rearrest convictions	9	5%	6	4%
Defendants <u>without</u> rearrest convictions	162	95%	146	96%
TOTAL released defendants	171	100%	152	100%

program (and were not located by the program's followup efforts), we repeated the analysis, excluding these defendants. The findings were the same: the experimental and control groups were comparable, and there were no significant differences between them for failure to appear or pretrial criminality rates.

Additionally, because the experimental group included both defendants who were referred to treatment programs or other services and defendants who were required only to call the program twice a week, we compared the outcomes of these groups to determine the impact of the greater supervision associated with services referral. Once again, there were no significant differences in failure to appear or pretrial criminality rates for the two groups. (The groups were comparable for all characteristics listed in Table 4.2 except occupation and number of prior arrests: the group referred to services had a higher percentage of craftsmen, operatives and laborers; it also had more prior arrests.)

There are several possible explanations for the apparent lack of impact of program supervision activities on defendant behavior during the pretrial period. First, supervision activities may simply have no effect on failure to appear or pretrial criminality. Past studies have had mixed findings concerning the impact of supervision,¹⁸ and further analysis of this topic is now being sponsored by the National Institute

¹⁸ For example, 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. District of Columbia Bail Agency, How Does Pretrial Supervision Affect Pretrial Performance? (Washington, D.C.: D.C. Bail Agency, May 1978), p. 2. An analysis of supervised defendants in Philadelphia found they had lower failure to appear rates, and were no more likely to be rearrested pretrial, than defendants in the comparison groups studied. Herbert Miller, et al., Evaluation of Conditional Release Program (Washington, D.C.: Institute for Criminal Law and Procedure, Georgetown University Law Center, 1976).

of Justice.¹⁹

Second, program followup may indeed have an effect on defendant behavior, but the impact of minimum supervision may be as great as the effect of more intensive supervision. It should be remembered in this regard that the Baltimore experiment did not include a group receiving no program followup; the experiment tested "minimal" versus "more intensive" supervision, rather than "some" supervision versus "none."

Another possible explanation is that supervision may need to be applied very selectively. 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 defendants. If this is the case, a better experimental test would be to identify a group of defendants thought to need supervision and randomly select half of them to be supervised. Such an approach is roughly similar to the one in the NIJ-²⁰ funded experimental test of supervised release now in progress.

Finally, the impact of supervision—and particularly of treatment or services—may occur over a time period longer than the pretrial period. The scope of our study precludes identifying any such longer term impact that might result from supervision.

We do not know which of these possibilities best explains the supervision findings. Further study of this topic, perhaps through additional experiments, could help answer this question.

Another topic of interest is the relationship of point system scores

¹⁹ For more information see Test Design: Supervised Pretrial Release (Washington, D.C.: U.S. Department of Justice, National Institute of Justice, February 1980).

²⁰ Ibid.

to release outcomes, failure to appear and pretrial criminality. Table 4.10 shows that the rate of release tended to increase with the number of points. Additionally, at each point level more defendants secured release in the experimental than in the control group. Table 4.11 compares failure to appear and pretrial criminality rates by point score totals (also see Appendix E). As shown, pretrial rearrest and conviction rates do tend to decline somewhat as point totals increase. No such trend exists for failure to appear, however.

Another observation based on Table 4.11 concerns the large number of defendants with zero points. These are primarily defendants for whom verification could not be obtained. Thus, their low scores do not necessarily reflect poor release risks, based on the items included in the point scale.

C: Comparison With Cross-Sectional Analysis

As with Tucson, Baltimore City was included in the cross-sectional analysis, discussed in Volume I. Consequently, we can compare the characteristics of the defendants in the experiment with those of the defendants in the earlier analysis, which was based on a random sample of all arrestees in the jurisdiction.

Because the experiment was limited to defendants with too few points to qualify for an own recognizance release recommendation under normal program procedures, we expected the experimental defendants to have weaker community ties than the cross-sectional defendants. This was indeed the case. Experimental defendants had lived at their present address a much shorter period of time, were less likely to be married and were more likely to live alone or with an unrelated person. They

TABLE 4.10

RATE OF RELEASE BY POINT SCORES

POINT SCORE	Experimental Group (n=148)		Control Group (n=158)	
	Total Number of Defendants	Percent Released	Total Number of Defendants	Percent Released
One or less	7	86%	8	75%
2	15	93%	22	86%
3	12	100%	28	89%
4	39	97%	28	96%
5	30	97%	36	92%
6 ^a	15	100%	7	100%
More than 6 ^a	18	97%	15	100%
TOTAL	136	97%	144	92%

^aTechnically, these defendants should not have been included in the experiment. Their point scores were initially calculated incorrectly: at the time release decisions were made, the defendants were thought to have fewer than six points. Because the defendants were processed as if they had fewer than six points, we included them in the study.

TABLE 4.11
FAILURE TO APPEAR AND PRETRIAL CRIMINALITY
RATES BY POINT SCORES

Point Score	Intensive Supervision Group				Minimal Supervision Group			
	Total Number of Defendants	Failure To Appear Rate	Pretrial Rearrest Rate	Pretrial Rearrest Conviction Rate	Total Number of Defendants	Failure To Appear Rate	Pretrial Rearrest Rate	Pretrial Rearrest Conviction Rate
Less Than 0	7	29%	14%	14%	5	20%	0%	0%
0	82	15%	5%	4%	76	18%	11%	7%
1	8	50%	13%	13%	4	25%	0	0%
2	15	0%	13%	13%	7	14%	29%	14%
3	9	11%	33%	11%	8	25%	13%	0%
4	13	31%	0%	0%	16	13%	13%	0%
5	13	8%	0%	0%	17	24%	0%	0%
6	9	22%	11%	11%	9	11%	0%	0%
More Than 6	15	20%	7%	0%	10	0%	10%	0%
TOTAL	171	17%	8%	5%	152	17%	9%	4%

were also more likely to be service workers and less likely to be laborers. In terms of charge, they were more likely to have been accused of economic or drug crimes and less likely to have been charged with crimes against public order. Finally, in the analysis of release impact (first random assignment) they were more likely to be white; and in the analysis of supervision impact (second random assignment) they were more likely to be women.

Table 4.12 compares the outcomes for the experimental and cross-sectional defendants. As shown, more experimental defendants were released, and they gained release more quickly than had cross-sectional defendants. Although more program group defendants secured nonfinancial (own recognition) release, fewer control group defendants did so (vis-a-vis cross-sectional defendants). Post-release outcomes show a much higher failure to appear rate for experimental defendants, when compared to cross-sectional defendants, but virtually identical pretrial rearrest and pretrial re-arrest conviction rates.

D. Cost-Effectiveness

The cost-effectiveness analysis based on the Baltimore experiment considers the effects of variations in program operations (e.g., extending own recognizance release recommendations to defendants with fewer than six points and providing more intensive versus minimal follow-up after defendants are released). This is in contrast to the other experimental sites, where the impact of having a program at all was tested.

One way to assess the cost-effectiveness of the Baltimore program variations is to note the strong impact on all release outcomes of

TABLE 4.12
COMPARISON OF OUTCOMES FOR DEFENDANTS
IN CROSS-SECTIONAL AND EXPERIMENTAL STUDIES

PART I. RELEASE OUTCOMES

OUTCOME	CROSS-SECTIONAL STUDY GROUP (n=811)	EXPERIMENTAL STUDY	
		Program Group (n=148)	Control Group (n=158)
Rate of Release	87%	97%	92%
Speed of Release	3.5 days	0.7 days	2.8 days
Rate of Nonfinancial Release	80%	92%	72%

PART II. POST-RELEASE OUTCOMES

OUTCOME	CROSS-SECTIONAL STUDY GROUP (n=703)	EXPERIMENTAL STUDY	
		Program Group (n=171)	Control Group (n=152)
Failure To Appear Rate	6%	17%	17%
Pretrial Rearrest Rate	8%	8%	9%
Pretrial Conviction for Rearrest Rate	4%	5%	4%

extending own recognizance release recommendations to selected defendants lacking six points: more defendants were released; they were released more quickly; they were more likely to secure nonfinancial release; and release outcomes seemed more equitable for defendants of different ethnicity and employment status. The increases in release rates did not lead to increased failure to appear or pretrial criminality rates. Moreover, the program processing costs were minimal, because the only change was in the release recommendation; interview and verification activities proceeded in their usual manner. Thus, the expansion of own recognizance release recommendation eligibility to selected defendants having fewer than six points would appear to be a highly cost-effective program change.

In the supervision analysis, defendants with minimal followup had the same rates of failure to appear and pretrial criminality as defendants with more intensive supervision. Because minimal followup is also the less expensive alternative, it would appear to be the more cost-effective approach.

Another way of analyzing cost-effectiveness is to consider the costs to the criminal justice system, as was done in the other experimental sites. However, in part because of the large amount of staff time devoted to solving implementation problems in Baltimore, we were unable to develop unit cost data for that site. To provide at least a rough cost-effectiveness estimate, we used the mean cost estimates from the other three experimental sites. These estimates were applied to the actual experimental outcomes data for both the program and control groups. Thus, for example, the cost of serving a bench warrant is the

same for both groups (and is based on the mean cost for the other three sites), but the number of bench warrants served reflects the documented experience of each group.

Tables 4.13-4.16 present the cost estimates for the program and control groups. As in the other sites, these estimates cannot be compared directly for the two groups, because (1) the group sizes are different and (2) the costs of failure to appear and pretrial rearrest should be weighted to reflect the lack of significant differences between the two groups.²¹ Table 4.17 provides the weighted cost data for the two groups. As shown, costs for the program group were approximately \$52,000 and for the control group, \$33,000. The cost differential between the two groups (\$19,000) is less than program costs alone (\$28,000).

When the two program variations tested are considered separately, the findings confirm the conclusions reached earlier. The extension of own recognizance release recommendations to additional defendants did not affect program interview costs but saved almost 300 days of detention costs, valued (probably somewhat conservatively) at about \$2,000. More intensive supervision, on the other hand, added almost \$26,000 to program costs and \$1,500 to failure to appear costs, while saving only about \$6,000 in pretrial arrest costs.

Pretrial arrest costs were higher for the control group, due largely to the more severe dispositions for convicted defendants. Court reactions to failure to appear, however, were more serious for

²¹ Detention is significantly different between the groups.

TABLE 4.13
COST SUMMARY, PROGRAM GROUP,
BALTIMORE EXPERIMENT

ITEM	COST
<u>Detention</u>	
191 days @ \$7.11	\$ 1,358.01
<u>Failure To Appear (FTA)</u>	
• 31 occurrences	
—District Court and prosecutor costs, 26 cases @ \$8.05	209.30
—Public Defender costs, District Court, 12 cases @ \$2.33	27.96
—Supreme Bench and prosecutor costs, 5 cases @ \$11.28	56.40
—Public Defender costs, Supreme Bench, 1 case @ \$2.48	2.48
—Bench warrant issued, 23 cases in District Court @ \$0.78	17.94
—Bench warrant issued, 4 cases at Supreme Bench @ \$1.87	7.48
—Bench warrant served, 13 cases, District Court, @ \$37.29	484.77
—Bench warrant served, 2 cases, Supreme Bench, @ \$55.45	110.90
—No unsuccessful attempts to serve bench warrants	
• 21 returns to court	
—District Court and prosecutor costs, 19 cases @ \$14.60	277.40
—Public Defender costs, District Court, 12 cases @ \$8.51	102.12
—Supreme Bench and prosecutor costs (no Public Defenders involved), 2 cases @ \$23.75	47.50
—Bond forfeiture hearing, District Court, 1 case @ \$11.49	11.49
—Bond forfeiture hearing, Supreme Bench, 1 case @ \$24.45	24.45
—Program costs, 5 re-interviews @ \$13.00	65.00
—Detention resulting from FTA, 269 days @ \$7.11	1,912.59
—No bond forfeitures executed	
• Prosecution of FTA	
—15 cases @ \$119.00	1,785.00
Subtotal, FTA costs	5,142.78

(Continued)

TABLE 4.13 (continued)
COST SUMMARY, PROGRAM GROUP,
BALTIMORE EXPERIMENT

<u>Pretrial Arrest</u>	
• Apprehension, 13 cases @ \$12.89	167.57
• Booking, 13 cases @ \$20.60	267.80
• Program costs	
—11 interviews @ \$13.00	143.00
• Detention resulting from pretrial arrest, 493 days @ \$7.11	3,505.23
• Court processing costs	
—Detailed in Table 4.14	7,015.65
—No jury costs	
• Sentencing costs	
—Pre-sentence investigations: 4 convicted defendants, District Court, @ \$50.00; 4 convicted defendants, Supreme Bench, @ \$94.00	200.00 376.00
—Probation supervision: 2 defendants, Supreme Bench, 5 years @ \$190.67 per year	953.35
—No incarceration	
—Less fines for 3 defendants (assumed paid)	- 150.00
Subtotal, PTA costs	\$ 12,478.60
<u>Program</u>	
• Interview and verification, 148 defendants @ \$13.00	1,924.00
• Supervision: surveillance, 124 defendants @ \$100.00; referral to services, 47 defendants @ \$300.00	12,400.00 14,100.00
Subtotal, program costs	\$ 28,424.00
TOTAL COSTS	\$ 47,403.39

the program group. Detention resulting from failure to appear averaged almost nine days for each occurrence in the program group, as compared with only five days for the control group.

TABLE 4.14
COURT PROCESSING COSTS FOR PRETRIAL ARRESTS,
PROGRAM GROUP, BALTIMORE EXPERIMENT

Note: No defendant had more than one pretrial arrest. No pretrial arrests were adjudicated on the same day as the original charge.

Court	Outcome	Attorney	Total Number of Cases	Cost
District	Dismissal	Public Defender	3	3 @ \$107.66 = \$322.98
District	Dismissal	Other than Public Defender	1	1 @ \$69.33 = \$69.33
District	Convicted by judge	Public Defender	1	1 @ \$625.34 = \$625.34
District	Convicted by judge	Other than Public Defender	3	3 @ \$342.67 = \$1,028.01
Supreme Bench	Dismissal	Public Defender	1	1 @ \$322.66 = \$322.66
Supreme Bench	Plea	Public Defender	1	1 @ \$777.33 = \$777.33
Supreme Bench	Plea	Other than Public Defender	1	1 @ \$526.00 = \$526.00
Supreme Bench	Convicted by judge	Other than Public Defender	2	2 @ \$1,672.00 = \$3,344.00
TOTAL COST =				\$7,015.65

TABLE 4.15
COST SUMMARY, CONTROL GROUP,
BALTIMORE EXPERIMENT

ITEM	COST
<u>Detention</u>	
493 days @ \$7.11	\$ 3,505.23
<u>Failure To Appear (FTA)</u>	
• 26 occurrences	
—District Court and prosecutor costs, 18 cases @ \$8.05	144.90
—Public Defender costs, District Court, 2 cases @ \$2.33	4.66
—Supreme Bench and prosecutor costs, 8 cases @ \$11.28	90.24
—Public Defender costs, Supreme Bench, 3 cases @ \$2.48	7.44
—Bench warrant issued, 17 cases in District Court @ \$0.78	13.26
—Bench warrant issued, 8 cases at Supreme Bench @ \$1.87	14.96
—Bench warrant served, 8 cases, District Court, @ \$37.29	298.32
—Bench warrant served, 3 cases, Supreme Bench, @ \$55.45	166.35
—No unsuccessful attempts to serve bench warrants	
• 14 returns to court	
—District court and prosecutor costs, 10 cases @ \$14.60	146.00
—Public Defender costs, District Court, 2 cases @ \$8.51	17.02
—Supreme Bench and prosecutor costs, 4 cases @ \$23.75	95.00
—Public Defender costs, Supreme Bench, 1 case @ \$9.38	9.38
—Bond forfeiture hearing, District Court, 1 case @ \$11.49	11.49
—Program costs, 5 re-interviews @ \$13.00	65.00
—Detention resulting from FTA, 127 days @ \$7.11	902.97
—No bond forfeitures executed	
• Prosecution of FTA	
—9 cases @ \$119.00	1,071.00
Subtotal, FTA costs	\$ 3,057.99

(Continued)

TABLE 4.15(continued)
COST SUMMARY, CONTROL GROUP,
BALTIMORE EXPERIMENT

<u>Pretrial Arrest (PTA)</u>	
• Apprehension, 16 cases @ \$12.89	206.24
• Booking, 16 cases @ \$20.60	329.60
• Program costs	
—14 interviews @ \$13.00	182.00
• Detention resulting from PTA, 167 days @ \$7.11	1,187.37
• Court processing costs	
—Detailed in Table 4.16	10,407.35
—No jury costs	
• Sentencing costs	
—Pre sentence investigations: 5 convicted defendants, District Court, @ \$50.00; 1 convicted defendant, Supreme Bench, @ \$94.00	250.00
—Probation supervision: 3 defendants, District Court, 3½ years @ \$75.67 per year	94.00
—Incarceration, 2 defendants, 37 months @ \$213 per month	264.85
—Less fines for 3 defendants (assumed paid)	7,881.00
	- 250.00
Subtotal, PTA costs	20,552.41
<u>Program</u>	
• Interview and verification, 158 defendants @ \$13.00	2,054.00
• Supervision, minimum followup for 152 defendants @ \$5.00	760.00
Subtotal, program costs	2,814.00
TOTAL COSTS	\$ 29,929.63

TABLE 4.16
COURT PROCESSING COSTS FOR PRETRIAL ARRESTS,
CONTROL GROUP, BALTIMORE EXPERIMENT

Note: No pretrial arrest was adjudicated on the same day as another pretrial arrest for the same defendant.

Court	Outcome	Attorney	Total Number of Cases	Number of Cases Adjudicated on Same Day as Original Charge	Cost
District	Dismissal	Public Defender	1	1	$\frac{1}{2} @ \$107.66 = \53.83
District	Dismissal	Other than Public Defender	3	0	$3 @ \$69.33 = \207.99
District	Convicted by judge	Public Defender	4	0	$4 @ \$625.34 = \$2,501.36$
District	Convicted by judge	Other than Public Defender	1	0	$1 @ \$342.67 = \342.67
District	Acquitted by judge	Public Defender	1	0	$1 @ \$625.34 = \625.34
District	Open*	Public Defender	1	0	$1 @ \$417.84 = \417.84
Supreme Bench	Dismissal	Public Defender	1	0	$1 @ \$322.66 = \322.66
Supreme Bench	Convicted by judge	Other than Public Defender	1	0	$1 @ \$1,672.00 = \$1,672.00$
Supreme Bench	Open*	Public Defender	2	0	$2 @ \$1,582.33 = \$3,164.66$
Supreme Bench	Open*	Other than Public Defender	1	0	$1 @ \$1,099.00 = \$1,099.00$
TOTAL COSTS =					\$10,407.35
*Costs of open cases were estimated as one-half the cost of a plea plus one-half the cost of a trial.					

TABLE 4.17
COMPARISON OF COSTS FOR PROGRAM AND
CONTROL GROUPS, BALTIMORE EXPERIMENT

Cost Category	Program Group		Control Group	
	Quantity	Cost	Quantity	Cost
<u>Unweighted</u>				
Detention	191 days, 148 defts	\$1,358	493 days, 158 defts	\$3,505
Failure to appear	31 FTA, 171 defts	\$5,143	26 FTA, 152 defts	\$3,058
Pretrial arrest	13 PTA, 171 defts	\$12,479	16 PTA, 152 defts	\$20,552
Program—Interview Followup	148 (Interview) 171 (Followup)	\$1,924 \$26,500	158 (Interview) 152 (Followup)	\$2,054 \$760
TOTAL		\$47,404		\$29,929
<u>Weighted To Reflect Equal Group Sizes</u>				
Detention	204 days, 158 defts	\$1,453	493 days, 158 defts	\$3,505
Failure to appear	31 FTA, 171 defts	\$5,143	29 FTA, 171 defts	\$3,440
Pretrial arrest	13 PTA, 171 defts	\$12,479	18 PTA, 171 defts	\$23,121
Program—Interview Followup	158 (Interview) 171 (Followup)	\$2,059 \$26,500	158 (Interview) 171 (Followup)	\$2,054 \$855
TOTAL		\$47,634		\$32,975
<u>Weighted To Reflect Equal FTA and PTA</u>				
Detention	204 days, 158 defts	\$1,453	493 days, 158 defts	\$3,505
Failure to appear	31 FTA, 171 defts	\$5,143	31 FTA, 171 defts	\$3,681
Pretrial arrest	18 PTA, 171 defts	\$17,221	18 PTA, 171 defts	\$23,121
Program—Interview Followup	158 (Interview) 171 (Followup)	\$2,059 \$26,500	158 (Interview) 171 (Followup)	\$2,054 855
TOTAL		\$52,376		\$33,216

V. THE LINCOLN EXPERIMENT

A. Background

The Lincoln pretrial release program operates through the Intake Service Center of the Corrections Division.²² After arrest defendants charged with misdemeanor offenses who are not released through citation by the arresting officer are brought to the City Jail for booking. Upon booking, Intake Service Center staff screen each arrestee for possible pre-arraignment release. Most misdemeanor defendants are eligible for release to the custody of an attorney or a responsible party; such release is effected when the third party appears at the jail and signs for the defendant. Arrestees may also be released by posting bond (either full or deposit), according to the preset bail schedule.

If defendants do not secure release through any of these mechanisms, they may be screened for own recognizance release. All misdemeanor defendants are eligible for such screening, provided they have a Lancaster County (Lincoln area) address and are not subject to "hold status" for failure to appear, probation violation or other reason. However, the program operates only 18 hours per week, during the peak misdemeanor booking hours over weekends. Program staff estimated that they interviewed only about 40% of the arrestees who would be contacted if the program operated around-the-clock.

Defendants screened for own recognizance (O.R.) release are asked about their residence, family ties, employment and criminal record. They are also asked for at least one reference, who is telephoned for

²²For a more detailed discussion of pretrial release procedures in Lincoln, see Lisa Crowley, Delivery System Analysis of Lancaster County (Lincoln), Nebraska, Working Paper No. 9, February 1980.

verification purposes. The program also conducts a State record check by teletype. A point system is used to evaluate the information obtained, with seven verified points needed for O.R. release eligibility. The program is itself authorized to release eligible defendants, who sign a "Promise to Appear" form in which they agree to appear for subsequent court dates.

The program considers itself responsible for defendants only until the time of arraignment, which usually occurs within 48 hours of release. If a defendant fails to appear for arraignment, the program attempts to contact the defendant and encourage the person to return to court. No followup beyond arraignment is conducted.

At arraignment the judge is responsible for reviewing the release conditions of all defendants whose cases were not settled at that time. This review occurs, regardless of whether a defendant was released prior to arraignment. No information on the defendant is forwarded to the court by the Intake Service Center. The judge sometimes refers defendants to the Municipal Probation Department for a Bond Investigation. These investigations take place while the arraignment proceedings continue, with the results returned to the judge by the end of the session. Only rarely are attempts made to verify the information. In other cases the judge simply questions the defendant about employment, residence, family ties, etc. and makes a release decision on that basis.

Thus, the pretrial release program has limited involvement in the pretrial release process. The program's goal is to provide a cost-effective means of releasing defendants prior to arraignment to assist in maintaining a manageable level of jail population. The experiment

implemented in Lincoln was designed to test the extent to which the program's efforts to release defendants prior to arraignment and the costs associated with those efforts were offset by savings in detention costs or other costs borne by the criminal justice system.

The experiment involved the expansion of program activities, so that interviews could be conducted during an additional 40 hours per week. This permitted interviewing during weekday evenings and expanded the weekend coverage as well. Days were randomly assigned as "program" or "research" days. On program days defendants received routine program processing. On research days all defendants were part of the control group. These defendants were interviewed only, with this information sent to us for inclusion in the data base.

Because of the relatively low volume of defendants processed by the program, we thought it would take about six months to obtain 400 defendants for the experimental study. This estimate was based on recent workload data for the program and its estimates of the size of the "overflow" group not being contacted.

Although six months was a long time period, Lincoln offered a number of advantages as a site for study. First, it was a program that released defendants on its own authority soon after booking. The potential cost-effectiveness of this approach seemed worthy of analysis. Second, no other site in that part of the country had been included in any of the components of the pretrial release evaluation. Small jurisdictions had been considered too small—or too limited in their program eligibility criteria—for inclusion in the cross-sectional analyses, and larger sites had had various problems with record-keeping systems, etc., that made them inappropriate. Thus, Lincoln filled that

regional gap in our sample. Finally, the Lincoln program was run by the Corrections Division, and we had studied few such programs. Most had been under the jurisdiction of the courts.

Thus, the disadvantages of a small volume of defendants in Lincoln seemed offset by other factors. In fact the volume was considerably smaller than we had estimated. After nine months there were only 130 defendants in the study. The most likely explanation for this seems to be that the estimate was high for the size of the "overflow" group of defendants missed due to limited hours of program operation. It is also possible, however, that other release practices in the jurisdiction simply changed over time (e.g., the police may have released more defendants on citations), so that the program faced a declining volume of defendants eligible for its services. Unfortunately, adequate data do not exist at the local level for us to evaluate these alternative explanations conclusively.

Another difficulty with the Lincoln experiment is that the size of the experimental group was much larger than that of the control group. Nevertheless, as discussed in the following section, the groups were overwhelmingly comparable in terms of the various background characteristics for which they were analyzed.

B. Outcomes of Experiment

Tables 5.1 and 5.2 consider the comparability of the experimental and control groups (see Appendix E for detailed data). As shown, for the 18 characteristics with reasonably complete data, the groups were equivalent for 17 of them. Only age at arrest showed a significant difference: defendants in the experimental group were younger than those in the control group.

As indicated in Table 5.3, defendants in the experimental group were much more likely to be released than members of the control group: 77% of the experimental group defendants were released prior to trial, as compared with only 47% of the control group defendants. There were no important differences between the groups in terms of the speed or type of release for released defendants (see Appendix E for more information on the speed of release). If type of release is considered for all defendants, rather than released defendants, there is a significant difference between the experimental and control groups: more defendants were released on own recognizance in the experimental group (72%, as compared to 44% in the control group). This difference stems from the vastly different release rates for the two groups.

Because the experimental and control groups differ for defendants' ages at arrest, it is possible that the release impact shown in Table 5.3 is due to this age disparity. To consider this possibility, we analyzed the rate of release for older versus younger defendants. As indicated in Table 5.4, release was more likely to be secured by defendants in the experimental group, regardless of age. For older defendants 71% were released in the experimental group and only 40% in the control group, a statistically significant difference. However, for younger defendants the difference

TABLE 5.1
PERCENTAGES OF CASES IN EXPERIMENTAL AND CONTROL GROUPS
WITH DATA FOR SELECTED CHARACTERISTICS

CHARACTERISTIC	EXPERIMENTAL GROUP (n=94)	CONTROL GROUP (n=36)
<u>Community Ties</u>		
Local residence status	99%	100%
Years of local residence	79%	75%
Months at present address	94%	92%
Marital status	97%	94%
Family support	76%	89%
With whom defendant lives	94%	94%
Employment status	97%	94%
Income level (employed defendants only)	2%	0%
Public assistance	79%	83%
Occupation	64%	75%
<u>Criminality</u>		
Current charge	100%	100%
Number of prior arrests	100% ^a	100% ^a
Number of prior convictions	100% ^a	100% ^a
Criminal justice system status at time of arrest	96%	89%
Age at first adult arrest	59%	72%
<u>Demographic Characteristics</u>		
Age at arrest	98%	100%
Ethnicity	64%	78%
Sex	99%	100%
<u>Other</u>		
Education	89%	89%
^a Includes cases with missing information.		

TABLE 5.2
SUMMARY OF COMPARABILITY OF EXPERIMENTAL
AND CONTROL GROUPS
(n=130)

CHARACTERISTIC	SIGNIFICANCE LEVEL
<u>Community Ties</u>	
Local residence status	N.S. *
Years of local residence	N.S.
Months at present address	N.S.
Marital status	N.S.
Family support	N.S.
With whom defendant lives	N.S.
Employment status	N.S.
Public assistance	N.S.
Occupation	N.S.
<u>Criminality</u>	
Current charge	N.S.
Number of prior arrests	N.S.
Number of prior convictions	N.S.
Criminal justice system status at time of arrest	N.S.
Age at first adult arrest	N.S.
<u>Demographic Characteristics</u>	
Age at arrest	.03
Ethnicity	N.S.
Sex	N.S.
<u>Other</u>	
Education	N.S.
* Indicates the two groups were not significantly different (.05 statistical level).	

TABLE 5.3
RATE, SPEED AND TYPE OF RELEASE FOR
EXPERIMENTAL AND CONTROL GROUPS

OUTCOME	EXPERIMENTAL GROUP (n=94)		CONTROL GROUP (n=36)	
	Number	Percent	Number	Percent
<u>Rate of Release^a</u>				
Defendants released	72	77%	17	47%
Defendants <u>not</u> released	22	23%	19	53%
TOTAL	94	100%	36	100%
<u>Speed of Release</u>				
Mean number of days from arrest to release <u>b/</u>	3.4		2.1	
<u>Type of Release</u>				
Own recognizance	68	94%	16	94%
Bond	4	6%	1	6%
TOTAL	72	100%	17	100%
^a Significant at the .003 level.				
^b n=66 for experimental group, and n=15 for control group.				

between experimental and control groups did not reach the level of statistical significance used as the cut-off point for our analyses; the significance level of .09 is, however, considered of "borderline" importance. Thus, this analysis suggests that age differences between the experimental and control groups do not account for the extent of the release rate difference between the two groups.

TABLE 5.4
RATE OF RELEASE, BY AGE AT ARREST

AGE AT ARREST	EXPERIMENTAL GROUP		CONTROL GROUP	
	Number	Percent	Number	Percent
<u>Under 30 Years of Age</u> ^a				
Defendants released	47	81%	9	56%
Defendants <u>not</u> released	11	19%	7	44%
TOTAL	58	100%	16	100%
<u>30 Years of Age or Older</u> ^b				
Defendants released	24	71%	8	40%
Defendants <u>not</u> released	10	29%	12	60%
TOTAL	34	100%	20	100%
^a Significant at the .09 level.				
^b Significant at the .05 level.				

Additional insight about the release rate difference can be gained from Table 5.5, which considers program impact for cases settled at the first court appearance versus those disposed of at a later hearing. As indicated, the experimental group had a much higher rate of release than the control group for cases settled at the first appearance. This result stems from the nature of the program: it can release defendants before arraignment, and such an early release is difficult to obtain in any other way (for defendants reaching this CJS processing stage). For cases that continued beyond arraignment there was no significant difference in the release rates of experimental versus control group defendants. Thus, the program impact on release rates is due primarily to its ability to release defendants who would otherwise be detained until the next day, when their cases would be settled. A subsequent section of this chapter considers whether these savings in detention costs, coupled with other program-related savings, offset the program's costs.

Tables 5.6—5.8 consider the equity of release for defendants of different ethnicity and employment status. As indicated in Table 5.6, the only significant difference in the experimental group was that it took unemployed defendants longer to gain release. Before considering the findings for the control group (see Table 5.7), it should be noted that the control group is relatively small in total and thus that comparisons of subgroups of defendants within it are based on very few cases. As indicated in Table 5.7, unemployed defendants in the control group were less likely to secure release, and those who were released required much longer to gain release. Thus, as summarized in Table 5.8, there was apparently some bias against unemployed defendants in terms of release outcomes for both the experimental and control groups. When post-release outcomes (e.g., failure to appear and pretrial criminality) were compared for released defendants who were unemployed versus employed, no significant differences were

TABLE 5.5
RATE OF RELEASE, BY CASE DISPOSITION POINT

CASE DISPOSITION POINT	EXPERIMENTAL GROUP		CONTROL GROUP	
	Number	Percent	Number	Percent
<u>Settled at First Court Appearance^a</u>				
Defendants released	35	69%	4	21%
Defendants <u>not</u> released	16	31%	15	79%
TOTAL	51	100%	19	100%
<u>Settled After First Court Appearance</u>				
Defendants released	37	86%	13	76.5%
Defendants <u>not</u> released	6	14%	4	23.5%
TOTAL	43	100%	17	100.0%
^a Significant at the .005 level.				

TABLE 5.6
EQUITY OF RELEASE FOR EXPERIMENTAL GROUP
(n=94)

OUTCOME	ETHNICITY				EMPLOYMENT STATUS			
	WHITE		MINORITY		EMPLOYED OR SUBSTITUTES		UNEMPLOYED	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
<u>Rate of Release (all defendants)</u>								
Defendants released	33	75%	39	78%	50	78%	20	74%
Defendants <u>not</u> released	11	25%	11	22%	14	22%	7	6%
TOTAL	44	100%	50	100%	64	100%	27	100%
<u>Speed of Release</u> ^a								
Mean number of days from arrest to release <u>b</u> /	0.5		5.9		0.6		9.7	
<u>Type of Release (for released defendants)</u>								
Nonfinancial	32	97%	36	92%	48	96%	20	100%
Financial	1	3%	3	8%	2	4%	0	0
TOTAL	33	100%	39	100%	50	100%	20	100%

^aSignificant at the .04 level for employment status; not significant for ethnicity.

^bn=31 for white, n=35 for minority, n=44 for employed, n=20 for unemployed

TABLE 5.7
EQUITY OF RELEASE FOR CONTROL GROUP
(n= 36)

OUTCOME	ETHNICITY				EMPLOYMENT STATUS			
	White		Minority		Employed or Substitutes		Unemployed	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
<u>Rate of Release (All Defendants)^a</u>								
Defendants released	11	55%	6	37.5%	14	70%	3	21%
Defendants <u>not</u> released	9	45%	10	62.5%	6	30%	11	79%
TOTAL	20	100%	16	100.0%	20	100%	14	100%
<u>Speed of Release^b</u>								
Mean number of days from arrest to release <u>c</u>	2.2		2.0		0.9		7.0	
<u>Type of Release (for Released Defendants)</u>								
Nonfinancial	11	100%	5	83%	14	100%	2	67%
Financial	0	0%	1	17%	0	0%	1	33%
TOTAL	11	100%	6	100%	14	100%	3	100%
^a Statistically significant at the .01 level for employment status; not significant for ethnicity. ^b Significant at the .007 level for employment status; not significant for ethnicity. ^c n=11 for white, n=4 for minority, n=12 for employed, n=3 for unemployed.								

TABLE 5.8
SUMMARY OF EQUITY OF RELEASE FOR
EXPERIMENTAL AND CONTROL GROUPS

Outcome	Experimental Group (n= 94)		Control Group (n= 36)	
	Ethnicity	Employment Status	Ethnicity	Employment Status
Rate of Release	N.S.	N.S.	N.S.	.01
Speed of Release	N.S.	.04	N.S.	.007
Type of Release	N.S.	N.S.	N.S.	N.S.

found. Even though this analysis was necessarily limited to released defendants, it suggests that unemployed persons do not pose a greater release risk than employed defendants. If this is the case, then the apparent bias against unemployed defendants in terms of release outcomes—a bias which was greater in the control group, though present in the experimental group as well—may be inequitable.

Table 5.9 compares the characteristics of released defendants in the experimental and control groups (see Appendix E for detailed data). There were two important differences: released defendants in the control group were much more likely to have been married and to be living with spouses or relatives.

As indicated in Table 5.10, there were no significant differences between the experimental and control groups for failure to appear or pretrial criminality rates. To assess whether this might be partly due to the different characteristics of released defendants in the two groups, we compared the failure to appear and pretrial criminality rates while holding constant first marital status and then living arrangement. No important differences were found, which suggests that the program in fact had no statistically significant impact on failure to appear or pretrial criminality rates. While program operations did not result in lower failure to appear or pretrial criminality rates, they also did not increase those rates—even though many more defendants secured release as a result of program operations.

Another topic of interest is the impact of program recommendations on outcomes. Because the program can itself release defendants, a recommendation for release has an obvious and immediate impact on release outcomes, as shown in Table 5.11. However, there were no differences in

TABLE 5.9
SUMMARY OF COMPARABILITY OF RELEASED DEFENDANTS
IN THE EXPERIMENTAL AND CONTROL GROUPS
(n=89)

CHARACTERISTIC	SIGNIFICANCE LEVEL
<u>Community Ties</u>	
Local residence status	N.S.
Years of local residence	N.S.
Months at present address	N.S.
Marital status	.02
Family support	N.S.
With whom defendant lives	.01
Employment status	N.S.
Public assistance	N.S.
Occupation	N.S.
<u>Criminality</u>	
Current charge	N.S.
Number of prior arrests	N.S.
Number of prior convictions	N.S.
Criminal justice system status at time of arrest	N.S.
Age at first adult arrest	N.S.
<u>Demographic Characteristics</u>	
Age at arrest	N.S.
Ethnicity	N.S.
Sex	N.S.
<u>Other</u>	
Education	N.S.

TABLE 5.10
FAILURE TO APPEAR AND PRETRIAL CRIMINALITY
FOR EXPERIMENTAL AND CONTROL GROUPS

OUTCOME	EXPERIMENTAL GROUP (n=72)		CONTROL GROUP (n=17)	
	Number	Percent	Number	Percent
<u>Failure to Appear (FTA)</u>				
Defendants who FTA	7	10%	3	18%
Defendants who do <u>not</u> FTA	65	90%	14	82%
TOTAL released defendants	72	100%	17	100%
<u>Pretrial Criminality</u>				
Defendants with rearrests	7	10%	2	12%
Defendants <u>without</u> rearrests	65	90%	15	88%
TOTAL released defendants	72	100%	17	100%
Defendants with rearrest convictions	5	7%	1	6%
Defendants <u>without</u> rearrest convictions	67	93%	16	94%
TOTAL released defendants	72	100%	17	100%

TABLE 5.11
OUTCOMES BY PROGRAM RECOMMENDATION
(EXPERIMENTAL GROUP ONLY)

OUTCOME	OWN RECOGNIZANCE (n=51)		OTHER (n=43)	
	Number	Percent	Number	Percent
<u>Rate of Release^a</u>				
Defendants released	51	100%	21	49%
Defendants <u>not</u> released	0	0%	22	51%
TOTAL	51	100%	43	100%
<u>Type of Release</u>				
Own Recognizance	49	96%	19	90.5%
Bond	2	4%	2	9.5%
TOTAL released defendants	51	100%	21	100.0%
<u>Failure to Appear (FTA)</u>				
Defendants who FTA	7	14%	0	0%
Defendants who do <u>not</u> FTA	44	86%	21	100%
TOTAL released defendants	51	100%	21	100%
<u>Pretrial Criminality</u>				
Defendants with rearrests	5	10%	2	9.5%
Defendants <u>without</u> rearrests	46	90%	19	90.5%
TOTAL released defendants	51	100%	21	100.0%
Defendants with rearrest convictions	3	6%	2	9.5%
Defendants <u>without</u> rearrest convictions	48	94%	19	90.5%
TOTAL released defendants	51	100%	21	100.0%
^a Significant at the .0000 level.				

the type of release or rates of failure to appear or pretrial criminality of released defendants who received an own recognizance (O.R.) release recommendation, as compared with defendants having other recommendations. This suggests that program criteria for developing release recommendations are not strongly associated with post-release defendant behavior. This is supported by the information presented in Table 5.12, comparing post-release outcomes with point system scores: released defendants with too few points to qualify for an O.R. release recommendation had rates of failure to appear and pretrial criminality that were no worse than those of defendants with higher point scores. (It is possible, of course, that defendants not released were worse risks and would, if released, have had much worse outcomes and thus have "validated" the point system criteria. Although this possibility could not be tested, the fact that no important differences were found among released defendants having different point totals makes us doubt its likelihood.)

TABLE 5.12
COMPARISON OF OUTCOMES FOR DEFENDANTS HAVING
TOO FEW POINTS VERSUS ENOUGH POINTS TO QUALIFY
FOR AN OWN RECOGNIZANCE RELEASE RECOMMENDATION

Outcome	Experimental Group				Control Group			
	Too Few Points		Enough Points		Too Few Points		Enough Points	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
<u>Rate of Release</u> ^a								
Defendants released	11	39%	59	92%	3	18%	13	81%
Defendants <u>not</u> released	17	61%	5	8%	14	82%	3	19%
TOTAL	28	100%	64	100%	17	100%	16	100%
<u>Failure to Appear (FTA)</u>								
Defendants who FTA	1	9%	6	10%	1	33%	2	15%
Defendants who do <u>not</u> FTA	10	91%	53	90%	2	57%	11	85%
TOTAL released defendants	11	100%	59	100%	3	100%	13	100%
<u>Pretrial Criminality</u>								
Defendants with rearrests	1	9%	6	10%	1	33%	1	8%
Defendants <u>without</u> rearrests	10	91%	53	90%	2	67%	12	92%
TOTAL released defendants	11	100%	59	100%	3	100%	13	100%
Defendants with rearrest convictions	1	9%	4	7%	0	0%	1	8%
Defendants <u>without</u> rearrest convictions	10	91%	55	93%	3	100%	12	92%
TOTAL released defendants	11	100%	59	100%	3	100%	13	100%
^a Significant at the .0000 level for the experimental group and at the .0009 level for the control group.								

C. Cost-Effectiveness of Program

At one level the cost-effectiveness of the Lincoln program can be assessed by observing that its operations resulted in more defendants securing release before trial (due mainly to the program's ability to release defendants before arraignment) and in somewhat greater fairness in the release decisions for unemployed defendants. These outcomes were not associated with different rates of failure to appear or pretrial criminality, in comparison with the control group. If the improved outcomes associated with program operations are valued at a sum at least as great as the program's budget, then the program would be considered cost-effective.

Another way of assessing cost-effectiveness is to consider costs to the criminal justice system, when the program operates versus does not. Table 5.13 summarizes these costs for the program (experimental) group. As shown, total costs were approximately \$10,000, with about half of this amount attributable to the program's interview, verification and release activities. Detention and pretrial arrests were the next most expensive items, with failure to appear accounting for only about 2% of total costs.

Costs for the control group, presented in Table 5.15, were approximately \$3,000. About two-thirds of these expenses were for detention. Pretrial arrests were the next most costly item, with failure to appear costs once again relatively low.

The costs for the program and control groups cannot be compared directly, because (1) the groups are of different size, and (2) the costs of detention and failure to appear should be weighted to reflect the lack of statistically significant differences between the two groups (see Table 5.10 and the accompanying discussion).²³ The necessary adjustments are shown in Table

²³The number of pretrial arrests is identical after the groups are weighted to reflect equal sizes.

TABLE 5.13
COST SUMMARY, PROGRAM GROUP,
LINCOLN EXPERIMENT
(n=94)

ITEM	COST
<u>Detention</u>	
375 days @ \$7 per day	\$ 2,625.00
<u>Failure To Appear (FTA)</u>	
• 9 occurrences	
—Municipal Court and City Attorney costs, 7 cases @ \$3.99	27.93
—County Court and County Attorney costs, 2 cases @ \$7.55	15.10
—Public Defender costs, 1 case @ \$2.77	2.77
—Bench warrant issued, 6 cases in Municipal Court @ \$1.06	6.36
—Bench warrant service, 1 attempt @ \$33	33.00
• 5 returns to court	
—Municipal Court and City Attorney costs, 4 cases @ \$11.88	47.52
—County Court and County Attorney costs, 1 case @ \$19.98	19.98
—Public Defender costs, 1 case @ \$8.18	8.18
—Bond forfeiture hearing, County Court, 1 case @ \$31.00	31.00
—No prosecution of FTA; no detention resulting from FTA; no bond forfeitures executed	
Subtotal, FTA costs	\$ 191.84
<u>Pretrial Arrest (PTA)</u>	
• Apprehension, 10 cases @ \$15.00	150.00
• Booking, 10 cases @ \$45.00	450.00
• Program costs	
—1 interview and verification @ \$46.80	46.80
• Detention, 10 days @ \$7 per day	70.00
• Court processing costs	
—Detailed in Table 5.14	1,337.50
—Jury costs, 1 trial day @ \$20 per day per juror	240.00
• Sentencing costs	
—No probation costs	
—No incarceration costs	
—Less fines for 6 defendants	-385.00
Subtotal, PTA costs	\$ 1,909.30
<u>Program</u>	
• Interview and verification, 94 defendants @ \$46.80	4,399.20
• Release, 51 defendants @ \$18.28*	932.28
Subtotal, Program costs	\$ 5,331.48
TOTAL COSTS	\$ 10,057.62
*Although a total of 68 defendants were released on own recognizance, only 51 of them were released by the program.	

TABLE 5.14
COURT PROCESSING COSTS FOR PRETRIAL ARRESTS,
PROGRAM GROUP, LINCOLN

Note: All pretrial arrests were tried in Municipal Court.

Outcome	Attorney	Total Number of Cases	Number of Cases Adjudicated on Same Day As:		Cost
			Original Arrest	Other Pretrial Arrest	
Dismissal	Public Defender	1	0	0	1 @ \$54 = \$54
Dismissal	Other than Public Defender	1	0	0	1 @ \$31 = \$31
Plea	Public Defender	1	0	0	1 @ \$116 = \$116
Plea	Other than Public Defender	1	1	0	½ @ \$93 = \$46.50
Convicted by judge	Public Defender	2*	1	1	2/3 @ \$333 = \$222
Convicted by judge	Other than Public Defender	1	0	0	1 @ \$310 = \$310
Convicted by jury	Other than Public Defender	1	1	0	½ @ \$310 = \$155
Open**	Other than Public Defender	2	0	0	2 @ \$201.50 = \$403
TOTAL COST = \$1,337.50					
*Same defendant; both the pretrial arrests and the original charge were adjudicated on the same day, so 2/3 of costs were allocated to pretrial arrest. **Costs of open cases were estimated as one-half the cost of a plea plus one-half the cost of a trial.					

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TABLE 5.15
COST SUMMARY, CONTROL GROUP,
LINCOLN EXPERIMENT
(n=36)

ITEM	COST
<u>Detention</u>	
297 days @ \$7 per day	\$ 2,079.00
<u>Failure to Appear (FTA)</u>	
• 4 occurrences	
—Municipal Court and City Attorney costs (no Public Defenders), 2 cases @ \$3.99	7.98
—County Court, County Attorney and Public Defender costs, 2 cases @ \$10.32	20.64
—Bench warrant issued, 1 case in Municipal Court @ \$1.06	1.06
—No bench warrants served	
• 3 returns to court	
—Municipal Court and City Attorney costs, 1 case @ \$11.88	11.88
—County Court, County Attorney and Public Defender costs, 2 cases @ \$28.16	56.32
—Bond forfeiture hearing, Municipal Court, 1 case @ \$11.49	11.49
—Bond forfeiture hearing, County Court, 2 cases @ \$31.00	62.00
—No prosecution of FTA; no detention resulting from FTA; no bond forfeitures executed	
Subtotal, FTA costs	\$ 171.37
<u>Pretrial Arrest (PTA)</u>	
• Apprehension, 4 cases @ \$15.00	60.00
• Booking, 4 cases @ \$45.00	180.00
• Detention, 3 days @ \$7 per day	21.00
• Court processing costs	
—Detailed in Table 5.16	623.50
—No jury costs	
• Sentencing costs	
—No probation costs	
—No incarceration costs	
—Less fine for 1 defendant	-10.00
Subtotal, PTA costs	\$ 874.50
TOTAL COSTS	\$ 3,124.87

TABLE 5.16
COURT PROCESSING COSTS FOR PRETRIAL ARRESTS,
CONTROL GROUP, LINCOLN

Note: All pretrial arrests were tried in Municipal Court.

Outcome	Attorney	Total Number of Cases	Number of Cases Adjudicated on Same Day As:		Cost
			Original Arrest	Other Pretrial Arrest	
Dismissal	Public Defender	1	0	0	1 @ \$54 = \$54
Convicted by judge	Public Defender	1	1	0	½ @ \$333 = \$166.50
Open*	Other than Public Defender	2	0	0	2 @ \$201.50 = \$403
TOTAL COST = \$623.50					
*Costs of open cases were estimated as one-half the cost of a plea plus one-half the cost of a trial.					

5.17. As indicated, weighted total costs for the program group were about one-half more than for the control group. Although the control group had somewhat higher failure to appear and rearrest costs, these additional costs were not enough to offset the costs of program operations.

It should be noted that the cost comparisons of Table 5.17 necessarily exclude any "intangible" benefits from the program's operations, such as greater fairness in the release process. Also, it should be remembered that programs such as the one in Lincoln that interview relatively small numbers of defendants often have relatively high costs of operation. As discussed in Volume I (see chapter II), such programs cannot achieve the economies of scale available to larger programs. This may help explain why the cost comparisons of Table 5.17 are not more favorable to the program.

TABLE 5.17
COMPARISON OF COSTS FOR PROGRAM AND
CONTROL GROUPS, LINCOLN EXPERIMENT

COST CATEGORY	PROGRAM GROUP		CONTROL GROUP	
	Quantity	Cost	Quantity	Cost
<u>Unweighted</u>				
Detention	375 days	\$2,625	297 days	\$2,079
Failure to appear	9 cases	192	4 cases	171
Pretrial arrest	10 cases	1,909	4 cases	875
Program	N.A.	5,331	N.A.	0
TOTAL		\$10,057		\$3,125
Number of defendants	94		36	
<u>Weighted To Reflect Equal Group Sizes</u>				
Detention	375 days	\$2,625	775 days	\$5,426
Failure to appear	9 cases	192	10 cases	446
Pretrial arrest	10 cases	1,909	10 cases	2,284
Program	N.A.	5,331	N.A.	0
TOTAL		\$10,057		\$8,156
Number of defendants	94		94	
<u>Weighted To Reflect Equal Detention and FTA</u>				
Detention	775 days	\$5,434	775 days	\$5,246
Failure to appear	10 cases	213	10 cases	446
Pretrial arrest	10 cases	1,909	10 cases	2,284
Program	N.A.	5,331	N.A.	0
TOTAL		\$12,887		\$8,156
Number of defendants	94		94	

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VI. THE BEAUMONT—PORT ARTHUR EXPERIMENT

A. Background

The Jefferson County Pretrial Information Program, serving the Beaumont—Port Arthur area, was initiated in 1974 with Federal funds (LEAA) dispensed through the Texas Criminal Justice Council.²⁴ The request for funding came as a result of a 1973 article passed by the Texas Legislature permitting county governments to establish personal bond programs. The county government assumed funding responsibility for the program upon the completion of the three-year Federal grant.

The primary goal of the program is to assist indigents in securing personal bond releases from the County Jail. Bail is currently the only form of pretrial release in Jefferson County; there is no own recognizance release. A personal bond is the least restrictive method of posting bail, requiring the defendant to post only a three percent premium on the bail amount (subject to a \$20 minimum fee), and can be effected only through program intervention. The 3% fees are retained by the court and help offset the costs of the program; no portion of the fee is returned to the defendant.

The program conducts most of its interviews at the County Jail after a defendant has been arraigned and bail has been set. The program has no contact with city jails where many defendants are initially booked. As a result, it cannot process many potentially eligible defendants, who post bond through bondsmen and bonding attorneys soon after initial booking.

The program operates from 8:00 AM to 5:00 PM, Monday through Friday. With the exception of several excluded charges, including capital offenses,

²⁴For more information on the program, see Lisa Crowley, Delivery System Analysis of Jefferson County (Beaumont—Port Arthur), Texas, Working Paper No. 10, February 1980.

all defendants arrested for State and county violations are eligible for program interview. The interview includes questions concerning the defendant's family background, employment, other income and expenses, and criminal history. The defendant is also asked to supply as many as four references. Verification of the information is done by telephone, and national and local criminal record checks are facilitated by a computer in the jail basement.

Evaluations are subjective (no point system), based conjunctively on the defendant's verified community ties, the nature of the current charge and the severity of the prior record. Recommendations are made only for personal bond release and are effected by the presentation of the interview and recommendation to the appropriate judge, who must approve the release. The program may, at this time, request a reconsideration of the bail amount only for those defendants approved for personal bond release.

All defendants released on personal bonds agree to retain private counsel in addition to payment of the 3% fee. They are also required to maintain weekly contact with the program, either by telephone or personal visit. The program notifies the defendants by telephone and often by mail of upcoming court dates and generally monitors the defendants' compliance with release conditions. If the defendant fails to comply with any of the conditions of release, such as by failing to appear, failing to report or failing to retain counsel, the program can file an Affidavit for Release of Surety (AFRS), resulting in the defendant's arrest and return to jail. For the period October 1978 to August 1979, actions in response to non-compliance were taken against 15.3% (57) of the defendants released on personal bonds.

Because the Beaumont—Port Arthur program was not processing all potentially eligible defendants, the jurisdiction was an appropriate one for an experimental study. Program operations could be expanded and a control group established without reducing the number of defendants processed by the program. Although we considered an expansion to the pre-arraignment level, the logistics of working at the various J.P. court locations were too cumbersome to implement. Instead, the program expanded its staff to provide for more interviewing capability at the County Jail. The program initially expanded its hours of operation as well but soon concluded that this was unnecessary and returned to daytime, weekday interviewing only.

All defendants interviewed during the time period of the experiment were randomly assigned to one of two groups. The program (experimental) group was processed by the program, using its normal procedures. The control group did not receive program processing (interviews with control group defendants were sent to us for inclusion in the data base only).

Although we had anticipated obtaining about 800 defendants for the study within a 4-6 month period, only about 200 defendants ultimately participated in the study. The original estimate of 800 had been based on an analysis of County Jail bookings. However, after the experiment began, we learned that many of these bookings did not represent potential program clients. This was because the defendants had arranged for bonds before they were transferred to the County Jail and were simply waiting for their releases to be effected. Thus, they were not interested in becoming program clients. An additional, though less significant, reason for fewer experimental defendants than anticipated was a decline in the number of arrests after the experiment began.

B. Outcomes of Experiment

As shown in Table 6.1, data were available for more than half the defendants in each group for all 19 of the background characteristics considered. Table 6.2 indicates that the groups were not comparable (.05 level) for six of these characteristics. Thus, Beaumont-Port Arthur was the only experimental site showing a major lack of comparability between the experimental and control groups, a circumstance that both complicates and lessens the strength of subsequent analyses. As detailed in Appendix E, defendants in the experimental group were more likely to have families they were supporting, to have an occupation other than laborer, to have fewer prior arrests, to have been older at the time of their first adult arrests, to be female and to have more education.

Table 6.3 compares the rate and speed of release for the experimental and control groups (see Appendix E for detailed data on speed of release).²⁵ The program appears to have had a highly significant impact on release rates: 86% of the experimental group secured release, but only 57% of the control group did so.

Because of the lack of comparability between the experimental and control groups, it is important to consider whether the difference in release rates is in fact likely to have been due to program operations or rather was the result of the different characteristics of the defendants in the two groups. Consequently, the rate of release was compared between the two groups while controlling, one at a time, for the six differences in defendant background characteristics noted earlier. Results appear in Table 6.4. The percentage released is higher in the experimental group

²⁵Type of release was not compared, because the nature of the program ensured a difference: defendants could be released on 3% bond only if they were in the experimental group, except in unusual cases.

TABLE 6.1

PERCENTAGES OF CASES IN EXPERIMENTAL AND CONTROL GROUPS WITH DATA FOR SELECTED CHARACTERISTICS

CHARACTERISTIC	EXPERIMENTAL GROUP (n=132)	CONTROL GROUP (n=61)
<u>Community Ties</u>		
Local residence status	100%	100%
Years of local residence	89%	97%
Months at present address	98%	97%
Marital status	98%	100%
Family support	62%	70%
With whom defendant lives	100%	98%
Employment status	100%	100%
Income level	63%	54%
Public assistance	54%	56%
Occupation	77%	79%
<u>Criminality</u>		
Current charge	100%	100%
Number of prior arrests	100% ^a	100% ^a
Number of prior convictions	100% ^a	100% ^a
Criminal justice system status at time of arrest	95%	95%
Age at first adult arrest	78%	69%
<u>Demographic Characteristics</u>		
Age at arrest	100%	100%
Ethnicity	100%	100%
Sex	100%	100%
<u>Other</u>		
Education	98%	98%

^aIncludes cases with missing information.

TABLE 6.2

SUMMARY OF COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS (n=193)

CHARACTERISTIC	SIGNIFICANCE LEVEL
<u>Community Ties</u>	
Local residence status	N.S.*
Years of local residence	N.S.
Months at present address	N.S.
Marital status	N.S.
Family support	.02
With whom defendant lives	N.S.
Employment status	N.S.
Income level	N.S.
Public assistance	N.S.
Occupation	.01
<u>Criminality</u>	
Current charge	N.S.
Number of prior arrests	.01
Number of prior convictions	N.S.
Criminal justice system status at time of arrest	N.S.
Age at first adult arrest	.04
<u>Demographic Characteristics</u>	
Age at arrest	N.S.
Ethnicity	N.S.
Sex	.01
<u>Other</u>	
Education	.01

* Indicates the two groups were not significantly different (.05 statistical level).

TABLE 6.3

RATE AND SPEED OF RELEASE FOR EXPERIMENTAL AND CONTROL GROUPS

Outcome	Experimental Group (n=132)		Control Group (n=61)	
	Number	Percent	Number	Percent
<u>Rate of Release</u> ^a				
Defendants released	113	86%	35	57%
Defendants <u>not</u> released	19	14%	26	43%
TOTAL	132	100%	61	100%
<u>Speed of Release</u>				
Mean number of days from arrest to release <u>b/</u>	7.1		5.5	

^aSignificant at the .0000 level.

^bn=102 for experimental group and n=34 for control group.

TABLE 6.4
RATE OF RELEASE BY SELECTED DEFENDANT CHARACTERISTICS,
BEAUMONT-PORT ARTHUR EXPERIMENT

CHARACTERISTIC	EXPERIMENTAL GROUP		CONTROL GROUP		STATISTICAL SIGNIFICANCE LEVEL
	Number Released	Percent	Number Released	Percent	
All Defendants	113	86%	35	57%	.0000
<u>Family Support</u>					
Supports family	36	95%	12	71%	.0408
Does not support family	36	82%	15	58%	.0555
<u>Occupation</u>					
Other than laborer	63	94%	11	61%	.0010
Laborer	24	71%	16	53%	.2444
<u>Number of Prior Arrests</u>					
None	56	95%	13	62%	.0007
One through three	41	77%	12	60%	.1030
Four or more	16	70%	10	50%	.3192
<u>Age at First Adult Arrest</u>					
21 or younger	36	74%	13	50%	.0755
22 or older	77	93%	22	63%	.0002
<u>Sex</u>					
Male	84	83%	33	58%	.0010
Female	29	94%	2	50%	.0816
<u>Education</u>					
More than high school	32	94%	4	57%	.0368
High school graduate	41	91%	8	44%	.0002
Less than high school	38	75%	22	63%	.3592

for every sub-group of defendants considered; however, the differences do not always reach a level of statistical significance. In general, it appears that the impact of program processing was not able to override the adverse effect on release of (1) employment as a laborer, (2) a longer prior record, or (3) low education. In all other instances, program processing seems to have had a significant impact on the release rate.

Another impact of the program, inherent in its operations, is that it permits defendants to secure release by posting a smaller percentage of the bond amount than would otherwise be required. Table 6.5 summarizes these savings to defendants. As shown, defendants released through the program paid \$6,712.50 in fees; release through a bondsman or attorney would have cost at least \$20,762.50.²⁶ Thus, although there were no differences in the total bond amounts set for the experimental and control groups, the experimental group realized a significant savings due to the lower program fees.

Tables 6.6—6.8 summarize the findings regarding equity of release. As indicated, there were no differences in either group in the rate or speed of release for white versus minority defendants or for employed versus unemployed defendants.

Table 6.9 compares the characteristics of released defendants in the experimental and control groups. As was the case for the comparison of all defendants, there are major differences between the two groups (see Appendix E for detailed data). Released defendants in the experimental group had lived at their present address for a shorter period of time, were more likely to be supporting families, were less likely to be laborers, had fewer prior arrests, were more likely to be women and were better educated.

²⁶This amount was based on a 10% bonding fee. Reportedly, actual fees are often higher (12%—15%).

TABLE 6.5
FEES PAID BY DEFENDANTS RELEASED THROUGH PROGRAM,
COMPARED WITH ESTIMATED FEES OF BONDING AGENTS

Bond Amount	Number of Defendants Released Through Program	Program Fee	Total Fees	Estimated Bonding Agents' Fee	Total Estimated Fees
\$250 or less	5	\$ 20.00	\$ 100.00	\$ 12.50	\$ 62.50
\$250—\$500	46	20.00	920.00	37.50	1,725.00
\$501—\$1,000	9	22.50	202.50	75.00	675.00
\$1,001—\$1,500	5	37.50	187.50	125.00	625.00
\$1,501—\$2,000	5	52.50	262.50	175.00	875.00
\$2,001—\$2,999	4	75.00	300.00	250.00	1,000.00
\$3,000—\$4,999	2	120.00	240.00	400.00	800.00
\$5,000—\$9,999	12	225.00	2,700.00	750.00	9,000.00
\$10,000 or more	6	300.00	1,800.00	1,000.00	6,000.00
TOTAL	94	XX	\$6,712.50	XX	\$20,762.50

TABLE 6.6
EQUITY OF RELEASE FOR EXPERIMENTAL GROUP
(n=132)

OUTCOME	ETHNICITY				EMPLOYMENT STATUS			
	WHITE		MINORITY		EMPLOYED OR SUBSTITUTES		UNEMPLOYED	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
<u>Rate of Release (all defendants)</u>								
Defendants released	58	91%	55	81%	84	89%	29	76%
Defendants <u>not</u> released	6	9%	13	19%	10	11%	9	24%
TOTAL	64	100%	68	100%	94	100%	38	100%
<u>Speed of Release (released defendants)</u>								
Mean number of days from arrest to release ^a	5.7		8.5		5.3		11.0	

^an=49 for white; n=53 for minority; n=69 for employed; n=27 for unemployed.

TABLE 6.7
EQUITY OF RELEASE FOR CONTROL GROUP
(n=61)

OUTCOME	ETHNICITY				EMPLOYMENT STATUS			
	WHITE		MINORITY		EMPLOYED OR SUBSTITUTES		UNEMPLOYED	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
<u>Rate of Release (all defendants)</u>								
Defendants released	17	71%	18	49%	25	64%	10	46%
Defendants <u>not</u> released	7	29%	19	51%	14	36%	12	54%
TOTAL	24	100%	37	100%	39	100%	22	100%
<u>Speed of Release (released defendants)</u>								
Mean number of days from arrest to release <u>a/</u>	7.1		5.3		5.3		5.9	

^an=15 for white; n=15 for minority; n=24 for employed; n=10 for unemployed.

TABLE 6.8
SUMMARY OF EQUITY OF RELEASE FOR
EXPERIMENTAL AND CONTROL GROUPS

OUTCOME	EXPERIMENTAL GROUP (n=132)		CONTROL GROUP (n=61)	
	Ethnicity	Employment Status	Ethnicity	Employment Status
Rate of Release	N.S.	N.S.	N.S.	N.S.
Speed of Release	N.S.	N.S.	N.S.	N.S.

TABLE 6.9
SUMMARY OF COMPARABILITY OF RELEASED DEFENDANTS
IN THE EXPERIMENTAL AND CONTROL GROUPS
(n=148)

CHARACTERISTIC	SIGNIFICANCE LEVEL
<u>Community Ties</u>	
Local residence status	N.S.
Years of local residence	N.S.
Months at present address	.05
Marital status	N.S.
Family support	.04
With whom defendant lives	N.S.
Employment status	N.S.
Income level (employed defendants only)	N.S.
Public assistance	N.S.
Occupation	.01
<u>Criminality</u>	
Current charge	N.S.
Number of prior arrests	.02
Number of prior convictions	N.S.
Criminal justice system status at time of arrest	N.S.
Age at first adult arrest	N.S.
<u>Demographic Characteristics</u>	
Age at arrest	N.S.
Ethnicity	N.S.
Sex	.02
<u>Other</u>	
Education	.03

TABLE 6.10
FAILURE TO APPEAR AND PRETRIAL CRIMINALITY
FOR EXPERIMENTAL AND CONTROL GROUPS

OUTCOME	EXPERIMENTAL GROUP (n=113)		CONTROL GROUP (n= 35)	
	Number	Percent	Number	Percent
<u>Failure to Appear (FTA)</u>				
Defendants who FTA	16	14%	7	20%
Defendants who do <u>not</u> FTA	97	86%	28	80%
TOTAL released defendants	113	100%	35	100%
<u>Pretrial Criminality</u> ^a				
Defendants with rearrests	6	5%	5	14%
Defendants <u>without</u> rearrests	107	95%	30	86%
TOTAL released defendants	113	100%	35	100%
Defendants with rearrest convictions	3	3%	5	14%
Defendants <u>without</u> rearrest convictions	110	97%	30	86%
TOTAL released defendants	113	100%	35	100%
^a Significant at the .03 level for convictions; not significant for arrests.				

As shown in Table 6.10, there were no statistically significant (.05 level) differences between the experimental and control groups for failure to appear and pretrial rearrest rates. However, defendants in the experimental group were much less likely to be convicted of pretrial arrest charges: only 3% of the released defendants in the experimental group were convicted for pretrial arrests, as compared with 14% of the released defendants in the control group.

Thus, the higher release rates in the experimental group did not result in higher rates of failure to appear or pretrial criminality. Indeed, if conviction for pretrial arrest is considered the most appropriate measure of pretrial criminality, because only convictions reflect findings of guilt, then pretrial crime was actually less in the group with the higher release rate.

The post-release outcomes may, of course, be due to the different characteristics of defendants in the two groups, rather than to program impact. Because of the relatively small number of defendants who failed to appear or were rearrested pretrial, we cannot test this possibility through controls of the type used in the analysis of release rate differences.

The impact of program recommendations is shown in Table 6.11. They had a highly significant impact on the rate of release: all defendants recommended for program release secured release, as compared with 76.5% of the other defendants in the experimental group. For released defendants, program recommendations were not strongly correlated with failure to appear or pretrial criminality rates. Released defendants recommended for program release and those with other recommendations were equally likely to fail to appear, be rearrested or be convicted of a pretrial arrest.

C. Cost-Effectiveness of Program

At one level, the cost-effectiveness of the Beaumont-Port Arthur

TABLE 6.11
OUTCOMES BY PROGRAM RECOMMENDATION
(EXPERIMENTAL GROUP ONLY)

Outcome	Program Release (n= 51)		Other (n= 81)	
	Number	Percent	Number	Percent
<u>Rate of Release</u> ^a				
Defendants released	51	100%	62	76.5%
Defendants <u>not</u> released	0	0%	19	23.5%
TOTAL	51	100%	81	100.0%
<u>Failure To Appear (FTA)</u>				
Defendants who FTA	5	10%	11	18%
Defendants who do <u>not</u> FTA	46	90%	51	82%
TOTAL released defendants	51	100%	62	100%
<u>Pretrial Criminality</u>				
Defendants with rearrests	2	4%	4	6.5%
Defendants <u>without</u> rearrests	49	96%	58	93.5%
TOTAL released defendants	51	100%	62	100.0%
Defendants with rearrest convictions	2	4%	1	2%
Defendants <u>without</u> rearrest convictions	49	96%	61	98%
TOTAL released defendants	51	100%	62	100%

^aSignificant at the .0005 level.

program can be assessed by comparing its costs and impacts and making a value judgment about whether those impacts are sufficiently large to offset program costs. Tangible program effects include the following:

- On the whole, more defendants were released. Exceptions were defendants with longer prior records, low education or a laborer's occupation.
- Defendants were able to secure release at a much reduced fee (3% through the program, versus at least 10% through a bondsman or attorney).
- The increased rate of release did not result in higher rates of failure to appear or pretrial criminality. Indeed, the rate of convictions for pretrial arrests was significantly lower in the experimental than in the control group. However, these findings may be at least partly due to the different characteristics of released defendants in the two groups, rather than to program impact alone.

If these benefits, plus any intangible program effects not included in the present study, were deemed worth the program's costs, then the program would be considered a cost-effective one.

Another way to analyze cost-effectiveness is to consider the costs to the criminal justice system of having the program operate versus not operate. Tables 6.12—6.15 summarize the cost data for the program (experimental) and control groups. Cost estimation details appear in Appendix B. Most items were calculated as in the other sites studied. The major difference between Beaumont-Port Arthur and the other sites is that the program collects fees from released defendants. Consequently, these fees are used to offset part of the costs for the program group. One other cost difference for Beaumont-Port Arthur is that the jurisdiction levies a \$15 per month fee on each defendant on probation. Thus, the costs to the criminal justice system of probation supervision are much less than in the other sites studied.

TABLE 6.12
COST SUMMARY, PROGRAM GROUP,
BEAUMONT-PORT ARTHUR EXPERIMENT
(n = 132)

Item	Cost
<u>Detention</u>	
1,622 days at \$4.32 per day	\$ 7,007.04
<u>Failure To Appear (FTA)</u>	
• 18 occurrences (assumed not at trial)	
—District Court, 10 cases @ \$20.08	200.80
—County Court, 8 cases @ \$19.49	155.92
—Bench warrant served, 7 cases @ \$15.66 (\$7.66 for service and \$8.00 for booking)	109.62
—Bench warrant service attempt, 4 cases @ \$7.66	30.64
• 14 returns to court	
—District Court, 9 cases @ \$27.11	243.99
—County Court, 5 cases @ \$24.89	124.45
—No prosecution of FTA	
—Detention resulting from FTA, 94 days @ \$4.32	406.08
—Less bond forfeitures executed in 3 cases	-2,000.00
Subtotal, FTA costs	-\$728.50
<u>Pretrial Arrest (PTA)</u>	
• Apprehension, 8 cases @ \$7.66	61.28
• Booking, 8 cases @ \$8.00	64.00
• Program costs, 1 interview @ \$35.93	35.93
• Detention resulting from PTA, 60 days @ \$4.32	259.20
• Court processing costs	
—Detailed in Table 6.12	2,873.00
—No jury costs	
• Sentencing costs	
—Pre-Sentence Investigations: 3 convicted defendants (4 cases); weighted, 1.67 @ \$68.00	113.56
—Probation Supervision: 2 defendants, 6 years @ \$177.00 less \$15.00 per month reimbursement from defendants	-18.00
—Incarceration: 2 defendants, 85.5 months @\$129.60	11,080.80
—Less fine for 1 defendant (paid)	-300.00
Subtotal, PTA costs	\$14,169.77
<u>Program</u>	
• Interview, 132 defendants @ \$35.93	4,742.76
• Followup, 94 defendants @ \$63.18	5,938.92
• Detention resulting from violation of release conditions, 10 days @ \$4.32	43.20
• Less fees @ 3% of bond amounts	-6,712.50
Subtotal, Program Costs	\$4,012.38
TOTAL COSTS	\$24,460.69

TABLE 6.13
COURT PROCESSING COSTS FOR PRETRIAL ARRESTS,
PROGRAM GROUP, BEAUMONT-PORT ARTHUR

Note: All attorneys were privately retained.

Court	Outcome	Total Number of Cases	Number of Cases Adjudicated on Same Day As:		Cost
			Original Arrest	Other PTA	
District	Plea	2	1	0	1½ @ \$372 = \$558.00
District	Open*	2	0	0	2 @ \$806 = \$1612.00
County	Plea	2	1	0	1½ @ \$177 = \$265.50
County	Open*	1	0	0	1 @ \$383.50 = \$383.50
Justice	Dismissal	1	0	0	1 @ \$54 = \$54.00
TOTAL COST					\$2,873.00
*Costs of open cases were estimated as one-half the cost of a plea plus one-half the cost of a trial.					

TABLE 6.14
COST SUMMARY, CONTROL GROUP,
BEAUMONT—PORT ARTHUR EXPERIMENT
(n=61)

ITEM	COST
<u>Detention</u>	
1,629 days @ \$4.32 per day	\$ 7,037.28
<u>Failure To Appear (FTA)</u>	
• 9 occurrences (assumed not at trial)	
—District Court, 7 cases @ \$ 20.08	140.56
—County Court, 2 cases @ \$ 19.49	38.98
—Bench warrant served, 3 cases @ \$7.66 for service and \$ 8.00 for booking	46.98
—Bench warrant service attempt, 2 cases @ \$ 7.66	15.32
• 7 returns to court	
—District Court, 6 cases @ \$ 27.11	162.66
—County Court, 1 case @ \$ 24.89	24.89
—No prosecution of FTA	
—Detention resulting from FTA, 24 days @ \$ 4.32	103.68
—Less bond forfeitures executed in 2 cases	- 3,500.00
Subtotal, FTA costs	-\$ 2,966.93
<u>Pretrial Arrest (PTA)</u>	
• Apprehension, 8 cases @ \$ 7.66	61.28
• Booking, 8 cases @ 8.00	64.00
• No detention resulting from PTA	
• Court processing costs	
—Detailed in Table 6.14	1,438.24
—No jury costs	
• Sentencing costs	
—Pre-Sentence Investigations:	
5 convicted defendants (7 cases); weighted, 4.67 @ \$ 68.00	317.56
—No probation supervision costs	
—Incarceration: 3 defendants, 109 months, @ \$ 129.60	14,126.40
—Less fines for 2 defendants (paid)	-858.00
Subtotal, PTA costs	\$ 15,149.48
TOTAL COSTS	\$ 19,219.83

TABLE 6.15
COURT PROCESSING COSTS FOR PRETRIAL ARRESTS,
CONTROL GROUP, BEAUMONT-PORT ARTHUR

Note: All attorneys were privately retained.

Court	Outcome	Total Number of Cases	Number of Cases Adjudicated on Same Day As:		Cost
			Original Arrest	Other PTA	
District	Plea	3	2	1	1.17 @ \$372 = \$435.24*
County	Dismissal	1	0	0	1 @ \$59 = \$59.00
County	Plea	3	0	1	2 @ \$177 = \$354.00
County	Convicted by judge	1	0	0	1 @ \$590 = \$590.00
TOTAL COST					\$1,438.24
*One defendant had 2 PTA's, both settled with original charge; two-thirds of costs were allocated to PTA. The other defendant in this category had one PTA, settled with original charge; one-half of costs were allocated to PTA.					

Costs for the program group totaled about \$24,000, with more than half accounted for by pretrial arrest expenses (largely the incarceration costs for two defendants). Costs for detention were about half the pretrial arrest costs, with program costs the next most expensive item. Failure to appear (FTA) actually generated funds for the criminal justice system, because the bond forfeitures collected more than offset FTA-related expenses.

Costs for the control group amounted to about \$19,000. As with the program group, pretrial arrest was the most expensive cost category; most of those costs stemmed from incarceration expenses for three defendants. Detention accounted for about one-third of total costs, and failure to appear generated revenue for the criminal justice system.

Costs for the program and control groups cannot be compared directly, because (1) the groups are of different size, and (2) the costs of failure to appear should be weighted to reflect the lack of statistically significant differences between the two groups.²⁷ The necessary adjustments are shown in Table 6.16. As indicated, total costs for the two groups were about equal. Thus, the costs of operating the program are apparently offset by saved failure to appear, pretrial arrest and detention costs.

²⁷ For both the number of pretrial arrests (though not the number of defendants with pretrial arrests, as shown in Table 6.10) and detention, the differences between the groups were statistically significant.

TABLE 6.16
COMPARISON OF COSTS FOR PROGRAM AND CONTROL GROUPS,
BEAUMONT-PORT ARTHUR EXPERIMENT

Cost Category	Program Group		Control Group	
	Quantity	Cost	Quantity	Cost
<u>Unweighted</u>				
Detention	1622 da.	\$7,007	1629 da.	\$7,037
Failure to appear	18 cases	-729	9 cases	-2,967
Pretrial arrest:				
Arrest	8 cases	3,293	8 cases	1,564
Sentencing	4 cases	10,876	7 cases	13,586
Program	N.A.	4,012	N.A.	0
TOTAL	132 def'ts.	\$24,459	61 def'ts.	\$19,220
<u>Weighted To Reflect Equal Group Sizes</u>				
Detention	1622 da.	\$7,007	3519 da.	\$15,200
Failure to appear	18 cases	-729	19 cases	-6,409
Pretrial arrest:				
Arrest	8 cases	3,293	17 cases	3,378
Sentencing	4 cases	10,876	15 cases	29,346
Program	N.A.	4,012	N.A.	0
TOTAL	132 def'ts.	\$24,459	132 def'ts.	\$41,515
<u>Weighted To Reflect Equal FTA and Pretrial Arrest</u>				
Detention	1622 da.	\$7,007	3519 da.	\$15,200
Failure to appear	19 cases	-773	19 cases	-6,409
Pretrial arrest:				
Arrest	8 cases	3,293	17 cases	3,378
Sentencing	4 cases	10,876	15 cases	29,346
Program	N.A.	4,012	N.A.	0
TOTAL	132 def'ts.	\$24,415	132 def'ts.	\$41,515

VII. SUMMARY, CONCLUSIONS AND RECOMMENDATIONS

A. Program Impact on Release Outcomes

Four of the five programs studied experimentally showed an impact on release outcomes; only the Tucson misdemeanor program did not. As shown in Table 7.1, 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 measure affected favorably. Lincoln was next (two measures affected), followed by Beaumont-Port Arthur (one measure affected) and Tucson (one measure affected for the felony study and none for the misdemeanor experiment).

The apparent effect of program operations on release equity deserves special comment. In two sites the program 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.²⁸ In the remaining three experiments there were no differences in release rates for minority versus white defendants in either the program or control group.²⁹

The analysis of equity of release by employment status found two sites where program operations were associated with more equitable

²⁸ No other factors were identified that might account for this difference (e.g., the other characteristics of white versus minority defendants in the two groups were quite similar).

²⁹ Although Tucson misdemeanor release rates were significantly higher for minority defendants in the program group, this was due to their higher employment rate.

TABLE 7.1
SUMMARY OF PROGRAM IMPACT ON RELEASE OUTCOMES

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

OUTCOME	TUCSON FELONIES	TUCSON MISDEMEANORS	BALTIMORE CITY	LINCOLN	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 Ethnicity	+	0	+	0	0
By Employment Status	0	0	+	+	0

*Not applicable.

release of unemployed defendants than occurred in the control group. In two of the remaining experiments (the Tucson felony and misdemeanor analyses), both the program and control groups showed unemployed defendants detained at a significantly higher rate than employed persons. Only one experiment had equivalent release rates for unemployed and employed defendants in both the program and control groups. This suggests that equity of release by employment status (our proxy for income level) may remain a problem after 20 years experience with bail reforms designed to eliminate the discrimination against poor persons that was inherent in a money bail system. Although pretrial release programs reduced this inequity in some of the jurisdictions studied, they did not always do so.

B. Program Impact on Failure To Appear and Pretrial Criminality

Table 7.2 summarizes program impact on post-release outcomes, specifically, failure to appear, pretrial rearrest, and pretrial rearrest conviction rates. For the three experiments where only a pre-release random assignment occurred, there was one instance of significant outcomes difference between the program and control groups: in Beaumont-Port Arthur released program group defendants had lower rates of rearrest conviction than control group defendants. Thus, in none of these three sites were program operations associated with worse failure to appear 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 program group defendants. Thus, the release of additional defendants did not lead to increased disruption of court operations

TABLE 7.2
SUMMARY OF PROGRAM IMPACT ON FAILURE TO APPEAR AND
PRETRIAL CRIMINALITY

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

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

(through higher failure to appear rates) or to greater harm to community safety (through higher pretrial criminality rates).

For the two sites where random assignment occurred after release, so that the impact of program followup activities could be tested, no differences were found between the program and control groups for either failure to appear or pretrial criminality. Although this suggests that program followup after release has no impact on defendant behavior, one must remember that the tests of followup impact were quite limited in scope. In one jurisdiction, mail/telephone notification of coming court dates was tested, and in the other site the impact of minimal versus more intensive supervision was analyzed. Thus, although our findings suggest that programs' post-release activities may have little effect, these findings cannot be considered conclusive.

This issue is now receiving further scrutiny under an NIJ-sponsored "test design" of the impact of supervised release. When finished, that study will provide additional insight about the effect of programs' post-release activities on defendants' outcomes.

C. Cost-Effectiveness of Programs

Program cost-effectiveness, measured by comparing costs to the criminal justice system for the program and control groups, varied widely. The most cost-effective programs were not necessarily those that showed the greatest impact on defendant outcomes.

As shown in Table 7.3, the most cost-effective program operated at the Tucson felony level, where program group costs were only 38% of control group costs. Both the Beaumont-Port Arthur program and Baltimore City's changed release criteria (first random assignment) were also cost-effective. Baltimore City's post-release followup (second random assignment), the Lincoln program and the Tucson misdemeanor program resulted in

TABLE 7.3
SUMMARY OF COST-EFFECTIVENESS DATA

EXPERIMENT	PROGRAM GROUP COSTS	CONTROL GROUP COSTS	RATIO OF PROGRAM TO CONTROL GROUP COSTS
Tucson Felony	\$ 54,870	\$ 145,431	0.38
Beaumont-Port Arthur	24,415	41,515	0.59
Baltimore City:			
Release	3,512	5,559	0.63
Post-Release	48,864	27,657	1.77
Lincoln	12,887	8,156	1.58
Tucson Misdemeanor	24,020	5,117	4.69

higher costs for the program group; those costs exceeded control group costs by 58% to 369%.

The 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. Nor was Baltimore City's more intensive supervision cost-effective, when compared with the minimal supervision received by the control group.

Additional insight concerning cost implications of program operations can be gained by considering the relative contributions of various cost elements to total costs. Table 7.4 presents this information. As shown, failure to appear was the least costly category in all sites except Baltimore City. Indeed, in one jurisdiction (Beaumont-Port Arthur) failure to appear actually generated revenue, because of the court's policy of collecting on bond forfeitures. These collections more than offset the additional CJS processing costs incurred by the failures to appear.

Although actual failure to appear (FTA) costs were relatively low, it is important to point out that potential FTA costs are quite high. In the jurisdictions studied, prosecution for FTA was rare; during our 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 for failure to appear is a cost-effective one for the criminal justice system.

Program costs ranged from 16% to 74% of total costs for the program groups studied. These costs were major contributors to total costs in each of the experiments where programs were not cost-effective.

TABLE 7.4
PERCENTAGE OF TOTAL COSTS, BY MAJOR COST CATEGORIES

Note: Percentages may not add to 100% due to rounding.

	TUCSON FELONY		BEAUMONT- PORT ARTHUR		BALTIMORE CITY		LINCOLN		TUCSON MISDEMEANOR	
COST CATEGORY	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group	Program Group	Control Group
Detention	47%	18%	29%	37%	3%	11%	42%	67%	16%	76%
Failure to Appear	1%	1%	-3%	-15%	10%	11%	2%	5%	2%	6%
Pretrial Arrest	33%	81%	58%	79%	33%	70%	15%	28%	8%	18%
Program	19%	1%	16%	0%	55%	9%	41%	0%	74%	0%
TOTAL	100%	100%	100%	100%	100%	100%	100%	100%	100%	100%

For the control groups studied, the highest cost category was pretrial arrest in the experiments that included felony defendants and detention in the two misdemeanor-only experiments. The high pretrial arrest costs in the experiments including felony cases 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. For the Tucson felony control group, 78% of the total pretrial arrest costs were due to incarceration costs (for six defendants, sentenced to a total of more than 25 years in prison). Comparable percentages for Beaumont-Port Arthur and Baltimore City were 90% and 40%, respectively.

For the misdemeanor-only experiments, pretrial arrests costs were much less important contributors to total costs. Moreover, sentencing costs often reflected a profit for the criminal justice system, because of the more frequent use of fines and less common use of incarceration as sentences for misdemeanants.

This analysis of cost elements suggests that, in cost terms at least, there is little need for concern about failure to appear. On the other hand, pretrial arrest seems to be much more costly to the criminal justice system than is commonly realized. Because much of this cost is due to the incarceration of a few defendants, jurisdictions seeking cost savings may need to take a closer look at whether the benefits of those expenses merit their costs.

Another aspect of the analysis of sentencing costs is the way those costs differ for the program 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 program group. In particular,

the extent of incarceration in the control group was much greater than in the program group. This suggests that the operations of pretrial release programs may lead 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 occurs is not known. Programs may serve as advocates for rearrested defendants and help ameliorate sentences by providing information about defendants' circumstances that would not otherwise be available. It is also possible that the mere existence of more complete information about defendants has a positive effect on sentences, without the need for programs to serve as advocates. Alternatively, programs may affect defendant behavior so that less serious crimes are committed, with the difference in sentencing severity reflecting this fact. Finally, it is possible that our findings for three sites would not be replicated if additional jurisdictions were studied. Because of the importance of the topic, we recommend that it receive further study.

As noted earlier, the two misdemeanor-only programs were not found to be cost-effective. This occurred for two main reasons. First, most defendants charged with misdemeanors were released relatively quickly in both the program and control groups. Thus, there was little opportunity for program operations to generate savings in detention costs. Second, the rearrested defendants were rearrested on relatively minor charges 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.³⁰

At the felony level the more serious nature of the cases led to higher bonds and the potential for much longer periods of detention. Additionally, rearrested defendants were rearrested on more serious charges, having more severe penalties. Thus, felony-level programs—by affecting detention and pretrial arrest costs—were able to generate savings that could offset their costs.

In addition to the lack of cost-effectiveness for misdemeanor programs, we found that the post-release followup activities studied were not cost-effective. Neither of the two experimental tests of program followup activities 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 of the Tucson misdemeanor program vis-a-vis the control group (see Table 7.3).

Finally, the limitations of the cost-effectiveness analysis presented above should be stressed once again. The analysis was based on a consideration of costs to the criminal justice system only and thus excludes many possible costs and benefits that might reasonably be included in an analysis conducted from a different perspective. Moreover, the cost estimates derived were often very rough ones, because of the difficulties of obtaining the data needed for more accurate estimates from the sites studied. Thus, the cost-effectiveness findings should be viewed as suggestive, rather than definitive, and should be subjected to further testing

³⁰This suggests that misdemeanor-level programs might be cost-effective in jurisdictions where defendants charged with misdemeanors are detained for long periods of time or where they are rearrested for charges carrying severe penalties.

in other jurisdictions.

D. Conclusions

The experimental analysis of program impact leads to the following conclusions:

- The pretrial release programs studied had a positive effect on the release of defendants pending trial. Program operations were associated with higher release rates, higher rates of nonfinancial release, speedier release and/or greater equity of release in four of the five experiments conducted.
- The improvements in release outcomes were not offset by higher rates of failure to appear or pretrial criminality for defendants processed by the program.
- There is no evidence from our study that more intensive post-release followup has a positive effect on defendants' failure to appear or pretrial criminality rates. However, our experimental tests of followup activities were quite limited in scope, and the findings may not be broadly applicable to many jurisdictions.
- There may be great potential for programs to affect release outcomes, without increasing failure to appear or pretrial criminality rates, through the use of less restrictive release recommendation criteria. In the one jurisdiction where we tested the effect of relaxing the program's point system criteria, every release outcome measure was affected positively, and rates of failure to appear and pretrial criminality were no higher than in the control group.
- Unemployed defendants had more difficulty securing release than employed defendants. This suggests that the bail reform movement's goal of eliminating release inequities associated with income level has yet to be achieved.
- Release inequities associated with ethnicity were less common than those associated with employment status. Moreover, program operations eliminated the release inequities associated with ethnicity.
- Programs that included defendants charged with felonies were more cost-effective than programs that processed only misdemeanors.
- Failure to appear was not very costly to the criminal justice system relative to other major categories of costs. Indeed, one jurisdiction made a profit on failure to appear as a result of bond forfeiture collections.

- Failure to appear could become very costly to the criminal justice system, if prosecution for failure to appear became more widespread.
- Pretrial arrest was relatively costly for the experiments that included defendants charged with felonies. These costs were largely due to the sentences imposed on defendants convicted of pretrial arrests, especially when the sentences involved incarceration.
- Pretrial arrest costs were much higher for the control group than the program group in the three experiments that included defendants charged with felonies. This difference was largely due to much harsher sentences for the defendants convicted of pretrial arrests in the control group vis-a-vis the program group. The reasons for this difference could not be ascertained.

Several conclusions apply to the research tasks, including:

- It is possible, but difficult, to implement experimental designs testing the impact of pretrial release program activities.
- Although many programs were concerned about the possibility of a lawsuit based on "denial of service" and refused to participate in experiments for that reason, no legal challenges arose during any of the five experiments. This suggests that program fears about possible lawsuits may be much greater than the actual likelihood that such lawsuits will occur. If so, special efforts may be needed to allay program fears, if additional experimental studies are contemplated.
- The experimental analyses resulted in findings and conclusions about program impact that could not have been developed through other types of analyses.
- Data needed for a completely accurate, comprehensive analysis of program cost-effectiveness were not available locally and probably could have been developed only at considerable expense. Any future efforts to conduct cost-effectiveness analyses of programs should be cognizant of this limitation.

E. Recommendations

The following recommendations are based on the findings and conclusions from the experimental analyses:

- Pretrial release programs should continue to be supported, particularly those that process felony defendants, with their greater potential for cost savings due to program operations.

- Programs should scrutinize their post-release followup activities to determine whether they are effective. Our findings suggest that such activities may have little impact on defendant behavior but be quite expensive to conduct.
- Programs should give careful consideration to adopting less restrictive release recommendation criteria. The findings from the Baltimore City experiment suggest that such action might increase program impact on release outcomes, while not increasing failure to appear or pretrial criminality rates.
- Jurisdictions seeking ways to cut criminal justice system costs should consider whether incarceration can be decreased, either pretrial detention or post-adjudication imprisonment. Such costs were quite large in some of the sites studied. Also, if the jurisdiction prosecutes defendants for failure to appear, it should consider whether this practice merits continuation, given its rather high costs.
- Additional analysis should be conducted of the equity of release by employment or income status. In particular, programs should study whether their operations perpetuate release inequities for poor defendants.
- Additional analysis should be conducted to determine program impact on sentencing costs for defendants convicted of pretrial arrests and to ascertain the underlying reasons for such impact.
- Because of the wide variation found in the types and levels of program impact, other jurisdictions would be well advised to evaluate their programs and determine their specific effects.
- Programs should use experimental designs to test the effect of possible changes in program operations (e.g., providing different levels of supervision or implementing less restrictive release recommendation criteria). Such tests of program variations would not raise "denial of services" concerns and could provide great insight about the program activities that are most effective within a specific jurisdiction.

PRETRIAL RELEASE: A NATIONAL EVALUATION
OF PRACTICES AND OUTCOMES

VOLUME II
The Impact of Pretrial Release Programs:
A Study of Four Jurisdictions

—APPENDICES—

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EXPERIMENTAL DESIGN AND IMPLEMENTATION

This appendix discusses the design and implementation of the experimental analysis of pretrial release. Among the topics considered are the reasons for selecting an experimental design, the difficulties in locating jurisdictions where experiments could be undertaken, how these difficulties were handled, problems that arose during the implementation of the experiments, the ways these problems were resolved, and an assessment of our experiences in conducting experimental research.

A. Reasons for Selection of Experimental Design

Because of the wide range of issues to be addressed in the national evaluation of pretrial release, several different types of analyses were undertaken. These included both cross-sectional and experimental analyses of defendant outcomes as well as assessments of the "delivery systems" used for pretrial release decisions.

The cross-sectional analyses were based on eight jurisdictions in which random samples of arrested defendants were selected and studied, after the fact, using data from existing records. Questions addressed included:

- What was the extent of criminality among pretrial releasees?
- What were the failure-to-appear rates of releasees?
- Were different types of release, such as money bail and release on recognizance, associated with different rates of criminality or failure-to-appear?
- Were certain defendant characteristics (e.g., age, race, sex, current charge, prior criminal record, community ties) associated with different rates of criminality or failure-to-appear?

The cross-sectional analyses, while providing much insight about all release processes and outcomes, could not adequately consider issues related to program impact. Such issues include:

- What impact do pretrial release programs have on the rates of release of defendants awaiting trial? On the types of release (e.g., own recognizance, deposit bond, money bail)? On the speed with which release is secured?
- What impact do pretrial release programs have on the "equity" of the release process (i.e., are certain types of defendants, such as poor individuals or members of minority groups, more likely to secure release when formal programs exist)?
- Do programs result in the release of defendants with a significantly different level of risk than defendants released in the absence of program activities?
- What is the extent of criminality for pretrial releasees who receive program followup, as compared with released defendants who do not?
- What are the failure-to-appear rates for pretrial releasees who receive program followup versus those who do not?
- What costs and benefits are associated with program operations?

An experimental design was considered the most appropriate way to analyze these issues. Such a design, by randomly assigning defendants either to be processed by a program or not, would permit assessment of program impact through a straightforward comparison of outcomes for the groups.

The benefits of experimental analysis have been widely cited in the literature concerning research methods.¹ An experimental design is the research approach most likely to avoid ambiguous findings and the

¹See, for example, Howard E. Freeman, "The Present Status of Evaluation Research," reprinted in Marcia Guttentag and Shalom Saar, ed., Evaluation Studies Review Annual, Vol. 2 (Beverly Hills, California: Sage Publications, Inc., 1977) and Henry W. Riecken and Robert F. Boruch, ed., Social Experimentation: A Method for Planning and Evaluating Social Intervention (New York City, N.Y.: Academic Press, Inc., 1974).

resultant, possibly interminable and unresolvable, arguments over interpretation of study results. By its nature, an experimental design usually leads to unequivocal findings of unchallenged validity.

Besides the general agreement among researchers that an experimental design is the best way to assess program impact, there has been widespread agreement that such analysis is needed in the pretrial release field. The spread of pretrial release programs began after a controlled experiment, the Manhattan Bail Project, demonstrated in the early 1960's that selected defendants released on non-financial conditions would appear for court dates at least as reliably as defendants released on bail. As a result of these findings, many jurisdictions started pretrial release programs. Currently, more than 100 such programs exist throughout the country; most are funded by some level of government.

Although the Manhattan Bail Project led to the widespread adoption of pretrial release programs, it did not result in similar replication of experimental analysis designed to evaluate program impact. Indeed, after a comprehensive review of the "state of knowledge" regarding pretrial release, the National Center for State Courts concluded:²

Most of the questions that were unanswered a decade ago are still unanswered....The most glaring problem is the lack of comparative analysis on the performance of the money bail system vis-a-vis the various alternatives to it.... [T]he most reliable way of doing such a comparative analysis is through controlled experiments. None have been conducted over the past decade.... It is somewhat ironic that the bail reform movement, which received so much of its early impetus from the dissemination of the results of the control group experiment conducted by the Manhattan Bail Project, should have so totally ignored the potential benefits of well designed research studies during the past decade.

²Wayne H. Thomas, Jr., et al., Pretrial Release Programs, National Evaluation Program Phase I Summary Report (Washington, D.C.: Law Enforcement Assistance Administration, April 1977).

The need for experimental analysis of pretrial release programs has been emphasized by other researchers as well. At the end of an extensive study of pretrial services, including both release and diversion, Abt Associates concluded that many important questions concerning program impact remained to be answered. Abt further stated:³

Questions such as these require the use of controlled field experiments. Properly executed, this design produces results of virtually unimpeachable validity. The random assignment of a sufficient number of subjects to treatment and non-treatment status provides assurance that the groups under observation are similar in all respects and that any observed differences in outcomes may be attributed to the treatment delivered. Without equivalent groups, any differences that emerge can be attributed to the effects of selection rather than the particular treatment strategy.

Such analysis of program impact was considered especially important at this time. Because pretrial release programs have existed for a number of years, it has been suggested that judges may have "internalized" program criteria. Thus, they might question defendants about their community ties, if there were no programs providing this information.⁴ An experimental analysis would permit an assessment of the extent to which such judicial behavior occurred.

B. The Experimental Design

The experiments were initially planned to operate as follows:

- Two groups of defendants would be analyzed: a group which was processed by the program (i.e., the "experimental" group) and a group which was not processed by the program (i.e., the "control" group). Any defendant would have an equal chance to be in either group.

³Abt Associates, Inc., Pretrial Services: An Evaluation of Policy Related Research, A Report Prepared for the National Science Foundation (Cambridge, Mass.: Abt Associates, Inc., 1974), Volume 1, p. 5.

⁴Although such information would not be verified, it was not clear that lack of verification would affect release decisions and outcomes.

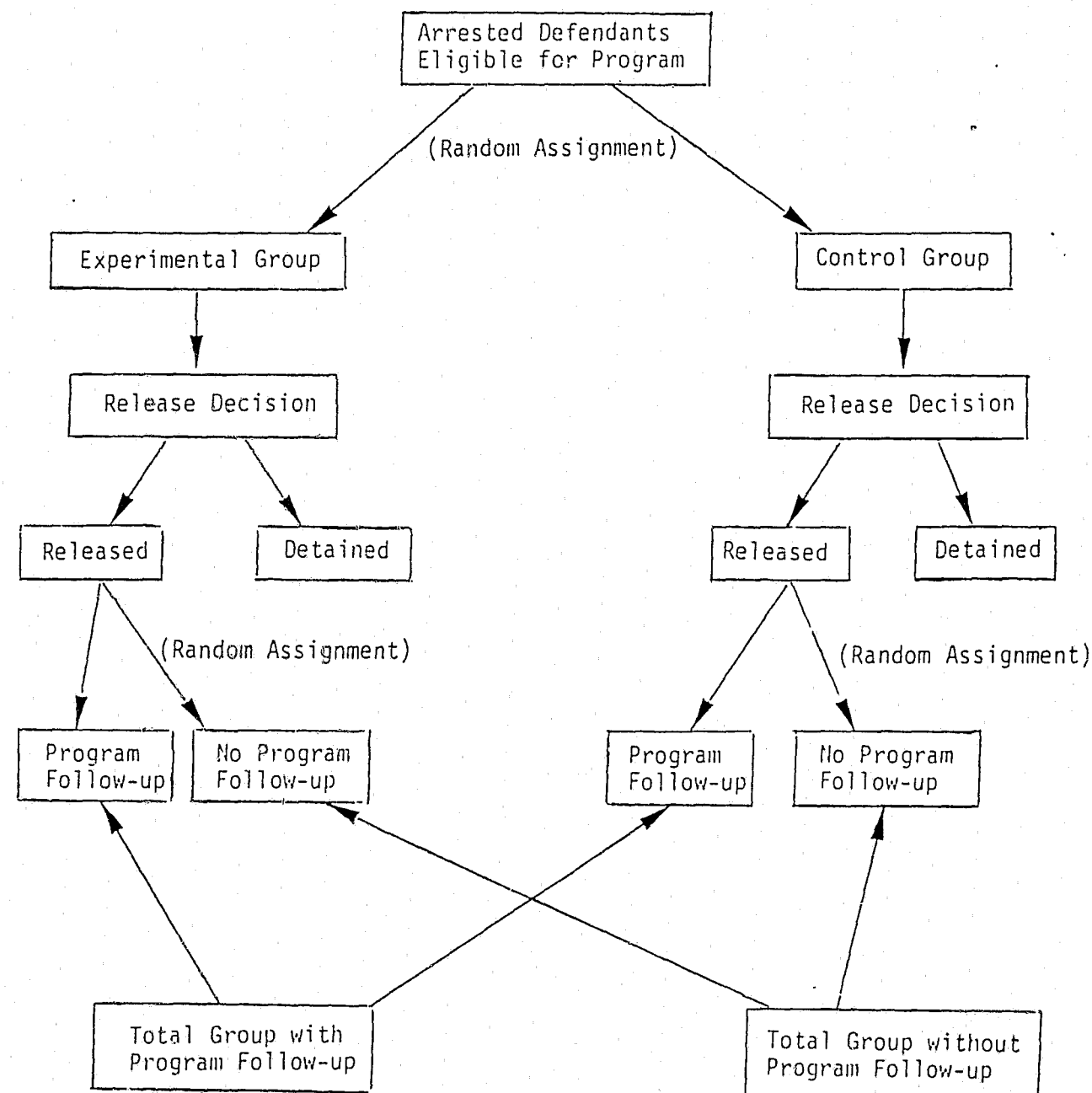
- Members of the "program" group would receive normal program processing, while members of the "non-program" group would be processed by the criminal justice system without program involvement.
- After each group contained about 400 defendants, the program would return to its normal operating procedures.
- Outcomes of defendants in both groups would be tracked. Such outcomes would include release, failure to appear and pretrial criminality.
- A comparison of the outcomes for the "program" and the "non-program" groups would indicate the program's impact (since the only difference between the two groups of defendants would presumably be that one was processed by the program and the other was not).

Because this design provided for random assignment of defendants before release decisions were made, defendants in the program and control groups who secured release might not be comparable. If they were not, any differences in their failure to appear or pretrial criminality rates might be due to differences in defendant characteristics, rather than program impact. Consequently, the design shown in Figure 1 was developed, based on discussions with National Institute of Justice officials. In addition to the random assignment before release decisions were made, a second random assignment would be made after release. Released defendants in each of the original experimental and control groups would be randomly assigned either to a group for which the program provided its normal follow-up and supervision services or to a group for which these services were not provided. This design would permit the failure-to-appear and pretrial criminality rates to be compared for groups which were similar in terms of their exposure to program activities during the post-release period. Thus, the impact of these program activities could be isolated and assessed.

Although we recognized that this design would be more difficult to

Figure 1

Revised Research Design for Pretrial Release Evaluation



implement, we thought its advantages outweighed its problems. Eventually, it was successfully implemented in two of the four experimental sites. In the other two jurisdictions only the first-stage random assignment (before release) occurred.

C. Criticisms of Experimental Design

Some persons—particularly staff of large, well-established pretrial release programs—rejected the premises of the experimental analysis. They thought that lack of program impact might be found under experimental conditions but could be explained as a program "carry-over" effect. This was considered especially likely by them, because the experimental design required the programs to process some defendants at the same time that the control group was not processed. Thus, judges would be "reminded" of program criteria throughout the experimental period and might apply these criteria to the control group. If the programs were to cease operations altogether, it was thought that the program carry-over effect would fade over time, with judges eventually reverting to "pre-program" decision-making behavior.⁵

Although this criticism may be valid, programs should nevertheless recognize the possibility of achieving the same level of impact through modified, less costly operations. If there is little evidence of program impact when an experiment is conducted, it may be prudent to redirect

⁵ We looked at this issue to a limited extent in our analysis of "defunct" programs, although that analysis faced the limitation that defunct programs may not be representative of programs that show greater durability.

program activities. For example, if the control group members were handled in the same way as defendants processed by the program, and if this were attributed to a carry-over effect of the program, this suggests that the program may not need to process every defendant in order to have an impact on all defendants. Operations could be systematically cut back, with the effects tested, until a point was reached where release outcomes were affected by the decline in program processing. Similarly, if no impact occurs soon after a program's demise, perhaps a program needs to operate only intermittently, rather than continuously.

An additional concern raised about the experiments was that they could not address the issue of "false positives," that is, the extent to which defendants were being detained who could safely have been released. To have addressed this issue would have required the release of some defendants who were being detained, and judges were not willing to make such releases on a random basis. They believed that certain defendants were being detained for good reasons, and they were not willing to expose the community (or themselves) to the possible risks involved in a test of the validity of those reasons.

Although we were unable to conduct experimental analyses regarding "false positives," we think it is an important issue and one that should receive further study. Our findings—and the general findings over time that as more defendants are released, failure to appear and pretrial criminality rates remain at reasonable levels—suggest that it may be possible to release still more defendants without significantly increasing the level of "harm" (as reflected in failure to appear and pretrial criminality rates). Additionally, a few small-scale studies in which defendants initially selected for detention were later released (on a

random basis) found that those defendants did not pose substantially greater release risks than other released defendants. Nevertheless, most of the judges we interviewed expressed extreme reservations about the possibility of randomized release of defendants currently being detained. These reservations were expressed, despite the fact that most detained defendants had been given at least a theoretical means of securing release, namely, the posting of bail.⁶

D. Site Selection Difficulties

Identification of sites amenable to experimental analysis proved a difficult task. Despite the fact that pretrial release programs were largely an outgrowth of the experimental Manhattan Bail Project, the concept of experimentation was not a familiar one to many of the programs we contacted. Often the programs expressed an interest in participating in the study, but wanted to select a "comparison" group rather than a "control" group. For example, programs suggested comparing program defendants directly with various groups of defendants not processed (e.g., defendants missed by the program because of resource constraints or persons arrested before the program began).

Even programs in agreement about the benefits of experimental analyses were often reluctant to participate (if not openly hostile to the idea). These programs usually expressed concern about the ethics and legality of structuring a control group of defendants who would not receive the program's "services." The more convinced a program is that its services are beneficial, the more likely it is to question the merits of establishing

⁶The "false positives" issue could be tested for most defendants in most jurisdictions by establishing a bail fund and using it to secure the release of randomly selected defendants.

a control group.

In certain cases (e.g., in the State of Kentucky), programs operate under statutes that require all defendants to be interviewed and information relevant to release determinations to be provided to the court. Under such conditions, a controlled experiment might be unlawful. In other places (e.g., Rochester, New York), there was no statutory requirement to interview all defendants and provide release-related information to the court, but the program had done so for a number of years. Such programs viewed selection of a control group as a "denial of service" to defendants. Because of the frequency with which this issue arose, it is considered in some detail below.

E. The "Denial of Service" Issue

An important issue posed by experimental analysis is whether "services" will be "denied" to members of the control group and, if so, whether this constitutes an unfair or unlawful practice. An experimental study of a program in a related field illustrates one way that this issue has been treated. The following discussion describes that situation and compares it with the pretrial release experiment.

The Court Employment Project (CEP) diverted defendants from the criminal justice system, provided them with employment services and led to a dismissal of charges for defendants who successfully completed the program. When proposing experimental analysis of the program, researchers noted that CEP could not serve all the defendants eligible for it, due to resource constraints. Therefore, procedures for selecting a control group could be viewed as altering the manner in which defendants were chosen for program services; the procedures would not affect the total size of the served group or the fact that some defendants would go unserved.

In this case, however, an opinion by LEAA's General Counsel indicated that a straightforward random assignment of defendants to groups served versus not served by the program might be unconstitutional. This opinion was based largely on the fact that the prosecutor agreed not to prosecute those defendants who successfully completed the program. LEAA's General Counsel stated that:⁷

It is generally recognized that a prosecutor is given wide latitude in exercising his discretion in determining whether or not to prosecute....However, the prosecutor's discretion must be based upon a justifiable standard....Where a justifiable standard is not used, the exercise of discretion is arbitrary and subject to challenge as a denial of equal protection or due process. In addition, where a classification is used, it must rest upon real differences which are relevant to the purpose for which the classification is made....

This office is of the opinion that the use of a random assignment procedure to determine whether or not to prosecute is not a justifiable standard. The selection of control group participants is not based upon real differences.

When the proposed random assignment of defendants was rejected, the research staff sought another defendant assignment mechanism which would still yield a useful analysis. Such a mechanism was found and works as follows:

- More defendants are screened for eligibility by the CEP staff than can be admitted into the program.
- Within a given time period, eligible defendants are assigned to CEP as long as there are openings. When no more slots are available, the remaining defendants screened during that time period are placed in a non-CEP group (i.e., a "control" or "overflow" group).

⁷Memorandum from LEAA's Office of General Counsel to Cheryl V. Martorana, Acting Chief, Courts Division, National Institute of Law Enforcement and Criminal Justice, dated December 12, 1974.

- The time periods during which defendants are assigned to CEP and non-CEP groups are of varying lengths (e.g., 5 hours, 2 hours, 4 hours, etc.), determined on a random basis by the research staff. Thus, CEP staff cannot bias the program and overflow groups by assuring that certain defendants will be placed in one group or the other. Although the lengths of the time periods are randomly determined, within a time period defendants are assigned to CEP on a "first come, first served" basis until the CEP slots available for that time period have been filled.

In addition, the research was associated with another change in CEP's procedures: the screening of eligible defendants was expanded to include persons previously missed (e.g., defendants arrested on weekends). Thus, it could be argued that the net effect of the selection procedures for identifying CEP and overflow groups was to make CEP's selection of clients more equitable. During the research period any defendant would have an "equal probability" of becoming a CEP client.

Although this experimental approach to analysis of CEP's impact was accepted by researchers, program staff and attorneys concerned with protecting defendants' rights, there are some important differences between the CEP program and a typical pretrial release program. For example, the prosecutor played a key role in the CEP program but is usually not involved in pretrial release decisions. Thus, the legal objections to simple random assignment raised by LEAA's General Counsel might not apply to pretrial release programs, since those objections were based largely on the prosecutor's role in CEP.

In addition, the criminal processing of defendants selected for CEP participation ceases—permanently, if defendants are successful in the program, but at least temporarily even for those who fail—while no such cessation occurs for members of the control group. This situation was quite different from the one proposed for the experimental analysis

of pretrial release programs, where release determinations would be made by the court for all defendants in both groups. Thus, no defendant would be denied the possibility of release as a result of the experiment.

Although it could be argued that a defendant who received a favorable recommendation for release might be more likely to secure release than a similarly situated defendant in the control group, such an outcome was not a foregone conclusion. Indeed, some researchers have suggested that, once a pretrial release program "sensitizes" judges to the circumstances under which defendants can "safely" be released on nonfinancial conditions, the program itself is no longer needed:⁸ it could be abolished, and there would be little change in release outcomes. An experiment would provide a test of this hypothesis.

Although program officials were concerned that "denial of service" to someone in the control group might "harm" the defendant, it is not clear that a pretrial release program's "services" will always be of benefit to a defendant. Indeed, it can be argued that some defendants are harmed by virtue of receiving a program's "services." For example, a defendant who does not receive a favorable program recommendation for release, in a jurisdiction where such recommendations are common, may be much less likely to secure release than if no release-related information were provided to the court for any defendants.⁹ Thus, without experimental analysis, the extent to which the activities of a pretrial release program constitute a service or a benefit—and a benefit to whom—cannot be determined.

⁸See, for example, Wayne H. Thomas, Jr., et al., op. cit., pp. 34-36.

⁹Medical literature provides many examples of cases where the "treated" group had worse outcomes than the control group.

F. Possible Legal Challenges

Some programs expressed concern that an experiment might be challenged in the courts. It is somewhat ironic that experimental findings that showed the greatest program impact would also provide the strongest case for a successful legal challenge after the experiment ended (i.e., a defendant in the control group could claim unfair denial of service only if the study results documented program impact on release outcomes).

Program concerns regarding possible lawsuits were particularly difficult ones to address, because the areas of law that were involved appeared to lack clearly established precedents. Thus, it was difficult to estimate either the likelihood that formal legal proceedings would be initiated against the experiment or the probability that such a case would be won. The mere possibility of a lawsuit, however unlikely, probably discouraged some programs from participating in the study.

One program was especially concerned about the financial implications of a court case. Although program and criminal justice system officials in the jurisdiction thought such a case was unlikely to occur, and even less likely to be won by a plaintiff, they would still face the legal costs of responding to the lawsuit. They were not willing to assume full responsibility for such a financial burden and suggested that the Federal government share this risk.

This led us to discuss with National Institute of Justice (NIJ) officials whether they could provide "indemnification" to a grantee or to a program participating in a grantee's research study. Federal precedent for such indemnification existed with the Department of Health, Education and Welfare, which had assumed financial responsibility for any lawsuit resulting from at least one experimental study in the health

area. In that case, the contractor had been required to obtain insurance against financial damages from possible lawsuits, and no firm had been willing to provide such insurance. Consequently, the government became, in effect, the insurer.

Because the issue of indemnification could have many ramifications for NIJ, and because it did not seem likely to be resolved both favorably and quickly, we sought other solutions as well. One was "partial indemnification": assurance for a jurisdiction that legal fees not exceeding a certain sum (\$5,000 or so) would be borne by the Federal government (perhaps through its grantee), if participation in the study led to a court case. This would have avoided the financially open-ended liability from full indemnification, while assuring a program that it would not be fully responsible for legal costs arising from the study.

We also considered the possibility of buying insurance to cover the costs of lawsuits. However, preliminary inquiries suggested that our situation was not amenable to such a solution. Insurance companies usually provide insurance against situations that have a small probability of happening to any single client (i.e., fire, automobile accident, death at an early age), though many clients are at risk. Thus, the funds collected from persons who do not experience the adverse circumstances insured against will more than offset the payments to those who do. In our situation, there were not a large number of potential clients seeking similar coverage. Consequently, there was no way to spread the risk of payment over a large number of insurance policies. Although the probability of payment might be small in our case, if a lawsuit did arise, the insurance company would be unable to cover its costs with premiums paid by other organizations. Thus, the insurance possibility seemed a remote

one and was not actively pursued beyond preliminary inquiries.¹⁰

We also considered various ways in which our experiments might be structured to make a legal challenge as unlikely as possible. Some opposition to experimental analysis stemmed from concern that certain defendants in the control group might be detained when, if they had been in the program group, they would have been released. One way considered to avoid this outcome was to identify defendants in the control group who did not secure release at the initial court appearance. For those defendants, program-developed information relating to release could be provided at the bail review hearing (which many jurisdictions hold within 24 hours or so).

Such an approach would have minimized possible harm to defendants, but it had certain limitations. Program impact on initial release rates could have been assessed, but it would have been impossible to determine whether control group defendants released at the bail review hearing would have eventually secured release without the program's intervention. This limitation would also have affected the analysis of defendant outcomes (e.g., court appearance rates, pretrial criminality rates), although different assumptions could have been made about the eventual release of defendants initially detained to test the sensitivity of study findings to this factor.

This approach also faced other problems. For example, there might have been some prejudice against defendants initially detained, so that

¹⁰More recently, insurance companies have apparently begun writing more policies to insure corporations against possible lawsuits for damages (e.g., class action suits brought against firms for marketing harmful products). Thus, obtaining insurance against lawsuits in connection with experimental studies might be more feasible today.

they were less likely to secure release than if the program information had been provided at the first court appearance. Additionally, if some defendants in the control group had secured release at the bail review hearing, as a result of the program information provided, they would have been detained longer than if the program information had been provided initially. Thus, while this approach might have minimized possible harm to defendants in the control group, it would not have eliminated the possibility altogether. Consequently, because of its limitations, we did not try to implement this approach.

Another variation considered was to test the impact of providing "much" versus "little" information to release decision-makers. However, this was thought less desirable than an experiment testing the impact of "program" information versus "no program" information, so we continued our efforts to find suitable sites for that test. The experiment implemented in Baltimore City does, though, analyze the effect of a variation in program operations, rather than of "program" versus "no program" situations. Additionally, because of a State law requiring provision of certain information to the judge, the Tucson felony experiment tested the provision of this information versus the "full program services" situation, including verification and recommendation.

Although programs raised many concerns about possible lawsuits, no legal challenges arose in any of the sites that participated in the experimental analyses. This suggests that programs' fears about possible lawsuits greatly exceed the likelihood that such court cases will develop.

G. Site Selection

To help avoid possible problems associated with the "denial of services" to defendants and alleviate program concerns about legal challenges,

we sought sites that could expand their operations into an "overflow" group of defendants. As a result of such expansion, a control group of defendants could be selected without reducing the total number of persons processed by the program.

Several possible overflow situations seemed amenable to experimental analysis. For example, some programs conducted interviews only during certain hours (often only on weekdays), and other programs excluded many charges from eligibility. Such programs were likely candidates to expand their operations on an experimental basis for a short period of time. However, they would need additional resources to support their expanded activities.

Because of the National Institute of Justice's continued interest in experimentation, it agreed to make funds available to participating programs to expand their operations. In some cases the experimental procedures were limited to the overflow group only (e.g., the Tucson felony experiment covered "late arrests" only), but in other sites the entire (expanded) population processed by the program was subjected to experimental procedures (e.g., Beaumont-Port Arthur).

We also considered establishing new programs in jurisdictions lacking them, rather than expanding on-going programs. However, the special problems of setting up new programs, and the somewhat unusual conditions faced during any start-up period, discouraged active consideration of this possibility. We did support a new program for the Tucson misdemeanor study, but that program both (1) operated under the umbrella of the existing felony program and (2) was staffed by many of the same persons who had worked with a similar misdemeanor program that had ended the preceding year.

Altogether, about two dozen programs were contacted personally before four participating jurisdictions were located. We made site visits to five jurisdictions that ultimately did not participate in the experiments as well as to the four sites that agreed to do so. Additionally, we talked to directors and staff of many more programs at annual conferences, including the National Symposium on Pretrial Services and the meeting of the New York Association of Pretrial Services Agencies. Also, at the 1978 Symposium in San Diego, California, programs were informed of our study, and we talked to many staffmembers about the possibility of participating in the experiments. Moreover, the Pretrial Services Resource Center included an item in its monthly newsletter describing our study and our search for programs. Finally, we used various "word-of-mouth" contacts, particularly through our Advisory Panel and the Resource Center, to identify jurisdictions where experimentation might be feasible.

Ultimately, as discussed in the individual chapters of this volume, experimental analyses began in four sites: Baltimore City, Maryland; Lincoln, Nebraska; Beaumont-Port Arthur, Texas; and Tucson, Arizona. The search for programs had begun in January 1978, and the first experiment began in September of that year.

H. Study Implementation

To implement the experiments, we first met with appropriate local officials to explain the study's goals and overall approach. After necessary approvals were obtained, we worked with program staff to develop step-by-step procedures and to insure that these were fully understood. The procedures were written, so that program staff could refer to them whenever necessary (see Appendix D).

We were particularly concerned that the program understand the need to follow the procedures exactly, because we would usually not be on-site ourselves while the experiments were in progress. We had frequent telephone conversations with program staff, especially during the early days of the experiments, to help insure that possible problems would be resolved before they became serious ones.

Data collection for the cross-sectional study was underway in Tucson at the time the experiments began there, so our staff was able to assist with initial implementation difficulties on-site in that case. We also spent a considerable amount of time on-site in Baltimore City while the experiment was in progress there. Because of the short distance between Baltimore City and our offices, we were able to provide more on-site assistance and monitoring there than in the other experimental jurisdictions.

One problem to which we gave special attention, in part because we would not be able to provide on-site monitoring, was the randomization method. If possible, we wanted a randomization approach that would permit us to check for errors. If we used a system that involved reference to a list of random numbers (e.g., opening an envelope for each study defendant to determine if the person should be in the program or control group), there would be no way for us to ascertain whether the random order was in fact followed. It would be relatively easy for individual staff members to place a few defendants they "liked" in the program group by manipulating the order of processing until that occurred (e.g., opening envelopes until the next program case appeared, then "going back" to pick up the control cases skipped).

Although an individual staff person might think, despite our admonitions to the contrary, that manipulating the groups for a few defendants

would have little effect on the overall results of the experiment, the compounded effect of several staffmembers behaving similarly could destroy group comparability.¹¹ Thus, we sought a randomization approach that might avoid these difficulties. At the same time, we needed an approach that could be implemented easily, so that staffmembers would make few mistakes using it and so that program processing of defendants would not be delayed (hence, we could not have program staff call us for each defendant, to determine the appropriate group placement).

The approach developed is based on the defendant's month and day of birth. The days of the year were randomized and placed on two lists. One list was designated the "program group" and given to program staff. At the time of interview the program staffperson was to check the defendant's date of birth against the list. If the date appeared on the list, the defendant was placed in the program group; if not on the list, the control group. Because date of birth usually appeared in most records, the accuracy of the randomization could be checked easily. Because program staff knew this, we thought that knowledge alone would discourage attempts to manipulate group composition.

It was, of course, possible that "word would get around" to defendants, so they would lie about their birthdates. However, we considered this unlikely, in part because the experiments were relatively short in duration in most cases.

¹¹Based on our discussions with staffmembers at participating programs, we had no reason to think that any of them would try to manipulate the randomization procedures. Nevertheless, we considered it desirable to remove all possible temptation to do so (particularly since any such temptation might arise several weeks later, when memories of our urgings about the importance of the experimental design had faded and interviewer sympathy for a particular defendant might be strong).

For those sites where a second random assignment occurred after release decisions had been made, we developed a second list of randomized birthdates. The program group of the second list contained half the birthdates from the program group of the first list and half the birthdates from the control group of the first list. Thus, the post-release groups would be equally divided between defendants processed by the program before release and those not processed.

We had some concern that the two lists might get switched, inadvertently, during part of the experiment. We took two precautions to try to avoid this. First, we tried to the extent possible to have individual staff members work only with one list. Thus, interviewers were concerned only with the list for the first random assignment; they had no reason even to see the second list. Similarly, the staff handling post-release followup activities had no reason to be concerned about the earlier randomization. To implement this, of course, required a division of labor between pre-release and post-release activities. For the programs studied, such a division of labor was relatively easy to obtain.

The second precaution was to have each list printed on paper of different color. During training, staff were told to refer to the "yellow" or the "blue" list, as appropriate. Thus, if lists became mixed at some point, staff members should know immediately whether they had the correct one.

This randomization approach seems to have worked effectively. In the two largest experiments (Tucson and Baltimore City), the number of defendants in each group was roughly equal, and the characteristics of defendants in the two groups were equivalent. This occurred for the second randomization as well as the first in each of these sites.

In Beaumont-Port Arthur, one of the two smaller experimental sites, the randomization results were not as satisfactory: the program group was much larger than the control group (132 defendants versus 61), and the two groups were dissimilar for six of the nineteen defendant characteristics compared. We cannot explain this result. It is possible that the size of the experiment was simply too small to obtain comparability; if the group sizes had been greater, perhaps defendant differences would have eventually balanced out. This explanation is not completely satisfactory, however, because of the imbalance in group sizes. In the larger sites the group sizes were more nearly equivalent at all stages of the study.

Another possibility is, of course, that the program staff manipulated the group composition. For example, staff could have screened out certain defendants from participation in the study: control group defendants who seemed like good risks could have been skipped, as well as program group defendants who seemed like poor risks. Because the program conducts post-arraignment interviews at the jail, and because many jailed defendants post bond without program involvement, it would be difficult for us to identify such biased program selection of defendants. (The other programs studied required interviewing of all defendants before arraignment, so selection bias in the overall pool of defendants would have been virtually impossible. The Beaumont-Port Arthur program, on the other hand, has much more discretion in its interviewing procedures, in part because the jurisdiction considers pretrial release a "privilege," rather than a "right."¹²)

¹²See the discussion in Lisa Crowley, Delivery System Analysis of Jefferson County (Beaumont-Port Arthur), Texas, Working Paper Number 10, February 1980.

Although program staff could have manipulated the study design, that explanation too is unsatisfactory. Because it is a small program, the director is aware of all staff activities and indeed conducts much of the interviewing himself. In our judgment he is unlikely to have biased the study. In our many discussions with him, he clearly understood the research design and the reasons for it, and he also clearly believed that his program would be proved effective in any impartial test of its impact. Given these facts, there would be little reason for him to attempt to bias the defendant groups and every reason to avoid any actions that might lead to questionable study results.

Thus, for Beaumont-Port Arthur, we are left with a puzzling finding regarding group composition and no fully satisfactory explanation for it.

In Lincoln a different randomization approach was used, because program staff objected to the randomized birthdates mechanism. Their reason was that rearrested defendants would always fall in the same group. They wanted a method that would provide a method for a rearrested person to be in the program group at some point in the study, rather than confining some defendants always to the control group based on dates of birth. Consequently, we randomized the days of the year for the six-month period we thought the experiment would require¹³ and designated each day as a "program" or "research" day (see Appendix D for more information). On program days, the program carried out routine processing of defendants. On research days, defendants were interviewed and the completed forms were sent to us for inclusion in the data base; no further program processing occurred.

¹³In fact the experiment ran for nine months.

We had some concerns about this approach. One was that arresting officers might bias the groups if they knew which days had "program" versus "research" designations. For example, a defendant arrested late at night (e.g., 11 P.M.) might be delayed for booking until early the next morning (e.g., 1 A.M.), if the officer wished to affect the defendant's chances for program processing. Because of this concern, we asked program staff to keep the list of program and research days as confidential as possible—not, for example, to post it in their offices at the jail.

It would, of course, be even easier for program staff to conduct such manipulation of the groups. We attempted to deal with this possibility by stressing the need for strict adherence to the study design in order to obtain valid findings. In this site, as in most others, we considered the program director the key to successful implementation. Because the director seemed to understand the possible difficulty, and the need to avoid it, we were reasonably confident that the study design would be implemented without interviewers' biasing the group composition.

One problem we did not anticipate stemmed from the fact that some days would have no defendants, due to the small volume of defendants eligible for program processing. When a new director took over the program, an unnoticed design modification occurred: whenever there were no defendants on a "program" day, the next day was re-designated a program day.

The fact that this occurred without our being aware of it reflects the fact that Lincoln received the least monitoring of the various experimental sites. It was the easiest experiment to implement, had

very straightforward procedures and was a low-volume site. Because we had two more complex experiments underway in other sites at the same time, Lincoln received relatively little attention.

The re-designation of some "research" days as "program" days may partly account for the imbalance in the number of defendants in the program (n=94) versus control (n=36) groups. Despite this imbalance, the two groups had equivalent characteristics. There was only one significant difference between the two groups for the eighteen characteristics compared: program group defendants were younger than members of the control group.¹⁴

While Lincoln illustrates that problems may arise when monitoring is somewhat lax, Baltimore City shows the ways that intensive monitoring can help identify problems early, so that they can be resolved before becoming serious. In Baltimore we were faced with implementing our most complex experiment in the largest jurisdiction studied. The study required interviewers to screen defendants for study eligibility, based on point system scores, charge and other criteria. Once an eligible defendant was identified, the birthdates list was used to determine whether the person would be processed as a "program" or "control" group defendant. Finally, all defendants released on own recognizance who had fewer than five points were to be referred to the program's downtown headquarters for supervision screening.

Besides these multi-step procedures, the Baltimore experiment required consistent implementation by approximately 35 interviewers,

¹⁴It is possible, of course, that this comparability merely reflects the overall homogeneity of eligible arrestees in the Lincoln area. When compared with Beaumont-Port Arthur defendants, for example, the Lincoln study groups were much less diverse.

working three shifts, stationed in ten different physical locations throughout the city. To have the experimental procedures implemented successfully under these conditions required some special actions on our part as well as unswerving commitment by the program's top staff.

It should also be noted that our experiment was conducted during a time when the program experienced somewhat higher turnover and higher vacancy rates than usual. This placed considerable stress on the remaining staff and gave them less time to devote to mastering complicated experimental procedures.

Interviewers were trained three times before consistent implementation of procedures was obtained. In part this occurred because there were a number of special problems that individual interviewers handled differently; once these were identified (e.g., whether to refer a defendant whose case would be settled the next day to the office for supervision screening), consistent procedures for all interviewers to follow could be developed. Additionally, some procedures were modified during the retraining process. For example, we had initially planned to extend study eligibility to defendants with unverified addresses. However, interviewers objected so strenuously—and the initial interviews showed so many errors in the handling of these cases—that we excluded those defendants.

Besides interviewer training, we also developed more extensive monitoring procedures. Completed interview forms were reviewed on a daily basis in Baltimore and again, on a weekly basis, in Washington for correct handling. When errors were found, they were called to the attention of the shift supervisor, who discussed them with the interviewer. In this way any remaining confusion about study procedures was identified and resolved.

There were some unexpected findings that resulted from the monitoring activities. One is that some interviewers were not calculating point scores correctly. Although interviewers are not bound by the point score in terms of release recommendations (the program considers point scores as "guides" rather than "rules"), it was nevertheless surprising to find that some interviewers did not understand the point scoring system.

We also found that a higher percentage of defendants received six points or more after the experiment started than had been the case during a baseline analysis, completed a short time earlier to estimate the likely volume of study cases. We suspect that in "borderline" cases, more interviewers were exercising their discretion in favor of defendants, so as to avoid the special study procedures for persons with fewer than six points.

I. Assessment of Implementation

In general the experiments were implemented most successfully in the two sites (Tucson and Baltimore City) that had been part of the earlier cross-sectional analyses. In those sites we obtained a larger number of study cases, and there were no problems with group comparability. Those were also the sites where two-stage randomization procedures were employed. Our extensive familiarity with these sites as a result of the cross-sectional study was a great aid to successful experimental implementation. Additionally, the program staffs may have been somewhat more at ease working with us, as a result of our earlier dealings with them.

This suggests that time allocated to developing a detailed understanding of a jurisdiction to be studied may be time well-spent, even

though it may delay actual implementation of experimental procedures. In our case, the difficulties of finding participating sites may have made us too eager to begin the analysis and not sufficiently circumspect in our screening of all jurisdictions for appropriateness and volume of defendants.

Additionally, we should probably have made periodic site visits to the participating jurisdictions, rather than relying primarily on telephone contact and review of mailed interview forms to identify potential problems. Such site visits might have permitted us, for example, to identify and correct some error in, or misunderstanding of, study procedures that led to the imbalance of group sizes in Lincoln and Beaumont-Port Arthur.

J. Factors Affecting Successful Implementation

Despite the implementation difficulties of our experiments, they were on the whole successfully completed and resulted in a number of significant findings. Based on our experience, we found several major factors responsible for successful implementation. Two of the most important of these were persistence and flexibility. Often program objections could be handled, once they were clearly understood, in ways that did not affect the validity of the study.

Programs themselves sometimes suggested feasible alternatives, as our discussions continued. For example, the Tucson program identified the "late arrests" group as an overflow situation, after we had initially concluded that the site did not meet the overflow criterion. Similarly, the Lincoln staff was uneasy about using birthdates as a randomization mechanism, but had no problem using randomized days of the week, an equally acceptable approach.

Additionally, there is no substitute for cooperative top staff in the jurisdictions studied. The more time researchers spend with them, explaining the reasons for the study procedures, the more likely they are to understand and be supportive of the researchers' needs. We do not subscribe to the theory that program staff should know as little as possible about the study, so that it will be difficult for them to manipulate its outcome. Our experience indicates that supportive top staff (and local criminal justice system officials as well) are vital for successful study implementation.

In this case we received unusually high levels of cooperation from officials in all the participating jurisdictions. Without the continuing interest and commitment of program directors and staff as well as the appropriate criminal justice system officials, no experiments would have been completed successfully. It was the willingness of many persons in the local jurisdictions to help us solve various implementation difficulties that ultimately assured the experiments' success.

K. Was It Worth It?

It is reasonable to ask whether the experimental findings warranted the level of effort required to implement the analyses. In our judgment the answer is an affirmative one. The experimental findings regarding program impact seem much more straightforward to us than the results of the cross-sectional analyses. Moreover, some of the findings could only have been derived from an experimental approach, such as the cost-effectiveness comparisons and the results regarding the equity of release decisions with and without a program.

It is too soon to determine whether programs will change their operations, as a result of the experimental findings. For example, the

findings suggest that current post-release followup activities may have little effect on defendant behavior. Although Tucson has decided not to continue the notification activities begun as part of the experimental test, it remains to be seen whether other jurisdictions will make similar changes—or, alternatively, will conduct similar tests of program impact.

L. Actions To Encourage Experimentation

Because experimental analysis often provides the best evaluative information about a program, the National Institute of Justice (NIJ) may wish to undertake activities that would create a more supportive environment for such analyses. Legal precedents concerning the conditions under which experimental analysis can be conducted are far from well-established. More researchers might be encouraged to consider experimental designs, if this area were better defined legally. Thus, NIJ may wish to have a legal scholar (or perhaps more than one) review the legal issues related to experimental analysis.¹⁵

If NIJ continues to support experimentation, through its Test Design program as well as research studies, there may eventually be some type of court case. It may be to NIJ's advantage to prepare for this possibility now. Moreover, NIJ may wish to consider whether a "test case" should be brought that might resolve many of the legal issues now debated (and sometimes used as a basis either for researchers' failing to consider the possibility of experimental analysis or for program staffs' reluctance to participate in it).

¹⁵At the first Advisory Panel meeting for the pretrial release evaluation, Barry Mahoney suggested that this would be an important activity, possibly affecting a broad spectrum of potential research efforts.

Additionally, NIJ may wish to pursue the possibility of acquiring insurance—for itself, grantees or participating sites—against the costs of any lawsuits that might arise as a result of experimental analyses. Such insurance would remove programs' financial concerns about possible liabilities of undetermined magnitude.

Another way of gaining greater acceptance for experimentation might be to work with programs as they are starting their operations. It is likely that many activities could be phased in, so that experimental analyses could be conducted. However, there will still be a need for experimental analyses of on-going programs. New programs can be expected to face different problems, perhaps resulting in different outcomes, than programs that have become well established. Unless ability to survive is alone considered a sufficient evaluation of a program, then older programs will need to be assessed as well as newer ones. Resolution of the legal issues posed by experimental research would facilitate, though by no means assure, experimentation involving older, better established programs.

M. Concluding Remarks

Many objections to experimental analysis stem from concern that it may be "unethical" to establish a control group of individuals who are not processed by the program whose impact is being evaluated. In response to this concern, Robert Boruch has observed that failure to discover whether a program is effective may itself be unethical.¹⁶

¹⁶Robert F. Boruch, "On Common Contentions about Randomized Field Experiments," reprinted in Marcia Guttentag and Shalom Saar, ed., Evaluation Studies Review Annual, Vol. 1 (Beverly Hills, Calif.: Sage Publications, Inc., 1976), p. 187.

[I]f one relies solely on nonrandomized assessments to make judgments about the efficacy of a program, subsequent decisions may be entirely inappropriate. Insofar as a failure to obtain unequivocal data on effects then leads to decisions which are wrong and ultimately damaging, that failure may violate good standards of both social and professional ethics.

APPENDIX B
METHODOLOGY FOR COST-EFFECTIVENESS ANALYSIS

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I. INTRODUCTION

A. Background

An important consideration regarding public expenditures is whether the funds have been used in a cost-effective manner. This is of particular concern during times of budget cutbacks and public clamor for reduced taxes. Because of the widespread interest in cost-effectiveness issues, we conducted a brief analysis of the experimental sites. This appendix presents the methodology used for that analysis.

It should be noted that we studied the cost-effectiveness of pretrial release programs, not the cost-effectiveness of pretrial release practices.¹ These may differ, because many persons (e.g., judges, attorneys, bondsmen) besides program staff affect release outcomes. Additionally, we considered cost-effectiveness issues from the viewpoint of the criminal justice system (CJS), rather than 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 they were accrued by the CJS. This perspective permits consideration of the impact of pretrial release program activities on other CJS agencies but does not require us to make questionable assumptions about such program effects as "saved" defendants' wages, decreased welfare costs for defendants' families or costs incurred by victims of pretrial crimes.

The experimental sites provide a good basis for analysis of the cost-effectiveness of programs, because defendant outcomes in the absence

¹See 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.

of program activities can be compared with outcomes for similar defendants processed by the program. This avoids a major difficulty with past cost-effectiveness analyses of pretrial release programs, namely, the assumption that if defendants had not been released they would have been detained for a time period equal to the actual time that elapsed between arrest and disposition of their cases.² In fact, it is reasonable to think that (1) some defendants would have secured release eventually, e.g., by posting bail, and (2) defendants might have had their cases disposed of more quickly if they had actually been detained until final case disposition, rather than released. In the experimental sites the extent of detention can simply be directly compared for the "program" and "no-program" groups; assumptions about eventual release outcomes are unnecessary.

Another major problem with past studies is they often included fixed costs, particularly when counting savings in detention costs, rather than limiting the analysis to the marginal costs of the activity. Thus, costs that could not be avoided were included along with the variable costs that could in fact have been saved.

To some extent, the limitations of past studies reflect the poor data available for such analyses. Records are usually not maintained in ways that facilitate such analyses, and the work involved in developing reasonable estimates of marginal costs may be excessive. These problems with existing records have affected the present study as well. Although we attempted, whenever possible, to develop marginal cost estimates, we

²See the discussion in Wayne H. Thomas, Jr., et al., National Evaluation Program Phase I Summary Report (Washington, D.C.: U.S. Department of Justice, LEAA, April 1977), p. 46.

were sometimes limited to average cost data, as discussed in later sections.

Table 1 illustrates the way the cost-effectiveness analysis was structured. As shown, cost estimates of five types were derived for the experimental and control groups: jail costs, failure-to-appear costs, pretrial crime costs, program costs and other costs. These costs were summed, weighted to reflect equal group sizes and compared to determine the group with the lesser cost and thus the more cost-effective approach. Each cost category is described below, followed by discussions of the detailed cost estimates for each site.

TABLE 1
SUMMARY OF APPROACH FOR COST-EFFECTIVENESS ANALYSIS
OF PRETRIAL RELEASE PROGRAMS

ITEM	EXPERIMENTAL GROUP	CONTROL GROUP
Jail Costs		
Failure-to-appear costs		
Pretrial crime costs		
Program costs		
Other costs		
Total costs (unweighted)		
Total weighted costs		

B. Jail Costs

Jail personnel were asked to estimate the variable cost per day of detention. These costs included food, clothing, linen, etc., and

excluded fixed costs (e.g., capital expenditures, jail administration, etc.). By considering all personnel expenditures as fixed, these estimates reflect a lower bound on jail costs. Because some staff reductions could probably occur if jail populations were permanently reduced, an upper bound was also estimated. This was based on the total operating budget of the jail, less a portion of the staff costs.

These cost estimates per inmate-day were then multiplied by the number of days of detention for each of the defendants in the experimental and control groups to estimate total jail costs. Such estimates are probably somewhat conservative. For example, the jurisdictions we studied typically charged other areas more than the variable costs per day for housing their prisoners. In Tucson, reimbursements from other jurisdictions were billed at \$21 per day, as compared with our variable costs estimate of \$10 per day.

Additionally, if a new jail would have to be built to accommodate an increased inmate population, then capital costs should be included in the jail cost estimates. Although some of the jails in the experimental jurisdictions were overcrowded, we did not consider the capital costs of new construction in our cost estimates.

Our estimates are also conservative because of their focus on criminal justice system costs alone. A recent study of the full costs of incarceration in New York City found those costs to be much higher than generally estimated.³ Direct costs of the Department of Correction

³Coopers and Lybrand, The Cost of Incarceration in New York City (Hackensack, N.J.: National Council on Crime and Delinquency, 1978). See also Carl M. Loeb, Jr., "The Cost of Jailing in New York City," Crime and Delinquency, October 1978, pp. 446-452.

averaged \$58.12 per prisoner day, and outside services provided by the Department of Health, General Services Department, Department of Special Services, etc., totaled an additional \$13.75 per prisoner day. Moreover, additional costs of more than \$55.00 per day were generated as a result of lost earnings, higher welfare payments, lost real estate taxes and similar expenses.

C. Failure-To-Appeal Costs

Failure-to-appear (FTA) costs may occur at three different points in time: when the FTA occurs, when (and if) efforts are made to return the defendant to court and when the defendant returns to court. Costs associated with the occurrence of FTA include the costs of a delay in court proceedings, while the defendant's name is repeatedly called, or the additional costs of processing routine paperwork. In some jurisdictions the occurrence of FTA results in virtually no immediate costs: the defendant's name is called, and if no one answers, the next case is called within a few seconds. Thus, the characteristics of each jurisdiction had to be considered in estimating the costs associated only with the occurrence of FTA. Additionally, costs of FTA occurrence varied by the point at which the FTA occurred: costs were typically less for an FTA at arraignment than for one at trial.

Responses to failure to appear varied by individual defendant. Court records were reviewed as part of the defendant outcomes data collection activities and information was acquired for the following responses (by defendant):

CONTINUED

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- no action taken;
- release revoked;
- bench warrant issued;
- bond ordered forfeited;
- FTA prosecuted;
- warning mailed;
- conditions added; and
- bail set.

Costs were estimated for each of these responses, based on the staff time required for criminal justice system personnel to implement each action. Knowledgeable local officials (e.g., judges, court clerks, prosecuting attorneys, public defenders, police officers) were asked to approximate the staff time required for each action and the average salary of the CJS staff involved. Fringe benefits were added to these salary estimates, based on the overall fringe benefit rates used in the annual budget.

This approach is equivalent to estimating the marginal opportunity costs for each action. Marginal costs are assumed to consist of the staff time (and related fringe benefit costs) consumed by the action. Additionally, it is assumed that if the staff time were not being used in that fashion, it would be productively allocated to another important CJS task (e.g., prosecuting attorneys would spend more time preparing their cases for trial, judges would spend more time deciding on sentences, etc.).

Several of the FTA responses required special care and/or additional information to estimate their costs accurately. These were:

- (1) Release revoked: In this case it was important to know if the defendant was apprehended and detained, and, if so, the

length of the detention. These data were collected for each of the study defendants.

- (2) Bench warrant issued: In this case it was important to know if efforts were made to serve the bench warrant, or if it had been issued only. This information was sought from court records. Costs of serving (or attempting to serve) bench warrants were estimated by police department officials. Additionally, when a bench warrant was served, the defendant was usually booked again, thus generating an additional booking cost.
- (3) Bond ordered forfeited: In this case it was important to know whether the forfeiture was actually enforced (and, if so, for what amount) or whether the forfeiture was merely ordered. Data on the forfeitures shown in the court records were acquired for the study defendants. The forfeiture amounts actually recovered were then subtracted from the costs of FTA.
- (4) Conditions added: For this action it was important to know if efforts were made to enforce these conditions. If so, e.g., if the pretrial release (PTR) program were charged with responsibility for supervising the defendant more closely, then these costs had to be estimated and included in the analysis. Consequently, court records were reviewed to determine whether the added conditions involved added costs for the pretrial release program.
- (5) Bail set: When bail was set, it was important to know if the bail had been made. If not, there was an increase in

detention costs. Consequently, study defendants who had bail set (or increased) in response to FTA were tracked to determine whether detention resulted and, if so, the length of the detention.

- (6) FTA prosecuted: In this case, costs to the CJS include the costs of prosecution. These costs depend, in turn, upon the court in which the FTA occurred.

Finally, failure to appear may result in additional costs at the time a defendant returns to court. When a defendant is returned to court through rearrest (either for FTA or on another charge), an additional court appearance usually occurs. Even when no additional appearance is involved, the court proceedings at the time of return are usually somewhat longer than they would otherwise have been: often the reason for the failure to appear and the pros and cons of changed released conditions are discussed in court; also, special paperwork may be required. On the average, these additional costs at the time of return were estimated as requiring approximately 20 minutes of court time. Details of cost estimation for specific sites are discussed in later sections.

D. Pretrial Arrest Costs

The approach for estimating pretrial arrest costs was similar to that for failure to appear: each component of arrest costs was identified and separately estimated. The five major cost components were:

- apprehension;
- booking;
- pretrial release;
- court processing; and
- sentencing.

Police officials were asked to estimate the costs of apprehension of a typical defendant. They were also asked to estimate the booking costs for a typical defendant.

Additional pretrial release program costs were incurred when a defendant was processed by the program after the rearrest. Thus, the costs of program processing for a typical case were included for rearrested defendants handled again by the program.⁴ Also, the conditions of release sometimes changed as a result of rearrest, and some of these changes had cost implications. In cases where detention was ordered, or resulted from setting or increasing a bond amount, the length of the detention was multiplied by the marginal jail costs per day to estimate detention costs. In cases where bond forfeiture was ordered, the amount actually recovered was determined from court records. In cases where conditions were added or supervision increased, the CJS staff time required (if any) to enforce such restrictions was estimated and costs applied.

Court processing costs varied according to the methods used to obtain the dispositions. Where possible, estimates were derived for four major methods:

- pleas;
- dismissals;
- trials by judges; and
- trials by juries.

⁴This does not constitute double-counting of program costs, because (as described later) those costs are estimated on a workload unit basis and applied to the number of workload units associated with the experiment.

Estimates were usually based on processing costs for a typical case. Because of the nature of the available data, we were rarely able to estimate the marginal costs of court processing and had to use average costs estimates instead.

Sentences for pretrial arrests that resulted in convictions also affected the cost analysis. Specifically, incarceration and probation sentences increased the CJS costs associated with the arrests, and fines decreased those costs. The marginal costs of jail per day per inmate were used to estimate incarceration costs. One difficulty with estimating incarceration costs was that the served length of incarceration could be less than that sentenced. Although information was sought on the average length of time served by charge, such data were not readily available.

For probation, costs were estimated on the basis of the hours of a probation officer's time spent on a typical case each month, multiplied by the associated salary and fringe benefit costs, multiplied by the length of the probation. The cost of an average Pre-Sentence Investigation (PSI) was estimated in a similar manner.

For fines, an effort was made to determine the amounts actually paid by each defendant. If defendants served time in jail in lieu of paying fines, the length of the incarceration was recorded, and the costs included in the analysis.

E. Program Costs

Unlike the cost categories discussed earlier, where marginal cost estimates were of greatest usefulness, program cost estimates reflected

average costs. This was because the experimental procedures were designed to study the impact of a pretrial release program, as compared with the absence of such a program. Since if there were no program, there would be neither fixed nor variable costs, average cost estimates were the appropriate ones for this part of the analysis. (The earlier cost estimates, on the other hand, assumed that the various CJS components—courts, jails, police departments, etc.—would exist and incur certain fixed costs, without regard to the existence or demise of a pretrial release program; thus, marginal costs were of primary interest.)

In each jurisdiction, total program costs were split into those that affected the interview-verification-recommendation process and those that occurred after the release decision had been made (e.g., processing the released defendant, notifying the person of coming court dates, etc.). The technique used for deriving these costs was to estimate the total staff time spent on each function and to apply those percentages to the total budget. Average costs per defendant were then developed by dividing the pre-release cost figure by the number of defendants interviewed and the post-release estimate by the number of defendants involved in the post-release activities. For example, if a jurisdiction spent 75% of its staff time on pre-release activities, and 25% on post-release functions and had an annual budget of \$300,000, then the cost estimates by activity would be \$225,000, pre-release; and \$75,000, post-release. If the annual workload were 30,000 interviews and 20,000 post-release followup cases, then average costs would be \$7.50, pre-release; and \$3.75, post-release.

These estimates of average costs per function were applied to the defendants studied in the experimental jurisdictions to develop a total

program cost estimate. Thus, the total cost estimate reflected the fact that program functions (and costs) varied by type of release (e.g., bailed defendants usually received no post-release program services).

Any treatment costs associated with released defendants were excluded from the analysis. This was because those costs were borne by social service programs, not by the CJS. Additionally, to assess the "true" costs of treatment would have required consideration of the benefits resulting from treatment—a task beyond the scope of the present project.

One additional cost consideration applied to the Beaumont-Port Arthur program, which collects fees in connection with program-recommended releases. These fees were offset against program costs.

F. Other Cost Considerations

Another source of CJS costs is the court's reaction to cases where release conditions are violated (e.g., if a defendant does not comply with the supervision requirements imposed). Data on these responses were collected for the individual defendants studied and, when those responses had CJS cost implications, the costs were included in the analysis. This occurred only rarely, however.

Another cost consideration concerns deposit bonds, since these generate fees that are retained by the court. Such fees were deducted from total costs.

Additional fees imposed in certain jurisdictions were also deducted from total costs. For example, some sites required defendants represented by a public defender to make small payments (e.g., \$15 or so) for these services, and Beaumont-Port Arthur collected a monthly fee from probationers.

As the above discussion indicates, the use of a CJS cost perspective excludes many possible costs and benefits which might result from program operations and release decisions. These include:

- 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);
- "GNP" costs and lost taxes, caused by detained defendants' inability to work (and contribute to GNP or the tax base);
- "bitterness" costs stemming from the detention of innocent defendants;
- welfare costs attributable to families formerly supported by detained defendants;
- costs of decreased "civil liberties" for detained defendants; and
- any costs of pretrial crime which are not borne by the CJS (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).

Although analyses of these and similar costs would be useful for some purposes, such analyses were beyond the scope of the present study.

Finally, it should be stressed that costs, however they are defined and calculated, are not the only considerations that are appropriate for overall analysis of pretrial release programs. Such considerations do, however, provide a useful component of such analysis and were included in the national pretrial release evaluation for that reason.

The following sections discuss the cost estimates for specific sites. Summary data were acquired from interviews with local CJS officials and from various documents and records (e.g., annual reports, court records). Additionally, selected data on individual defendants were obtained during the collection of data for the defendant outcomes analysis. Because of the nature of the available data, many of the cost estimates

are very rough ones, as discussed subsequently; these estimates should be viewed as "ball-park" figures, rather than as precise calculations.

The data collection for the cost analysis (excluding individual defendant data) required approximately one person-week in each site. Analysis of these data took another two to four person-weeks per site. If more resources had been allocated to the cost-effectiveness analysis, it is possible that more precise estimates might have been developed. However, such a level of precision was not deemed necessary for the present study, when alternative uses of the study's resources were taken into consideration.

II. TUCSON COST ESTIMATES

A. Jail Costs

Jail costs were estimated in several ways. Jail staff reported that 1978 costs were \$21 per day, and that this amount was charged to other jurisdictions. This sum is a high estimate of jail costs, because it is not based solely on variable costs. The variable costs of food, clothing, linen and medical care amounted to \$4.53 per inmate-day. This is a low estimate, because some staff costs should be considered variable and included as well. When adjusted to include some staff costs, jail costs were an estimated \$10 per day per inmate.

B. Failure-To-Appear Costs

Failure to appear (FTA) costs were calculated separately for Superior Court and City Court, because of the different costs of CJS personnel. Table 2 shows the staff costs for Superior Court personnel.

TABLE 2
PERSONNEL COSTS, SUPERIOR COURT

Court Personnel	Salary and Fringe Costs Per Hour
Judge	\$25.08
Bailiff	6.45
Court reporter	11.56
Courtroom clerk	7.92
Judge's secretary	7.16
Senior clerk	4.97

Court costs when an FTA occurs vary, depending on whether the FTA occurs at arraignment or at trial. For an FTA at arraignment, the

defendant's name is called; the case is set aside; the name is called again; and a summons or bench warrant is issued. This consumes about five minutes of time each for the judge, court clerk, court reporter and bailiff at a total cost of \$4.07.

For an FTA at trial, about two hours would be lost waiting for the defendant, trying to locate him or her, issuing a bench warrant, and hearing motions to continue. The judge, court clerk, court reporter and bailiff would be involved, at a total cost of \$102.02. For a jury trial, there would be additional juror reimbursement costs of \$36 (12 jurors X \$1.50 per hour X 2 hours). Also, a portion of the jury fee costs⁵ of the Superior Court budget should be allocated to a jury trial FTA. These costs averaged \$321.27 per trial-day in 1978, or \$47.66 per trial-hour. A two-hour loss due to FTA would therefore cost an estimated \$95.32. Thus, total additional FTA costs for jury trials would amount to \$131.32 (\$36.00 + \$95.32), for a total cost of \$233.34 (\$131.32 + \$102.02).

Any FTA also generates costs for the senior clerk, who issues the summons or bench warrant and, if necessary, sends a copy to the sheriff. This takes 15-30 minutes, at an average cost of \$1.87.

When a defendant returns to court after failing to appear, the judge's secretary calls the defense and prosecuting attorneys to notify them. This takes about five minutes and costs 57¢. In court, there will be a discussion of the FTA and perhaps an additional court appearance. On the average, about 20 minutes of time will be involved for the judge, court reporter, court clerk and bailiff, at a cost of \$16.83. Thus,

⁵These costs include food, lodging, etc., associated with jury trials.

total costs upon return of the defendant amount to \$17.40 (\$16.83 + \$0.57).

If there is a separate hearing for modification of release conditions, an additional 15 minutes of time would be required for the judge, court reporter, court clerk and bailiff, at a cost of \$12.75, plus 10 minutes of time for the senior clerk to file the motion and set the hearing data, at a cost of 80¢, for a total cost of \$13.55. For a bond forfeiture hearing, the same staff time would be required, plus an additional 10 minutes of clerical time to process the motion, for a total cost of \$14.35.

If the FTA is prosecuted, there would be additional court processing costs; the FTA would not be merged with the original case. As discussed later, Superior Court processing costs are estimated at \$122 for a dismissal, \$366 for a plea and \$1,220 for a trial.

FTA also generates costs for the County Attorney (prosecutor) and, for defendants represented by the Public Defender, the Public Defender. For the County Attorney's staff, hourly salary and fringe costs are:

- attorney—\$12.54; and
- clerk—\$4.72.

An FTA at arraignment consumes about two minutes of time for the attorney and clerk, while the defendant's name is called, and about 20 minutes of time for the clerk who is involved in issuing the summons, for a total cost of \$2.15.

If an FTA occurs at trial (a rare event), the attorney would lose about three minutes of time before going to work on a backup case, and the clerk would need about 20 minutes to help prepare the bench warrant, for a total cost of \$2.20.

Upon return of the defendant, about 25 extra minutes of time would be required for the attorney, and 20 extra minutes for the clerk, at a total cost of \$6.75. Additionally, if a motion for bond evaluation is heard, 20 minutes of attorney time will be required, at a cost of \$4.18. If a bond forfeiture motion is filed, its preparation requires 15 minutes of attorney time (\$3.14) and 10 minutes of clerical time (76¢) and the court hearing requires another 15 minutes of attorney time (\$3.14), for a total cost of \$7.04.

Although rarely done, if the FTA is prosecuted, costs would involve a 30-minute attorney review to decide to prosecute (\$6.27), a 30-minute attorney submission to the Grand Jury (\$6.27), one hour of clerical preparation and filing-time (\$4.72), and about half the usual costs of court processing (discussed later). Thus, prosecution costs would total \$17.26 plus \$109 for a dismissal, \$327 for a plea and \$1,090 for a trial.

For the Public Defender's Office, hourly salary and fringe costs are as follows:

- attorney—\$10.58;
- legal secretary—\$6.17; and
- criminal investigator—\$11.17.

An FTA at arraignment consumes about 2 minutes of attorney time, costing 35¢. If an FTA occurs at trial, the attorney would lose about two-and-a-half hours, waiting and preparing a written motion for continuance, at a cost of \$26.45.

Additionally, an effort would be made to locate the defendant. At a minimum, the attorney would make a five-minute phone call, and the secretary would send a letter (taking about 10 minutes), at a cost of \$1.84. If the defendant was not located in this way, a criminal

investigator could spend 2½ hours following up on the case, at a cost of \$29.77.

For defendants returned to court, the attorney will conduct an interview at the jail (30 minutes). Additionally, the court appearance following an FTA will be more involved than otherwise and require an extra 30 minutes of attorney time. Thus, total additional costs upon return of the defendant will amount to \$10.58.

If there is a motion to modify release conditions, this will require 30 minutes of secretarial time, 30 minutes of attorney preparation time and 30 minutes of attorney court hearing time, for a total cost of \$13.67. If there is a bond forfeiture hearing, approximately 20 minutes of attorney time will be needed (five minutes for review and 15 minutes for the court hearing), at a cost of \$3.50.

According to the Public Defender's office, no separate prosecutions for FTA in Superior Court had ever been handled by their office.

FTA may affect police costs as well, because the police must serve bench warrants. Based on workload data for the Felony and Fugitive Warrant Division of the Tucson Police Department for the October 1978—July 1979 period, it cost \$63.70 to serve a felony warrant (6.37 hours at \$10.00 per hour). Additionally, when a warrant was successfully served, the defendant was usually booked again, at a cost of \$9.00 (derivation discussed later).

Table 3 summarizes FTA costs for the court, County Attorney, Public Defender and police for Superior Court.

FTA costs for City Court are summarized in Table 4. Court costs are based on the following hourly salary and fringe rates:

- judge—\$15.61;
- bailiff—\$7.23;

TABLE 3
COSTS OF FTA IN SUPERIOR COURT, TUCSON

Item	Court Personnel	County Attorney	Public Defender	Police
<u>Occurrence of FTA:</u>				
Arraignment	\$ 4.07	\$ 2.15	\$.35	\$ 0
Court trial	\$102.02	\$ 2.20	\$ 26.45	0
Jury trial	\$233.34	\$ 2.20	\$ 26.45	0
Bench warrant	\$ 1.87	0	0	\$ 63.70 (service) \$ 9.00 (booking)
Efforts to locate defendant	0	0	\$ 1.84 to \$29.77	0
<u>Return of Defendant after FTA:</u>				
Additional costs at first court appearance after FTA	\$ 17.40	\$ 6.75	\$ 10.58	0
Hearing for modification of release conditions	\$ 13.55	\$ 4.18	\$ 13.67	0
Bond forfeiture hearing	\$ 14.35	\$ 7.04	\$ 3.50	0
<u>Prosecution of FTA:</u>				
Preparation	0	\$ 17.26	0	0
Dismissal	\$122.00	\$109.00	N.A.	0
Plea	\$366.00	\$327.00	N.A.	0
Trial	\$1,220.00	\$1,090.00	N.A.	0

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TABLE 4
COSTS OF FTA IN CITY OR JUSTICE COURT, TUCSON

Item	Court Personnel	City Attorney	Public Defender*	Police
<u>Occurrence of FTA:</u>				
Arraignment	\$ 0.49	\$ 0.17	\$ 0.29	\$ 0
Court trial	5.19	0.75	\$ 22.10	0
Jury trial	9.65	\$ 0.75	\$ 22.10	0
Bench warrant	0.50	0	0	\$ 9.20 (service) \$ 9.00 (booking)
Efforts to locate defendant	0	0	\$ 1.60 to \$27.85	0
<u>Return of Defendant after FTA:</u>				
Additional costs at first court appearance after FTA	\$ 5.58	\$ 1.46**	\$ 8.84	0
<u>Prosecution of FTA:</u>				
Dismissal	\$ 5.00	\$ 2.00	\$ 4.42	0
Plea	3.00	5.00	4.42	0
Court trial	15.00	16.00	N.A.	0
Jury trial	102.00	16.00	N.A.	0

*For Justice Court only. Court-appointed attorneys are used in City Court.

**If the defendant returned voluntarily: 85¢. If the FTA occurred at trial: \$4.01 plus \$4.58 per witness subpoena.

- court clerk—\$5.98; and
- warrant clerk—\$6.07.

For an FTA at arraignment approximately one minute is lost by the judge, bailiff and court clerk, at a cost of 49¢. For an FTA at trial an additional 10 minutes is lost, for a total cost of \$5.19. If a jury is involved, there are additional costs of juror's fees (12 jurors X 10 minutes X \$9.00 per day = \$2.16) and jury maintenance (\$16,775 annual cost ÷ 146 trials = \$114.90 per trial, averaging 8 hours; 10 minutes ÷ 8 hours = 2% X \$114.90 = \$2.30), amounting to \$4.46. Also, each occurrence of FTA generates about two minutes' work for the court clerk, and three minutes' work for the warrant clerk at a total cost of 50¢.

Several costs are associated with the defendant's return to court. When a warrant is returned, the warrant clerk matches it with the defendant's file (five minutes), the court clerk swears out a new complaint (two minutes) and the judge signs the complaint (one minute), for a cost of 96¢. Also, the next appearance requires an extra ten minutes of time from the judge, bailiff and court clerk, at a cost of \$4.62, so that total costs for the defendant's return to court are \$5.58.

If the FTA is prosecuted, the case will be merged with the original case, providing the defendant's plea is the same in both. Processing costs will be approximately one-fourth those of a typical case (discussed later), or \$3.00 for a plea, \$5.00 for a dismissal, \$15.00 for a court trial and \$102.00 for a jury trial.

For the City Attorney (prosecutor), costs are based on the following hourly salary and fringe costs:

- attorney—\$10.57; and
- secretary—\$5.00.

For an FTA at arraignment an attorney loses about one minute of time, at a cost of 17¢. For an FTA at trial an attorney loses about two minutes of time and a secretary, five minutes (filing the paperwork), for a cost of 75¢.

Upon return of a defendant an additional six minutes of attorney time is used and five minutes of secretarial time, for a cost of \$1.46. If the defendant returned voluntarily, the costs would be less: only five minutes of attorney time would be required, for the motion to quash the bench warrant, and costs would thus be 85¢. If the FTA occurred at trial, additional costs are incurred upon the defendant's return. The attorney must check the new trial date with witnesses, make sure there are no other conflicts and check the computer for correct entries; this requires about 15 minutes to do, at an additional cost of \$2.55. Also, the City Attorney's office estimates that each subpoena issued costs about \$4.58. So the costs of a defendant's return to court after FTA at trial total \$4.01 (\$1.46 + \$2.55) plus \$4.58 per witness subpoena.

If the FTA is prosecuted, it will likely be merged with the original case. Costs are estimated as one-fourth the usual case processing costs (discussed below), or \$2.00 for a dismissal, \$5.00 for a plea, and \$16.00 for a trial.

For the Public Defender's office, hourly salary and fringe costs are:

- attorney—\$8.84;
- legal secretary—\$5.64; and
- criminal investigator—\$11.14.

For an FTA at arraignment approximately two minutes of attorney time is lost, at a cost of 29¢. For an FTA at trial about two and one-half hours

of attorney time is used, while waiting for the defendant and preparing a written motion for continuance; this costs \$22.10. Additionally, efforts will be made to locate the defendant. Such efforts range from a five-minute call by an attorney and a letter sent by a secretary (ten minutes), at a total cost of \$1.60, to a two and one-half hour followup by a criminal investigator, at a cost of \$27.85.

For defendants returned to court the attorney will conduct an interview at the jail (30 minutes). Additionally, the court appearance following the FTA will require an extra 30 minutes of attorney time. Thus, total additional costs upon return of the defendant will amount to \$8.84.

If there is a motion to modify release conditions, this will require 30 minutes of secretarial time, 30 minutes of attorney preparation time and 30 minutes of attorney court hearing time, for a total cost of \$11.66. If there is a bond forfeiture hearing, approximately 20 minutes of attorney time will be needed (five minutes for review and 15 minutes for the court hearing), at a cost of \$2.91.

If the FTA is prosecuted, it is likely to be settled through a plea bargain involving the original charge as well. Consequently, additional Public Defender costs are minimal. About thirty minutes of attorney time would be required, at a cost of \$4.42.

Police costs for serving bench warrants were estimated from workload data for the Misdemeanor Warrant Division of the Tucson Police Department for the period August 1978—July 1979. The average warrant took 55.2 minutes of police officer time, at a cost of \$10.00 per hour, for a per-warrant cost of \$9.20. Booking costs (discussed below) when warrants were successfully served averaged \$9.00 per defendant.

C. Pretrial Arrest Costs

Costs of apprehension by the police depend upon whether the officer is dispatched or on-site and whether the situation is an emergency. For emergency situations (e.g., burglaries in progress, family fights), the average time from dispatch to clearing is 39.35 minutes and from arrival on-site to clearing, 58.0 minutes. For situations that are not emergencies, dispatch time averages 40 minutes and on-site, 55.5 minutes. Two officers, with salary and fringe costs of \$10.00 each, are involved in each case. The average apprehension cost for the four situations described above is \$16.00 per arrest.

Booking costs were based on hourly salary and fringe costs of \$7.58 for detention officers working in the Sheriff's Department and \$10.00 for officers of the Tucson Police Department. Booking takes about 30 minutes of detention officers' time (five minutes for the booking officer, 15 minutes for the floor jailer and ten minutes for the identification technician) and an additional 30 minutes for the police officer. Thus, total booking costs are \$8.79 (\$3.79 + \$5.00).

Court processing costs for pretrial arrests depend on the court, the way the case is settled (i.e., by dismissal, plea, trial by judge or jury), and whether the pretrial arrest is adjudicated through separate court appearances or is handled jointly with the original charge. Table 5 summarizes these processing costs.

Costs for the County Attorney were derived from workload estimates and budget data for 1978. The County Attorney's office weights dismissals, pleas and trials as follows:

- dismissal—one workload unit;
- plea—three workload units; and
- trial (either judge or jury)—ten workload units.

TABLE 5
COURT PROCESSING COSTS, TUCSON

CJS Organization	Dismissal	Plea	Trial
<u>Felony Cases:</u>			
County Attorney	\$ 218	\$ 654	\$ 2,180
Superior Court	122	366	1,220 ^a
Public Defender	156	468	1,560
Court-Appointed Attorney	250	250	250
<u>Misdemeanor Cases:</u>			
City Attorney	\$ 7	\$ 21	\$ 70
City Court	18	12	\$58, court \$407, jury
Justice Court	24	71	237
Public Defender ^b	60	180	700
Court-Appointed Attorney	c	c	c

^a\$976 for a court trial and \$1,464 for a jury trial (plus costs of the jury).

^bJustice Court only.

^cCosts are included in City Court processing costs. Court-appointed attorneys cost an average of \$103 per case.

On this basis, the office had 7,015 workload units in 1978, and an adult, criminal, felony court budget of \$1,529,270. Thus, each workload unit cost \$218.00. A dismissal (one unit) cost \$218.00; a plea (three units), \$654.00; and a trial (ten units), \$2,180.00.

The same weighting scheme was applied to the Superior Court workload and budget to derive its costs:

- dismissal—\$122.00;
- plea—\$366.00; and
- trial—\$1,220.00.

Trial costs were considered to be about 50% more costly for jury trials than for court trials, so jury trial costs were estimated at \$1,464.00 and court trial costs at \$976.00. Additionally, jury trials incur juror costs of \$12 per day per juror plus \$381.27 per trial-day for jury maintenance costs.

Costs for a public defender were based on workload estimates and budget data, with the budget reduced by the amount of fees collected from defendants. One workload unit was valued at \$156.00, so that a dismissal cost \$156.00; plea, \$468.00; and trial, \$1,560.00. Costs for court-appointed attorneys average \$250.00 per case.

Estimates for misdemeanor cases were more difficult to derive. For example, the City Attorney's office did not maintain separate budget or workload data for criminal, as compared to civil, cases. Rough allocations of both were made, and the weighting system used for felony cases was applied to these estimates. Resulting costs were approximately \$7.00 per workload unit, or \$7.00 per dismissal, \$21.00 per plea and \$70.00 per trial.

To derive estimates of the City Court processing costs, the City Court Administrator allocated individual cost items to jury trials,

court trials, pleas, and dismissals. For example, the jury trial cost estimate was developed as shown in Table 6.

TABLE 6
JURY TRIAL COST ESTIMATE, CITY COURT

Item	Amount
10% of cost for magistrates	\$15,741
14% of cost for judicial service	7,466
100% of jury cost	16,775
10% of cost for lawyers for indigents	11,313
5% of cost of other professional services	1,424
10% of cost of caseflow management	<u>6,674</u>
Total cost estimate	\$59,393
Total number of jury trials	146
Cost per jury trial	\$407

A similar approach was used to develop the cost estimates for court trial, \$58.00; plea, \$12.00; and dismissal, \$18.00.⁶

Costs for Justice Court were easier to derive and were based on straightforward comparisons of workload units and the budget for criminal cases. The cost of one workload unit was \$23.68, so dismissal cost an estimated \$24.00; plea, \$71.00; and trial, \$237.00.

The Public Defender handles misdemeanors only for Justice Court; indigent defendants in City Court are represented by court-appointed attorneys. The costs of court-appointed attorneys (averaging \$103 per case) are included in the estimates of City Court processing costs.

⁶Costs of dismissals are higher than pleas, because the dismissal proceeding in City Court is somewhat more formal and includes some additional steps.

Public Defender costs for Justice Court were based on workload and budget data, with the budget reduced by the amount of the fees collected from defendants. One workload unit cost \$60.00, so a dismissal cost an estimated \$60.00; plea, \$180.00; and trial, \$600.00.

Additional pretrial arrest costs stem from the sentences imposed on defendants convicted of pretrial arrests. For incarcerated defendants, the daily costs of detention (discussed earlier) were used to estimate the costs of incarceration. For defendants placed on probation, costs were developed from Probation Department data. Table 7 summarizes the probation cost estimates. Costs of Pre-Sentence Investigations (PSI's) were developed on the basis of the person-hours of staff time required to conduct them in 1978. For Superior Court 10.5 hours of probation officer time at \$9.01 per hour were required to research and write the PSI report and appear in court, and 2.14 hours of clerical time at \$5.43 per hour were also needed, for a total cost of \$106.23 per PSI. For misdemeanor cases 5.86 hours of officer time at \$8.56 per hour were required, with clerical time provided by the court. Thus, PSI costs were estimated at \$50.16 (5.86 hours X \$8.56 per hour). PSI's are conducted for all convicted defendants.

TABLE 7
PROBATION COSTS, TUCSON

Court	Pre-Sentence Investigations	Supervision
Superior Court (felonies)	\$106	\$242
City Court (misdemeanors)	\$ 50	\$ 4

Average supervision costs were derived by estimating the total costs applicable to supervision and dividing by the annual supervised caseload. Average costs were \$242 per case per year for felonies and \$4 for misdemeanors.

D. Program Costs

Table 8 summarizes the derivation of program costs by function for the felony program. As shown, the cost for interview and verification averaged \$24.72; for tracking, \$53.96; and for supervision, \$88.31 per investigation and \$176.62 per supervised release. Approximately half the interview and verification costs were for the interview; and the other half, for verification. Thus, the cost of the interview alone was estimated at \$12.36; and verification, also \$12.36.

We hired and paid the staff for the misdemeanor program for the duration of the experiment. Thus, these costs can be easily identified. Other costs (e.g., overhead, fringe benefits) were estimated as bearing the same relationship to staff costs for the misdemeanor program as they do for the felony program. Thus, staff costs were assumed to be 61% of total costs for the misdemeanor program. As shown in Table 9, the cost per interview averaged \$30.20, and the cost of followup notification averaged \$68.75 for the misdemeanor program.

TABLE 8
FELONY PROGRAM COSTS, BY FUNCTION

Item	Interview and Verification	Tracking	Supervision	Overhead	TOTAL
Annual program costs ^a	\$ 34,184	\$ 22,790	\$ 22,790	\$67,416 ^b	\$147,180
Allocation of overhead to functions	29,663	19,551	18,202	-67,416	0
Imputed costs of volunteers (at \$3.50 per hour)	29,200	14,600	7,300	0	51,100
Costs of CETA workers	10,723	10,723	16,085	0	37,531
TOTAL	\$103,770	\$ 67,664	\$ 64,377	0	\$235,811
Workload units	4,197 interviews	1,254 defend- ants released on O.R.	729 units ^c	0	N.A.
Cost per workload unit	\$24.72	\$53.96	\$88.31, investigation \$176.62, supervision	0	N.A.

^aAnnualized, based on costs through May 28, 1979.

^bIncludes administration, supervision and other costs not directly applicable to specific program functions.

^c471 supervision investigations, weighted at 1 each; and 129 supervised releases, weighted at 2 each.

TABLE 9
MISDEMEANOR PROGRAM COSTS, BY FUNCTION

Item	Interview	Notification	Overhead	TOTAL
Cost of salaries	\$ 5,925	\$ 5,140	\$ 3,456	\$14,521
Estimated additional costs			9,284	9,284
Allocation of overhead to functions	6,880	5,860	-12,740	0
TOTAL	\$12,805	\$11,000	0	\$23,805
Workload units	424 interviews	160 defend- ants notified	0	N.A.
Cost per workload unit	\$30.20	\$68.75	0	N.A.

III. LINCOLN COST ESTIMATES

A. Jail Costs

Local officials estimated total jail costs at \$18 per day per inmate. Operating costs were \$10.58 per day per inmate, calculated by dividing the annual number of inmate-days into the annual budget for operating costs. Because operating costs include some staff salaries which must be considered fixed, since a core staff is needed to operate the jail at any inmate level, jail costs were further reduced to an estimated \$7.00 per day for variable costs only.

B. Failure-to-Appear Costs

As in Tucson, failure to appear (FTA) costs for Lincoln vary by the court in which they occur. Table 10 summarizes these cost estimates for Municipal and County court as well as for the prosecuting and defense attorneys.

Hourly salary and fringe costs used to derive these estimates for Municipal Court are:

- judge—\$17.00;
- bailiff—\$8.70;
- clerk/stenographer—\$5.67; and
- docket clerk—\$6.00.

When an FTA occurs, approximately five minutes of time is lost by the judge, bailiff and docket clerk, while the defendant's name is called and in some cases motions are heard. Additionally, five minutes of clerical time is needed to issue the bench warrant. Thus, total costs when an FTA occurs are \$3.10.

When the defendant returns to court, approximately 15 additional minutes are used by the judge, bailiff and docket clerk, at a total

TABLE 10
COSTS OF FAILURE TO APPEAR (FTA), LINCOLN

Part I. Municipal Court

Item	Court Personnel	City Attorney	Public Defender	Police
<u>Occurrence of FTA:</u>				
Lost court time	\$ 3.10	\$ 0.89	\$2.77	0
Bench warrant	0	1.06	0	\$33 (service) \$45 (booking)
<u>Return of Defendant after FTA:</u>				
Additional costs at first court appearance after FTA	7.93	3.95	8.18	0
Bond forfeiture hearing	7.93	3.56	3.50	0
Prosecution of FTA	N.A.	N.A.	N.A.	N.A.

Part II. County Court

Item	Court Personnel	County Attorney	Public Defender	Police
<u>Occurrence of FTA:</u>				
Lost court time	\$ 3.81 ^a	\$ 3.74	\$2.77	0
Bench warrant	0	0	0	\$33 (service) \$45 (booking)
<u>Return of Defendant after FTA:</u>				
Additional costs at first court appearance after FTA	13.70	6.28	8.18	0
Bond forfeiture hearing	13.70	13.80	3.50	0
Prosecution of FTA	N.A.	N.A.	N.A.	N.A.

^aAbout \$56 for failure to appear at trial (rare).

cost of \$7.93. If there is a separate bond forfeiture hearing, this too will require about 15 minutes of court time, at a cost of \$7.93. There is no prosecution for failure to appear in Municipal Court.

Cost estimates for the City Attorney are based on the following hourly salary and fringe rates:

- attorney—\$10.70; and
- clerk—\$4.70.

When an FTA occurs, five minutes of attorney time is lost in court, at a cost of 89¢. If a bench warrant is issued, an additional six minutes of attorney time is needed, at a cost of \$1.06.

Upon return of the defendant, approximately five minutes of clerical time is needed to obtain the defendant's folder and about 20 additional minutes of attorney time is used to review the case, at a cost of \$3.95. If a bond forfeiture hearing occurs, it will require another 20 minutes of attorney time, at a cost of \$3.56.

Public Defender costs are estimated on the basis of the following hourly salary and fringe costs:

- attorney—\$14.00; and
- clerk—\$5.00.

When an FTA occurs, approximately 10 minutes of attorney time is used, waiting for the defendant in court and requesting that the matter be continued as well as trying to locate the defendant. Also, five minutes of clerical time is used, filing papers relating to the FTA. Thus, total costs are \$2.77.

Upon the defendant's return to court, approximately 35 additional minutes of attorney time, at a cost of \$8.18, is spent in court, in conference and in the preparation and filing of motions related to the

FTA. If a bond forfeiture hearing is held, an additional 15 minutes of attorney time at a cost of \$3.50 is required.

Police costs related to FTA consist of the costs of serving bench warrants and of booking the defendants located. The Lincoln police department has a special warrant unit, which has average labor-related costs of \$28.00 per warrant. The mileage costs for an average warrant are estimated at \$5.00, so total costs are \$33.00. Booking costs are estimated as costing \$35.00 for detention officials (who check the defendant's record, computerize additional data, search the defendant and make an inventory of the items held) and about \$10.00 for the arresting officer (roughly one hour of time at \$9.69 salary and fringe costs per hour).

For the County Court, hourly salary and fringe rates are:

- judge—\$17.00;
- clerk—\$4.50; and
- records clerk—\$10.40.⁷

When an FTA occurs, usually about 5 minutes is lost by the judge and records clerk while waiting for the defendant and an additional minute while ordering a bench warrant, for a cost of \$2.73. Also, about 15 minutes of clerical time is needed to type and file the paperwork at a cost of \$1.08. Thus, total costs are \$3.81. If the FTA occurred at trial, two hours of productivity might be lost at a cost of \$54.80, but such FTA's are rare.

Upon the defendant's return to court, approximately 30 additional minutes of time is spent on the case by the judge and records clerk, at

⁷There is no full-time bailiff in County Court.

a cost of \$13.70. If there is a separate bond forfeiture hearing, this too will take about 30 minutes and cost \$13.70. FTA is usually not prosecuted at the misdemeanor level.

For the County Attorney salary and fringe costs are:

- attorney—\$11.00; and
- clerk—\$4.60.

When an FTA occurs, about 12 minutes of attorney time is involved (both in and out of court) and about 20 minutes of clerical time, for a total cost of \$3.74 (including time associated with getting a bench warrant issued).

When the defendant returns to court, approximately 30 additional minutes of attorney time and ten additional minutes of clerical time is used, for a cost of \$6.28. If there is a separate bond forfeiture hearing, about 30 minutes of attorney time will be spent in preparation and another 30 minutes in court. Also, 30 minutes of clerical time will be needed, so total costs will be \$13.80 (\$11.00 for the attorney and \$2.80 for the clerk).

Costs for the public defender and police are the same as in Municipal Court.

C. Pretrial Arrest Costs

Costs of apprehension by the police vary considerably, depending on the nature of the offense. The minimum cost would entail about 30 minutes of police officer time at \$9.70 per hour, or \$4.85 total.

At the other extreme, two officers might spend 3½ hours each responding to an assault, at a cost of \$63.00. Charges like disturbing the peace or drunkenness would involve about 1½ hours of officer time, for a cost

of about \$15.00. Booking costs, as discussed in connection with failure to appear, were \$45.00 per case.

As in Tucson, court processing costs for pretrial arrests vary by the court and the way the case is settled (i.e., dismissal, plea, trial). Table 11 summarizes these costs.

TABLE 11
COURT PROCESSING COSTS, LINCOLN

CJS Organization	Dismissal	Plea	Trial
<u>Municipal Court Cases:</u>			
Court	\$26	\$ 78	\$260 ^a
City Attorney	5	15	50
Public Defender ^b	23	23	23
<u>County Court Cases:</u>			
Court	\$14	\$ 42	\$140 ^a
County Attorney	48	144	480
Public Defender ^b	86	86	86
^a Plus \$20 per day per juror for jury trials.			
^b Net of \$25 per case average reimbursement from defendants.			

Cost estimates for Municipal Court were derived from the total 1978-79 criminal budget (estimated) and weighted workload data (using the same weights as in Tucson: dismissal = 1, plea = 3, and trial = 10). On this basis costs were \$26.00 for a dismissal, \$78.00 for a plea and \$260.00 for a trial. Additionally, a jury trial involves reimbursements to jurors at the rate of \$20 per day per juror.

For the City Attorney similar budget and caseload data were used. A dismissal cost an estimated \$5.00; a plea, \$15.00; and a trial, \$50.00. For the Public Defender, sufficient workload data by case outcome were not available to develop separate cost estimates for dismissals, pleas and trials. Overall, the office handled 752 Municipal Court cases (both misdemeanor and traffic charges) at a cost of \$36,185.00, or \$48.11 per case. Because defendant reimbursement to the Public Defender averaged \$25.00 per case, this sum was deducted from the total costs, leaving a per case cost of about \$23.00.

For the County Court budget and workload data yielded cost estimates of \$14.00 per workload unit, or \$14.00 for a dismissal, \$42.00 for a plea and \$140.00 for a trial (plus jury costs for jury trials). Similarly derived estimates for the County Attorney were \$48.00 per dismissal, \$144.00 per plea, and \$480.00 per trial. For the Public Defender's office, overall budget and caseload data yielded a per-case cost estimate of \$111.00 for County Court misdemeanor cases. An average defendant reimbursement of \$25.00 was subtracted from this sum, for a net cost of \$86.00.

Because our experiment was limited to misdemeanor charges, we did not make a major effort to determine the costs of felony case processing. Rough estimates, based on total budget and workload data, are as follows:

- District Court—\$148.00 per case;
- County Attorney—\$200.00 per case; and
- Public Defender—\$197.00 per case (\$222.00 total less \$25.00 defendant reimbursement).

Additional pretrial arrest costs stem from the sentences imposed on defendants convicted of pretrial arrests. For incarcerated defendants,

the daily costs of detention (discussed earlier) were used to estimate the costs of incarceration. For defendants placed on probation, cost estimates were developed from Probation Department data. Table 12 summarizes these estimates. Municipal Court Pre-Sentence Investigation (PSI) costs were based on an average of 4½ hours time for a probation officer at \$6.50 per hour plus 30 minutes clerical time at \$5.30 per hour, for a total cost of \$31.90. PSI's are not ordered for all convicted defendants; they are most commonly done in alcohol-related cases. Supervision costs were derived from annual budget and caseload data and averaged \$46.00 per case per year.

Cost estimates for County Court were based on annual budget and caseload data and averaged \$108.00 per PSI (ordered for about 75% of convicted defendants) and \$153.00 per case per year for supervision.

D. Program Costs

Table 13 summarizes the derivation of program costs by function. Program costs consist of two screeners who work 20 hours per week at \$3.50 per hour, for a total annual cost of \$7,280.00. The program also receives clerical support amounting to about eight hours per week at a salary and fringe cost of \$4.47 per hour, or \$1,860.00 per year.

Program workload was estimated at:

- 45% for screening and interview;
- 40% for verification; and
- 15% related to the release of defendants.

The program interviewed 166 defendants (annual basis) and released 75 persons, at a cost per interview of \$24.78, per verification attempt of \$22.02, and per release of \$18.28.

TABLE 12
PROBATION COSTS, LINCOLN

Court	Pre-Sentence Investigations	Supervision
Municipal Court	\$ 32	\$ 46
County Court	108	153

TABLE 13
PROGRAM COSTS, LINCOLN

Item	Interview	Verification	Release	TOTAL
Program Costs	\$3,276	\$2,912	\$1,092	\$7,280
Additional Clerical Costs	837	744	279	1,860
TOTAL	\$4,113	\$3,656	\$1,371	\$9,140
Workload units	166 interviews	166 attempts	75 releasees	N.A.
Cost per work- load unit	\$24.78	\$22.02	\$18.28	N.A.

IV. BEAUMONT-PORT ARTHUR COST ESTIMATES

A. Jail Costs

Local officials estimated the current total costs of detention at \$12.00 per day. Variable costs of food, medical care and clothing were estimated at \$3.17 per day. The most reasonable cost estimate probably lies between these two amounts, because some staff costs should be considered variable and some, fixed. As part of a local research study conducted several years ago,⁸ variable detention costs were estimated to be 36% of the total detention costs; on this basis the costs would be \$4.32 per day (36% of \$12.00).

B. Failure-To-Appear Costs

Table 14 summarizes failure-to-appear costs for courts on the Beaumont-Port Arthur area. For the District Court, hourly salary and fringe costs are:

- judge—\$23.70;
- bailiff—\$7.90;
- court reporter—\$10.07; and
- clerk—\$5.73.

When an FTA occurs, approximately five minutes of court time is lost, by the judge, bailiff and court reporter, at a cost of \$3.45. Additionally, about one hour and 15 minutes of clerical time is used, at a cost of \$7.16. Thus, total costs for FTA occurrence are \$10.61. When the defendant returns to court, approximately 30 additional minutes of court time will be needed, at a cost of \$20.80.

⁸ Ronald J. Pry, A Descriptive Study of Pretrial Release on Personal Bond in Jefferson County, Texas, 1975, p.18.

For the County Court, hourly salary and fringe costs are:

- judge—\$20.68;
- bailiff—\$6.39;
- court reporter—\$10.07; and
- clerk—\$5.56.

When an FTA occurs, approximately five minutes of court time is lost, involving the judge, bailiff and court reporter, at a cost of \$3.07. Also, about one hour and 15 minutes of clerical time will be needed to type a capias, prepare a judgment, etc., at a cost of \$6.95. Thus, total costs for an FTA occurrence are \$10.02. If the FTA occurs at trial, a wait of two hours may occur, at a cost (for judge, bailiff and court reporter) of \$74.28 (plus \$6.95 clerical costs). Upon the defendant's return, about 30 additional minutes of court time (judge, bailiff, court reporter) will be used, at a cost of \$18.58.

District Attorney costs for both courts are estimated on the basis of hourly salary and fringe costs of \$12.62 per attorney. When an FTA occurs, about 40 minutes of attorney time is used in court and in preparation of a judgment, at a cost of \$9.47. Upon the defendant's return to court, an additional 30 minutes of attorney time is used, at a cost of \$6.31.

No additional defense attorney costs are incurred by the CJS when FTA occurs. There is no public defender in the area, and court-appointed attorneys are paid by the case.

Police costs associated with FTA consist of the costs of serving bench warrants and booking apprehended defendants. The average cost per warrant is \$7.66, calculated by dividing the total salary costs for the warrant squad by the number of warrants served. Booking costs are \$8 per case.

TABLE 14
FAILURE-TO-APPEAR COSTS, BEAUMONT-PORT ARTHUR

Item	Upon Occurrence of FTA	Upon Return of Defendant
District Court	\$10.61	\$20.80
County Court	10.02 ^a	18.58
District Attorney	9.47	6.31
Police	0	7.66 (warrant service) 8.00 (booking)

^aFor FTA at trial, costs are around \$80.00.

TABLE 15
COURT PROCESSING COSTS, BEAUMONT-PORT ARTHUR

CJS Organization	Dismissal	Plea	Trial
<u>District Court:</u>			
Court	\$ 80	\$240	\$800
District Attorney	44	132	440
Court-appointed attorney	200	200	500
<u>County Court:</u>			
Court	\$ 15	\$ 45	\$150
District Attorney	44	132	440
Court-appointed attorney	125	125	125
<u>Justice Court:</u>			
Court	\$ 10	\$ 30	\$100
District Attorney	44	132	440
Court-appointed attorney	75	75	75

C. Pretrial Arrest Costs

Because costs of apprehension could not be derived, the cost of serving a warrant (\$7.66) was used as an estimate (probably a low one). Booking costs were \$8.00 per case.

Court processing costs are summarized in Table 15. These estimates were derived from annual budget and workload information, weighted as in Tucson and Lincoln (i.e., dismissal = one workload unit; plea = three units; trial = ten units). The cost per workload unit was \$80.00 for District Court, \$15.00 for County Court and \$10.00 for Justice Court. Thus, dismissals, pleas and trials cost \$80.00, \$240.00 and \$800.00, respectively, in District Court; \$15.00, \$45.00, and \$150.00 in County Court; and \$10.00, \$30.00, and \$100.00 in Justice Court. (Jury costs are included in the cost estimates for trials.)

District Attorney costs were also estimated from budget and weighted workload data (for all courts combined). Resulting costs were \$44.00 per workload unit, or \$44.00 for a dismissal, \$132.00 for a plea and \$440.00 for a trial.

Average payments to court-appointed attorneys are \$200.00 per case in District Court, \$125.00 per case in County Court and \$75.00 per case in J.P. Court. Also, when District Court cases go to trial, court-appointed attorneys are paid an average of an additional \$300.00 per case.

Pretrial arrest costs also result from the sentences imposed on defendants convicted of pretrial arrests. For incarcerated defendants, the daily costs of detention (discussed earlier) were used to estimate the costs of incarceration. For defendants placed on probation, cost estimates were developed from Probation Department data and are shown in Table 16. The cost of a Pre-Sentence Investigation was estimated at

\$68.00, based on budget and workload data for this function. PSI's are conducted for all defendants convicted of felonies and about 15% of those convicted of misdemeanors.

TABLE 16
PROBATION COSTS, BEAUMONT-PORT ARTHUR

Court	Pre-Sentence Investigation	Supervision
All courts	\$68	\$177 ^a
^a Probationers pay \$15.00 per month, which is not reflected in this figure.		

Supervision costs were also estimated from total budget and workload data and average \$177.00 per case per year. This is offset by the \$15.00 per month fees probationers pay while on probation. The average length of a probation sentence for a misdemeanor is six months, and early termination is rare. For felony cases, the average length of probation is about seven years, with approximately 15% of the cases terminated early.

D. Program Costs

Table 17 summarizes the derivation of program costs by function. These costs are for three full-time employees, including one paid through C.E.T.A. The cost per defendant for interview and verification is about \$36.00 and for followup after release, about \$68.00. The program also collects a fee from each defendant released through the program; this fee is 3% of the bond amount (no defendants are released without bond); however, a fee of \$20.00 is required on bonds of \$500.00 or less. These fees reduce the CJS costs of the program's operations.

TABLE 17
PROGRAM COSTS, BEAUMONT-PORT ARTHUR

Item	Interview and Verification	Followup	Administration	TOTAL
Staff Costs	\$13,325	\$13,206	\$ 8,844	\$35,375
Allocation of admini- stration to functions	4,422	4,422	-8,844	0
TOTAL	\$17,747	\$17,628	0	\$35,375
Workload units	494 interviews	279 defen- dants re- leased through program	N.A.	N.A.
Costs per workload unit	\$35.93	\$63.18	N.A.	N.A.

V. BALTIMORE CITY COST ESTIMATES

A. Introduction

Because we were unable to derive detailed cost data for Baltimore City but wished to develop at least a rough cost-effectiveness analysis for the program changes tested, we used the mean unit costs from the other experimental sites for estimating purposes. This approach has obvious disadvantages, since the cost structure of Baltimore's criminal justice system may be quite different than that of the other sites. However, this drawback is partly offset by the fact that the unit costs are applied to the actual experimental outcomes data for both the program and control groups. Thus, for example, the cost of serving a bench warrant is the same for both groups (and is based on the mean cost for the other three sites), but the number of bench warrants served reflects the documented experience of each group.

Although far from ideal, this approach seemed more satisfactory than conducting no cost-effectiveness analysis of Baltimore City. Because the analysis is based partly on the mean costs from the three other sites, these estimates are derived below.

B. Cost Estimates

Table 18 summarizes the cost estimates for detention, failure to appear and pretrial arrest. For program costs we used the \$13 per interview estimate developed as part of the cross-sectional study (see Volume I, Chapter II). Surveillance costs were estimated as \$100 per case; referral to services, \$300 per case; and minimum followup (for the control group), \$5 per case. These are very imprecise estimates, based on our cross-sectional analyses and our observations of the relative time expended on different activities by program staff during the experiment.

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TABLE 18
COST ESTIMATES FOR BALTIMORE CITY

Item	Tucson	Lincoln	Beaumont - Port Arthur	Mean (Baltimore City Estimate)
A. Variable Daily Jail Costs	\$10.00	\$ 7.00	\$ 4.32	\$ 7.11
B. Failure To Appear				
1. Occurrence				
• Higher Court				
—Without Public Defender	6.22	7.55	20.08	11.28
—Additional Public Defender Costs	2.19	2.77	NA	2.48
• Lower court				
—Without Public Defender	0.66	3.99	19.49	8.05
—Additional Public Defender Costs	1.89	2.77	NA	2.33
• Bench warrant issued				
—Higher court	1.87	NA	NA	1.87
—Lower court	0.50	1.06	NA	0.78
• Bench warrant served				
—Higher court	72.70	78.00	15.66	55.45
—Lower court	18.20	78.00	15.66	37.29
• Bench warrant service attempted				
—Higher court	63.70	33.00	7.66	34.79
—Lower court	9.20	33.00	7.66	16.62
2. Return to Court				
• Higher court				
—Without Public Defender	24.15	19.98	27.11	23.75
—Additional Public Defender Costs	10.58	8.18	NA	9.38
• Lower court				
—Without Public Defender	7.04	11.88	24.89	14.60
—Additional Public Defender Costs	8.84	8.18	NA	8.51

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TABLE 18 (CONTINUED)
COST ESTIMATES FOR BALTIMORE CITY

Item	Tucson	Lincoln	Beaumont - Port Arthur	Mean (Baltimore City Estimate)
• Bond forfeiture hearing				
—Higher court, without Public Defender	\$21.39	\$27.50	\$ NA	\$24.45
—Higher court, additional Public Defender costs	3.50	3.50	NA	3.50
—Lower court, without Public Defender	NA	11.49	NA	11.49
—Lower court, additional Public Defender costs	NA	3.50	NA	3.50
3. Prosecution of FTA				
• Higher court				
—Dismissal, without Public Defender	231.00	NA	NA	231.00 ^a
—Dismissal, additional Public Defender costs	NA	NA	NA	NA
—Plea, without Public Defender	693.00	NA	NA	693.00
—Plea, additional Public Defender costs	NA	NA	NA	NA
—Trial, without Public Defender	2310.00	NA	NA	2310.00
—Trial, additional Public Defender costs	NA	NA	NA	NA
• Lower court				
—Dismissal, without Public Defender	7.00	NA	NA	7.00 ^a
—Dismissal, additional Public Defender costs	4.42	NA	NA	4.42
—Plea, without Public Defender	8.00	NA	NA	8.00
—Plea, additional Public Defender costs	4.42	NA	NA	4.42
—Trial, without Public Defender	31.00	NA	NA	31.00
—Trial, additional Public Defender costs	NA	NA	NA	NA

TABLE 18 (CONTINUED)
COST ESTIMATES FOR BALTIMORE CITY

Item	Tucson	Lincoln	Beaumont- Port Arthur	Mean (Baltimore City Estimate)
<u>C. Pretrial Arrest</u>				
1. Apprehension	\$16.00	\$15.00	\$ 7.66	\$12.89
2. Booking	8.79	45.00	8.00	20.60
3. Court Processing Costs				
• Higher court				
—Dismissal, without Public Defender	340.00	62.00	124.00	175.33
—Dismissal, addi- tional Public Defender costs	156.00	86.00	200.00 ^b	147.33
—Plea, without Public Defender	1020.00	186.00	372.00	526.00
—Plea, additional Public Defender costs	468.00	86.00	200.00 ^b	251.33
—Trial, without Public Defender	3156.00	620.00	1240.00	1672.00
—Trial, additional Public Defender costs	1560.00	86.00	500.00 ^b	715.33
• Lower court				
—Dismissal, without Public Defender	25.00	31.00	59.00	38.33
—Dismissal, addi- tional Public Defender costs	60.00	23.00	125.00 ^b	69.33
—Plea, without Public Defender	33.00	93.00	177.00	101.00
—Plea, additional Public Defender costs	180.00	23.00	125.00 ^b	109.33
—Trial, without Public Defender	128.00	310.00	590.00	342.67
—Trial, additional Public Defender costs	700.00	23.00	125.00 ^b	282.67

TABLE 18 (CONTINUED)
COST ESTIMATES FOR BALTIMORE CITY

Item	Tucson	Lincoln	Beaumont- Port Arthur	Mean (Baltimore City Estimate)
4. Sentencing Costs				
• Pre-Sentence Investigations				
—Higher court	\$106.00	\$108.00	\$68.00	\$94.00
—Lower court	50.00	32.00	68.00	50.00
• Probation Supervision (Annual Cost per Case)				
—Higher court	242.00	153.00	177.00	190.67
—Lower court	4.00	46.00	177.00	75.67

^aMean of \$119 used for Baltimore estimate.^bCourt-appointed attorney.

APPENDIX C
METHODOLOGY FOR DEFENDANT
OUTCOMES AND DELIVERY SYSTEM ANALYSES

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APPENDIX C
METHODOLOGY FOR DEFENDANT
OUTCOMES AND DELIVERY SYSTEM ANALYSES

The data needed for the defendant outcomes analysis was almost identical to the information required for cross-sectional analyses. A few data elements (e.g., number of dependents) were deleted, because the information acquired in the cross-sectional study had been very incomplete. Also, several data elements were added for use in the cost-effectiveness analysis. This information included days of detention resulting from failure to appear or pretrial rearrest, whether bond forfeitures were collected or merely ordered, and the length of incarceration or probation sentenced for pretrial rearrest convictions. The data collection guide appears at the end of this appendix.

Although the data needs were largely the same as for the cross-sectional study, a different data collection approach was used. Participating programs sent copies of completed interview sheets to our office, where the information was coded in machine-readable form. After sufficient time had elapsed so that most cases would have reached final disposition, data collectors were hired locally and trained by our field supervisor, one of the persons who had collected data for the cross-sectional study. The local data collectors recorded the followup information needed on "summary sheets" and sent these to our office, where the data were coded and merged with the interview information. Each completed case was checked by the field supervisor for completeness and consistency before it was added to the data base.*

* In Baltimore the data were coded by the data collectors, as the information was acquired. A person who had collected data for the cross-sectional study served as first-line supervisor of the Baltimore data collection activities and was on-site each day to answer questions that might arise. The field supervisor also assisted in resolving Baltimore data collection problems.

The data collection guide appears at the end of this appendix, along with the summary sheets used by the local data collectors. The interview forms used by participating programs appear in Appendix D.

After the data were collected, the experimental and control groups were compared for 19 characteristics to determine whether they were equivalent. Then, release outcomes (rate of release, speed of release, type of release, equity of release) were compared for the two groups. Similarly, the characteristics of released defendants were compared to determine if the groups were equivalent; then, post-release outcomes (failure to appear, pretrial rearrest, conviction for pretrial rearrest) were compared. When groups were not equivalent, controls were exercised to study the effect of the groups' differences on outcomes.

The delivery system analyses for the experimental sites were patterned after those of the cross-sectional study. Additional questions were asked about possible changes in CJS procedures or officials' behavior that might affect the outcomes of the experiments. (Appendix F summarizes these findings for Tucson.) Also, data on criminal justice system costs were sought for inclusion in the cost-effectiveness analysis. Because most of the questions asked were identical to those of the cross-sectional study, the interview guides are not repeated in this appendix.

The delivery system information was used to prepare case studies of Lincoln and Beaumont-Port Arthur, the two sites not included in the cross-sectional analyses. Additionally, important findings from the delivery system analysis are included in the relevant chapters of Volume II.

A brief cost-effectiveness analysis was conducted, using information derived from both the delivery system interviews and the data on individual defendants. Appendix B discusses the approach for this analysis in detail.

DATA COLLECTION GUIDE

DEFENDANT/SI: RECORDING FORM
FOR EXPERIMENTAL SITES

Instructions: When data for a question is missing, always fill in ALL the columns with "0's".

INFORMATION	CODES
A. IDENTIFIERS	
<u>1</u> Coding Sheet No. (1)	
<u>2</u> Site I.D. No. (2) (3)	06 = Tucson 09 = Lincoln 10 = Baltimore 11 = Beaumont
<u>4</u> Defendant I.D. No. (4) (5) (6) (7)	02 = experimental group 03 = control group 04 = experimental group (Tucson felony study) 05 = control group (Tucson felony study)
<u>8</u> Study Type (8)	
B. DEFENDANT DEMOGRAPHIC INFORMATION	
<u>9</u> Date of Birth (9) (10) (11) (12)	Month, Day—Use two digits for each (viz., 02 12 = February 12th)
<u>13</u> Age at time of arrest for this (13) (14) offense	Use two digits, e.g., <u>3</u> <u>0</u>
<u>15</u> Ethnicity (15)	Black = 1 Oriental = 4 Hispanic Surname = 2 Other = 5 White = 3 American Indian = 6
<u>16</u> Sex (16)	Male = 1 Female = 2
C. BACKGROUND INFORMATION	
<u>17</u> Marital Status (17)	Married, living with spouse = 1 Married, not living with spouse = 2 Divorced = 3 Widowed = 4 Never Married = 5
<u>18</u> Does Defendant (D) support family? (18)	Yes, Alimony = 1 Yes, Supports Child = 2 Yes, Alimony and Child Support = 3 Yes, Other type of family support = 4 No = 5
<u>19</u> With whom does (D) live? (19)	Parent = 1 Other Relative = 4 Spouse = 2 Non-Family Person = 5 Guardian = 3 Alone = 6
<u>20</u> BLANK (20)	
<u>21</u> BLANK (21) (22)	

Local Resident?
(23)

Yes = 1; No = 2

If (23) answer is "1",

Number of years of local
(24) (25) residence?

Use two digits.

Number of months at present
(26) (27) (28) address

Use three digits. 1 month or less = 001

Is person an alien?
(29)

Yes, legal = 1 Foreign-born, status
Yes, illegal = 2 Unknown = 4
No = 3

Employed at time of arrest?
(30)

Yes = 1
No = 2
Housewife = 3
Fulltime Student = 4
Employed and Student = 5

If (30) answer is "1",

What is occupation?
(31) (32)

Professional/Technical = 01
Managers/Administrators = 02
Sales/Retail = 03
Clerical = 04
Craftsmen/Foremen = 05
Operatives (except Transportation) = 06
Transportation Equipment Operatives = 07
Laborers (except Farm) = 08
Farm Workers = 09
Service workers = 10
Private household workers = 11
Armed Forces = 12
Use three digits. 1 month or less = 001

If (30) answer is "2",

What was the last occupation?
(33) (34)

Number of months at job
(35) (36) (37) if (30) answer is "1"

Record amount, using three digits.

Estimated weekly earnings
(38) (39) (40) from job, if (30) is "1"

Use three digits

If unemployed, number of
(41) (42) (43) months unemployed

BLANK
(44)

Is defendant or family receiving
(45) any form of public assistance?
(excluding unemployment compensation)

Yes = 1
No = 2

INFORMATION	CODES
<p>Educational achievement to date (46)</p>	<p>Graduate School or Professional School = 1 College Grad. = 2 Some College = 3 Technical or Vocational School in addition to High School Graduate = 4 High School Grad. = 5 Technical or Voca- tional without H.S. Grad. = 6 Some H.S. = 7 Less than High School = 8</p>
<p>D. CRIMINAL HISTORY</p> <p>Does Defendant have a juvenile arrest (47) record?</p> <p>Is Defendant a veteran? (48)</p> <p>Age at first <u>adult</u> arrest (49) (50)</p> <p>Number of prior <u>adult</u> arrests (51) (52)</p> <p>Number of prior <u>adult</u> convictions. (53) (54)</p> <p>BLANK (55) (56) (57)</p> <p>Do not complete (58-63) if defendant has less than 3 prior arrests or if defendant has not been arrested at least twice for at least one partic- ular charge.</p> <p>Most frequent charge in total (58) (59) record.</p> <p>Second most frequent charge (60) (61)</p> <p>Third most frequent charge (62) (63)</p> <p>Date of most recent (64) (65) (66) (67) previous arrest</p> <p>Time of Arrest for (68) (69) (70) (71) Current Arrest (charge under study)</p>	<p>Yes = 1, No = 2</p> <p>Yes = 1, No = 0</p> <p>If only date of arrest is available, record it on New Codes Sheet for later conversion.</p> <p>Use Crime Codes Sheets</p> <p>Use Continuous 24-hr. times (e.g., 1415=2:15 PM)</p>

INFORMATION	CODES
<p>BLANK</p> <p>(72) (73) (74)</p> <p>Has the defendant failed to appear in (75) the past?</p> <p>If (75) answer is "1", How many times? (76)</p>	<p>Yes = 1</p> <p>No = 2</p> <p>Record Number</p>
<p>E. PRESENT ARREST AND RELEASE FOR PREVIOUS CHARGE</p> <p>Date of Arrest</p> <p>(77) (78) (79) (80)</p> <p>IDENTIFIERS</p> <p>2 Coding Sheet Number (81)</p> <p>Site I.D. Number</p> <p>(82) (83)</p> <p>Defendant I.D. No.</p> <p>(84) (85) (86) (87)</p> <p>Status at time of arrest</p> <p>(88)</p> <p>If answer to (86) is "3" or "4", list Probation or Parole Officer's name on Names Sheet.</p> <p>If answer to (88) is other than "1", list most serious charges for which released prior to arrest under study:</p> <p>(89) (90)</p> <p>(91) (92)</p> <p>(93) (94)</p> <p>If answer to (88) is "2", what type of (95) release?</p> <p>Date of Previous Release</p> <p>(96) (97) (98) (99)</p>	<p>Use Data Coding Sheet</p> <p>No other CJS involvement = 1</p> <p>On PTR for another charge = 2</p> <p>On probation = 3 On parole = 4</p> <p>2 and 3 = 5 Outstanding Warrant=6</p> <p>6 and 2 = 7 6 and 3 = 8</p> <p>4 and 2 = 9</p> <p>Use Crimes Codes Sheet</p> <p>(If answer to 88 above involves two statuses i.e., 5 = 2+3, and the charge for each status is known, code the charges for the numbers they represent in order from lowest to highest, i.e., 89-90=crime for 2, 91-92=charge for 3)</p> <p>Bail bond = 1 Conditional Release=5</p> <p>ROR = 2 Supervised Release=6</p> <p>Deposit bail = 3 Citation release=7</p> <p>Unsecured bond = 4 Do Not Release=8</p> <p>Summons Warrant=9</p> <p>Use Date Coding Sheet</p>

INFORMATION	CODES
F. RELEASE INFORMATION (CHARGE UNDER STUDY)	
Was defendant given release option pending trial? (100)	By judge = 1; PTR Program acting on its own = 2; By Bail Commissioner = 3; By Magistrate Other than Judge = 4; Don't Know by Whom = 5; Not Released = 6; Arresting Officer = 7; Pre-set Bail = 8; Sheriff's Dep't. = 9;
Date of Release (101) (102) (103) (104)	Use Date Coding Sheet If (D) failed to get release put zeros in (101)-(104)
Time of Release (105) (106) (107) (108)	Use continuous 24-hour times (e.g., 1415= 2:15 PM)
Original Type of Release by Officials (109)	Bail = 1; ROR = 2; Dep. Bail = 3; Unsec. Bond = 4; Cond. R. = 5; Supervised Rel. = 6; Citation Rel. = 7; Release Denied = 8
If money involved (i.e., the answer to previous question is code "1", "3", "4" or "6"), specify amount. (110)	Below \$250 = 1; \$251-\$500 = 2; \$501-\$1000 = 3; \$1,001-\$1,500 = 4; \$1,501-\$2,000 = 5; \$2,001-\$2,999 = 6; \$3,000-\$4,999 = 7; \$5,000-\$9,999 = 8; \$10,000 or more = 9
If percentage involved, specify (111)	5% = 1; 10% = 2; 15% = 3; 20% = 4; 25% = 5; 20%-50% = 6; Over 50% = 7
Type of release by officials after all reconsiderations, if different (112)	Use codes in (109)
Date of last reconsideration of release conditions (113) (114) (115) (116)	Use Date Coding Sheet
New amount, if appropriate (117)	Use codes in (110)
New percentage, if appropriate (118)	Use codes in (111)
If there were conditions or supervision, indicate the kind [for Baltimore, this refers to final supervision conditions] (119)	Report to drug treatment program during pretrial period = 1; Alcohol program = 2; Report to manpower/job program [diversion] = 3; Supervisory Custody = 4; Surveillance = 5; Mental Health = 6; Minimum call-ins = 7; Normal call-ins = 8
Was someone in PTR program charged with the responsibility of seeing to it that the defendant got to court at the proper time? (120)	Yes, Personally Responsible [Supervision Group] = 1; Yes, General Program Responsibility [Normal Call-ins, errors under study procedure] = 2; No [Minimum Call-ins] = 3; No [excluded from supervision study] = 4; [Brackets = Baltimore study]

BLANK
(121)

INFORMATION	CODES
If raising of resources was necessary for release, how did defendant raise them? (122)	Other than bondsman = 1; Bondsman = 2
BLANK (123)	
Program Involvement:	
Was defendant interviewed by PTR Program? (124)	Yes = 1; No = 2
When was this done (if done), for first time? (125)	Immediately after arrest = 1; After first bail hearing = 2; At time of subsequent arrest = 3;
If the answer to (124) is yes, how many hours after arrest? (126) (127)	
How was information verified? (128)	Telephoned references = 1; Visited references = 2; Checked files = 3; 1 and 2 = 4; 1 and 3 = 5; 2 and 3 = 6; 1, 2 and 3 = 7; Did not verify anything = 8
If checked, how many references were checked? (129)	Indicate number (9 or more = 9)
Was information gathered on the defendant available and presented to release source? (130)	Yes = 1; No = 2
What release recommendation was made by Program? (131)	Indicate type using (95) codes; except Do Not Recommend = 9
If money involved, specify amount recommended (132)	Use codes in (110)
If percentage involved, specify percentage recommended (133)	Use codes in (111)
If conditions or supervision were recommended, what were they? [for Baltimore this refers to initial supervision referral] (134)	Report to drug treatment program during pretrial period = 1; Alcohol program = 2; Report to manpower/job program = 3; Supervisory Custody = 4; Report to PTR = 5; 5 and 1, 2, or 3 = 6; 4 and 5 = 7; Counseling/Mental Health = 8; Surveillance = 9
Has defendant been employed during the pretrial release period? (135)	Use codes in (30). 0 if not released; (D) believed he could get job back = 6; (D) believed he will lose job = 7; Note: Answer (136), (137) and (138) only if (135) is 1, 5 or 6.
If Yes, list occupation during that time (136) (137)	Use codes in (31) and (32)

C-10

INFORMATION	CODES
Is this the same job defendant held (138) prior to arrest?	Yes = 1; No = 2; Unemployed = 0
G. PROGRAM INTERVENTION (If Applicable)	
Frequency of program contact with (D) (139)	Once a week or more = 1; Less than 1, but at least once a month = 2; Less than once a month = 3
Average length of contact (140)	Less than 15 minutes = 1; 16-30 minutes = 2; 31-60 minutes = 3; more than 1 hr. = 4
Primary mode of contact (141)	Telephone = 1; In person at Program Office = 2; In person, elsewhere = 3; Mail = 4; 1 and 2 = 5
Types of service provided to (D) by (142) Program	Reminded (D) of court dates = 1; Counseled (D) = 2; Referred (D) to services = 3; Monitored (D) compliance with conditions of release = 4; 1 and 4 = 5; 1, 3 and 4 = 6; 1, 2, 3, 4 = 7 List Combinations or others on New Codes Sheet
Did (D) comply with release conditions? (143)	Yes = 1; PTC = 2; FTA = 3; Non-report = 4; Failure to retain attorney = 5
If no, what action did program take? (144) (Record additional responses, combinations or modifications on New Code Sheet)	Tried to locate (D) by phone = 1 Tried to locate (D) by mail = 2 Tried to locate (D) by personal visit = 3 Issued petition for release revocation/affadavit for release of surety (AFRS) = 4 Issued petition for bench warrant = 5 Notified police of fugitive status = 6 Lectured (D)/reviewed circumstances = 7 1, 2 or 3 and 4 = 8 1, 2 or 3 and 7 = 9
If this action resulted in (145) (146) (147) detention, what was the length of detention?	Record number in days (i.e., 001, 010)
H. WEIGHT OF EVIDENCE	
Number of witnesses against (D) (148) (149)	
Number of witnesses for (D) (150) (151)	
Was a police officer a witness? (152)	Yes, but not only witness = 1; Yes, only witness = 2; No = 3
Were weapons or apparatus used? (153)	Yes, weapons involved, found in possession = 1; Yes, weapons involved, not found in possession (includes involved, unknown if found in possession) = 2; Yes, apparatus involved, found in possession = 3; Yes, apparatus involved, not found in possession = 4

C-11

INFORMATION	CODES
Has (D) confessed to crime charged? (this (154) refers to pretrial confession)	Yes, both involved and found in poss. = 5 Yes, both involved, not found in poss. = 6 No = 7
Was (D) caught in the act or at the scene (155) of the crime charged?	Yes = 1; No = 2
Type of representation (156)	Private attorney = 1; Public Defender = 2; Court appointed private attorney = 3; Self-representation ("pro se") = 4; Status Unknown = 5
Relation of victim to (D) (157)	Immediate family = 1; Prior acquaintance = 2 = 3; Commercial = 4; Non-commercial = 5; Police Officer = 6; No victim = 7; Multiple Victims = 8
I. COURT APPEARANCES AND DISPOSITIONS FOR THREE MOST SERIOUS CHARGES FOR WHICH TRIED	
What is the most serious charge (158) (159)	Use Crime Codes Sheet
BLANK (160)	
3 Coding Sheet No. (161)	
Site I.D. No. (162) (163)	
Defendant I.D. No. (164) (165) (166) (167)	
Date of first scheduled (168) (169) (170) (171) appearance on this charge	Use Date Coding Sheet
Number of real scheduled appearances for (172) this charge	Record number. 9 or more = 9
BLANK (173)	
Number of all postponements for this charge (174)	Record number. 9 or more = 9
In which court was (D) tried? (NOTE: If (175) more than one court, record the court in which disposition occurred)	Municipal Court [Lincoln] = 1; County Court [Lincoln, Beaumont] = 2; District Court [Lincoln, Beaumont, Baltimore] = 3; J.P. Court [Tucson, Beaumont] = 4; City Court [Tucson] = 5; Superior Court [Tucson] = 6; Supreme Bench [Baltimore] = 7

INFORMATION	CODES
<div>(176) (177) (178) (179)Date of disposition for most serious charge</div> <div>(180) (181)Charge for which tried</div> <div>(182)Outcome of trial</div> <div>(183) (184)Type of Sentence</div> <div>(185) (186) (187)Length of unsuspended incarceration</div> <div>(188) (189) (190)Length of unsuspended Probation</div> <div>(191) (192)What is the second most serious charge?</div> <div>(193) (194) (195) (196)Date of first scheduled appearance on this charge?</div> <div>(197)Number of real scheduled appearances on this charge</div> <div>(198)Number of all postponements for this charge</div> <div>(199)(If more than one court, record court in which disposition occurred)</div>	<div>Use Date Coding Sheet</div> <div>Use Crime Codes Sheet (Record change, reduction, etc. from original charge)</div> <div>Pled no contest = 1; Pled Guilty = 2; Dismissed or not prosecuted = 3; Acquitted, Judge = 4; Acquitted, Jury = 5; Found Guilty, Judge = 6; Found Guilty, Jury = 7; Not Convicted (other than 3, 4 or 5) = 8; Bail Forfeited in Lieu of Appearance = 9</div> <div>Incarceration = 01 02, 04, 05 = 14 Probation = 02 01, 02, 04, 05 = 15 Suspended Sent. = 03 01, 02, 04 = 16 Fine = 04 "Volunteer" Services = 17 Treatment = 05 01, 02, 05 = 18 02 and 03 = 06 01 and 02 = 19 02, 03, 04 = 07 04 and 05 = 20 01, 04 = 08 01 and 05 = 21 02, 03, 05 = 09 01, 02, 03, 04 = 22 02 and 04 = 10 02, 03, 04, 05 = 23 03 and 04 = 11 01, 02, 03 = 24 05 and 07 = 12 01, 02, 03, 05 = 25 02 and 05 = 13 Time Served = 26</div> <div>Record number of <u>days</u></div> <div>Record number of <u>months</u></div> <div>Use Crime Codes Sheet</div> <div>Use Date Coding Sheet</div> <div>Record Number. 9 or more = 9</div> <div>Record Number. 9 or more = 9</div> <div>Municipal Ct. [Lincoln] = 1 County Ct. [Lincoln, Beaumont] = 2 District Ct. [Lincoln, Beaumont, Balt]=3 J.P. Ct. [Tucson, Beaumont] = 4 City Ct. [Tucson] = 5 Superior Ct. [Tucson] = 6 Supreme Bench [Baltimore] = 7</div>

INFORMATION	CODES
<div>(200) (201) (202) (203)Date of disposition for second most serious charge</div> <div>(204) (205)Charge for which tried</div> <div>(206)Outcome of trial</div> <div>(207) (208)Type of sentence</div> <div>(209) (210) (211)Length of Unsuspended Incarceration</div> <div>(212) (213) (214)Length of Unsuspended Probation</div> <div>(215) (216)What is the third most serious charge?</div> <div>(217) (218) (219) (220)Date of first scheduled appearance on this charge</div> <div>(221)Number of real scheduled appearances for this charge</div> <div>(222)BLANK</div> <div>(223)Number of all postponements for this charge</div> <div>(224)In which court was defendant tried? (If more than one court, record court in which disposition occurred)</div>	<div>Use Date Coding Sheet</div> <div>Use Crime Codes Sheet (record change, reduction, etc., from original charge)</div> <div>Pled no contest = 1; Pled Guilty = 2; Dismissed or not Prosecuted = 3; Acquitted, Judge = 4; Acquitted, Jury= 5; Found Guilty, Judge = 6; Found Guilty, Jury = 7; Not convicted (other than 3, 4 or 5) = 8; Bail forfeited in lieu of appearance = 9</div> <div>Incarceration = 01 02, 04, 05 = 14 Probation = 02 01, 02, 04, 05 = 15 Suspended Sent. = 03 01, 02, 04 = 16 Fine = 04 "Volunteer" Services = 17 Treatment = 05 01, 02, 05 = 18 02 and 03 = 06 01 and 02 = 19 02, 03, 04 = 07 04 and 05 = 20 01, 04 = 08 01 and 05 = 21 02, 03, 05 = 09 01, 02, 03, 04 = 22 02 and 04 = 10 02, 03, 04, 05 = 23 03 and 04 = 11 01, 02, 03 = 24 05 and 07 = 12 01, 02, 03, 05 = 25 02 and 05 = 13 Time Served = 26</div> <div>Record number of <u>days</u></div> <div>Record number of <u>months</u></div> <div>Use Crime Codes Sheet</div> <div>Use Date Coding Sheet</div> <div>Record number. 9 or more = 9</div> <div>Record number 9 or more = 9</div> <div>Municipal Ct. [Lincoln] = 1 County Ct. [Lincoln, Beaumont] = 2 District Ct. [Lincoln, Beaumont, Balt]=3 J.P. Ct. [Tucson, Beaumont] = 4 City Ct. [Tucson] = 5 Superior Ct. [Tucson] = 6 Supreme Bench [Baltimore] = 7</div>

INFORMATION	CODES
<p>Date of disposition for (225) (226) (227) (228) third most serious charge</p> <p>Charge for which tried (229) (230)</p> <p>Outcome of trial (231)</p> <p>Type of Sentence (232) (233)</p> <p>Length of Unsuspended (234) (235) (236) Incarceration</p> <p>Length of Unsuspended (237) (238) (239) Probation</p> <p>What was the total amount of Unsuspended (240) Fines (excluding court costs) for ALL charges (1st, 2nd and 3rd most serious charges) RECORD EXACT AMOUNT ON NEW CODES SHEET</p> <p>Identification: Code Sheet No. (241)</p> <p>Site I.D. No. (242) (243)</p> <p>Defendant I.D. No. (244) (245) (246) (247)</p> <p>BLANK (248)</p>	<p>Use Date Coding Sheet</p> <p>Use Crime Code Sheets (record changes, reduction, etc. from original charge)</p> <p>Pled no contest = 1; Pled Guilty = 2; Dismissed or not prosecuted = 3; Acquitted, Judge = 4; Acquitted, Jury = 5; Found Guilty, Judge = 6; Found Guilty, Jury = 7; Not Convicted (other than 3, 4 or 5) = 8; Bail Forfeited in Lieu of Appearance = 9</p> <p>Incarceration = 01 02, 04, 05 = 14 Probation = 02 01, 02, 04, 05 = 15 Suspended Sent. = 03 01, 02, 04 = 16 Fine = 04 "Volunteer" Services = 17 Treatment = 05 01, 02, 05 = 18 02 and 03 = 06 01 and 02 = 19 02, 03, 04 = 07 04 and 05 = 20 01, 04 = 08 01 and 05 = 21 02, 03, 05 = 09 01, 02, 03, 04 = 22 02 and 04 = 10 02, 03, 04, 05 = 23 03 and 04 = 11 01, 02, 03 = 24 05 and 07 = 12 01, 02, 03, 05 = 25 02 and 05 = 13 Time Served = 26</p> <p>Record number of <u>days</u>.</p> <p>Record number of <u>months</u>.</p> <p>Below \$250 = 1 \$251-\$500 = 2 \$501-\$1,000 = 3 \$1,001-\$1,500 = 4 \$1,501-\$2,000 = 5 \$2,001-\$2,999 = 6 \$3,000-\$4,999 = 7 \$5,000-\$9,999 = 8 \$10,000 or more = 9</p>

INFORMATION	CODES
<p>J. FAILURE TO APPEAR</p> <p>Date of first failure (249) (250) (251) (252) to appear for the most serious charge</p> <p>Date of next actual (253) (254) (255) (256) court appearance</p> <p>How did (D) get to court after this FTA? (257)</p> <p>(258) } (259) } Actions taken by court in response (260) } to the FTA</p> <p>If any of these actions (261) (262) (263) resulted in further detention of the defendant, what was the length of detention?</p> <p>If bond forfeiture was ordered, was it (264) actually executed?</p> <p>If "Yes", for what amount? (Record (265) exact amount on New Codes Sheet)</p> <p>If bench warrant was issued, was it (266) actually served?</p> <p>If (D) was rearrested for the FTA, was (267) she/he interviewed again by the pretrial release program?</p> <p>If "conditions added" involved the pretrial (268) release program, what were they? (Record additional responses, modifications or combinations on New Codes Sheet)</p> <p>Reason for FTA (269)</p>	<p>Use Date Coding Sheet</p> <p>Use Date Coding Sheet</p> <p>Own volition, contacted court within 30 days = 1; Own volition, other = 2; Arrested and in jail = 3; Located by Program = 4; By bondsman = 5; (D) Still at large = 6; (D) Forfeited bail in Lieu of Appearance = 7; Tried in Absence = 8; Located by attorney = 9</p> <p>None = 1; Release revoked = 2; Bench Warrant = 3; Bond forfeited = 4; FTA prosecuted = 5; Mailed warning = 6; Conditions added = 7; Set Bail = 8; Bench warrant subsequently recalled = 9</p> <p>Record the number of <u>days</u></p> <p>Yes = 1; No = 2; Don't know = 3</p> <p>Below \$250 = 1 \$251-\$500 = 2 \$501-\$1,000 = 3 \$1,001-\$1,500 = 4 \$1,501-\$2,000 = 5 \$2,001-\$2,999 = 6 \$3,000-\$4,999 = 7 \$5,000-\$9,999 = 8 \$10,000 or more = 9</p> <p>Yes = 1; No = 2; No, but service attempted = 3; Don't know = 4</p> <p>Yes = 1; No = 2; Don't know = 3</p> <p>Required to report more often to program = 1; Required to report in person to program = 2; Placed on program's supervised release [Tucson]=3</p> <p>Scheduled for conflicting appearance = 1; In jail = 2; Ill = 3; Unknown = 4; Court error = 5</p>

INFORMATION	CODES
(270) (271) (272) (273) Date of second failure to appear for the most serious charge.	Use Date Coding Sheet
(274) (275) (276) (277) Date of next actual court appearance.	Use Date Coding Sheet
(278) How did (D) get to court after this FTA?	Own volition, contacted court within 30 days = 1; Own volition, other = 2; Arrested and in jail = 3; Located by Program = 4; By bondsman = 5; (D) Still at large = 6; (D) Forfeited bail in Lieu of Appearance = 7; Tried in Absence = 8; Located by attorney = 9
(279) } (280) } Actions taken by court in (281) } response to the FTA	None = 1; Release revoked = 2; Bench Warrant = 3; Bond forfeited = 4; FTA prosecuted = 5; Mailed warning = 6; Conditions added = 7; Set Bail = 8; Bench warrant subsequently recalled = 9
(282) (283) (284) If any of these actions resulted in further detention of the defendant, what was the length of detention?	Record the number of <u>days</u>
(285) If bond forfeiture was ordered, was it actually executed?	Yes = 1; No = 2; Don't know = 3
(286) If "yes", for what amount? (Record exact amount on New Codes Sheet)	Below \$250 = 1 \$251-\$500 = 2 \$501-\$1,000 = 3 \$1,001-\$1,500 = 4 \$1,501-\$2,000 = 5 \$2,001-\$2,999 = 6 \$3,000-\$4,999 = 7 \$5,000-\$9,999 = 8 \$10,000 or more = 9
(287) If bench warrant was issued, was it actually served?	Yes = 1; No = 2; No, but service attempted = 3; Don't know = 4
(288) If (D) was rearrested for the FTA, was she/he interviewed again by the pretrial release program?	Yes = 1; No = 2; Don't know = 3
(289) If "conditions added" involved the pretrial release program, what were they? Record additional responses, modifications and conditions on New Codes Sheet)	Required to report more often to program = 1; Required to report in person to program = 2; Placed on program's supervised release [Tucson]=3
(290) Reason for FTA	Scheduled for conflicting appearance = 1; In jail = 2; Ill = 3; Unknown = 4; Court error = 5

INFORMATION	CODES
(291) (292) (293) (294) Date of third failure to appear for the most serious charge	Use Date Coding Sheet
(295) (296) (297) (298) Date of next actual court appearance	Use Date Coding Sheet
(299) How did (D) get to court after this FTA?	Own volition, contacted court within 30 days = 1; Own volition, other = 2; Arrested and in jail = 3; Located by Program = 4; By bondsman = 5; (D) Still at large = 6; (D) Forfeited bail in Lieu of Appearance = 7; Tried in Absence = 8; Located by attorney = 9
(300) } (301) } Actions taken by court in (302) } response to the FTA	None = 1; Release revoked = 2; Bench Warrant = 3; Bond forfeited = 4; FTA prosecuted = 5; Mailed warning = 6; Conditions added = 7; Set Bail = 8; Bench warrant subsequently recalled = 9
(303) (304) (305) If any of these actions resulted in further detention of (D), what was the length of detention?	Record the number of <u>days</u>
(306) If bond forfeiture was ordered, was it actually executed?	Yes = 1; No = 2; Don't know = 3
(307) If "yes", for what amount? (Record <u>exact</u> amount on New Codes Sheet)	Below \$250 = 1 \$251-\$500 = 2 \$501-\$1,000 = 3 \$1,001-\$1,500 = 4 \$1,501-\$2,000 = 5 \$2,001-\$2,999 = 6 \$3,000-\$4,999 = 7 \$5,000-\$9,999 = 8 \$10,000 or more = 9
(308) If bench warrant was issued, was it actually served?	Yes = 1; No = 2; No, but service attempted = 3; Don't know = 4
(309) If defendant was rearrested for the FTA, was she/he interviewed again by the pretrial release program?	Yes = 1; No = 2; Don't know = 3
(310) If "conditions added" involved the pretrial release program, what were they? Record additional responses, modifications and conditions on New Codes Sheet)	Required to report more often to program = 1; Required to report in person to program = 2; Placed on program's supervised release [Tucson]=3
(311) Reason for FTA	Scheduled for conflicting appearance = 1; In jail = 2; Ill = 3; Unknown = 4; Court error = 5

INFORMATION

CODES

K. PRETRIAL CRIMINALITY

Date of first new arrest

(312) (313) (314) (315)

Use Date Coding Sheet

Most serious charge for which re-
(316) (317) arrested

Use Crime Codes Sheet

Total number of charges for the arrest
(318)In which court was defendant tried?
(319) (If more than one court, record court
in which disposition occurred)Municipal Ct. [Lincoln] = 1
County Ct. [Lincoln, Beaumont] = 2
District Ct. [Lincoln, Beaumont, Balt]=3
J.P. Ct. [Tucson, Beaumont] = 4
City Ct. [Tucson] = 5
Superior Ct. [Tucson] = 6
Supreme Bench [Baltimore] = 7How did release conditions change as a
(320) result of this arrest?Detained = 1; Bond forfeited = 2;
Conditions added = 3; Supervision
increased = 4; Bond Increased = 5;
ROR recinded and bail set = 6; Treatment
ordered = 7; No change = 8; Unsecured
Bond status recinded and bail set = 9

5 Coding Sheet No.

(321)

Site I.D. No.

(322) (323)

Defendant I.D. No.

(324) (325) (326) (327)

If any of these actions resulted
(328) (329) (330) in further detention of (D),
what was the length of detention?Record number of daysIf bond forfeiture was ordered, was it
(331) actually executed?

Yes = 1; No = 2; Don't Know = 3

If "Yes", for what amount? (Record
(332) exact amount on New Codes Sheet)Below \$250 = 1 \$251-\$500 = 2
\$501-\$1,000 = 3 \$1,001-\$1,500 = 4
\$1,501-\$2,000 = 5 \$2,001-\$2,999 = 6
\$3,000-\$4,999 = 7 \$5,000-\$9,000 = 8
\$10,000 or more = 9Was defendant reinterviewed by the program
(333) as a result of this arrest?

Yes = 1; No = 2

If "conditions added" or "supervision
(334) increased" involved the pretrial release
program, what did it include? (Record other
responses, modifications or changes on New
Codes Sheet)Required (D) to report more often to
program = 1; Required (D) to report in
person to program = 2; Placed (D) on
program's supervised release [Tucson]=3

INFORMATION

CODES

Date of disposition for
(335) (336) (337) (338) this arrestOutcome of trial
(339)

Use Date Coding Sheet

Pled no contest = 1; Pled Guilty = 2;
Dismissed or not prosecuted = 3;
Acquitted, Judge = 4; Acquitted, Jury =
5; Found Guilty, Judge = 6; Found Guilty,
Jury = 7; Not Convicted (other than 3, 4
or 5) = 8; Bail Forfeited in Lieu of
Appearance = 9If judge or jury trial, what was
(340) (341) the number of trial days (include
jury examination days). Record
number of trial days even if that
trial subsequently ended in dismissal
or plea. (Note whether it was judge
or jury trial on New Codes Sheet)Most serious charge for which tried
(342) (343) (Record charge, reduction, etc.,
from charge at arrest)

Use Crime Codes Sheet

Type of Sentence
(344) (345)Incarceration = 01 01, 02, 04 = 16
Probation = 02 "Volunteer" Services = 17
Suspended Sent. = 03 01, 02, 05 = 18
Fine = 04 01 and 02 = 19
Treatment = 05 04 and 05 = 20
02 and 03 = 06 01 and 05 = 21
02, 03, 04 = 07 01, 02, 03, 04 = 22
01 and 04 = 08 02, 03, 04, 05 = 23
02, 03, 05 = 09 01, 02, 03 = 24
02 and 04 = 10 01, 02, 03, 05 = 25
03 and 04 = 11 Time Served = 26
05 and 07 = 12 Choice of fine or equivalent
02 and 05 = 13 incarceration = 27
02, 04, 05 = 14 04 and 26 = 28
01, 02, 04, 05 = 15 02, 04, 26 = 29
01, 02, 04, 26 = 30 01, 02, 26 = 31Total length of unsuspended
(346) (347) (348) incarceration for all charges
stemming from this arrestRecord number of daysTotal length of unsuspended
(349) (350) (351) probation for all charges
stemming from this arrest.Record number of monthsTotal amount of unsuspended fines for all
(352) charges stemming from this arrest (Record
exact amount on New Codes Sheet)Below \$250 = 1 \$251-\$500 = 2
\$501-\$1,000 = 3 \$1,001-\$1,500 = 4
\$1,501-\$2,000 = 5 \$2,001-\$2,999 = 6
\$3,000-\$4,999 = 7 \$5,000-\$9,000 = 8
\$10,000 or more = 9

INFORMATION	CODES
If there is an indication that (D) paid (353) fine, for what amount? (Record exact amount on New Codes Sheet)	Use codes as in (352)
(354) (355) (356) (357) Date of 2nd new arrest	Use Date Coding Sheet
(358) (359) Most serious charge for which re-arrested	Use Crime Codes
(360) Total number of charges for the arrest	
(361) What court was (D) tried in? (If more than one court, record court in which disposition occurred)	Municipal Ct. [Lincoln] = 1 County Ct. [Lincoln, Beaumont] = 2 District Ct. [Lincoln, Beaumont, Balt]=3 J.P. Ct. [Tucson, Beaumont] = 4 City Ct. [Tucson] = 5 Superior Ct. [Tucson] = 6 Supreme Bench [Baltimore] = 7
(362) How did release conditions change as a result of this arrest	Detained = 1; Bond forfeited = 2; Conditions added = 3; Supervision increased = 4; Bond Increased = 5; ROR recinded and bail set = 6; Treatment ordered = 7; No change = 8; Unsecured Bond status recinded and bail set = 9
(363) (364) (365) If any of these actions resulted in further detention of (D), what was the length of incarceration?	Record number of <u>days</u>
(366) If bond forfeiture was ordered, was it actually executed?	Yes = 1; No = 2; Don't Know = 3
(367) If "yes", for what amount? (Record exact amount on New Codes Sheet)	Below \$250 = 1 \$251-\$500 = 2 \$501-\$1,000 = 3 \$1,001-\$1,500 = 4 \$1,501-\$2,000 = 5 \$2,001-\$2,999 = 6 \$3,000-\$4,999 = 7 \$5,000-\$9,000 = 8 \$10,000 or more = 9
(368) Was (D) reinterviewed by the program as a result of this arrest?	Yes = 1; No = 2
(369) If "conditions added" or "supervision increased" involved the pretrial release program, what did it involve? (Record other responses, changes or modifications on New Codes Sheet)	Required (D) to report more often to program = 1; Required (D) to report in person to program = 2; Placed (D) on program's supervised release [Tucson]=3

INFORMATION	CODES
(370) (371) (372) (373) Date of disposition for this arrest	Use Date Coding Sheet
(374) Outcome of trial	Pled no contest = 1; Pled Guilty = 2; Dismissed or not prosecuted = 3; Acquitted, Judge = 4; Acquitted, Jury = 5; Found Guilty, Judge = 6; Found Guilty, Jury = 7; Not Convicted (other than 3, 4 or 5) = 8; Bail Forfeited in Lieu of Appearance = 9
(375) (376) If judge or jury trial, what was the number of trial days (include jury examination days). Record number of trial days even if trial subsequently ended in dismissal or plea. (Note whether it was judge or jury trial on New Codes Sheet)	
(377) (378) Most serous charge for which tried (Record charge, reduction, etc. from charge at arrest)	Use Crime Codes Sheet
(379) (380) Type of Sentence	Incarceration = 01 01, 02, 04 = 16 Probation = 02 "Volunteer" Services = 17 Suspended Sent. = 03 01, 02, 05 = 18 Fine = 04 01 and 02 = 19 Treatment = 05 04 and 05 = 20 02 and 03 = 06 01 and 05 = 21 02, 03, 04 = 07 01, 02, 03, 04 = 22 01 and 04 = 08 02, 03, 04, 05 = 23 02, 03, 05 = 09 01, 02, 03 = 24 02 and 04 = 10 01, 02, 03, 05 = 25 03 and 04 = 11 Time Served = 26 05 and 07 = 12 Choice of fine or equivalent incarceration = 27 02 and 05 = 13 04 and 26 = 28 02, 04, 05 = 14 01, 02, 04, 05 = 15 02, 04, 26 = 29 01, 02, 04, 26 = 30 01, 02, 26 = 31
(381) (382) (383) Total length of unsuspended incarceration for all charges stemming from this arrest.	Record number of <u>days</u>
(384) (385) (386) Total length of unsuspended probation for all charges stemming from this arrest.	Record number of <u>months</u>
(387) Total amount of unsuspended fines for all charges stemming from this arrest. (Record exact amount on New Codes Sheet)	Below \$250 = 1 \$251-\$500 = 2 \$501-\$1,000 = 3 \$1,001-\$1,500 = 4 \$1,501-\$2,000 = 5 \$2,001-\$2,999 = 6 \$3,000-\$4,999 = 7 \$5,000-\$9,000 = 8 \$10,000 or more = 9

INFORMATION	CODES
If there is an indication that (D) paid (388) fine, for what amount? (Record exact amount on New Code Sheet)	Use codes as (387)
Date of 3rd new arrest (389) (390) (391) (392)	Use Date Coding Sheet
Most serious charge for which re- (393) (394) arrested	Use Crime Codes
Total number of charges for the arrest (395)	
In which court was defendant tried? (396) (If more than one court, record court in which disposition occurred)	Municipal Ct. [Lincoln] = 1 County Ct. [Lincoln, Beaumont] = 2 District Ct. [Lincoln, Beaumont, Balt]=3 J.P. Ct. [Tucson, Beaumont] = 4 City Ct. [Tucson] = 5 Superior Ct. [Tucson] = 6 Supreme Bench [Baltimore] = 7
How did release conditions change as a (397) result of this arrest?	Detained = 1; Bond forfeited = 2; Conditions added = 3; Supervision increased = 4; Bond Increased = 5; ROR recinded and bail set = 6; Treatment ordered = 7; No change = 8; Unsecured Bond status recinded and bail set = 9
If any of these actions resulted (398) (399) (400) in further detention of (D), what was the length of detention?	Record number of <u>days</u>
6 Code Sheet Number. (1)	
Site I.D. No. (2) (3)	
Defendant I.D. No. (4) (5) (6) (7)	
If bond forfeiture was ordered, was it (8) actually executed?	Yes = 1; No = 2; Don't Know = 3
If "yes", for what amount? (Record (9) exact amount on New Codes Sheet)	Below \$250 = 1 \$251-\$500 = 2 \$501-\$1,000 = 3 \$1,001-\$1,500 = 4 \$1,501-\$2,000 = 5 \$2,001-\$2,999 = 6 \$3,000-\$4,999 = 7 \$5,000-\$9,000 = 8 \$10,000 or more = 9
Was (D) reinterviewed by the program as (10) a result of this arrest?	Yes = 1; No = 2;

INFORMATION	CODES
If "conditions added" or supervision (11) increased" involved the pretrial release program, what did it involve? (Record other responses, changes or modifications on New Codes Sheet)	Required (D) to report more often to program = 1; Required (D) to report in person to program = 2; Placed (D) on program's supervised release [Tucson]=3
Date of disposition for (12) (13) (14) (15) this arrest.	Use Date Coding Sheet
Outcome of trial (16)	Use Crime Codes Sheet (Record change, reduction, etc. from original charge) Pled no contest = 1; Pled Guilty = 2; Dismissed or not prosecuted = 3; Acquitted, Judge = 4; Acquitted, Jury = 5; Found Guilty, Judge = 6; Found Guilty, Jury = 7; Not Convicted (other than 3, 4 or 5) = 8; Bail Forfeited in Lieu of Appearance = 9
If judge or jury trial, what was the (17) (18) number of trial days (include jury examination days). Record number of trial days even if trial subsequently ended in dismissal or plea. (Note whether it was judge or jury trial on New Codes Sheet)	
Most serious charge for which tried (19) (20) (Record change, reduction, etc., from charge at arrest)	Use Crime Codes
Type of Sentence (21) (22)	Incarceration = 01 01, 02, 04 = 16 Probation = 02 "Volunteer" Services = 17 Suspended Sent. = 03 01, 02, 05 = 18 Fine = 04 01 and 02 = 19 Treatment = 05 04 and 05 = 20 02 and 03 = 06 01 and 05 = 21 02, 03, 04 = 07 01, 02, 03, 04 = 22 01 and 04 = 08 02, 03, 04, 05 = 23 02, 03, 05 = 09 01, 02, 03 = 24 02 and 04 = 10 01, 02, 03, 05 = 25 03 and 04 = 11 Time Served = 26 05 and 07 = 12 Choice of fine or equivalent 02 and 05 = 13 incarceration = 27 02, 04, 05 = 14 04 and 26 = 28 01, 02, 04, 05 = 15 02, 04, 26 = 29 01, 02, 04, 26 = 30 01, 02, 26 = 31
Total length of unsuspended (23) (24) (25) incarceration for all charges stemming from this arrest.	Record number of <u>days</u>
Total length of unsuspended (26) (27) (28) probation for all charges stemming from this arrest.	Record number of <u>months</u>

INFORMATION	CODES
(29) Total amount of unsuspended fines for all charges stemming from this arrest. (Record exact amount on New Codes Sheet)	Below \$250 = 1 \$251-\$500 = 2 \$501-\$1,000 = 3 \$1,001-\$1,500 = 4 \$1,501-\$2,000 = 5 \$2,001-\$2,999 = 6 \$3,000-\$4,999 = 7 \$5,000-\$9,000 = 8 \$10,000 or more = 9
(30) If there is an indication that (D) paid fine, for what amount? (Record exact amount on New Code Sheet)	Use codes as above (29)
(31) (32) (33) (34) Date of 4th New Arrest	Use Date Coding Sheet
(35) (36) Most serious charge for which re-arrested.	Use Crime Codes
(37) Total number of charges for the arrest	
(38) In which court was (D) tried? (If more than one court, record court in which disposition occurred)	Municipal Ct. [Lincoln] = 1 County Ct. [Lincoln, Beaumont] = 2 District Ct. [Lincoln, Beaumont, Balt] = 3 J.P. Ct. [Tucson, Beaumont] = 4 City Ct. [Tucson] = 5 Superior Ct. [Tucson] = 6 Supreme Bench [Baltimore] = 7
(39) How did release conditions change as a result of this arrest?	Detained = 1; Bond forfeited = 2; Conditions added = 3; Supervision increased = 4; Bond Increased = 5; ROR recinded and bail set = 6; Treatment ordered = 7; No change = 8; Unsecured Bond status recinded and bail set = 9
(40) (41) (42) If any of these actions resulted in further detention of (D), what was the length of detention?	Record number of <u>days</u>
(43) If bond forfeiture was ordered, was it actually executed?	Yes = 1; No = 2; Don't Know = 3
(44) If "Yes", for what amount? (Record exact amount on New Codes Sheet)	Below \$250 = 1 \$251-\$500 = 2 \$501-\$1,000 = 3 \$1,001-\$1,500 = 4 \$1,501-\$2,000 = 5 \$2,001-\$2,999 = 6 \$3,000-\$4,999 = 7 \$5,000-\$9,000 = 8 \$10,000 or more = 9
(45) Was (D) reinterviewed by the program as a result of this arrest?	Yes = 1; No = 2
(46) If "conditions added" or "supervision increased" involved the pretrial release program, what did it involve? (Record other responses, changes or modifications on New Codes Sheet)	Required (D) to report more often to program = 1; Required (D) to report in person to program = 2; Placed (D) on program's supervised release [Tucson] = 3

INFORMATION	CODES
(47) (48) (49) (50) Date of disposition for this arrest.	Use Date Coding Sheet
(51) Outcome of trial	Pled no contest = 1; Pled Guilty = 2; Dismissed or not prosecuted = 3; Acquitted, Judge = 4; Acquitted, Jury = 5; Found Guilty, Judge = 6; Found Guilty, Jury = 7; Not Convicted (other than 3, 4 or 5) = 8; Bail Forfeited in Lieu of Appearance = 9
(52) (53) If judge or jury trial, what was the number of trial days (include jury examination days)? Record number of days even if trial subsequently ended in dismissal or plea. (Note whether it was judge or jury trial on New Codes Sheet)	
(54) (55) Most serious charge for which tried (Record charge, reduction, etc., from charge at arrest)	Use Crime Codes
(56) (57) Type of Sentence	Incarceration = 01 01, 02, 04 = 16 Probation = 02 "Volunteer" Services = 17 Suspended Sent. = 03 01, 02, 05 = 18 Fine = 04 01 and 02 = 19 Treatment = 05 04 and 05 = 20 02 and 03 = 06 01 and 05 = 21 02, 03, 04 = 07 01, 02, 03, 04 = 22 01 and 04 = 08 02, 03, 04, 05 = 23 02, 03, 05 = 09 01, 02, 03 = 24 02 and 04 = 10 01, 02, 03, 05 = 25 03 and 04 = 11 Time Served = 26 05 and 07 = 12 Choice of fine or equivalent 02 and 05 = 13 incarceration = 27 02, 04, 05 = 14 04 and 26 = 28 01, 02, 04, 05 = 15 02, 04, 26 = 29 01, 02, 04, 26 = 30 01, 02, 26 = 31
(58) (59) (60) Total length of unsuspended incarceration for all charges stemming from this arrest.	Record number of <u>days</u>
(61) (62) (63) Total length of unsuspended probation for all charges stemming from this arrest.	Record number of <u>months</u>
(64) Total amount of unsuspended fines for all charges stemming from this arrest. (Record exact amount on New Codes Sheet)	Below \$250 = 1 \$251-\$500 = 2 \$501-\$1,000 = 3 \$1,001-\$1,500 = 4 \$1,501-\$2,000 = 5 \$2,001-\$2,999 = 6 \$3,000-\$4,999 = 7 \$5,000-\$9,000 = 8 \$10,000 or more = 9

INFORMATION

CODES

If there is indication that (D) paid
(65) fine, for what amount? (Record exact
amount on New Code Sheet)

Use Codes as in (64)

L. POINT SCALES [Baltimore, Lincoln]

RECORD POINTS AWARDED OR SUBTRACTED
ON INTERVIEW FORMS

(space 35 on Lincoln interview)

(space 32 on Lincoln interview)

(space 40 on Lincoln interview)

(space 47 on Lincoln interview)

(space 52 on Lincoln interview)

(space 52 on Lincoln interview)

(space 51 on Lincoln interview)

(space 51 on Lincoln interview)

Residence

(66)

Time in Area

(67)

Family Ties

(68)

Employment/Substitutes

(69)

Miscellaneous [Baltimore]

(70) Discretionary [positive points-Lincoln]

Drug-Alcohol [Baltimore]

(71) Discretionary [negative points-Lincoln]

FTA/Escapes/Parole/Probation Conviction
(72) [Baltimore ONLY—Lincoln BLANK]

Prior Record—Negative Points

(73)

Prior Record—Positive Points

(74) [Lincoln ONLY—Baltimore BLANK]

DATE CODES SHEET

Instructions: Four digits will be used to code dates. The first digit will refer to the calendar year (e.g., 1977=7, 1976=6, etc.). The last three digits will show the month and day, as coded from the following table (e.g., March 14 = 074, November 1 = 306). Examples of date codes are:

March 14, 1977 7074
June 10, 1974 4162
November 1, 1976 6306
January 3, 1977 7003

Mo/Day	CD*	Mo/Day	CD	Mo/Day	CD	Mo/Day	CD	Mo/Day	CD	Mo/Day	CD
Jan. 1	001	Feb. 1	032	Mar. 1	061	Apr. 1	092	May 1	122	Jun. 1	153
2	002	2	033	2	062	2	093	2	123	2	154
3	003	3	034	3	063	3	094	3	124	3	155
4	004	4	035	4	064	4	095	4	125	4	156
5	005	5	036	5	065	5	096	5	126	5	157
6	006	6	037	6	066	6	097	6	127	6	158
7	007	7	038	7	067	7	098	7	128	7	159
8	008	8	039	8	068	8	099	8	129	8	160
9	009	9	040	9	069	9	100	9	130	9	161
10	010	10	041	10	070	10	101	10	131	10	162
11	011	11	042	11	071	11	102	11	132	11	163
12	012	12	043	12	072	12	103	12	133	12	164
13	013	13	044	13	073	13	104	13	134	13	165
14	014	14	045	14	074	14	105	14	135	14	166
15	015	15	046	15	075	15	106	15	136	15	167
16	016	16	047	16	076	16	107	16	137	16	168
17	017	17	048	17	077	17	108	17	138	17	169
18	018	18	049	18	078	18	109	18	139	18	170
19	019	19	050	19	079	19	110	19	140	19	171
20	020	20	051	20	080	20	111	20	141	20	172
21	021	21	052	21	081	21	112	21	142	21	173
22	022	22	053	22	082	22	113	22	143	22	174
23	023	23	054	23	083	23	114	23	144	23	175
24	024	24	055	24	084	24	115	24	145	24	176
25	025	25	056	25	085	25	116	25	146	25	177
26	026	26	057	26	086	26	117	26	147	26	178
27	027	27	058	27	087	27	118	27	148	27	179
28	028	28	059	28	088	28	119	28	149	28	180
29	029	29	060	29	089	29	120	29	150	29	181
30	030			30	090	30	121	30	151	30	182
31	031			31	091			31	152		

* CD = Code

(continued)

Mo/Day	CD*	Mo/Day	CD	Mo/Day	CD	Mo/Day	CD	Mo/Day	CD	Mo/Day	CD
Jul. 1	183	Aug. 1	214	Sep. 1	245	Oct. 1	275	Nov. 1	306	Dec. 1	336
2	184	2	215	2	246	2	276	2	307	2	337
3	185	3	216	3	247	3	277	3	308	3	338
4	186	4	217	4	248	4	278	4	309	4	339
5	187	5	218	5	249	5	279	5	310	5	340
6	188	6	219	6	250	6	280	6	311	6	341
7	189	7	220	7	251	7	281	7	312	7	342
8	190	8	221	8	252	8	282	8	313	8	343
9	191	9	222	9	253	9	283	9	314	9	344
10	192	10	223	10	254	10	284	10	315	10	345
11	193	11	224	11	255	11	285	11	316	11	346
12	194	12	225	12	256	12	286	12	317	12	347
13	195	13	226	13	257	13	287	13	318	13	348
14	196	14	227	14	258	14	288	14	319	14	349
15	197	15	228	15	259	15	289	15	320	15	350
16	198	16	229	16	260	16	290	16	321	16	351
17	199	17	230	17	261	17	291	17	322	17	352
18	200	18	231	18	262	18	292	18	323	18	353
19	201	19	232	19	263	19	293	19	324	19	354
20	202	20	233	20	264	20	294	20	325	20	355
21	203	21	234	21	265	21	295	21	326	21	356
22	204	22	235	22	266	22	296	22	327	22	357
23	205	23	236	23	267	23	297	23	328	23	358
24	206	24	237	24	268	24	298	24	329	24	359
25	207	25	238	25	269	25	299	25	330	25	360
26	208	26	239	26	270	26	300	26	331	26	361
27	209	27	240	27	271	27	301	27	332	27	362
28	210	28	241	28	272	28	302	28	333	28	363
29	211	29	242	29	273	29	303	29	334	29	364
30	212	30	243	30	274	30	304	30	335	30	365
31	213	31	244			31	305			31	366

*CD = CODE

STUDY TYPE: _____

[illegible]

Name of Data Collector _____

DATA COLLECTION PROCEDURES

A. Occupation (31-32) Section C

Examples:

Professional/Technical—Occupations where advanced school or training is necessary, e.g., lawyer, doctor, engineer, etc.
 Craftsmen—development of specific skills, e.g., electrician, painter, etc.
 Operatives—various machinists, crane operator, etc.
 Transportation Operatives—bus driver, chauffeur, taxi driver, etc.
 Laborer—construction, assembly-line workers, etc.
 Service—skilled and unskilled public-oriented service, e.g., nurses aide, waitress, janitor, parking lot attendant, security guard, etc.

Use new codes sheet if category cannot be determined

B. Crime Codes Sections D, E, I, K

Refer to Crime Code Sheet and Uniform Crime Reporting Sheet.

Some common unlisted examples:

Assault with a Deadly Weapon = Aggravated Assault (05)
 Breaking and Entering = Burglary (06)
 Shoplifting = Larceny/Theft (07)
 Unauthorized Use, Tampering = Auto Theft (08)
 Endangerment, Harrassment = Simple Assault (09)
 Bad/Worthless Check = Fraud (12)
 Criminal or Malicious Mischief, Destruction of Property = Vandalism (15)
 Sodomy, Child Molestation, Indecent Exposure, Peeping Tom = Other Sex Offenses = (18)
 Desertion, Non-Payment of Support = Offenses Against Family and Children = (21)
 Open Container, Drinking in Public, Alcohol Sales Violations = Liquor Laws (23)
 Public Nuisance, Disturbing the Peace = Disorderly Conduct (25)
 Loitering = Vagrancy (26)
 Any drug charges other than Marijuana or Hashish = Narcotics Distribution (19) or Possession (29)
 Possession of Burglary Tools = Apparatus (35)

40 = all other offenses not provided for in previous codes. Includes felonies and misdemeanors (e.g., obstructing justice, resisting arrest, kidnapping, trespassing).

32 = offenses particular to a region.

C. Criminal History Section D

If defendant has no previous criminal record, record zeros under age at first adult arrest as well as for remainder of section. Do not count minor traffic offenses as previous criminal history.

D. Types of Releases Section F

(100) = the person/office that made the last pretrial release decision for initial release from custody.

If defendant was not given release option, the sequence of codes is:
 (100) = 6; (101-108) = Zeros; (109) = 8

If defendant was given a release option, regardless of actuality of release, (e.g., bail granted, but not posted), a code other than "6" should be recorded in (100).

For cases disposed of at initial appearance prior to bail hearing when preset bail was the only option and the defendant did not post it prior to appearance, the sequence of codes is (100) = 8; (101-108) = Zeros; (109) = 1. This applies to all sites with preset bail schedules.

E. Program Intervention Section G

(14) Release compliance refers to conditions regarding court appearance, pretrial criminality, contact with treatment or pretrial release programs.

(144) Some possible responses to non-compliance: mail or telephone notifications, program or court rescinding recognizance, conditions added, new bail set, etc.

(145-7) Detention may result from recognizance revocation, time spent on release reconsideration, inability to post new bond, etc.

F. Weight of Evidence

(148-151) Count only witnesses where there is evidence that they testified or where there is an indication of intention to testify, such as a summons or subpoena.

G. Court Appearances and Dispositions Section I

(158-9) (101-2) 215-6) 1st, 2nd, 3rd Most serious charges
 This refers to charges as stated at arrest which were processed

(168-171) (193-6) (217-22) First Scheduled Appearance
 This refers to the first appearance where something occurred other than pretrial release proceedings, which are usually held automatically soon after arrest. This is usually not the initial appearance unless the case is dismissed, bound over to another court or there is an immediate plea.

(172) (197) (221) Real Appearance

A Real Appearance is one where:

- (1) The defendant was required to appear
- (2) The defendant did appear
- (3) Substantial proceedings took place (no postponement)

(174) (198) (223) Postponement

Includes pre-appearance and at-appearance postponements. An at-appearance postponement is one where no substantial proceedings took place aside from postponement.

(176-9) (200-3) (225-8) Date of Disposition

This refers to the date of outcome and not the date of sentencing, which may occur some time after. The pretrial period ends on this date. Consequently, arrests and failures to appear occurring in the period of post-disposition and pre-sentence are not included.

(180-1) (204-5) (229-30) Charge for Which Tried

Charges at arrest will not necessarily be the same as the charges for which a defendant was tried. Often concurrent cases are merged, charges are dropped, added or changed. If space isn't available, a single count for a given charge may represent several counts of the same charge.

"Charges for which tried" reflect a combination of the most serious charges of the case, the range of charges or the charges for which the defendant was ultimately convicted and sentenced.

Attempt, when possible, to make charges listed under (176-9) (200-3) (225-8) correspond to charges under (180-1) (201-5) (229-30).

When charges were changed from arrest to disposition, and there were codes for these new charges, then the codes were used. If the charge remained the same but was reduced from a felony to a misdemeanor or in degree, then 42 was used for charge for which tried.

EXAMPLE: Robbery Theft

$$\begin{array}{r} 0 \quad 4 \\ (158) \quad (159) \end{array} = \begin{array}{r} 0 \quad 7 \\ (180) \quad (181) \end{array}$$

Grand Theft Petty Theft

$$\begin{array}{r} 0 \quad 7 \\ (158) \quad (159) \end{array} = \begin{array}{r} 4 \quad 2 \\ (180) \quad (181) \end{array}$$

In the case of DWI, defendant often pleads guilty to a lesser traffic charge, such as reckless driving. This would be coded as $\frac{2}{(158)} \frac{2}{(159)} =$

$\frac{4}{(180)} \frac{2}{(181)}$

(182) (206) (231) Outcome of Trial

8 = Not Convicted

This refers to STET (dismissal subject to reopening), probation before verdict or jury (PBV, PBJ), or Diversion, where upon completion of or promise to attend diversion treatment program, charges are not prosecuted.

(183-4) (207-8) (232-3) Type of Sentence

Whatever part of the sentence is suspended, should not be coded as the sentence itself. For example, if sentence is 30 days jail suspended and one year probation, this should be coded as "suspended sentence" and "probation", not as "incarceration" and "probation".

(185-190) (209-214) (234-39) Length of Sentences

Record only amounts of unsuspended time. For cases with convictions of several charges for which concurrent sentences are given, divide the total length of the sentence among the charges.

II. Failure to Appear (FTA) Section J.

If defendant notified the court prior to scheduled appearance that he would be absent, but the judge was not notified and issued a bench warrant, this is not counted as an FTA.

If defendant contacted the court on the same day that the FTA occurred and a bench warrant was issued but then quashed or recalled, check these responses under "Actions".

If defendant failed to appear and was required to appear and did not contact the court, it is counted as an FTA and the court's response, which is usually noted on the court jacket, varies in each case.

FTA's are not considered pretrial crimes. If the FTA is prosecuted, this should be noted under "Actions", and the processing of the FTA as a separate charge should be recorded, if space is available, under "2nd or 3rd most serious charge".

(261-63) (282-84) (303-05) Length of subsequent detention may be determined by checking new bond receipts or other court, program and booking records.

(266) (287) (308) Service of bench warrant—date of service or attempts to serve may be noted by the law enforcement official on the copy of the bench warrant in the court folder.

I. Pretrial Criminality Section K

This refers to rearrests for new offenses when the offense and arrest occur after arrest or release for the study charge and prior to its disposition.

(320) (362) (397) (39) This refers to the changes from the last known arrest and not necessarily from the arrest under study, for those defendants with more than one rearrest.

(328-330) (363-65) (398-400) (40-42) If defendant was not re-released, calculate time between new detention and disposition of charge. (Also see note under FTA)

(353) (388) (30) (65) May be determined by receipts or by court papers indicating actions taken in response to non-payment.

C-34
CRIME CHART SHEET

murder and non-negligent (voluntary) manslaughter = 01
manslaughter by negligence (involuntary) = 02
forcible rape = 03
robbery = 04
aggravated assault = 05
burglary (including breaking and entering) = 06
larceny/theft (except auto) = 07
auto theft = 08
other assaults = 09
arson = 10
forgery/counterfeiting = 11
fraud = 12
embezzlement = 13
stolen property = 14
malicious destruction (vandalism) = 15
weapons = 16
prostitution and commercialized vice = 17
sex offenses (other than 03 and 17) = 18
narcotic drug distribution = 19
gambling = 20
offenses against family and children = 21
driving under the influence of liquor or narcotics = 22
liquor laws = 23
drunkenness = 24
disorderly conduct = 25
vagrancy = 26
suspicion = 27
failure to appear = 28
narcotics or paraphernalia possession = 29
marijuana distribution = 30
marijuana possession = 31
minor local offenses = 32
violation of probation or parole = 33
conspiracy = 34
possession of criminal apparatus excluding drugs = 35
other offenses = 40
reduced charge = 42

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OFFENSES IN UNIFORM CRIME REPORTING

Offenses in Uniform Crime Reporting are divided into two groupings designated as Part I and Part II offenses. The Part I offenses are as follows:

1. CRIMINAL HOMICIDE—(a) Murder and non-negligent manslaughter: All willful felonious homicides are distinguished from deaths caused by negligence. Excludes attempts to kill, assaults to kill, suicides, accidental deaths, or justifiable homicides. Justifiable homicides are limited to: (1) The killing of a person by a law enforcement officer in line of duty; and (2) The killing of a person in the act of committing a felony by a private citizen. (b) Manslaughter by negligence: Any death which the police investigation established was primarily attributable to gross negligence of some individual other than the victim.
2. FORCIBLE RAPE—The carnal knowledge of a female, forcibly and against her will in the categories of rape by force, assault to rape, and attempted rape. Excludes statutory offenses (no force used—victim under age of consent).
3. ROBBERY—Stealing or taking anything of value from the care, custody, or control of a person by force or violence or by putting in fear, such as strong-arm robbery, stickups, armed robbery, assaults to rob, and attempts to rob.
4. AGGRAVATED ASSAULT—Assault with intent to kill or for the purpose of inflicting severe bodily injury by shooting, cutting, stabbing, maiming, poisoning, scalding, or by the use of acids, explosives, or other means. Excludes simple assaults.
5. BURGLARY—BREAKING OR ENTERING—Burglary, housebreaking, safecracking, or any breaking or unlawful entry of a structure with the intent to commit a felony or a theft. Includes attempted forcible entry.
6. LARCENY-THEFT (except auto theft)—The unlawful taking, carrying, leading, or riding away of property from the possession or constructive possession of another. Thefts of bicycles, automobile accessories, shoplifting, pocket-picking, or any stealing of property or article which is not taken by force and violence or by fraud. Excludes embezzlement, "con" games, forgery, worthless checks, etc.
7. AUTO THEFT—Unlawful taking or stealing or attempted theft of a motor vehicle. A motor vehicle is a self-propelled vehicle that travels on the surface but not on rails. Specifically excluded from this category are motor boats, construction equipment, airplanes, and farming equipment.

The Part II offenses are:

8. OTHER ASSAULTS (SIMPLE)—Assaults which are not of an aggravated nature.
9. ARSON—Willful or malicious burning with or without intent to defraud. Includes attempts.
10. FORGERY AND COUNTERFEITING—Making, altering, uttering or possessing with intent to defraud anything false which is made to appear true. Includes attempts.
11. FRAUD—Fraudulent conversion and obtaining money or property by false pretenses. Includes bad checks except forgeries and counterfeiting. Also includes larceny by bailee.
12. EMBEZZLEMENT—Misappropriation or misapplication of money or property entrusted to one's care, custody, or control.
13. STOLEN PROPERTY; BUYING, RECEIVING, POSSESSING—Buying, receiving, and possessing stolen property and attempts.
14. VANDALISM—Willful or malicious destruction, injury, disfigurement, or defacement of property without consent of the owner or person having custody or control.
15. WEAPONS: CARRYING, POSSESSING, ETC.—All violations of regulations or statutes controlling the carrying, using, possessing, furnishing, and manufacturing of deadly weapons or silencers. Includes attempts.
16. PROSTITUTION AND COMMERCIALIZED VICE—Sex offenses of a commercialized nature and attempts, such as prostitution, keeping a bawdy house, procuring or transporting women for immoral purposes.
17. SEX OFFENSES (EXCEPT FORCIBLE RAPE, PROSTITUTION, AND COMMERCIALIZED VICE)—Statutory rape, offenses against chastity, common decency, morals, and the like. Includes attempts.
18. NARCOTIC DRUG LAWS—Offenses relating to narcotic drugs, such as unlawful possession, sale, use, growing, manufacturing, and making of narcotic drugs.
19. GAMBLING—Promoting, permitting, or engaging in gambling.
20. OFFENSES AGAINST THE FAMILY AND CHILDREN—Nonsupport, neglect, desertion, or abuse of family and children.
21. DRIVING UNDER THE INFLUENCE—Driving or operating any motor vehicle or common carrier while drunk or under the influence of liquor or narcotics.
22. LIQUOR LAWS—State or local liquor law violations, except "drunkenness" (class 21). Excludes Federal violations.

23. DRUNKENNESS—Drunkenness or intoxication.
24. DISORDERLY CONDUCT—Breach of the peace.
25. VAGRANCY—Vagabondage, begging, loitering, etc.
26. ALL OTHER OFFENSES—All violations of State or local laws, except classes 1-25 and traffic offenses.
27. SUSPICION—Arrests for no specific offense and released without formal charges being placed.
28. CURFEW AND LOITERING LAWS (JUVENILES)—Offenses relating to violation of local curfew or loitering ordinances where such laws exist.
29. RUNAWAY (JUVENILES)—Limited to juveniles taken into protective custody under provision of local statutes as runaways.

ANALYSIS OF PRETRIAL RELEASE IN THE TUCSON/PIMA COUNTY AREA:

EXPERIMENTAL PROCEDURES

INTRODUCTION

In order to analyze the impact of pretrial release programs, the Law Enforcement Assistance Administration (LEAA) has commissioned a multi-city study of release practices. This study, part of LEAA's National Evaluation Program, is being conducted by The Lazar Institute, a research organization based in Washington, D.C. The study considers several major pretrial release issues, including:

- What impact do pretrial release programs have upon release rates?
- Do programs result in increased "equity" of release (e.g., by leading to the release of more poor defendants than would otherwise occur)?
- What is the extent of criminality among pretrial releasees?
- To what extent do defendants on different types of pretrial release (e.g., own recognizance, money bail, supervised release) return for scheduled court appearances?
- How do the operations of pretrial release programs affect defendant outcomes (e.g., court appearance rates, pretrial criminality rates)?
- What costs and benefits are associated with pretrial release programs?

To address these issues, the study provides for experimental analysis, comparing defendants who are processed by a pretrial release program with an otherwise equivalent group of defendants who are not processed by the program. Analysis of outcomes (e.g., release rates, court appearance performance, pretrial criminality) for the two groups will permit assessment of program impact.

The "ideal" situation for implementing this research approach is in a jurisdiction where the pretrial release program is not able to interview all defendants who might potentially be eligible for the program's services (e.g., the program may lack the resources needed to provide full service to all eligible defendants). The defendants not served comprise an "overflow" group, which exceeds the program's service capacity. Under these conditions, a comparison group of defendants not served by the program can be identified through modifying the process for selecting "program" versus "overflow" defendants. This can be done in a way that

APPENDIX D

EXPERIMENTAL PROCEDURES

- Tucson, Arizona
- Baltimore, Maryland
- Lincoln, Nebraska
- Beaumont-Port Arthur, Texas

This appendix provides the procedures developed for use by program staff in implementing the various experiments.

insures each defendant of an equal opportunity to be in either group, while the program's overall client loads remain at the same level (or even increase).

The Tucson/Pima County area now has an "overflow" situation. For felony defendants, the overflow consists of defendants who are brought to court late; the pretrial release program run by the Correctional Volunteer Center (CVC) does not have sufficient time to provide full services to all of these defendants. At the misdemeanor level, no program services are currently provided to any defendants; thus, the "overflow" group consists of all misdemeanor arrestees.

These overflow situations provide the ideal conditions for conducting experimental analysis of program impact. A cooperative research effort involving Lazar, CVC and various Tucson/Pima County officials is planned for the area. The following sections of this paper describe the specific procedures to be used to implement such analysis at the felony and misdemeanor levels, respectively.

PROCEDURES FOR THE FELONY STUDY

1. All defendants except the "late arrests" group will be processed by CVC in its normal manner.
2. All defendants in the "late arrests" group will be interviewed by CVC.
3. If a defendant in the "late arrests" group was born on one of the days listed in Exhibit A (BIRTHDATES FOR PROGRAM GROUP), that defendant will receive full program services (e.g., verification of information and preparation of a release recommendation, provided to the court). To allow time for the provision of full services to these defendants, the program may sometimes have to ask the court to consider a defendant at a later time in the court session than would normally occur (e.g., after consideration of the out-of-custody cases has begun).
4. If a defendant in the "late arrests" group was NOT born on one of the days listed in Exhibit A (BIRTHDATES FOR PROGRAM GROUP), only the completed interview form will be provided to the court; no verification will be done, and no recommendation will be provided to the court. For these defendants it is important that the court not ask CVC to provide any information beyond the completed interview. Any such requests would result in the overflow group (i.e., the "minimum services" group) receiving more than minimum services; this would tend to diminish the extent of program impact when the overflow group is compared with the program group (i.e., the "full services" group).
5. Enter the names of all defendants interviewed in the "late arrests" group on the MASTER LIST (Exhibit B). Also record the defendant's date of birth and check the appropriate column to indicate whether the defendant is in the Program Group (which receives full program services) or the Overflow Group (which receives minimum services). Finally, indicate the study number for the defendant in the last column of the MASTER LIST. Study numbers should be assigned consecutively as defendants' names are entered on the MASTER LIST (for example, the first defendant will have study number 1, the second defendant will have study number 2, and so on). The MASTER LIST will be used as a control sheet to keep track of all defendants included in the experimental analysis at the felony level; thus, it is particularly important that the MASTER LIST be completely accurate and kept up-to-date.

6. A records check of criminal history, Statewide if possible, should be made for every defendant on the MASTER LIST (that is, for every defendant interviewed as part of the study). If the records check is the same as the information provided by the defendant during the interview, make a notation to that effect on the interview form (e.g., write "same records" above the list of past charges on the interview form). If the records check is different from the information provided by the defendant during the interview, complete a CRIMINAL HISTORY SUMMARY (Exhibit C) for the defendant.

7. Complete the VERIFICATION/RELEASE SUMMARY (Exhibit D):

- (a) Copy the names and study numbers of all defendants on the MASTER LIST onto the VERIFICATION/RELEASE SUMMARY sheet.
- (b) Check the appropriate columns to indicate the type of verification conducted and enter the number of references checked. Note that for defendants in the Overflow Group, the column checked should always be "checked files" (since criminal records should be checked for all defendants), and "None" should be entered for the number of references checked.
- (c) Provide the information indicated concerning the defendant's release. One or more of the following should be written in the "TYPE OF RELEASE" column:

- ROR;
- Third party custody, CVC;
- Third party custody, not CVC;
- Surety Bond;
- Unsecured appearance bond;
- Conditional release; or
- Other.

If the type of release is "Other," please describe it. If the defendant was released on bond, complete the columns showing the bond amount and the estimated percentage paid.

(d) Note that defendants may be released at various times and thus the release information on the VERIFICATION/RELEASE SUMMARY sheet may have to be checked and recorded over a period of several days.

8. Send COPIES of the following materials to Lazar each week:

- MASTER LIST (one list of all defendant interviewed as part of the study during the reporting period)
- INTERVIEW FORMS (one form for each defendant on the MASTER LIST)
- COVER SHEETS SHOWING THE PROGRAM'S RELEASE RECOMMENDATIONS (one sheet for each defendant on the MASTER LIST who received a program recommendation, that is, for each defendant in the "program group")
- CRIMINAL HISTORY SUMMARY (one form for each defendant whose interview information did not correspond with the criminal records check)
- VERIFICATION/RELEASE SUMMARY (send each sheet on which an entry has been made during the reporting period)

9. A target date of early November has been set for the start of the felony study. It is expected to take about four months to develop a group of 400 defendants (200 "program" defendants and 200 "overflow" defendants). At the end of that time, the CVC program would return to its normal operating procedures for all defendants. After sufficient time had elapsed for the study defendants' cases to reach final dispositions, Lazar staff would visit Tucson to collect data on court appearance performance, pretrial arrests and case dispositions for all defendants in the study group. Comparisons of outcomes for "program" versus "overflow" defendants should provide considerable insight concerning CVC's impact on the release process and on defendant outcomes.

CONTINUED

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PROCEDURES FOR THE MISDEMEANOR STUDY

1. The misdemeanor study will be limited to offenses tried in the City Courts of Tucson. Jail records will be reviewed to determine which arrestees should be included in the study. Very minor offenses will be excluded, such as:

- jaywalking;
- leaving the scene;
- reckless driving;
- traffic (except DWI); and
- trespassing.

Bench warrant arrests will also be excluded. Note that DWI is included, although other traffic offenses are excluded. If possible, defendants who are likely to bond out will be included in the group eligible for program interviews.

2. (a) If a defendant is not screened out on the basis of the review of jail records, enter the defendant's name and date of birth on the MASTER LIST (Exhibit E).
- (b) Check the defendant's date of birth against the list entitled BIRTHDATES FOR PROGRAM GROUP (Exhibit A).
- (c) If the defendant's birthdate appears on that list, the defendant will be in the Program Group. If the defendant's birthdate does not appear on that list, the defendant will be in the Overflow Group. Mark the appropriate column on the MASTER LIST with an X.
- (d) Next, record the study number for the defendant. Study numbers should be assigned consecutively as defendants' names are entered on the MASTER LIST (for example, the first defendant will have study number 1, the second defendant will have study number 2, and so on).
3. (a) Interview all defendants listed on the MASTER LIST. Use the interview form shown in Exhibit F, or a similar form.
- (b) If the defendant is in the Program Group, verify the interview information (by checking criminal records and talking to references) and prepare a release recommendation for the court. Use the recommendation form shown in Exhibit G, or a similar form.

- (c) If the defendant is in the Overflow Group, conduct the interview only; do not present either the interview information or a release recommendation to the court.
4. (a) Magistrates making the release decisions will tell all released defendants to report to the CVC staff person (stationed in the courtroom or right outside).
- (b) If the released defendant is included in the study (in either the Program Group or the Overflow Group), the CVC staff person will compare the defendant's date of birth with the list entitled BIRTHDATES FOR NOTIFICATION GROUP (Exhibit H). NOTE THAT THIS LIST OF BIRTHDATES IS A DIFFERENT ONE THAN WAS USED TO DEVELOP THE PROGRAM AND OVERFLOW GROUPS; THIS LIST INCLUDES HALF OF THE PROGRAM GROUP BIRTHDATES AND HALF OF THE OVERFLOW GROUP BIRTHDATES.
- (c) If the defendant's birthdate appears on the list entitled BIRTHDATES FOR NOTIFICATION GROUP, the CVC staff person will explain the program's notification procedures to the defendant.
- (d) If the defendant's birthdate does not appear on the list, the CVC staff person will tell the defendant that he or she is free to go.
5. A records check of criminal history should be made for every defendant on the MASTER LIST (that is, for every defendant interviewed as part of the study). If the records check is the same as the information provided by the defendant during the interview, make a notation to that effect on the interview form (e.g., write "same records" above the list of past charges on the interview form). If the records check is different from the information provided by the defendant during the interview, complete a CRIMINAL HISTORY SUMMARY (Exhibit C) for the defendant.
6. Complete the VERIFICATION/RELEASE SUMMARY (Exhibit D):
- (a) Copy the names and study numbers of all defendants on the MASTER LIST onto the VERIFICATION/RELEASE SUMMARY sheet.
- (b) Check the appropriate columns to indicate the type of verification conducted and enter the number of references checked. Note that for defendants in the Overflow Group, the column checked should always be "checked files" (since criminal records should be checked for all defendants), and "None" should be entered for the number of references checked.

- (c) Provide the information indicated concerning the defendant's release. One or more of the following should be written in the "TYPE OF RELEASE" column:

- ROR
- Third party custody;
- Surety bond;
- Unsecured appearance bond;
- Conditional release; or
- Other.

If the type of release is "Other," please describe it.
If the defendant was released on bond, complete the columns showing the bond amount and the estimated percentage paid.

- (d) Note that defendants may be released at various times and thus the release information on the VERIFICATION/RELEASE SUMMARY sheet may have to be checked and recorded over a period of several days.

7. For the defendants in the Notification Group, maintain records of the notification done. Complete the NOTIFICATION SUMMARY (Exhibit I) for each defendant.

8. Send COPIES of the following materials to Lazar each week:

- MASTER LIST (one list of all defendants interviewed as part of the study during the reporting period)
- INTERVIEW FORMS (one form for each defendant on the MASTER LIST)
- COVER SHEETS SHOWING THE PROGRAM'S RELEASE RECOMMENDATIONS (one sheet for each defendant on the MASTER LIST who received a program recommendation, that is, for each defendant in the Program Group)
- CRIMINAL HISTORY SUMMARY (one form for each defendant whose interview information did not correspond with the criminal records check)
- VERIFICATION/RELEASE SUMMARY (send each sheet on which an entry has been made during the reporting period)
- NOTIFICATION SUMMARY (send one sheet for each defendant whose case reached final disposition during the reporting period)

9. It is expected to take about eight weeks to develop a group of 300-400 defendants who are released awaiting final disposition of their cases. At the end of that time, only the notification activities would continue as part of this study; these activities are expected to continue for an additional eight weeks, at which time all cases in the study should have reached final disposition. At that time, Lazar staff would visit Tucson to collect data on pretrial arrests and case dispositions of all defendants and on the court appearance performance of defendants not in the notification group. Comparisons of outcomes for "program" versus "overflow" defendants and for "notified" versus "not notified" defendants should provide considerable insight concerning the impact of a formal pretrial release program on the misdemeanor release process and on defendant outcomes.

Figure 1 provides a summary of the overall approach to the Misdemeanor Study.

Figure 1

Summary of Approach for Misdemeanor Study

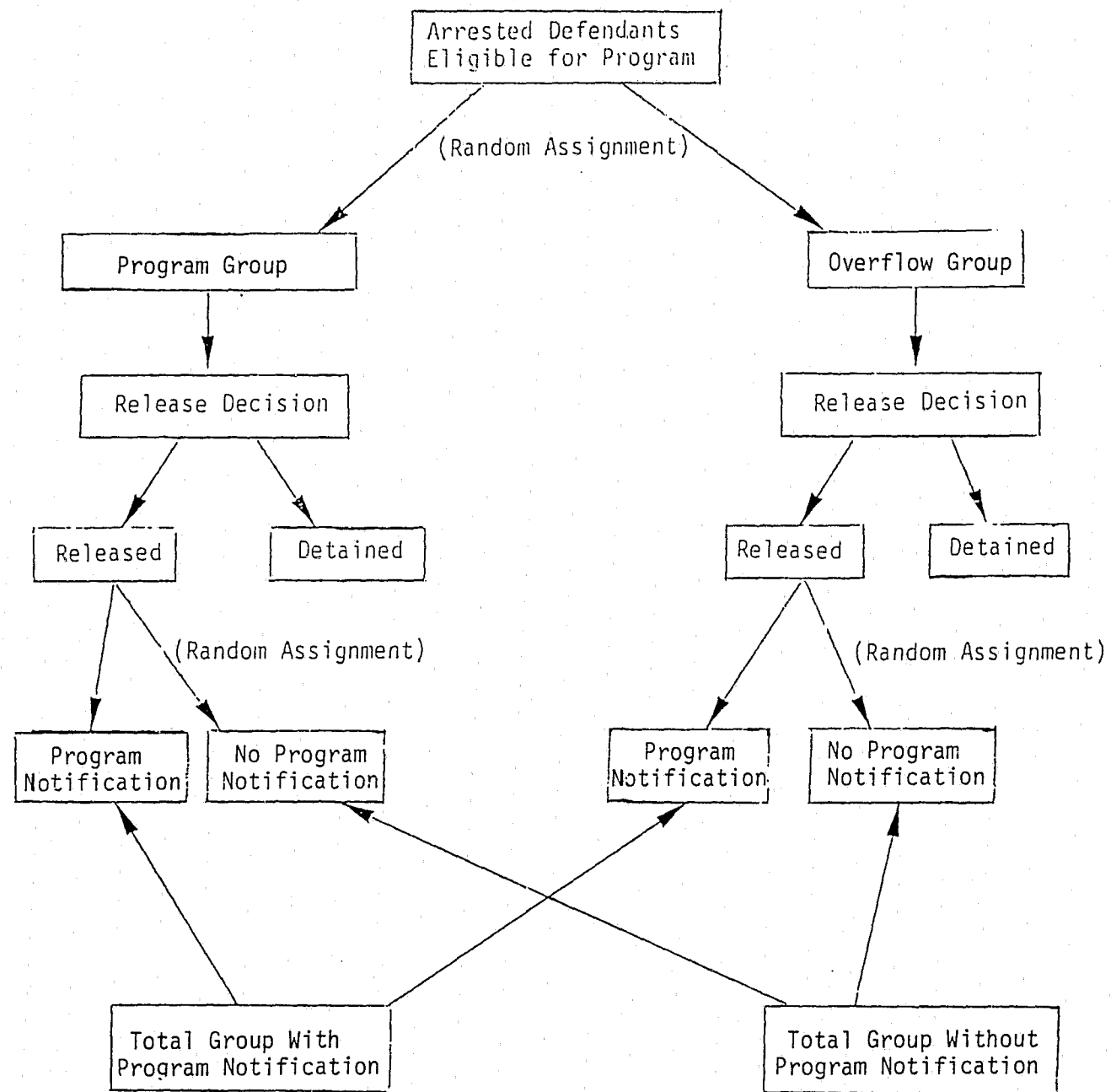


EXHIBIT A

BIRTHDATES FOR PROGRAM GROUP

January 2	May 1	September 2
" 3	" 2	" 3
" 4	" 3	" 4
" 5	" 4	" 5
" 6	" 5	" 6
" 7	" 6	" 7
" 8	" 7	" 8
" 9	" 8	" 9
" 10	" 9	" 10
" 11	" 10	" 11
" 12	" 11	" 12
" 13	" 12	" 13
" 14	" 13	" 14
" 15	" 14	" 15
" 16	" 15	" 16
" 17	" 16	" 17
" 18	" 17	" 18
" 19	" 18	" 19
" 20	" 19	" 20
" 21	" 20	" 21
" 22	" 21	" 22
" 23	" 22	" 23
" 24	" 23	" 24
" 25	" 24	" 25
" 26	" 25	" 26
" 27	" 26	" 27
" 28	" 27	" 28
" 29	" 28	" 29
" 30	" 29	" 30
February 5	June 3	October 3
" 6	" 4	" 4
" 7	" 5	" 5
" 8	" 6	" 6
" 9	" 7	" 7
" 10	" 8	" 8
" 11	" 9	" 9
" 12	" 10	" 10
" 13	" 11	" 11
" 14	" 12	" 12
" 15	" 13	" 13
" 16	" 14	" 14
" 17	" 15	" 15
" 18	" 16	" 16
" 19	" 17	" 17
" 20	" 18	" 18
" 21	" 19	" 19
" 22	" 20	" 20
" 23	" 21	" 21
" 24	" 22	" 22
" 25	" 23	" 23
" 26	" 24	" 24
" 27	" 25	" 25
" 28	" 26	" 26
" 29	" 27	" 27
" 30	" 28	" 28
March 4	July 4	November 2
" 5	" 5	" 3
" 6	" 6	" 4
" 7	" 7	" 5
" 8	" 8	" 6
" 9	" 9	" 7
" 10	" 10	" 8
" 11	" 11	" 9
" 12	" 12	" 10
" 13	" 13	" 11
" 14	" 14	" 12
" 15	" 15	" 13
" 16	" 16	" 14
" 17	" 17	" 15
" 18	" 18	" 16
" 19	" 19	" 17
" 20	" 20	" 18
" 21	" 21	" 19
" 22	" 22	" 20
" 23	" 23	" 21
" 24	" 24	" 22
" 25	" 25	" 23
" 26	" 26	" 24
" 27	" 27	" 25
" 28	" 28	" 26
" 29	" 29	" 27
April 1	August 1	December 2
" 2	" 2	" 3
" 3	" 3	" 4
" 4	" 4	" 5
" 5	" 5	" 6
" 6	" 6	" 7
" 7	" 7	" 8
" 8	" 8	" 9
" 9	" 9	" 10
" 10	" 10	" 11
" 11	" 11	" 12
" 12	" 12	" 13
" 13	" 13	" 14
" 14	" 14	" 15
" 15	" 15	" 16
" 16	" 16	" 17
" 17	" 17	" 18
" 18	" 18	" 19
" 19	" 19	" 20
" 20	" 20	" 21
" 21	" 21	" 22
" 22	" 22	" 23
" 23	" 23	" 24
" 24	" 24	" 25
" 25	" 25	" 26
" 26	" 26	" 27
" 27	" 27	" 28
" 28	" 28	" 29
" 29	" 29	" 30
" 30	" 30	" 31
" 31	" 31	" 31

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EXHIBIT B

MASTER LIST
PIMA COUNTY FELONY DEFENDANTS STUDY

Period from _____ to _____
(month, day, year) (month, day, year)

NAME	Date of Birth	Group (Check One)		Study Number
		Program	Overflow	

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EXHIBIT C

CRIMINAL HISTORY SUMMARY

STUDY NO. _____ NAME _____

AGE AT FIRST ADULT ARREST _____

NUMBER OF PRIOR ADULT ARRESTS _____

NUMBER OF PRIOR ADULT CONVICTIONS _____

TOTAL NUMBER OF MONTHS INCARCERATED AS PART OF SENTENCES SERVED FOR
ADULT CONVICTIONS _____ MONTHS

DATE OF MOST RECENT PREVIOUS ARREST _____

DATE OF RELEASE FROM MOST RECENT INCARCERATION _____

NUMBER OF MONTHS OF LAST INCARCERATION _____ MONTHS

MOST FREQUENT CHARGE IN TOTAL RECORD _____

HAS DEFENDANT FAILED TO APPEAR FOR COURT IN THE PAST?

____ DON'T KNOW ____ NO ____ YES ____ NUMBER OF TIMES

EXHIBIT D

VERIFICATION/RELEASE SUMMARY

Name	Study Number	Type of Verification (Check as many as apply)					Number of References Checked	Date of Release	Type(s) of Release*	Bond Amount	Estimated Percentage Paid
		Telephoned References	Visited References	Checked Files	Did not Verify Anything	Other (Please Describe)					

*INDICATE: ROR (Released on Recognizance)
 TPC, CVC (Third Party Custody, CVC)
 TPC, not CVC (Third Party Custody, not CVC)
 SB (Surety Bond)

UAB (Unsecured Appearance Bond)
 CR (Conditional Release)
 Other — describe

EXHIBIT F (CONTINUED) INTERIM COMPLAINT RELEASE QUESTIONNAIRE (PART II) Page 2 of 3

Does this alleged incident involve any member of your household? 1. ☐ Yes 2. ☐ No

If yes, is: 1. ☐ Victim 2. ☐ Codetendant
Are you welcome to return home? 1. ☐ Yes 2. ☐ No

If no, alternate residence?
Where Will You Go if Released Today?

Where are you expected and when?

Previous addresses, how long, with whom?

Are there any special problems, such as health, that you and your family are experiencing?

Total Time in the Tucson Area?
Years Months Days

What is Your Mailing Address? for court documents?
Street or P.O. Box

Total Time in Arizona?
Years Months Days

City State Zip

What is your employment status
(code as many as two)

1. ☐ Employed full time
2. ☐ Employed part-time
3. ☐ Unemployed seeking employment
4. ☐ Student
5. ☐ Self employed
6. ☐ Retired
7. ☐ Unemployed supported by family
8. ☐ Unemployed, not seeking employment
9. ☐ Employed as housewife

Employers Name _____ Position _____
Address _____ Phone _____

How many hours do you work per week?
May we contact your employer? ☐ Yes ☐ No
How many jobs have you worked in the last twelve months?

What is your monthly income in dollars from employment?
Do you have any other sources of income?
(Select three from choices offered below)

1. ☐ None
2. ☐ Social Security
3. ☐ Social Security disability
4. ☐ Unemployment compensation
5. ☐ Food stamps
6. ☐ Aid to dependent children
7. ☐ Workmen's compensation
8. ☐ Social Security and Veterans assistance
9. ☐ Money from friends
10. ☐ Money from relatives
11. ☐ Selling blood
12. ☐ Training stipend
13. ☐ Investment, business, property or rental income
14. ☐ Churches
15. ☐ Other (describe) _____

Total monthly income from all sources?

If you are a home owner what is the value of your home? (in dollars)
What is the outstanding mortgage amount? (in dollars)
What is your monthly outlay for housing?

1. ☐ Mortgage payment 2. ☐ Rent

What are your total savings in dollars on hand?

Where?
List your major debts or obligations:

Total amount of bills, debts, loans outstanding? (in dollars)
Total amount available to post bond (in dollars)

If you are unemployed, how long since your last job?
Years Months Days

If you are employed for less than two years or unemployed, what was your

Previous job?

Address at work

Phone _____

For how long? Years Months Days
From _____ To _____
Day _____ Month _____ Year _____

If you are unemployed, how long have you been seeking employment? Days Months Years
What kind of work do you do?
What kind of skills do you have?

What is the highest grade of school you completed?

What degrees or diplomas do you have?

1. ☐ None
2. ☐ High School Diploma
3. ☐ College
4. ☐ U.S.D.

Are you currently enrolled in _____?
1. ☐ High School 2. ☐ College 3. ☐ Vocational 4. ☐ Skills Training
Where? For how long?

How much of your income is devoted to the support of the household? 1. ☐ None 2. ☐ Some 3. ☐ Half 4. ☐ Most

How much does your contribution represent of the total household support? 1. ☐ None 2. ☐ Some 3. ☐ Half 4. ☐ Most

How many in your household are currently employed? 1. ☐ None 2. ☐ One 3. ☐ Two 4. ☐ Three 5. ☐ Four 6. ☐ Five 7. ☐ Six 8. ☐ Seven 9. ☐ Eight 10. ☐ Nine 11. ☐ Ten 12. ☐ Eleven 13. ☐ Twelve 14. ☐ Thirteen 15. ☐ Fourteen 16. ☐ Fifteen 17. ☐ Sixteen 18. ☐ Seventeen 19. ☐ Eighteen 20. ☐ Nineteen 21. ☐ Twenty

Is your spouse currently employed?

1. ☐ Not applicable 2. ☐ Yes 3. ☐ No
What is the length of your spouse's employment (in months)?

Family income monthly from employment

Total family income from all sources (monthly):

1. ☐ Non-Vet
2. ☐ Veteran
3. ☐ Active
4. ☐ Reserves
5. ☐ National Guard Service
6. ☐ Branch of service
7. ☐ Marine
8. ☐ Air Force
9. ☐ Army
10. ☐ Navy
11. ☐ Coast Guard

Type of discharge
1. ☐ Not applicable 2. ☐ Dishonorable 3. ☐ Honorable 4. ☐ Medical 5. ☐ General 6. ☐ Undesirable
Date of Discharge _____ Day _____ Month _____ Year _____

EXHIBIT F (CONTINUED)

Do you want the court to appoint a lawyer to represent you in this case? 1. ☐ Yes 2. ☐ No
Are you able to obtain the services of a lawyer without incurring substantial hardship to yourself or your family? 1. ☐ Yes 2. ☐ No If "no" state your reason

Veterans Administration Benefits
1. ☐ Not a veteran
2. ☐ Veteran currently receiving benefits
3. ☐ Veteran previous contact with VA currently not receiving benefits
4. ☐ Veteran not currently receiving benefits

Have you ever been arrested before? 1. ☐ Yes 2. ☐ No

Have you ever been released on
1. ☐ Bond 2. ☐ ROR 3. ☐ Neither
(code one or more)

Are you currently released on
1. ☐ Bond 2. ☐ ROR 3. ☐ Neither
(code one or more)

If you are currently pending on a felony charge, who is your lawyer?

Do you have any other property which is not needed by your family for day-to-day living which you could use to pay for an attorney?

Approximate value (in dollars);
Approximately how much can you afford to contribute to the cost of an attorney each month to represent you in this case?

When is your next court date? Month _____ Day _____ Year _____

Type of hearing
What is your Pima County Superior Court case number?

What are the charges pending against you?

LIST IN CHRONOLOGICAL ORDER THE HISTORY OF YOUR ADULT CRIMINAL CHARGES

Date	Charges	Jurisdiction	Disposition/Sentence	Date	Type (Probation Prison and Parole)	Actual Time Served Yrs Mos Days	Date Completed
1. _____	_____	_____	_____	_____	_____	_____	_____
2. _____	_____	_____	_____	_____	_____	_____	_____
3. _____	_____	_____	_____	_____	_____	_____	_____
4. _____	_____	_____	_____	_____	_____	_____	_____
5. _____	_____	_____	_____	_____	_____	_____	_____
6. _____	_____	_____	_____	_____	_____	_____	_____
7. _____	_____	_____	_____	_____	_____	_____	_____
8. _____	_____	_____	_____	_____	_____	_____	_____
9. _____	_____	_____	_____	_____	_____	_____	_____
10. _____	_____	_____	_____	_____	_____	_____	_____
11. _____	_____	_____	_____	_____	_____	_____	_____

What are the total number of prior felony arrests? _____

What are the number of prior felony convictions? _____

What are the total number of prior misdemeanor arrests?
Have you ever failed to appear for court?

If 1. ☐ Yes ☐ Felony ☐ Misdemeanor ☐ Traffic

Are you currently on probation? 1. ☐ Yes 2. ☐ No

Are you currently on parole? 1. ☐ Yes 2. ☐ No

Are you currently on diversion? 1. ☐ Yes 2. ☐ No

What is your Probation and/or Parole Officer's name?

- | | | | |
|--|---|---|--|
| 1. <input type="checkbox"/> New case | 6. <input type="checkbox"/> Out ROR—non local felony | 12. <input type="checkbox"/> Out bond—non local misdemeanor | 17. <input type="checkbox"/> Serving time—misdemeanor |
| 2. <input type="checkbox"/> On probation—felony | 7. <input type="checkbox"/> Out ROR—local misdemeanor | 13. <input type="checkbox"/> On diversion | 18. <input type="checkbox"/> Released to pay fine |
| 3. <input type="checkbox"/> On probation—misdemeanor | 8. <input type="checkbox"/> Out ROR—non local misdemeanor | 14. <input type="checkbox"/> In custody—pending felony | 19. <input type="checkbox"/> Work—release status |
| 4. <input type="checkbox"/> On parole—state | 9. <input type="checkbox"/> Out bond—local felony | 15. <input type="checkbox"/> In custody—pending misdemeanor | 20. <input type="checkbox"/> In custody—other jurisdiction pending charges |
| 5. <input type="checkbox"/> Federal probation/parole | 10. <input type="checkbox"/> Out bond—non local misdemeanor | 16. <input type="checkbox"/> Out bond—local misdemeanor | |
| 6. <input type="checkbox"/> Out ROR—local felony | 11. <input type="checkbox"/> Out bond—local misdemeanor | 17. <input type="checkbox"/> Serving time—felony | |

Do you have anyone else who can verify the information you have given?

Name	Relationship	Phone	Address
_____	_____	_____	_____
_____	_____	_____	_____
_____	_____	_____	_____

Who will be responsible for your return to court?

1. ☐ No one
2. ☐ Parents
3. ☐ Roommate
4. ☐ Girlfriend
5. ☐ Self
6. ☐ Sibling
7. ☐ Organization
8. ☐ Employer
9. ☐ Spouse
10. ☐ Friend(s)
11. ☐ Relative
12. ☐ Other

Is field verification required? 1. ☐ Yes 2. ☐ No

If Yes, please provide us with directions.

Person or Organization Relationship

Address Phone

Will they be in Court today? 1. ☐ Yes 2. ☐ No
Works at or can be located at

Address Phone

PIMA COUNTY PRE-TRIAL RELEASE PROJECT

EXHIBIT H

December	1
"	3
"	4
"	6
"	10
"	11
"	14
"	15
"	16
"	18
"	19
"	20
"	21
"	23
"	27
"	28

Project Staff

NOTIFICATION SUMMARY

[illegible]

INTRODUCTION

- What impact do pretrial release programs have upon release rates?
- Do programs result in increased "equity" of release (e.g., by leading to the release of more poor defendants than would otherwise occur)?
- What is the extent of criminality among pretrial releasees?
- To what extent do defendants on different types of pretrial release (e.g., own recognizance, money bail, supervised release) return for scheduled court appearances?
- How do the services of pretrial release programs affect defendant outcomes (e.g., court appearance rates, pretrial criminality rates)?
- What costs and benefits are associated with pretrial release programs?

In Baltimore this research approach will be implemented for defendants receiving five or fewer verified points on the point scale. Other defendants (i.e., those receiving a total of six or more points) will not be included in this study. Defendants receiving an appropriate point scale total will be randomly divided into two groups so that every such defendant has an equal opportunity of being in either group.

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LIST OF ACCEPTABLE CHARGES

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 (Exclude second time against same person)
 THEFT—UNDER \$500
 TRESPASSING (not a repeat on same establishment)
 UNLAWFUL ACTS—RELATED TO MINORS
 WELFARE FRAUD

LIST OF EXCLUSIONS (BESIDES CHARGE)

DETAINERS
 PRETRIAL REARRESTS
 PSYCHIATRIC CASES
 OUT-OF-STATE RESIDENTS
 UNVERIFIED ADDRESS
 FTA RECORD ONLY IF FLAGRANT VIOLATION (2 FTA'S WITH GUILTY
 DISPOSITIONS OR 4 FTA CHARGES SINCE 1/1/77)

PROCEDURES FOR INTERVIEWERS

1. All defendants with six or more points will be processed as usual.
2. If a defendant has five or less points, ALWAYS RECOMMEND O.R. if:
 - the charge is acceptable,
 - the listed exclusions do not apply, and
 - the defendant's birthdate is on the "Birthdates for Program Group" list.
3. Any recommendation may be made for a defendant with five or less points if:
 - the charge is not acceptable,
 - any listed exclusion does apply, or
 - the defendant's birthdate is not on the "Birthdates for Program Group" list.
4. ALL defendants with five or less points who are released on O.R. should be referred to the PRSD office, unless the charge is disorderly conduct or a similar charge which will be settled in court the next day.
5. It is essential that all interview forms for defendants with five or less points be completed in full during the time period of this study. In particular, the forms should be pointed up, and the recommendations and charges should be clearly indicated.
6. The folders for all defendants with five or less points should be marked in the upper left-hand corner, as follows:

☐ eligible for Survey

☒ not eligible for Survey

If the case is not eligible, the reason should be written next to the ☒ mark (e.g., charge, out-of-State, rearrest, FTA, etc.).

PROCEDURES FOR CENTRAL OFFICE PERSONNEL

If any defendant with five or less points is released on OR, Program Staff will compare the defendant's date of birth with the list entitled "BIRTHDATES FOR SUPERVISED GROUP." NOTE THAT THIS LIST OF BIRTHDATES IS A DIFFERENT ONE FROM THE LIST USED TO DEVELOP THE PROGRAM AND NON-PROGRAM GROUPS: IT INCLUDES HALF OF THE PROGRAM GROUP BIRTHDATES AND HALF OF THE NON-PROGRAM GROUP BIRTHDATES.

1. If the defendant's birthdate appears on the "BIRTHDATES FOR SUPERVISED GROUP" list, the defendant is to be given an appropriate form of supervision, such as being required to report to a referral program, come to the release program in person, be placed in someone's custody, etc. The minimum supervision for defendants in this group will be to call in at least twice a week. During these calls, defendants will be reminded of their next court dates, their addresses will be verified, etc. All defendants on the SUPERVISED birthdate list are to be assigned some form of supervision, beyond that now received by all defendants.
2. If the defendant's birthdate does not appear on the "BIRTHDATES FOR SUPERVISED GROUP" list, the defendant will be required to call in once a week ONLY. NO ADDITIONAL SUPERVISION will be required for any defendant in this group. Additionally, when these defendants call in, they will not be reminded of their next court dates; their addresses will be verified only.

Figure 1 presents a summary of the overall approach for the experimental analysis in Baltimore.

Figure 1: Summary of Approach for Baltimore Experiment

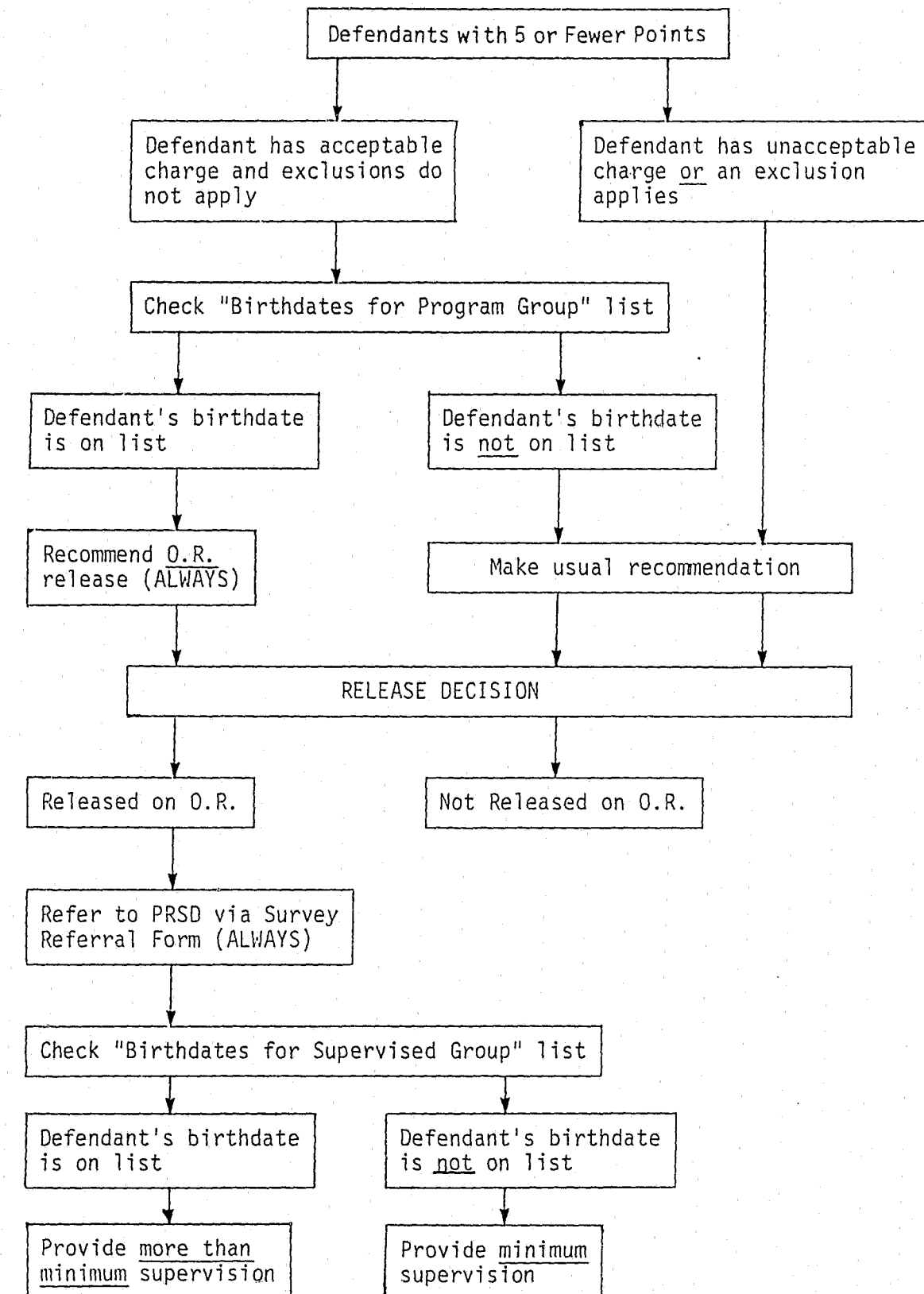


EXHIBIT 1

BIRTHDATES FOR PROGRAM GROUP

January 2	May 1	September 2
" 3	" 2	" 3
" 4	" 3	" 4
" 5	" 4	" 5
" 6	" 5	" 6
" 7	" 6	" 7
" 8	" 7	" 8
" 9	" 8	" 9
" 10	" 9	" 10
" 11	" 10	" 11
" 12	" 11	" 12
" 13	" 12	" 13
" 14	" 13	" 14
" 15	" 14	" 15
" 16	" 15	" 16
" 17	" 16	" 17
" 18	" 17	" 18
" 19	" 18	" 19
" 20	" 19	" 20
" 21	" 20	" 21
" 22	" 21	" 22
" 23	" 22	" 23
" 24	" 23	" 24
" 25	" 24	" 25
" 26	" 25	" 26
" 27	" 26	" 27
" 28	" 27	" 28
" 29	" 28	" 29
" 30	" 29	" 30
February 5	June 3	October 3
" 6	" 4	" 4
" 7	" 5	" 5
" 8	" 6	" 6
" 9	" 7	" 7
" 10	" 8	" 8
" 11	" 9	" 9
" 12	" 10	" 10
" 13	" 11	" 11
" 14	" 12	" 12
" 15	" 13	" 13
" 16	" 14	" 14
" 17	" 15	" 15
" 18	" 16	" 16
" 19	" 17	" 17
" 20	" 18	" 18
" 21	" 19	" 19
" 22	" 20	" 20
" 23	" 21	" 21
" 24	" 22	" 22
" 25	" 23	" 23
" 26	" 24	" 24
" 27	" 25	" 25
" 28	" 26	" 26
" 29	" 27	" 27
" 30	" 28	" 28
March 4	July 4	November 2
" 5	" 5	" 3
" 6	" 6	" 4
" 7	" 7	" 5
" 8	" 8	" 6
" 9	" 9	" 7
" 10	" 10	" 8
" 11	" 11	" 9
" 12	" 12	" 10
" 13	" 13	" 11
" 14	" 14	" 12
" 15	" 15	" 13
" 16	" 16	" 14
" 17	" 17	" 15
" 18	" 18	" 16
" 19	" 19	" 17
" 20	" 20	" 18
" 21	" 21	" 19
" 22	" 22	" 20
" 23	" 23	" 21
" 24	" 24	" 22
" 25	" 25	" 23
" 26	" 26	" 24
" 27	" 27	" 25
" 28	" 28	" 26
" 29	" 29	" 27
" 30	" 30	" 28
April 1	August 1	December 2
" 2	" 2	" 3
" 3	" 3	" 4
" 4	" 4	" 5
" 5	" 5	" 6
" 6	" 6	" 7
" 7	" 7	" 8
" 8	" 8	" 9
" 9	" 9	" 10
" 10	" 10	" 11
" 11	" 11	" 12
" 12	" 12	" 13
" 13	" 13	" 14
" 14	" 14	" 15
" 15	" 15	" 16
" 16	" 16	" 17
" 17	" 17	" 18
" 18	" 18	" 19
" 19	" 19	" 20
" 20	" 20	" 21
" 21	" 21	" 22
" 22	" 22	" 23
" 23	" 23	" 24
" 24	" 24	" 25
" 25	" 25	" 26
" 26	" 26	" 27
" 27	" 27	" 28
" 28	" 28	" 29
" 29	" 29	" 30
" 30	" 30	" 31

EXHIBIT B: MASTER LIST, BALTIMORE CITY DEFENDANTS STUDY

Date: _____

Note: Include all defendants with 5 or less points.

District	Name of Defendant	Point Total (If 5 or less)	Reason for Exclusion *	Date of Birth	Group (Check One)	
					Program	Non- Program

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*For example, if arrested on murder charge, write "murder"; if out-of-State resident, write "out-of-State."

EXHIBIT C: RELEASE DECISION

Complete for ALL STUDY GROUP Defendants					Complete ONLY for Defendants with RELEASE REVIEW			
Name of Defendant	Group (Check One) Program Non-Program		Commissioner's Release Decision*	Amount or Percentage, if Applicable	Date of Release	Release Decision at Reconsideration	Program Recommendation at Reconsideration (include amount or percentage if any)	Number of Points at Reconsideration

*Indicate ROR, bail bond, deposit bond, third party custody, conditional release or other (describe).

BIRTHDATES FOR SUPERVISED GROUP

January 1	April 1	July 1	October 3
" 3	" 2	" 3	" 4
" 5	" 3	" 4	" 5
" 6	" 4	" 6	" 6
" 7	" 6	" 8	" 8
" 11	" 7	" 11	" 9
" 12	" 8	" 13	" 10
" 14	" 11	" 14	" 11
" 15	" 12	" 15	" 13
" 17	" 15	" 16	" 14
" 18	" 17	" 17	" 17
" 20	" 20	" 18	" 19
" 22	" 21	" 19	" 21
" 25	" 25	" 23	" 22
" 29	" 27	" 25	" 23
" 31	" 30	" 26	" 25
		" 28	" 30
		" 31	" 31
February 1	May 1	August 2	November 2
" 3	" 3	" 3	" 4
" 5	" 5	" 5	" 6
" 6	" 7	" 10	" 8
" 7	" 8	" 11	" 10
" 9	" 10	" 12	" 15
" 12	" 11	" 13	" 19
" 17	" 13	" 14	" 20
" 19	" 15	" 16	" 21
" 20	" 16	" 18	" 22
" 21	" 17	" 20	" 23
" 23	" 19	" 22	" 24
" 25	" 26	" 26	" 27
" 27	" 27	" 27	" 29
" 28	" 30	" 29	
March 1	June 2	September 6	December 1
" 3	" 6	" 9	" 3
" 4	" 10	" 14	" 4
" 8	" 11	" 15	" 6
" 9	" 12	" 16	" 10
" 11	" 19	" 18	" 11
" 13	" 20	" 19	" 14
" 14	" 22	" 24	" 15
" 15	" 26	" 25	" 16
" 16	" 27	" 27	" 18
" 20	" 28	" 28	" 19
" 21	" 29		" 20
" 22			" 21
" 23			" 23
" 26			" 27
" 30			" 28
" 31			

EXHIBIT E: SUPERVISION SUMMARY

Name*	Date of Birth	In Supervised Group?		Type and Frequency of Supervision (e.g., call-in twice per week)	Comments
		Yes	No		

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*Include all defendants with five or fewer points RELEASED ON O.R.

[illegible]

To be recommended, a defendant needs

1. A verified Baltimore area address, AND
2. A minimum of 6 verified points from the following.

Int	Ver	
4	4	<u>RESIDENCE (In Baltimore Area)</u>
3	3	Present address 5 years or more
3	3	Present address 2 years OR Present and Prior 3 years
2	2	Present address 6 months OR Present and Prior 1 year
1	1	Present address 4 months OR Present and Prior 6 months.
1	1	<u>TIME IN BALTIMORE AREA</u>
		5 years or more (continuous)
3	3	<u>FAMILY TIES (In Baltimore Area)</u>
2	2	Lives with Parent, Spouse or Guardian.
1	1	Lives with Other Relative
		Lives with Non-family Person
4	4	<u>EMPLOYMENT OR SUBSTITUTES</u>
3	3	Present full-time job 3 years, where employer will take back
		Present full-time job 1 year or more
		OR Present and Prior full-time jobs 2 years
		OR Intermitent work with same company or union 3 years
2	2	Present full-time job 6 months or more
		OR Present and Prior full-time jobs 1 year
		OR Part-time job 1 year or more
		OR Full-time student 6 months or more
		OR Receiving Social Services, SSI, Social Security, Pension, or VA Disability Benefits 6 months or more
		OR Laid off during last 4 months from full-time job where employed at least 1 year
1	1	Current full-time job
		OR Part-time job 3 months or more
		OR Full-time student
		OR Part-time student 3 months or more
		OR Receiving Social Services, SSI, Social Security, Pension, VA Disability Benefits, or Unemployment Compensation
		OR Laid off during last 4 months
		OR Pending or receiving Workmen's Compensation
		OR Firm commitment to start work within next 2 weeks (Cannot be awarded unless fully explained in comments)
		OR Regular Family Support or Substantial Savings
1	1	<u>OTHER FACTORS</u> (Cannot be awarded unless fully explained in comments)
1	1	Poor Health, Over 60 years old, or Pregnant
		Extenuating Responsibilities (e.g. children, household, civic or church duties, etc.)
-2	-2	<u>DRUG OR ALCOHOL PROBLEM</u>
		Knowledge of drug addiction and/or alcoholism (Rebuttable with treatment condition)
-2	-2	<u>FTA, ESCAPE OR PAROLE/PROBATION VIOLATION</u>
-4	-4	Conviction of FTA, Escape or Parole/Probation Violation
		2 or more Convictions of FTA, Escape, Parole/Probation Violation, or Combination thereof
		<u>PRIOR RECORD</u>
		Negative points are assessed on the basis of the total number of offense points achieved. The units are as follows:
		8 Units - each Felony conviction
		2 Units - each Misdemeanor conviction (within last 7 years)
		1 Unit - each Misdemeanor conviction (over 7 years ago)
0	0	0 1 2
-1	-1	3 4 5 6 7 8
-2	-2	9 10 11 12 13 14
-3	-3	15 16 17 18 19 20
-4	-4	21 or more

ANALYSIS OF PRETRIAL RELEASE IN THE LINCOLN, NEBRASKA AREA:
PROCEDURES

INTRODUCTION

In order to analyze the impact of pretrial release programs, the Law Enforcement Assistance Administration (LEAA) has commissioned a multi-city study of release practices. This study, part of LEAA's National Evaluation Program, is being conducted by The Lazar Institute, a research organization based in Washington, D.C. The study considers several major pretrial release issues, including:

- What impact do pretrial release programs have upon release rates?
- Do programs result in increased "equity" of release (e.g., by leading to the release of more poor defendants than would otherwise occur)?
- What is the extent of criminality among pretrial releasees?
- To what extent do defendants on different types of pretrial release (e.g., own recognizance, money bail, supervised release) return for scheduled court appearances?
- How do the operations of pretrial release programs affect defendant outcomes (e.g., court appearance rates, pretrial criminality rates)?
- What costs and benefits are associated with pretrial release programs?

To address these issues, the study provides for experimental analysis, comparing defendants who are processed by a pretrial release program with an otherwise equivalent group of defendants who are not processed by the program. Analysis of outcomes (e.g., release rates, court appearance performance, pretrial criminality) for the two groups will permit assessment of program impact.

The "ideal" situation for implementing this research approach is in a jurisdiction where the pretrial release program is not able to interview all defendants who might potentially be eligible for the program's services (e.g., the program may lack the resources needed to provide full service to all eligible defendants). The defendants not served comprise an "overflow" group, which exceeds the program's service capacity. Under these conditions, a comparison group of defendants not served by the program can be identified through modifying the process for selecting "program" versus "overflow" defendants. This can be done in a way that insures each defendant of an equal

opportunity to be in either group, while the program's overall client loads remain at the same level (or even increase).

The Lincoln, Nebraska area now has an "overflow" situation. Since the pretrial release program operates only during selected weekend hours, the overflow consists of defendants who are arrested on weekdays. This overflow situation provides the ideal conditions for conducting experimental analysis of program impact. A cooperative research effort involving Lazar, the Division of Corrections in Lincoln, and various Lincoln officials is planned for the area. The following sections of this paper describe the proposed procedures for the study.

PROPOSED PROCEDURES

1. The Lazar Institute, under the terms of its grant from LEAA, will provide the Lincoln pretrial release program, run by the Division of Corrections, with the funds needed to expand its activities. As a result of this expansion, defendants would be interviewed throughout the week, rather than on weekends only.
2. (a) On some days each week, the program will operate as it does now: interviewing defendants, verifying information and making release recommendations. On other days each week, the program will conduct research only: defendants will be interviewed as part of the research project, and their criminal records will be checked, but no release recommendations will be made.
- (b) To determine whether it is a "program" day or a "research" day, pretrial release staff should consult EXHIBIT A (LIST OF PROGRAM AND RESEARCH DAYS FOR PRETRIAL RELEASE ANALYSIS IN LINCOLN, NEBRASKA). The distribution of program versus research days shown in Exhibit A was randomly generated, in accordance with valid statistical procedures. It is essential that the program and research days listed in Exhibit A be followed exactly during the experimental analysis period; any changes could bias the analysis and invalidate the research findings.
- (c) As Exhibit A indicates, over a 26-week period, there would be a total of 91 "program" days and 91 "research" days. There would also be 45 "program" weekend days. (Friday, Saturday and Sunday) and 33 "research" weekend days. Thus, the number of defendants in the "program" group may somewhat exceed the number in the "research" group.
- (d) The net result of these procedures should be that more defendants are processed by the pretrial release program than would be if the program continued to operate as it now does. This expectation is based on the program's estimate that at present it reaches about 40% of the potentially eligible defendants arrested each week. Expanding the program's operations from weekends only to daily should permit virtually all of the potentially eligible defendants to be interviewed. If half the defendants are interviewed on "program" days, then approximately 50% of the potentially eligible defendants would be processed by the program, as compared with the current 40%.

3. (a) On any given day, program staff will check the booking log at the jail to screen out any obviously inappropriate defendants (e.g., those with ineligible charges or who are in transit to another jurisdiction).
- (b) Any defendant not screened out when the booking log is checked will be interviewed by the pretrial release program staff. In addition, the defendant's criminal records will be checked. EXHIBIT B (INTERVIEW AND VERIFICATION WORK SHEET—PRE-TRIAL RELEASE PROJECT) should be used for the interview and criminal records' verifications. EXHIBIT B represents a revised version of the interview form now used by the program; additional questions have been added to permit more detailed analysis of the research findings.
- (c) The ANSWER CATEGORIES GUIDE (EXHIBIT C) should be used when completing each interview form. Please use only the categories listed there for the specific questions indicated; this is necessary so that the data from Lincoln will be comparable with the information obtained from other areas.
- (d) Note that the revised interview form includes a section ("Criminal Records Information") which is to be completed by checking criminal records, rather than by questioning the defendant.
- (e) If it is a "program" day, as determined by EXHIBIT A (LIST OF PROGRAM AND RESEARCH DAYS FOR PRETRIAL RELEASE ANALYSIS IN LINCOLN, NEBRASKA), the program staff will review the interview and criminal records information and attempt to secure the release of appropriate defendants, just as is currently done.
- (f) If it is a "research" day, as determined by EXHIBIT A, the program staff will only conduct the interviews and criminal records' checks; no attempts will be made to secure the release of any defendants on "research" days.
4. If a defendant is interviewed on a "research" day, it is essential that that defendant remain in the "research" group. The defendant should not be interviewed again on a subsequent "program" day, even if the person is still in jail on that day.
5. (a) Enter the names of all defendants interviewed on the MASTER LIST (EXHIBIT D).

7. Send COPIES of the following materials to Lazar each week:

- MASTER LIST (one list of all defendants contacted during the reporting period)
- INTERVIEW FORMS (one form for each defendant on the MASTER LIST)
- RELEASE SUMMARY (send each sheet on which an entry has been made during the reporting period)

8. (a) A target date of early December has been set for the start of the study. It is expected to take about six months (26 weeks) to develop a group of approximately 400 defendants (200 from "program" days and 200 from "research" days). At the end of that time, the pretrial release program would return to its normal operating procedures.
- (b) After sufficient time had elapsed for the study defendants' cases to reach final dispositions, Lazar staff would visit Lincoln to collect data on the court appearance performance, pretrial arrests and case dispositions for all defendants studied. To accomplish this analysis, the Lazar staff will need access to relevant court and police records on the defendants studied.
- (c) After all data have been collected, Lazar will prepare a report on the Lincoln program as well as a composite report on all sites included in the pretrial release evaluation study. Comparisons of outcomes for defendants on "program" versus "research" days should provide considerable insight concerning the Lincoln program's impact on the release process and on defendant outcomes.

- (b) Also record the date of the interview and check the appropriate column to indicate whether the defendant was interviewed on a "program" day or a "research" day.
- (c) Finally, indicate the study number for the defendant in the last column of the MASTER LIST. Study numbers should be assigned consecutively as defendants' names are entered on the MASTER LIST (for example, the first defendant will have study number 1, the second defendant will have study number 2, and so on).
- (d) The MASTER LIST will be used as a control sheet to keep track of all defendants included in the study; Thus, it is particularly important that the MASTER LIST be completely accurate and kept up-to-date.
6. Complete the RELEASE SUMMARY sheet (EXHIBIT E):
- (a) Copy the names and study numbers of all defendants on the MASTER LIST onto the RELEASE SUMMARY sheet.
- (b) Provide the information indicated concerning the defendant's release. One or more of the following should be written in the "TYPE OF RELEASE" column:
- ROR;
 - Surety bond;
 - 10% deposit bond;
 - conditional release;
 - third party custody; or
 - other.

If the type of release is "other," please describe it. If the defendant was released on bond, complete the columns showing the bond amount and the estimated percentage paid by the defendant.

- (c) Note that defendants may be released at various times and thus the information on the RELEASE SUMMARY sheet may have to be checked and recorded over a period of several days.

EXHIBIT A

LIST OF PROGRAM AND RESEARCH DAYS FOR PRETRIAL
RELEASE ANALYSIS IN LINCOLN, NEBRASKA

Week Number	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
1	Program	Program	Research	Program	Research	Research	Program
2	Program	Research	Program	Research	Research	Program	Program
3	Research	Program	Research	Program	Research	Research	Research
4	Program	Research	Research	Research	Research	Research	Research
5	Program	Research	Research	Research	Program	Program	Program
6	Research	Program	Program	Research	Program	Program	Research
7	Research	Program	Program	Research	Research	Program	Research
8	Program	Research	Research	Program	Program	Research	Program
9	Program	Program	Research	Research	Program	Research	Research
10	Program	Program	Research	Program	Research	Program	Program
11	Program	Research	Research	Research	Research	Program	Program
12	Program	Program	Research	Program	Program	Research	Research
13	Research	Research	Program	Research	Program	Research	Program
14	Research	Program	Program	Research	Research	Program	Research
15	Research	Research	Research	Research	Research	Program	Program
16	Research	Research	Program	Program	Program	Program	Program
17	Research	Program	Program	Research	Research	Research	Research
18	Program	Research	Program	Program	Research	Program	Research
19	Research	Program	Research	Program	Research	Program	Program
20	Program	Research	Program	Program	Research	Program	Program
21	Research	Program	Research	Research	Research	Program	Program
22	Program	Research	Research	Program	Research	Program	Program
23	Research	Program	Research	Program	Program	Program	Program
24	Program	Program	Research	Program	Research	Research	Research
25	Research	Program	Program	Program	Research	Program	Program
26	Research	Research	Research	Research	Program	Research	Program

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EXHIBIT A—CONTINUED

LIST OF PROGRAM AND RESEARCH DAYS FOR PRETRIAL
RELEASE ANALYSIS IN LINCOLN, NEBRASKA

Week Number	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday	Week Number
27	Program	Program	Research	Program	Research	Research	Program	27
28	Program	Research	Program	Research	Research	Research	Program	28
29	Research	Program	Research	Program	Research	Program	Program	29
30	Research	Program	Program	Program	Program	Research	Program	30
31	Program	Research	Program	Program	Research	Program	Research	31
32	Program	Program	Research	Research	Research	Program	Program	32
33	Research	Research	Program	Program	Program	Research	Research	33
34	Research	Research	Research	Program	Program	Program	Research	34
35	Program	Program	Research	Research	Research	Research	Research	35
36	Program	Research	Research	Research	Research	Research	Program	36
37	Research	Program	Program	Research	Program	Research	Research	37
38	Program	Program	Research	Program	Program	Research	Program	38
39	Research	Research	Program	Research	Program	Program	Research	39

INTERVIEW AND VERIFICATION WORK SHEET -					PRE-TRIAL RELEASE PROJECT							
1 - NAME (LFMI)					6 - VER NAME		3 - START DATE		4 - RELEASE NO.			
3 - CHARGES					DATE OF ARREST					9 - VER. CHOS.		
4 - BOND			8 - COURT/CL. NO.			6 - ARR. DATE			7 - ARR. TIME			
10 - SIGNED WAIVER <input type="checkbox"/> YES <input type="checkbox"/> NO		11 - DOUG. CTY. ADDRESS <input type="checkbox"/> YES <input type="checkbox"/> NO		12 - QUALIFIED CHARGES <input type="checkbox"/> YES <input type="checkbox"/> NO		50 - APPEARANCE QUAL. <input type="checkbox"/> YES <input type="checkbox"/> NO		11 - ATTORNEY <input type="checkbox"/> YES <input type="checkbox"/> NO				
14 - D.O.B.		15 - AGE		16 - SEX		17 - RACE		18 - HEIGHT		19 - WEIGHT		
20 - RESIDENCE ADDRESS					21 - MAIL <input type="checkbox"/> YES <input type="checkbox"/> NO		22 - ZIP		23 - RES. PH.		24 - OWN <input type="checkbox"/> YES <input type="checkbox"/> NO	
25 - MA. PH.					26 - HRS.							
INTERVIEW					PTS		OCCUPATION					
28 - DATE		29 - INTERVIEWER		30 - LOCATION		33 - DISPOSITION						
31 - LANCASTER COUNTY					32		41 - PRESENT COMPANY EMPLOYING					
33 - LENGTH PRE. ADD. (MO.)		34 - LENGTH PREV. ADD. (MO.)		35		42 - LGTH. PRE. 43 - TERMS		45 - LGTH. PREV. 46 - TERMS				
35 - PREVIOUS ADDRESS					40		44 - PREVIOUS COMPANY EMPLOYING					
TOTAL NO. YEARS OF LOCAL RESIDENCE		TOTAL NO. OF DEPENDENTS IN AREA		40		NO. OF MONTHS UNEMPLOYED		EDUCATION LEVEL				
37 - MARITAL STATUS <input type="checkbox"/> NEVER MARR. <input type="checkbox"/> MARR. <input type="checkbox"/> SEP. <input type="checkbox"/> DIV. <input type="checkbox"/> WID.					IS DEFENDANT OR FAMILY RECEIVING ANY FORM OF PUBLIC ASSISTANCE? <input type="checkbox"/> YES <input type="checkbox"/> NO							
SUPPORT OF FAMILY <input type="checkbox"/> ALIMONY <input type="checkbox"/> CHILD SUPPORT <input type="checkbox"/> OTHER FAMILY SUPPORT <input type="checkbox"/> NONE					CRIMINAL RECORDS INFORMATION							
38 - LIVING WITH <input type="checkbox"/> ALONE <input type="checkbox"/> WIFE <input type="checkbox"/> CHILDREN <input type="checkbox"/> PAR. <input type="checkbox"/> OTHER					47		AGE AT FIRST ADULT ARREST		NO. OF PRIOR ADULT ARRESTS		NO. OF PRIOR ADULT CONVICTIONS	
39 - OTHER FAMILY TIES <input type="checkbox"/> WIFE <input type="checkbox"/> CHILDREN <input type="checkbox"/> PAR. <input type="checkbox"/> OTHER					47		DATE OF MOST RECENT PREVIOUS ARREST		MOST FREQUENT CHARGE IN TOTAL RECORD			
ALIEN? <input type="checkbox"/> LEGAL <input type="checkbox"/> ILLEGAL <input type="checkbox"/> NO					VETERAN? <input type="checkbox"/> YES <input type="checkbox"/> NO		HAS DEFENDANT FAILED TO APPEAR FOR COURT IN THE PAST? <input type="checkbox"/> DON'T KNOW <input type="checkbox"/> NO <input type="checkbox"/> YES <input type="checkbox"/> NO. OF TIMES					
48 - CRIM. RECORD <input type="checkbox"/> YES <input type="checkbox"/> NO		49 - LAST YEAR <input type="checkbox"/> YES <input type="checkbox"/> NO		50 - NO. IN. LAST YR.		51		DATE OF INTERVIEW		TIME OF INTERVIEW		
PAST APPEARANCE RECORD					COURT DISPOSITION - FIRST APPEAR ONLY							
53 - PROBATION <input type="checkbox"/> YES <input type="checkbox"/> NO		54 - NOW <input type="checkbox"/> YES <input type="checkbox"/> NO		55 - VIOLATION <input type="checkbox"/> YES <input type="checkbox"/> NO		59 - CAPIAS OR OTHER FAILURE <input type="checkbox"/> YES <input type="checkbox"/> NO		115 - DATE		116 - RECOMMENDER		
56 - PAROLE <input type="checkbox"/> YES <input type="checkbox"/> NO		57 - NOW <input type="checkbox"/> YES <input type="checkbox"/> NO		58 - VIOLATION <input type="checkbox"/> YES <input type="checkbox"/> NO				118 - JUDGE		119 - SURETY BOND <input type="checkbox"/> YES <input type="checkbox"/> NO		
								120 - DISPOSITION				
NO. ITEM NO												
INTERVIEWER NOTES AND REMARKS												
I												
II												
III												
IV												
V												
VI												
VII												
VIII												
IX												

EXHIBIT C

ANSWER CATEGORIES GUIDE

RACE (ITEM 17)

Black
White
Hispanic Surname
Oriental
Other

OCCUPATION

Record so that occupation can be coded later as:

- Professional/managerial
- Sales/retail
- Clerical
- Craftsman/foreman
- Operatives
- Laborers (except farm)
- Farm workers
- Service workers
- Private household workers
- Armed Forces

PRESENT COMPANY EMPLOYING (ITEM 41)

Write housewife or student, where applicable, if not employed by a company or other employer

TERMS (ITEM 43)

Indicate estimated weekly earnings from job

EDUCATION LEVEL

- Less than high school
- Some high school
- Technical or vocational school without high school graduation
- High school graduate
- Technical or vocational school in addition to high school graduation
- Some college
- College graduate or more

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EXHIBIT D

MASTER LIST

Period from _____ to _____
(month, day, year) (month, day, year)

[illegible]

EXHIBIT E
RELEASE SUMMARY

Name	Study Number	Did Program Recommend Release?		Was Defendant Released In Accordance with Program Recommendation?		Date of Release	Type(s) of Release*	Bond Amount	Estimated Percentage Paid
				Yes	No				

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ANALYSIS OF PRETRIAL RELEASE IN BEAUMONT-PORT ARTHUR, TEXAS

Background

In recent years many communities have been exploring new ways of making pretrial release decisions. A widely adopted technique for facilitating these decisions has been to designate special staff who:

- interview defendants soon after arrest about their length of residence in the area, local relatives, employment history and similar matters reflecting community ties as well as about their criminal record;
- verify the information provided by defendants, through telephone calls to references and checks of police records;
- provide this information to the magistrates who decide which defendants will be released pending trial; and
- maintain contact with released defendants and remind them of court appearance dates.

Before such procedures were adopted many magistrates had been forced to make release decisions on the basis of very limited information about the defendant. Often this information consisted only of the police report about the arrest, perhaps supplemented by a quick check of police files to assess the defendant's prior criminality.

Usually, magistrates possessed no reliable data about the defendant's background, family ties and responsibilities, employment history, income bracket or (for a repeat offender) past performance at meeting court dates. In addition, information about the defendant's criminal record was often incomplete. Thus, release decisions were being made in isolation, without access to information which might indicate whether a particular defendant seemed to be a "good risk" for release.

Moreover, after release, defendants had no contact with the court until their first appearance dates. Until then, no one knew if the defendants were still in town, much less whether they intended to appear in court when scheduled.

The new techniques for providing information to the magistrates before the release decisions were made (and maintaining contact with defendants after release) were designed to mitigate these problems. Criminal justice officials in the communities adopting these techniques hoped that their introduction would lead to better release decisions, that is, the release of more defendants who were good risks and the detention of more defendants who were poor risks.

The National Evaluation Study

Although the new techniques have been widely adopted and are thought by many to be effective, there has not been adequate evaluation of their impact. To remedy this situation, the Law Enforcement Assistance Administration (LEAA) has engaged The Lazar Institute to conduct a National Evaluation Program "Phase II" study of pretrial release.

The evaluation will analyze the impact of the new release techniques on overall rates of release, the equity of release, subsequent court appearance rates and community safety. An important part of the study involves a comparison of the outcomes (e.g., whether released, court appearance performance, pretrial criminality) of defendants processed by the new techniques with the outcomes of a similar group of defendants who are not processed in this way.

Possible Participation By Beaumont

Beaumont, Texas, has been suggested as a possible jurisdiction for conducting such analysis. Recently, several criminal justice officials in Beaumont have expressed interest in possible changes in release procedures. For example, such changes were discussed with Mr. Bruce Beaudin, Co-Investigator of the Lazar evaluation, during his technical assistance visit to Beaumont as part of another project. In addition, Beaumont was represented at the National Symposium on Pretrial Services, held in San Diego in early April.

These expressions of interest in various pretrial release practices raise the possibility that Beaumont's participation in the national evaluation study would be mutually advantageous. The national evaluation study is seeking sites where experimental analysis of pretrial release changes can be conducted; in return for participation, Federal funds would be made available to cover the costs of a two- to three-month experiment. From the results of the experiment, Beaumont officials could determine whether changed release procedures merited adoption on a continuing basis.

Experimental Procedures and Benefits

The proposed experiment would operate as follows:

- Lazar Institute staff would work with appropriate Beaumont officials to hire and train individuals who would interview defendants soon after arrest. These individuals would also verify the information provided by the defendants.
- During the experimental period, interviewed defendants would be split into two groups: for Group A the interview information would be provided to the magistrate making the release decision. For Group B the interview information would not be provided to the magistrate; release decisions for these defendants would be based on the type of information available at the present time.
- All release decisions would be made by a magistrate; the proposed changes would affect only the information available to the magistrate when those decisions are made.

- The special experimental staff would maintain contact with the released defendants in Group A until the final disposition of their cases. Released defendants in Group B would be treated as they are now; no special contact would be maintained with them.
- At the end of the experimental period, Lazar Institute research staff would track the outcomes (e.g., whether released, court appearance performance, pretrial criminality) of defendants in both groups.
- Lazar Institute staff would prepare a report presenting the results of the analysis. From this report Beaumont officials could determine whether the new procedures (i.e., those applied to the defendants in Group A) or the old ones (applied to defendants in Group B) were more appropriate for use in Beaumont in the future.

Besides its possible usefulness to Beaumont, the proposed experiment would provide insight of value to other communities which may be considering changes in their current pretrial release procedures. Insight gleaned from the Beaumont analysis could also facilitate informed judgments at the national level about the value of various pretrial release procedures and the most appropriate roles for them within the criminal justice system. Thus, the results of Beaumont's participation in the national evaluation study could have a broad impact on pretrial release procedures and policies around the country.

STUDY PROCEDURES

The Interview and Release Process

1. Check the jail card copy to screen out any obviously inappropriate defendants.
2. If a defendant is not screened out, enter the defendant's name and date of birth on the MASTER LIST (Exhibit A).
3. (a) Check the date of birth against the list entitled BIRTHDATES FOR PROGRAM GROUP (Exhibit B).
(b) If the defendant's birthdate appears on that list, the defendant will be in the Program Group. If the defendant's birthdate does not appear on that list, the defendant will be in the Overflow Group. Mark the appropriate column on the MASTER LIST with an X. (The column entitled Special Case will be used only when the Pre-Trial Release Program Director so indicates; this is for very unusual cases that may arise during the four-month study period.)
(c) Next, fill in (on the MASTER LIST) the cause number, if it is known, and the study number. Study numbers should be assigned consecutively as defendants' names are entered on the MASTER LIST (for example, the first defendant will have study number 1, the second defendant will have study number 2, and so on).
4. For a defendant in the Program Group, follow the usual program procedures. Be sure to complete an INTERVIEW FORM (Exhibit C) and a VERIFICATION/RELEASE SUMMARY form (Exhibit D).
5. For a defendant in the Overflow Group, complete the SHORT INTERVIEW FORM to obtain background information needed for the study (see Exhibit E).
6. When completing any interview form, be sure to use the ANSWER CATEGORIES GUIDE for the items listed on it (see Exhibit F).

Other Activities

7. Complete the CRIMINAL HISTORY SUMMARY (Exhibit G) for every defendant on the MASTER LIST.
8. (a) Copy the names and study numbers of all defendants on the MASTER LIST onto the RELEASE INFORMATION FORM (Exhibit H).
(b) Fill in the RELEASE INFORMATION FORM, providing release information for each defendant shown on the MASTER LIST. Use program records for defendants released through the program and the daily jail sheet for all other defendants.

9. Check the daily jail sheet to see if any defendant on the MASTER LIST is rearrested. Fill in the REARREST DATA SHEET every time a defendant on the MASTER LIST is rearrested (see Exhibit I).
10. Send COPIES of the following materials to Lazar (once a week at first, less often later):
 - MASTER LIST (one list of all defendants contacted during the reporting period)
 - INTERVIEW FORMS (one form for each defendant on the MASTER LIST)
 - VERIFICATION/RELEASE SUMMARY FORMS (one form for each defendant in the Program Group)
 - CRIMINAL HISTORY SUMMARY FORMS (one form for each defendant on the MASTER LIST)
 - RELEASE INFORMATION FORMS (send each form on which an entry has been made during the reporting period)
 - REARREST DATA SHEET (send each form on which an entry has been made during the reporting period)

The Lazar Institute
1800 M Street, N.W.
Washington, D.C. 20036
(202) 785-4320

Refer questions to:
Mary Toborg
Martin Sorin
Raymond Milkman

MASTER LIST

Period from _____ to _____
(month, day, year) (month, day, year)

[illegible]

EXHIBIT B
BIRTHDATES FOR PROGRAM GROUP

January 2	May 1	September 2
" 3	" 2	" 3
" 4	" 3	" 4
" 5	" 4	" 5
" 6	" 5	" 6
" 7	" 6	" 7
" 8	" 7	" 8
" 9	" 8	" 9
" 10	" 9	" 10
" 11	" 10	" 11
" 12	" 11	" 12
" 13	" 12	" 13
" 14	" 13	" 14
" 15	" 14	" 15
" 16	" 15	" 16
" 17	" 16	" 17
" 18	" 17	" 18
" 19	" 18	" 19
" 20	" 19	" 20
" 21	" 20	" 21
" 22	" 21	" 22
" 23	" 22	" 23
" 24	" 23	" 24
" 25	" 24	" 25
" 26	" 25	" 26
" 27	" 26	" 27
" 28	" 27	" 28
" 29	" 28	" 29
" 30	" 29	" 30
February 5	June 3	October 3
" 6	" 4	" 4
" 7	" 5	" 5
" 8	" 6	" 6
" 9	" 7	" 7
" 10	" 8	" 8
" 11	" 9	" 9
" 12	" 10	" 10
" 13	" 11	" 11
" 14	" 12	" 12
" 15	" 13	" 13
" 16	" 14	" 14
" 17	" 15	" 15
" 18	" 16	" 16
" 19	" 17	" 17
" 20	" 18	" 18
" 21	" 19	" 19
" 22	" 20	" 20
" 23	" 21	" 21
" 24	" 22	" 22
" 25	" 23	" 23
" 26	" 24	" 24
" 27	" 25	" 25
" 28	" 26	" 26
" 29	" 27	" 27
" 30	" 28	" 28
March 4	July 4	November 2
" 5	" 5	" 3
" 6	" 6	" 4
" 7	" 7	" 5
" 8	" 8	" 6
" 9	" 9	" 7
" 10	" 10	" 8
" 11	" 11	" 9
" 12	" 12	" 10
" 13	" 13	" 11
" 14	" 14	" 12
" 15	" 15	" 13
" 16	" 16	" 14
" 17	" 17	" 15
" 18	" 18	" 16
" 19	" 19	" 17
" 20	" 20	" 18
" 21	" 21	" 19
" 22	" 22	" 20
" 23	" 23	" 21
" 24	" 24	" 22
" 25	" 25	" 23
" 26	" 26	" 24
" 27	" 27	" 25
" 28	" 28	" 26
" 29	" 29	" 27
" 30	" 30	" 28
April 1	August 1	December 2
" 2	" 2	" 3
" 3	" 3	" 4
" 4	" 4	" 5
" 5	" 5	" 6
" 6	" 6	" 7
" 7	" 7	" 8
" 8	" 8	" 9
" 9	" 9	" 10
" 10	" 10	" 11
" 11	" 11	" 12
" 12	" 12	" 13
" 13	" 13	" 14
" 14	" 14	" 15
" 15	" 15	" 16
" 16	" 16	" 17
" 17	" 17	" 18
" 18	" 18	" 19
" 19	" 19	" 20
" 20	" 20	" 21
" 21	" 21	" 22
" 22	" 22	" 23
" 23	" 23	" 24
" 24	" 24	" 25
" 25	" 25	" 26
" 26	" 26	" 27
" 27	" 27	" 28
" 28	" 28	" 29
" 29	" 29	" 30
" 30	" 30	" 31

INTERVIEW FORM		EXHIBIT C	
I.D. # _____		DATE OF ARREST _____	
NAME _____	OTHER NAMES _____	ARRESTED BY _____	
ADD _____	CHARGE 1 _____	2 _____	3 _____
LIVE _____	CAUSE 1 _____	2 _____	3 _____
W/WHOM _____	BOND 1 _____	2 _____	3 _____
HOW _____	COURT 1 _____	2 _____	3 _____
LONG _____ PH _____	TOTAL YEARS OF LOCAL RESIDENCE _____		
PRIOR RES _____	HOW LONG _____		
CHILDREN _____	FAMILY SUPPORT _____		
DOB _____	AGE _____	SEX _____	RACE _____
ED _____	SS# _____	DL# _____	MLTRY _____
ARREST RECORD			
Felonv		Misdemeanor	Juvenile
1 _____	_____	_____	_____
2 _____	_____	_____	_____
3 _____	_____	_____	_____
4 _____	_____	_____	_____
DISPOSITIONS			
1 _____	_____	_____	_____
2 _____	_____	_____	_____
3 _____	_____	_____	_____
4 _____	_____	_____	_____
PROBATION/PAROLE INFO _____			
EMPLOYMENT			
EMPLOYER _____	SUPV _____	HOW LONG _____	
WAGE _____	SKILLS _____	ADD OR PH _____	
IF UNEMPLOYED, WHY & HOW LONG? _____			
PAST EMPLOYER INFO _____			
INCOME & EXPENSES			
SPOUSE'S EMPLOYER _____	HOW LONG _____	INCOME _____	
OTHER SOURCES OF INCOME _____	DEBTS _____		
HOME: OWNED _____	RENT \$ _____	BUY \$ _____	PROPERTY OWNED _____
AMOUNT \$ _____			
REFERENCES			
Name _____	Relation _____	Address _____	Phone _____
_____	_____	_____	_____
_____	_____	_____	_____
_____	_____	_____	_____
INTERVIEWER _____	DATE, TIME _____	BACKGROUND CHECKED BY _____	DATE _____
ATTORNEY _____	OFFICE PH _____	HOME PH _____	

EXHIBIT D

I.D. # _____ NAME _____

TYPE OF VERIFICATION (CHECK ONE):

_____ Telephoned references only
 _____ Checked files only
 _____ Telephoned references and checked files
 _____ Did not verify anything
 _____ Other (please specify) _____

NUMBER OF REFERENCES CHECKED:

_____ One
_____ Two
_____ Three
_____ Four
_____ More than four

RECOMMENDED FOR RELEASE?

_____ Yes
No

BOND AMOUNT:

§ 101.101. The following shall be the rules and regulations of the Board of Directors:

DATE OF RELEASE:

(month, day, year)

RETURN FORM TO:

The Lazar Institute
1800 M Street, N.W.
Washington, D.C. 20036

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SHORT INTERVIEW FORM

EXHIBIT E

I.D. # _____ DATE OF ARREST _____
NAME XXXXXXXXXXXXXXXXXXXXXXXXXXXX ARRESTED
ADD _____ CHARGE 1 _____ 2 _____ 3 _____
LIVE _____ CAUSE 1 _____ 2 _____ 3 _____
W/WHOM _____ BOND 1 _____ 2 _____ 3 _____
HOW
LONG RHXXXXXXXXXXXXXXX COURT XXXXXXXXXXXXXXXXXXXX XXXXXXXXXXXXXXXXXX XXXXXXXXXXXXXXXXXX
PRIOR RES _____ HOW LONG _____ TOTAL YEARS OF
LOCAL RESIDENCE _____
CHILDREN _____ FAMILY SUPPORT _____
MAR.
DOB _____ AGE _____ SEX _____ RACE _____ STATUS _____ XXXXXXXXXXXXXXXXXXXX
ED _____ SS# _____ XXXXXXXXXXXXXXXXXXXX

	<u>Felony</u>	<u>ARREST RECORD</u> <u>Misdemeanor</u>	<u>Juvenile</u>
1			
2			
3			
4			

DISPOSITIONS

1			
2			
3			
4			

PROBATION/PAROLE INFO

EMPLOYMENT

EMPLOYER _____ SOCIAL SECURITY NUMBER _____ HOW LONG _____
WAGE _____ SKILLS _____ ADDRESS _____
IF UNEMPLOYED,
WHY & HOW LONG? _____
PAST
EMPLOYER INFO _____

INCOME & EXPENSES

INCOME & EXPENSES

DEDUCTIONS FOR CHARITABLE CONTRIBUTIONS XXXXXXXXXXXXXXXXXXXXXXXXXX
OTHER SOURCES OF INCOME DEDUCTIONS
NOT OWNED RENTALS EXCESS OF DEPRECIATION ON PROPERTY OWNED
ANNUAL

REFERENCES:

REFERENCES

[illegible]

LIVE W/WHOM

Parent
Spouse
Guardian
Other relative
Non-family person
Alone

PROBATION/PAROLE INFORMATION

On probation at time of arrest
On parole at time of arrest
Outstanding warrant at time of arrest
On pretrial release for another
charge at time of arrest

EMPLOYER

Include housewife or student,
where applicable

FAMILY SUPPORT

Supports family
Child support only
None

WAGE

Record so that weekly earnings can be
computed later on

RACE

Negro
White, Mexican-American
White, Other
Oriental
Other

SKILLS

Record so that occupation can be
determined later on:

Professional/managerial
Sales/retail
Clerical
Craftsman/foreman
Operatives
Laborers (except farm)
Farm workers
Service workers
Private household workers
Armed Forces

MARITAL STATUS

Married, living with spouse
Separated (married, not
living with spouse)
Divorced
Widowed
Never married

PAST EMPLOYER INFO

Include skills, so that occupation
can be determined later on
(see above item)

EDUCATION

Less than high school
Some high school
Technical or vocational
school without high
school graduation
High school graduate
Technical or vocational
school in addition to
high school graduation
Some college
College graduate or more

OTHER SOURCES OF INCOME

Show public assistance separately

CRIMINAL HISTORY SUMMARY

I.D.# _____ NAME _____

JUVENILE ARREST RECORD? ____ YES ____ NO ____ DON'T KNOW

AGE AT FIRST ADULT ARREST _____

NUMBER OF PRIOR ADULT ARRESTS _____

NUMBER OF PRIOR ADULT CONVICTIONS _____

TOTAL NUMBER OF MONTHS INCARCERATED AS PART OF SENTENCES SERVED FOR
ADULT CONVICTIONS _____ MONTHS

DATE OF MOST RECENT PREVIOUS ARREST _____

DATE OF RELEASE FROM MOST RECENT INCARCERATION _____

NUMBER OF MONTHS OF LAST INCARCERATION _____ MONTHS

MOST FREQUENT CHARGE IN TOTAL RECORD _____

HAS DEFENDANT FAILED TO APPEAR FOR COURT IN THE PAST?

____ DON'T KNOW ____ NO ____ YES ____ NUMBER OF TIMES

RETURN FORM TO: The Lazar Institute
1800 M Street, N.W.
Washington, D.C. 20036

RELEASE INFORMATION FORM

NAME	STUDY NUMBER	DATE OF RELEASE	BOND AMOUNT	ESTIMATED % PAID	TYPE OF RELEASE				
					PTR	BONDS- MAN	ATTY	CASH	OTHER (SPECIFY)

09-0

REARREST DATA SHEETEXHIBIT I

NAME	STUDY NO.	CAUSE NO.	DATE OF REARREST	CHARGE (IF KNOWN)

APPENDIX E
SUPPLEMENTARY TABLES

APPENDIX E
SUPPLEMENTARY TABLES

- E.1—Comparability of Experimental and Control Groups, Tucson Felony Experiment
- E.2—Days from Arrest to Release, Tucson Felony Experiment
- E.3—Bond Amounts, Tucson Felony Experiment
- E.4—Comparability of Released Defendants in Experimental and Control Groups, Tucson Felony Experiment
- E.5—Comparability of Experimental and Control Groups, Tucson Misdemeanor Experiment
- E.6—Bond Amounts, Tucson Misdemeanor Experiment
- E.7—Comparability of Released Defendants in Experimental and Control Groups, Tucson Misdemeanor Experiment (Notification Analysis)
- E.8—Comparability of Experimental and Control Groups, Baltimore Analysis of Release Impact
- E.9—Days from Arrest to Release, Baltimore Experiment
- E.10—Comparability of Experimental and Control Groups, Baltimore Analysis of Supervision Impact
- E.11—Defendant Outcomes by Point Score Totals
- E.12—Comparability of Experimental and Control Groups, Lincoln Experiment
- E.13—Days from Arrest to Release, Lincoln Experiment
- E.14—Comparability of Released Defendants in the Experimental and Control Groups, Lincoln Experiment
- E.15—Comparability of Experimental and Control Groups, Beaumont-Port Arthur Experiment
- E.16—Days from Arrest to Release, Beaumont-Port Arthur Experiment
- E.17—Comparability of Released Defendants in the Experimental and Control Groups, Beaumont-Port Arthur Experiment

TABLE E.1
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
TUCSON FELONY EXPERIMENT

Characteristic	Experimental Group (n = 130)		Control Group (n = 165)	
	Number	Percent	Number	Percent
<u>Part I: Community Ties</u>				
A. Local Residence				
Defendant is local resident	109	83%	142	89%
Defendant is <u>not</u> local resident	18	14%	17	11%
TOTAL	127	100%	159	100%
B. Years of Local Residence				
Mean number of years	12.8		14.0	
C. Months at Present Address				
Mean number of months	57.6		70.0	
D. Marital Status				
Married	22	19%	28	18%
Separated, divorced, widow(er)ed	18	15%	33	21%
Single	79	66%	93	60%
TOTAL	119	100%	154	100%
E. Family Support				
Supports nuclear family	36	30%	49	31%
Other	84	70%	109	69%
TOTAL	120	100%	158	100%
F. Living Arrangement				
Lives with parent or guardian	40	33%	56	37%
Lives with spouse	22	18%	27	18%
Lives with relatives	15	12%	15	10%
Lives with non-family person	24	20%	34	22%
Lives alone	21	17%	20	13%
TOTAL	122	100%	152	100%
G. Employment Status				
Employed or substitutes	65	51%	94	58%
Unemployed	63	49%	67	42%
TOTAL	128	100%	161	100%
H. Weekly Earnings (for employed defendants)				
Mean weekly earnings	\$169		\$202	
I. Occupation				
Professional and managerial	3	4%	6	6%
Sales and clerical	8	11%	13	13%
Craftspersons and operatives	28	40%	35	35%
Laborers	14	20%	29	28%
Service and other	18	25%	18	18%
TOTAL	71	100%	101	100%

(continued)

TABLE E.1 (CONTINUED)
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
TUCSON FELONY EXPERIMENT

Characteristic	Experimental Group (n = 130)		Control Group (n = 165)	
	Number	Percent	Number	Percent
<u>Part II: Criminality</u>				
A. Current Charge				
Part I	65	50%	79	48%
Part II	65	50%	86	52%
TOTAL	130	100%	165	100%
Crimes against persons	20	16%	37	22%
Economic crimes	55	43%	68	41%
Drug crimes	25	19%	28	17%
Crimes against public morality	6	5%	5	3%
Crimes against public order	1	1%	3	2%
Other crimes	22	17%	24	15%
TOTAL	129	100%	165	100%
B. Number of Prior Arrests				
Mean number of prior arrests	2.1		2.0	
C. Number of Prior Convictions				
Mean number of prior convictions	1.2		1.1	
D. Age at First Adult Arrest				
Mean number of years	21.9		21.3	
<u>Part III: Demographic Characteristics</u>				
A. Age at Arrest				
Mean number of years	28.0		27.3	
B. Ethnicity				
White	79	61%	94	58%
Minority	50	39%	68	42%
TOTAL	129	100%	162	100%
C. Sex				
Male	116	89%	144	88%
Female	14	11%	20	12%
TOTAL	130	100%	164	100%
<u>Part IV. Other</u>				
A. Education				
Less than high school	15	16%	32	25%
High school graduate	41	44%	47	36%
More than high school	37	40%	51	39%
TOTAL	93	100%	130	100%

TABLE E.2
DAYS FROM ARREST TO RELEASE, TUCSON FELONY EXPERIMENT

NUMBER OF DAYS	EXPERIMENTAL GROUP (n=130)		CONTROL GROUP (n=165)	
	Number	Percent	Number	Percent
0-1	77	69%	94	67%
2-3	5	4%	7	5%
4-7	4	4%	6	4%
8-14	4	4%	13	9%
15-28	7	6%	5	4%
29-56	7	6%	0	0%
57-84	2	2%	3	2%
Missing Information	6	5%	12	9%
Subtotal, released defendants	112	100%	140	100%
Defendants detained until final case disposition	18		25	
TOTAL DEFENDANTS	130		165	

TABLE E.3
BOND AMOUNTS, TUCSON FELONY EXPERIMENT

BOND AMOUNT	DEFENDANTS WHO MADE BAIL				DEFENDANTS WHO DID NOT MAKE BAIL			
	Experimental Group		Control Group		Experimental Group		Control Group	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
Under \$250	1	7%	0	0%	0	0%	1	6%
\$251-\$500	0	0%	1	4%	2	12%	1	6%
\$501-\$1,000	3	20%	3	13%	3	19%	0	0%
\$1,001-\$1,500	6	40%	8	35%	3	19%	5	29%
\$1,501-\$2,000	0	0%	0	0%	1	6%	0	0%
\$2,001-\$2,999	2	13%	2	9%	2	12%	4	24%
\$3,000-\$4,999	1	7%	5	22%	3	19%	1	6%
\$5,000-\$9,999	1	7%	3	13%	2	12%	3	18%
\$10,000 or more	1	7%	1	4%	0	0%	2	12%
TOTAL	15	100%	23	100%	16	100%	17	100%

TABLE E.4
COMPARABILITY OF RELEASED DEFENDANTS IN
EXPERIMENTAL AND CONTROL GROUPS, TUCSON FELONY EXPERIMENT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n = 112)		Control Group (n = 140)	
	Number	Percent	Number	Percent
<u>Part I. Community Ties</u>				
A. Local Residence				
Defendant is local resident	96	88%	125	91%
Defendant is <u>not</u> local resident	13	12%	12	9%
TOTAL	109	100%	137	100%
B. Years of Local Residence				
Mean number of years	13.3		14.8	
C. Months at Present Address				
Mean number of months	58.6		75.7	
D. Marital Status				
Married	18	18%	26	20%
Separated, divorced, widow(er)ed	14	14%	30	23%
Single	69	68%	76	57%
TOTAL	101	100%	132	100%
E. Family Support				
Supports nuclear family	31	30%	44	33%
Other	71	70%	91	67%
TOTAL	102	100%	135	100%
F. Living Arrangement				
Lives with parent or guardian	35	34%	51	39%
Lives with spouse	18	17%	25	19%
Lives with relatives	15	14%	12	9%
Lives with non-family person	21	20%	26	20%
Lives alone	15	14%	17	13%
TOTAL	104	100%	131	100%
G. Employment Status				
Employed or substitutes	60	54.5%	88	63%
Unemployed	50	45.5%	51	37%
TOTAL	110	100.0%	139	100%
H. Occupation				
Professional and Managerial	2	3%	6	6%
Sales and Clerical	6	9%	13	14%
Craftspersons and Operatives	27	42%	32	34%
Laborers	13	20%	26	28%
Service and Other	16	25%	17	18%
TOTAL	64	100%	94	100%

(continued)

TABLE E.4 (CONTINUED)
COMPARABILITY OF RELEASED DEFENDANTS IN
EXPERIMENTAL AND CONTROL GROUPS, TUCSON FELONY EXPERIMENT

Characteristic	Experimental Group (n = 112)		Control Group (n = 140)	
	Number	Percent	Number	Percent
<u>Part II. Criminality</u>				
A. Current Charge				
Crimes against persons	16	14%	31	22%
Economic crimes	50	45%	55	39%
Drug crimes	23	21%	26	19%
Crimes against public morality	4	4%	4	3%
Crimes against public order	1	1%	3	2%
Other crimes	17	15%	21	15%
TOTAL	111	100%	140	100%
B. Number of Prior Arrests				
Mean number of prior arrests	1.9		1.8	
C. Number of Prior Convictions				
Mean number of prior convictions	1.1		1.0	
D. Age at First Adult Arrest				
Mean number of years	22.1		20.6	
<u>Part III. Demographic Characteristics</u>				
A. Age at Arrest				
Mean number of years	28.2		27.7	
B. Ethnicity				
White	70	63%	85	62%
Minority	41	37%	53	38%
TOTAL	111	100%	138	100%
C. Sex				
Male	98	87.5%	122	87%
Female	14	12.5%	18	13%
TOTAL	112	100%	140	100%
<u>Part IV. Other</u>				
A. Education				
Less than high school	32	40%	41	37%
High school graduate	31	40%	36	32%
More than high school	17	21%	35	32%
TOTAL	81	100%	112	100%

TABLE E.5
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
TUCSON MISDEMEANOR EXPERIMENT

Characteristic	Experimental Group (n = 224)		Control Group (n = 200)	
	Number	Percent	Number	Percent
Part I. Community Ties				
A. Local Residence				
Defendant is local resident	206	92%	181	90.5%
Defendant is <u>not</u> local resident	18	8%	19	9.5%
TOTAL	224	100%	200	100.0%
B. Years of Local Residence				
Mean number of years	17.6		15.6	
C. Months at Present Address				
Mean number of months	55.7		51.1	
D. Marital Status				
Married	45	21%	31	16%
Separated, divorced, widow(er)ed	64	31%	50	26%
Single	102	48%	111	58%
TOTAL	211	100%	192	100%
E. Family Support				
Supports nuclear family	55	26%	48	26%
Other	157	74%	136	74%
TOTAL	212	100%	184	100%
F. Living Arrangement				
Lives with parent or guardian	53	24%	50	26%
Lives with spouse	48	22%	32	17%
Lives with relatives	19	9%	21	11%
Lives with non-family person	49	22%	41	21%
Lives alone	52	23%	49	25%
TOTAL	221	100%	193	100%
G. Employment Status				
Employed or substitutes	146	66%	129	65%
Unemployed	76	34%	69	35%
TOTAL	222	100%	198	100%
H. Weekly Earnings				
\$100 or less	35	30%	34	32%
\$101—\$200	57	49%	49	46%
\$201 or more	24	21%	23	22%
TOTAL	116	100%	106	100%

(continued)

TABLE E.5 (CONTINUED)
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
TUCSON MISDEMEANOR EXPERIMENT

Characteristic	Experimental Group (n = 224)		Control Group (n = 200)	
	Number	Percent	Number	Percent
I. Occupation				
Professional and managerial	17	9%	12	7%
Sales and clerical	13	7%	15	9%
Craftspersons and operatives	76	42%	60	36%
Laborers	42	23%	55	32%
Service and other	34	19%	27	16%
TOTAL	182	100%	169	100%
Part II. Criminality				
A. Current Charge				
Part I	25	11%	27	13.5%
Part II	199	89%	173	86.5%
TOTAL	224	100%	200	100.0%
Crimes against persons	10	4%	14	7%
Economic crimes	24	11%	26	13%
Drug crimes	1	0%	1	1%
Crimes against public morality	4	2%	6	3%
Crimes against public order	161	72%	130	65%
Other crimes	24	11%	22	11%
TOTAL	224	100%	199	100%
B. Number of Prior Arrests				
Mean number of prior arrests	2.3		2.5	
C. Number of Prior Convictions				
Mean number of prior convictions	1.4		1.5	
D. Criminal Justice System (CJS) Status at Time of Arrest				
Involved with CJS	38	26%	39	32%
Not involved with CJS	106	74%	84	68%
TOTAL	144	100%	123	100%
E. Age at First Adult Arrest				
Mean number of years	24.6		22.9	
Part III. Demographic Characteristics				
A. Age at Arrest				
Mean number of years	31.5		30.6	
B. Ethnicity				
White	108	51%	110	58%
Minority	102	49%	81	42%
TOTAL	210	100%	191	100%

(continued)

TABLE E.5 (CONTINUED)
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
TUCSON MISDEMEANOR EXPERIMENT

Characteristic	Experimental Group (n = 224)		Control Group (n = 200)	
	Number	Percent	Number	Percent
C. Sex				
Male	202	91%	188	94%
Female	21	9%	12	6%
TOTAL	223	100%	200	100%
Part IV. Other				
A. Education				
Less than high school	36	19%	29	18%
High school graduate	73	40%	71	44%
More than high school	76	41%	61	38%
TOTAL	185	100%	161	100%

TABLE E.6
BOND AMOUNTS, TUCSON MISDEMEANOR EXPERIMENT

Bond Amount	Defendants Who Made Bail				Defendants Who Did NOT Make Bail			
	Experimental Group		Control Group		Experimental Group		Control Group	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent
Under \$ 250	19	43%	18	50%	59	83%	54	84%
\$251-\$500	22	50%	18	50%	11	16%	9	14%
\$501-\$1,000	3	7%	0	0%	1	1%	1	2%
TOTAL	44	100%	36	100%	71	100%	64	100%

1.12
TABLE E.7
COMPARABILITY OF RELEASED DEFENDANTS IN EXPERIMENTAL
AND CONTROL GROUPS, TUCSON MISDEMEANOR EXPERIMENT
(Notification Analysis)

CHARACTERISTIC	Experimental Group (n=160)		Control Group (n=127)	
	Number	Percent	Number	Percent
<u>Part I. Community Ties</u>				
A. Local Residence				
Defendant is local resident	149	93%	123	97%
Defendant is <u>not</u> local resident	11	7%	4	3%
TOTAL	160	100%	127	100%
B. Years of Local Residence				
Mean number of years	16.8		16.3	
C. Months At Present Address				
Mean number of months	55.9		52.7	
D. Marital Status				
Married	38	24%	22	18%
Separated, divorced, widow(er)ed	44	28%	29	24%
Single	75	48%	71	58%
TOTAL	157	100%	122	100%
E. Family Support				
Supports nuclear family	44	29%	36	30%
Other	108	71%	84	70%
TOTAL	152	100%	120	100%
F. Living Arrangement				
Lives with parent or guardian	44	28%	31	25%
Lives with spouse	39	25%	24	19%
Lives with relatives	12	8%	15	12%
Lives with non-family person	37	24%	32	25%
Lives alone	23	15%	24	19%
TOTAL	155	100%	126	100%
G. Employment Status				
Employed or substitutes	120	75%	88	70%
Unemployed	40	25%	38	30%
TOTAL	160	100%	126	100%
ii. Weekly Earnings				
\$100 or less	34	29%	34	33%
\$101-\$200	61	52%	45	43%
\$201 or more	22	19%	25	24%
TOTAL	117	100%	104	100%
I. Occupation				
Professional and managerial	11	8%	12	10%
Sales and clerical	11	8%	7	6%
Craftspersons and operatives	69	51%	40	35%
Laborers	24	18%	31	27%
Service and other	21	15%	25	22%
TOTAL	136	100%	115	100%

(continued)

TABLE E 7 (CONTINUED)
COMPARABILITY OF RELEASED DEFENDANTS IN EXPERIMENTAL
AND CONTROL GROUPS, TUCSON MISDEMEANOR EXPERIMENT
(Notification Analysis)

CHARACTERISTIC	Experimental Group (n=160)		Control Group (n=127)	
	Number	Percent	Number	Percent
<u>Part II. Criminality</u>				
A. Current Charge				
Crimes against persons	8	5%	6	5%
Economic crimes	15	9%	11	9%
Drug crimes	1	1%	1	1%
Crimes against public morality	4	3%	3	2%
Crimes against public order	120	75%	92	73%
Other crimes	12	7%	13	10%
TOTAL	160	100%	126	100%
B. Number of Prior Arrests				
Mean number of prior arrests	2.3		2.2	
C. Number of Prior Convictions				
Mean number of prior convictions	1.3		1.1	
D. Criminal Justice System (CJS)				
Status at Time of Arrest				
Involved with CJS	30	23%	29	28%
Not involved with CJS	101	77%	76	72%
TOTAL	131	100%	105	100%
E. Age at First Adult Arrest				
Mean number of years	22.9		24.4	
<u>Part III. Demographic Characteristics</u>				
A. Age at Arrest				
Mean number of years	30.3		30.1	
B. Ethnicity				
White	73	47%	63	53%
Minority	82	53%	56	47%
TOTAL	155	100%	119	100%
C. Sex				
Male	139	87%	118	93%
Female	21	13%	9	7%
TOTAL	160	100%	127	100%
<u>Part IV. Other</u>				
A. Education				
Less than high school	29	22%	22	20%
High school graduate	46	35%	52	48%
More than high school	56	43%	35	32%
TOTAL	131	100%	109	100%

TABLE E.8
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
BALTIMORE ANALYSIS OF RELEASE IMPACT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n= 148)		Control Group (n=158)	
	Number	Percent	Number	Percent
<u>Part I. Community Ties</u>				
A. Local Residence				
Defendant is local resident	140	95%	152	97%
Defendant is <u>not</u> local resident	7	5%	5	3%
TOTAL	147	100%	157	100%
B. Years of Local Residence				
Mean number of years	20.4		21.7	
C. Months At Present Address				
Mean number of months	15.0		22.7	
D. Marital Status				
Married	7	5%	8	5%
Separated, divorced, widow(er)ed	43	29%	49	31%
Single	97	66%	101	64%
TOTAL	147	100%	158	100%
E. Family Support				
Supports nuclear family	23	16%	30	19%
Other	125	84%	126	81%
TOTAL	148	100%	156	100%
F. Living Arrangement				
Lives with parent or guardian	24	17%	25	16%
Lives with spouse	7	5%	7	5%
Lives with relatives	35	24%	28	18%
Lives with non-family person	52	36%	64	41%
Lives alone	26	18%	31	20%
TOTAL	144	100%	155	100%
G. Employment Status				
Employed or substitutes	70	47%	71	45%
Unemployed	78	53%	87	55%
TOTAL	148	100%	158	100%

(continued)

TABLE E.8 (CONTINUED)
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
BALTIMORE ANALYSIS OF RELEASE IMPACT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n= 148)		Control Group (n=158)	
	Number	Percent	Number	Percent
H. Weekly Earnings				
\$100 or less	9	17%	9	16%
\$101—\$200	34	63%	41	71%
\$201 or more	11	20%	8	14%
TOTAL	54	100%	58	100%
I. Occupation				
Professional and managerial	2	2%	9	7%
Sales and clerical	11	8%	8	6%
Craftspersons and operatives	47	35%	36	27%
Laborers	35	26%	41	30%
Service and other	40	30%	42	31%
TOTAL	135	100%	136	100%
Part II. Criminality				
A. Current Charge				
Part I	54	36.5%	35	22%
Part II	94	63.5%	123	78%
TOTAL	148	100%	158	100%
Crimes against persons	25	17%	36	23%
Economic crimes	52	35%	34	22%
Drug crimes	21	14%	29	18%
Crimes against public morality	12	8%	16	10%
Crimes against public order	26	18%	28	18%
Other crimes	12	8%	15	10%
TOTAL	148	100%	158	100%
B. Number or Prior Arrests				
Mean number of prior arrests	2.5		2.7	
C. Number of Prior Convictions				
Mean number of prior convictions	1.5		1.7	
D. Age at First Adult Arrest				
Mean number of years	22.6		24.1	
Part III. Demographic Characteristics				
A. Age at Arrest				
Mean number of years	26.9		28.2	

(continued)

TABLE E.8 (CONTINUED)
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
BALTIMORE ANALYSIS OF RELEASE IMPACT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n= 148)		Control Group (n=158)	
	Number	Percent	Number	Percent
B. Ethnicity				
White	56	38%	60	38%
Minority	92	62%	98	62%
TOTAL	148	100%	158	100%
C. Sex				
Male	118	80%	129	82%
Female	30	20%	29	18%
TOTAL	148	100%	158	100%
Part IV. Other				
A. Education				
Less than high school	83	58%	90	59%
High school graduate	47	33%	40	26%
More than high school	13	9%	22	14%
TOTAL	143	100%	152	100%

TABLE E.9
DAYS FROM ARREST TO RELEASE,
BALTIMORE EXPERIMENT

NUMBER OF DAYS	EXPERIMENTAL GROUP (n = 148)		CONTROL GROUP (n = 158)	
	Number	Percent	Number	Percent
0-1	128	89%	123	85%
2-3	6	4%	6	4%
4-7	3	2%	3	2%
8-14	1	1%	3	2%
15-28	0	0%	1	1%
29-56	0	0%	1	1%
57-84	0	0%	0	0%
85-112	0	0%	2	2%
Missing information	6	4%	6	4%
Subtotal, released defendants	144	100%	145	100%
Defendants detained until final case disposition	4		13	
TOTAL defendants	148		158	

TABLE E.10
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
BALTIMORE ANALYSIS OF SUPERVISION IMPACT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n= 171)		Control Group (n=152)	
	Number	Percent	Number	Percent
<u>Part I. Community Ties</u>				
A. Local Residence				
Defendant is local resident	165	97%	142	95%
Defendant is <u>not</u> local resident	5	3%	8	5%
TOTAL	170	100%	150	100%
B. Years of Local Residence				
Mean number of years	22.4		21.1	
C. Months at Present Address				
Mean number of months	46.0		33.5	
D. Marital Status				
Married	13	8%	14	9%
Separated, divorced, widow(er)ed	49	29%	45	30%
Single	108	64%	90	60%
TOTAL	170	100%	149	100%
E. Family Support				
Supports nuclear family	36	21%	31	21%
Other	135	79%	117	79%
TOTAL	171	100%	148	100%
F. Living Arrangement				
Lives with parent or guardian	45	27%	34	23%
Lives with spouse	12	7%	15	10%
Lives with relatives	28	17%	21	14%
Lives with non-family person	52	31%	55	37%
Lives alone	32	19%	23	16%
TOTAL	169	100%	148	100%
G. Employment Status				
Employment or substitutes	93	54%	75	50%
Unemployed	78	46%	76	50%
TOTAL	171	100%	151	100%

(continued)

TABLE E.10 (CONTINUED)
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
BALTIMORE ANALYSIS OF SUPERVISION IMPACT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n= 171)		Control Group (n=152)	
	Number	Percent	Number	Percent
H. Weekly Earnings				
\$100 or less	16	21%	9	17%
\$101—\$200	46	61%	29	55%
\$201 or more	13	17%	15	28%
TOTAL	75	100%	53	100%
I. Occupation				
Professional and managerial	3	2%	6	5%
Sales and clerical	10	7%	17	13%
Craftspersons and operatives	53	35%	43	32%
Laborers	33	22%	30	23%
Service and other	51	34%	37	28%
TOTAL	150	100%	133	100%
<u>Part II. Criminality</u>				
A. Current Charge				
Part I	56	33%	44	29%
Part II	115	67%	108	71%
TOTAL	171	100%	152	100%
Crimes against persons	39	23%	31	20%
Economic crimes	47	28%	43	28%
Drug crimes	26	15%	28	18%
Crimes against public morality	29	17%	27	18%
Crimes against public order	14	8%	13	9%
Other crimes	16	9%	10	7%
TOTAL	171	100%	152	100%
B. Number of Prior Arrests				
Mean number of prior arrests	2.1		2.0	
C. Number of Prior Convictions				
Mean number of prior convictions	1.2		1.1	
D. Age at First Adult Arrest				
Mean number of years	23.2		24.1	
<u>Part III. Demographic Characteristics</u>				
A. Age at Arrest				
Mean number of years	28.2		29.1	

(continued)

TABLE E.10 (CONTINUED)
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
BALTIMORE ANALYSIS OF SUPERVISION IMPACT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n= 171)		Control Group (n=152)	
	Number	Percent	Number	Percent
B. Ethnicity				
White	54	32%	62	41%
Minority	117	68%	90	59%
TOTAL	171	100%	152	100%
C. Sex				
Male	128	75%	115	76%
Female	43	25%	37	24%
TOTAL	171	100%	152	100%
Part IV. Other				
A. Education				
Less than high school	99	60%	83	56%
High school graduate	46	28%	36	25%
More than high school	20	12%	28	19%
TOTAL	165	100%	147	100%

TABLE E-11
DEFENDANT OUTCOMES BY POINT SCORE TOTALS

Outcome	2 or Fewer Points		3-4 Points		5 Points		6 or More Points		TOTAL	
	Number	Percent	Number	Percent	Number	Percent	Number	Percent	Number	Percent
Released	45	87%	102	95%	62	94%	55	100%	264	94%
Not Released	7	13%	5	5%	4	6%	0	0%	16	6%
TOTAL	52	100%	107	100%	66	100%	55	100%	280	100%
Failed To Appear	35	17%	9	20%	5	17%	6	14%	55	17%
Did Not Fail to Appear	169	83%	37	80%	25	83%	37	86%	268	83%
TOTAL	204	100%	46	100%	30	100%	43	100%	323	100%
Rearrested Pretrial	18	9%	6	13%	0	0%	3	7%	27	8%
Not Rearrested Pretrial	186	91%	40	87%	30	100%	40	93%	296	92%
TOTAL	204	100%	46	100%	30	100%	43	100%	323	100%
Convicted of Pre-trial Rearrest	13	6%	1	2%	0	0%	1	2%	15	5%
Not Convicted of Pretrial Rearrest	191	94%	45	98%	30	100%	42	98%	308	95%
TOTAL	204	100%	46	100%	30	100%	43	100%	323	100%

TABLE E.12
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
LINCOLN EXPERIMENT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n= 94)		Control Group (n= 36)	
	Number	Percent	Number	Percent
<u>Part I. Community Ties</u>				
A. Local Residence				
Defendant is local resident	90	97%	33	92%
Defendant is <u>not</u> local resident	3	3%	3	8%
TOTAL	93	100%	36	100%
B. Years of Local Residence	13.6		15.0	
Mean number of years				
C. Months at Present Address	24.1		37.9	
Mean number of months				
D. Marital Status				
Married	12	13%	9	27%
Separated, divorced, widow(er)ed	22	24%	12	35%
Single	57	63%	13	38%
TOTAL	91	100%	34	100%
E. Family support				
Supports nuclear family	11	15.5%	6	19%
Other	60	84.5%	26	81%
TOTAL	71	100.0%	32	100%
F. Living Arrangement				
Lives with parent or guardian	15	17%	2	6%
Lives with spouse	11	13%	9	27%
Lives with relatives	10	11%	3	9%
Lives with non-family person	29	33%	6	18%
Lives alone	23	26%	14	41%
TOTAL	88	100%	34	100%
G. Employment Status				
Employed or substitutes	64	70%	20	59%
Unemployed	27	30%	14	41%
TOTAL	91	100%	34	100%
H. Public Assistance Status				
On public assistance	11	15%	3	10%
Not on public assistance	63	85%	27	90%
TOTAL	74	100%	30	100%

(continued)

TABLE E.12 (CONTINUED)
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
LINCOLN EXPERIMENT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n= 94)		Control Group (n= 36)	
	Number	Percent	Number	Percent
I. Occupation				
Professional and managerial	2	3%	0	0%
Sales and clerical	2	3%	1	4%
Craftspersons and operatives	13	22%	12	44%
Laborers	26	43%	12	44%
Service and other	17	28%	2	7%
TOTAL	60	100%	27	100%
<u>Part II. Criminality</u>				
A. Current Charge				
Part I	8	8.5%	1	3%
Part II	86	91.5%	35	97%
TOTAL	94	100.0%	36	100%
Crimes against persons	4	4%	2	6%
Economic crimes	11	12%	2	6%
Drug crimes	3	3%	1	3%
Crimes against public morality	19	20%	5	14%
Crimes against public order	44	47%	17	47%
Other crimes	13	14%	9	25%
TOTAL	94	100%	36	100%
B. Number of Prior Arrests				
Mean number of prior arrests	1.8		2.3	
C. Number of Prior Convictions				
Mean number of prior convictions	1.1		1.9	
D. Criminal Justice System (CJS) Status at Time of Arrest				
Involved with CJS	4	4%	1	3%
Not involved with CJS	86	96%	31	97%
TOTAL	90	100%	32	100%
E. Age at First Adult Arrest				
Mean number of years	25.1		28.0	
<u>Part III. Demographic Characteristics</u>				
A. Age at Arrest ^a				
Mean number of years	30.3		36.0	

(continued)

TABLE E.12 (CONTINUED)
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
LINCOLN EXPERIMENT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n= 94)		Control Group (n=36)	
	Number	Percent	Number	Percent
B. Ethnicity				
White	44	73%	20	71%
Minority	16	27%	8	29%
TOTAL	60	100%	28	100%
C. Sex				
Male	83	89%	32	89%
Female	10	11%	4	11%
TOTAL	93	100%	36	100%
Part IV. Other				
A. Education				
Less than high school	34	40%	14	44%
High school graduate	23	27%	13	41%
More than high school	27	32%	5	16%
TOTAL	84	100%	32	100%
^a Significant at the .03 level.				

TABLE E.13
DAYS FROM ARREST TO RELEASE,
LINCOLN EXPERIMENT

NUMBER OF DAYS	EXPERIMENTAL GROUP (n=94)		CONTROL GROUP (n=36)	
	Number	Percent	Number	Percent
0-1	56	78%	12	71%
2-3	6	8%	0	0%
4-7	2	3%	2	12%
8-14	0	0%	1	6%
15-28	0	0%	0	0%
29-56	1	1%	0	0%
57-84	0	0%	0	0%
85-112	0	0%	0	0%
113-140	1	1%	0	0%
Missing Information	6	8%	2	12%
Subtotal, released defendants	72	100%	17	100%
Defendants detained until final case disposition	22		19	
TOTAL defendants	94		36	

CONTINUED

5 OF 6

TABLE E.14
COMPARABILITY OF RELEASED DEFENDANTS IN THE
EXPERIMENTAL AND CONTROL GROUPS, LINCOLN EXPERIMENT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n= 72)		Control Group (n= 17)	
	Number	Percent	Number	Percent
<u>Part I. Community Ties</u>				
A. Local Residence				
Defendant is local resident	70	99%	17	100%
Defendant is <u>not</u> local resident	1	1%	0	0%
TOTAL	71	100%	17	100%
B. Years of Local Residence				
Mean number of years	13.8		17.1	
C. Months at present address				
Mean number of months	24.1		49.5	
D. Marital Status ^a				
Married	10	14%	7	41%
Separated, divorced, widow(er)ed	19	27%	6	35%
Single	41	59%	4	24%
TOTAL	70	100%	17	100%
E. Family Support				
Supports nuclear family	10	17%	5	31%
Other	49	83%	11	69%
TOTAL	59	100%	16	100%
F. Living Arrangement ^b				
Lives with parent or guardian	11	16%	1	6%
Lives with spouse	9	13%	7	41%
Lives with relatives	9	13%	1	6%
Lives with non-family person	24	35%	1	6%
Lives alone	15	22%	7	41%
TOTAL	68	100%	17	100%
G. Employment Status				
Employed or substitutes	50	71%	14	82%
Unemployed	20	29%	3	18%
TOTAL	70	100%	17	100%
H. Public Assistance Status				
On public assistance	8	14%	2	13%
Not on public assistance	49	86%	13	87%
TOTAL	57	100%	15	100%

(continued)

TABLE E.14 (CONTINUED)
COMPARABILITY OF RELEASED DEFENDANTS IN THE
EXPERIMENTAL AND CONTROL GROUPS, LINCOLN EXPERIMENT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n= 72)		Control Group (n= 17)	
	Number	Percent	Number	Percent
I. Occupation				
Professional and managerial	2	4%	0	0%
Sales and clerical	1	2%	1	8%
Craftspersons and operatives	11	23%	7	58%
Laborers	20	42%	3	25%
Service and other	14	29%	1	8%
TOTAL	48	100%	12	100%
<u>Part II. Criminality</u>				
A. Current Charge				
Crimes against persons	3	4%	2	12%
Economic crimes	10	14%	1	6%
Drug crimes	2	3%	0	0%
Crimes against public morality	14	19%	4	24%
Crimes against public order	33	46%	7	41%
Other crimes	10	14%	3	18%
TOTAL	72	100%	17	100%
B. Number of Prior Arrests				
Mean number of prior arrests	1.8		1.9	
C. Number of Prior Convictions				
Mean number of prior convictions	1.1		1.5	
D. Criminal Justice System (CJS) Status at Time of Arrest				
Involved with CJS	2	3%	0	0%
Not involved with CJS	67	97%	15	100%
TOTAL	69	100%	15	100%
E. Age at First Adult Arrest				
Mean number of years	24.3		24.9	
<u>Part III. Demographic Characteristics</u>				
A. Age at Arrest				
Mean number of years	29.5		30.8	
B. Ethnicity				
White	33	70%	11	79%
Minority	14	30%	3	21%
TOTAL	47	100%	14	100%

(continued)

TABLE E.14 (CONTINUED)
COMPARABILITY OF RELEASED DEFENDANTS IN THE
EXPERIMENTAL AND CONTROL GROUPS, LINCOLN EXPERIMENT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n = 72)		Control Group (n = 17)	
	Number	Percent	Number	Percent
C. Sex				
Male	63	89%	14	82%
Female	8	11%	3	18%
TOTAL	71	100%	17	100%
Part IV. Other				
A. Education				
Less than high school	25	38%	5	31%
High school graduate	14	21%	9	56%
More than high school	27	41%	2	13%
TOTAL	66	100%	16	100%
^a Significant at the .02 level.				
^b Significant at the .01 level.				

TABLE E-15
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
BEAUMONT-PORT ARTHUR EXPERIMENT

Note: Percentages may not add to 100% due to rounding.

Characteristic	Experimental Group (n = 132)		Control Group (n = 61)	
	Number	Percent	Number	Percent
Part I. Community Ties				
A. Local Residence				
Defendant is local resident	131	99%	61	100%
Defendant is not local resident	1	1%	0	0%
TOTAL	132	100%	61	100%
B. Years of Local Residence				
Mean number of years	19.1		18.4	
C. Months at Present Address				
Mean number of months	47.6		68.9	
D. Marital Status				
Married	34	26%	14	23%
Separated, divorced, widow(er)ed	35	27%	15	24%
Single	60	47%	32	53%
TOTAL	129	100%	61	100%
E. Family Support ^a				
Supports nuclear family	30	37%	7	16%
Other	52	63%	36	84%
TOTAL	82	100%	43	100%
F. Living Arrangement				
Lives with parent or guardian	48	36%	26	43%
Lives with spouse	32	24%	12	20%
Lives with relatives	27	21%	14	23%
Lives with non-family person	12	9%	6	10%
Lives alone	13	10%	2	3%
TOTAL	132	100%	60	100%
G. Employment Status				
Employed or substitutes	94	71%	39	64%
Unemployed	38	29%	22	36%
TOTAL	132	100%	61	100%
H. Weekly Earnings				
\$100 or less	9	11%	2	6%
\$101-\$200	38	46%	14	42%
\$201 or more	36	43%	17	52%
TOTAL	83	100%	33	100%

(continued)

TABLE E-15 (CONTINUED)
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
BEAUMONT-PORT ARTHUR EXPERIMENT

Characteristic	Experimental Group (n = 132)		Control Group (n = 61)	
	Number	Percent	Number	Percent
I. Public Assistance Status				
On public assistance	11	15.5%	1	3%
Not on public assistance	60	84.5%	33	97%
TOTAL	71	100.0%	34	100%
J. Occupation ^b				
Professional and managerial	10	10%	1	2%
Sales and clerical	12	12%	1	2%
Craftspersons and operatives	28	28%	10	21%
Laborers	34	34%	30	63%
Service and other	17	17%	6	13%
TOTAL	101	100%	36	100%
Part II. Criminality				
A. Current Charge				
Part I	57	43%	38	62%
Part II	75	57%	23	38%
TOTAL	132	100%	61	100%
Crimes against persons	14	11%	8	13%
Economic crimes	77	58%	34	56%
Drug crimes	14	11%	6	10%
Crimes against public morality	1	1%	0	0%
Crimes against public order	14	11%	11	18%
Other crimes	12	9%	2	3%
TOTAL	132	100%	61	100%
B. Number of Prior Arrests ^b	1.5		2.3	
Mean number of prior arrests				
C. Number of Prior Convictions	0.7		1.0	
Mean number of prior convictions				
D. Criminal Justice System (CJS)				
Status at Time of Arrest				
Involved with CJS	7	6%	3	5%
Not involved with CJS	119	94%	55	95%
TOTAL	126	100%	58	100%
E. Age at First Adult Arrest ^c	24.5		21.5	
Mean number of years				
Part III. Demographic Characteristics				
A. Age at Arrest	28.0		25.7	
Mean number of years				

(continued)

TABLE E-15 (CONTINUED)
COMPARABILITY OF EXPERIMENTAL AND CONTROL GROUPS,
BEAUMONT-PORT ARTHUR EXPERIMENT

Characteristic	Experimental Group (n = 132)		Control Group (n = 61)	
	Number	Percent	Number	Percent
B. Ethnicity				
White	64	48.5%	24	39%
Minority	68	51.5%	37	61%
TOTAL	132	100.0%	61	100%
C. Sex ^b				
Male	101	76.5%	57	93%
Female	31	23.5%	4	7%
TOTAL	132	100.0%	61	100%
Part IV. Other				
A. Education ^b				
Less than high school	51	39%	35	58%
High school graduate	45	35%	18	30%
More than high school	34	26%	7	12%
TOTAL	130	100%	60	100%

^aSignificant at the .02 level.^bSignificant at the .01 level.^cSignificant at the .04 level.

TABLE E.16
DAYS FROM ARREST TO RELEASE,
BEAUMONT-PORT ARTHUR EXPERIMENT

Number of Days	Experimental Group (n = 132)		Control Group (n = 61)	
	Number	Percent	Number	Percent
0 - 1	67	59%	11	31%
2 - 3	12	11%	5	14%
4 - 7	2	2%	11	31%
8 - 14	7	6%	3	9%
15 - 28	3	3%	4	12%
29 - 56	9	8%	0	0%
57 - 84	2	2%	0	0%
Missing information	11	10%	1	3%
Subtotal, released defendants	113	100%	35	100%
Defendants detained until final case disposition	19		26	
TOTAL defendants	132		61	

TABLE E-17
COMPARABILITY OF RELEASED DEFENDANTS IN THE
EXPERIMENTAL AND CONTROL GROUPS,
BEAUMONT-PORT ARTHUR EXPERIMENT

Characteristic	Experimental Group (n=113)		Control Group (n=35)	
	Number	Percent	Number	Percent
<u>Part I. Community Ties</u>				
A. Local Residence				
Defendant is local resident	112	99%	35	100%
Defendant is <u>not</u> local resident	1	1%	0	0%
TOTAL	113	100%	35	100%
B. Years of Local Residence				
Mean number of years	19.2		17.1	
C. Months at Present Address ^a				
Mean number of months	48.0		78.7	
D. Marital Status				
Married	29	26%	9	26%
Separated, divorced, widow(er)ed	32	29%	10	28%
Single	49	45%	16	46%
TOTAL	110	100%	35	100%
E. Family Support ^b				
Supports nuclear family	29	40%	5	19%
Other	43	60%	22	81%
TOTAL	72	100%	27	100%
F. Living Arrangement				
Lives with parent or guardian	40	35%	12	35%
Lives with spouse	28	25%	9	26%
Lives with relatives	21	19%	8	24%
Lives with non-family person	11	10%	4	12%
Lives alone	13	11%	1	3%
TOTAL	113	100%	34	100%
G. Employment Status				
Employed or substitutes	84	74%	25	71%
Unemployed	29	26%	10	29%
TOTAL	113	100%	35	100%
H. Weekly Earnings				
\$100 or less	8	11%	0	0%
\$101—\$200	33	45%	8	40%
\$201 or more	32	44%	12	60%
TOTAL	73	100%	20	100%
I. Public Assistance Status				
On public assistance	10	16%	0	0%
Not on public assistance	51	84%	24	100%
TOTAL	61	100%	24	100%

TABLE E-17 (CONTINUED)

Characteristic	Experimental Group (n =113)		Control Group (n=35)	
	Number	Percent	Number	Percent
J. Occupation ^c				
Professional and managerial	10	11%	1	4%
Sales and clerical	12	14%	0	0%
Craftspersons and operatives	24	28%	8	30%
Laborers	24	28%	16	59%
Service and other	17	19%	2	7%
TOTAL	87	100%	27	100%
Part II. Criminality				
A. Current Charge				
Crimes against persons	12	11%	5	14%
Economic crimes	65	57%	16	46%
Drug crimes	13	11%	6	17%
Crimes against public morality	1	1%	0	0%
Crimes against public order	11	10%	7	20%
Other crimes	11	10%	1	3%
TOTAL	113	100%	35	100%
B. Number of Prior Arrests ^d				
Mean number of prior arrests		1.2		2.1
C. Number of Prior Convictions				
Mean number of prior convictions		0.6		0.7
D. Criminal Justice System (CJS)				
Status at Time of Arrest				
Involved with CJS	6	5.5%	2	6%
Not involved with CJS	103	94.5%	32	94%
TOTAL	109	100.0%	34	100%
E. Age at First Adult Arrest				
Mean number of years		25.4		21.8
Part III. Demographic Characteristics				
A. Age at Arrest				
Mean number of years		28.5		27.3
B. Ethnicity				
White	58	51%	17	49%
Minority	55	49%	18	51%
TOTAL	113	100%	35	100%
C. Sex ^d				
Male	84	74%	33	94%
Female	29	26%	2	6%
TOTAL	113	100%	35	100%

TABLE E-17 (CONTINUED)

Characteristic	Experimental Group (n= 113)		Control Group (n=35)	
	Number	Percent	Number	Percent
Part IV. Other				
A. Education ^e				
Less than high school	38	34%	22	65%
High school graduate	41	37%	8	24%
More than high school	32	29%	4	12%
TOTAL	111	100%	34	100%
^a Significant at the .05 level. ^b Significant at the .04 level. ^c Significant at the .01 level. ^d Significant at the .02 level. ^e Significant at the .03 level.				

APPENDIX F
 REVIEW OF THE PIMA COUNTY CRIMINAL JUSTICE SYSTEM
 DURING THE EXPERIMENTAL STUDY

APPENDIX F
 REVIEW OF THE PIMA COUNTY CRIMINAL JUSTICE SYSTEM
 DURING THE EXPERIMENTAL STUDY

A. Introduction

The implementation of the experimental study through Pima County's Correctional Volunteer Center (CVC) created a situation where two groups of defendants received differential program treatment (Table 1). Because the different branches of the criminal justice system were informed of the nature of the experiment and because the differential program treatment tended to overtly identify some defendants with a particular group, it was questioned whether the other branches (i.e., defense, prosecution, magistrates) in any way changed their normal operating procedures to compensate for this. In addition, a change in the criminal code, instituted two months prior to the start of the

TABLE 1
 EXPERIMENTAL PROCEDURES

Type of Study	Defendants Interviewed	Defendant Groups	Information Presented to Release Authority by CVC
Felony	All Felons Not Normally Interviewed By Existing Program	Program	Verified Interview and Recommendations
		Overflow	Unverified Interview
Misdemeanor	All City Court Misdemeanants	Program	Verified Interview and Recommendation
		Overflow	No Information

experimental study, is one which would have potentially affected release determinations. A mandatory sentence statute for previously convicted felons went into effect in October 1979, possibly increasing the risk of flight for those defendants falling in this category.

A brief review of the criminal justice system during the experiment was done four months after its completion in an attempt to ascertain differences between the cross-sectional and experimental studies as well as any possible biases within the experiment itself.

B. Commissioners

Commissioners are responsible for making the release decisions for all felons processed at weekday Initial Appearances. Two of the three commissioners serving during the period of the experiment also served during the sample period of the cross-sectional study.

It appears that all three commissioners proceeded in their release considerations without special regard to what "group" the defendant was in. Two commissioners claim to have not been cognizant that an experiment was even being conducted. This does not seem unreasonable: since only a subset of felony defendants were in the experiment, the number of purposefully unverified interviews presented may not have been recognizably greater on a daily basis than the number of unverified interviews normally presented due to the inability to verify. The number of verified interviews, even during the experiment, is much greater proportionately.

All the commissioners expressed a hesitancy to release defendants on personal recognizance when there was no verified information. However, as the law requires defendants to be released on the most lenient terms practical, the commissioners made efforts to determine what reported,

unverified information was reliable.

The commissioners typically asked the defendants in court questions regarding employment, marital status, community ties and criminal history and then checked to see if the answers corresponded to those on the CVC interview. One commissioner also mentioned comparing the information on the CVC interview with that on the defendant's financial statement, which occasionally highlighted discrepancies. A defendant's relative or friend present in court was viewed as somewhat helpful in verifying information, but there was some amount of skepticism on the part of the commissioners concerning the "sincerity" of the reference. It most often depended on exactly who the contact was, and the person's presence alone was said not to have much impact on the release determination. In lieu of hard data, the "gut reactions" of the commissioners were the basis of many decisions.

The commissioners did not detect any change in procedure on the part of either the public defenders or the county attorneys during the experiment. Time limitations prevent them from securing information of their own by the time of the initial appearance.

As the law permits consideration of the nature of the offense in determining release, all the commissioners stated that they were more likely to release defendants with non-violent crimes on personal recognizance without the aid of verified information. While risk of non-appearance is generally the most important concern, one commissioner's explanation for using offense as a criterion was that "the defendant may not show up, but at least he won't hurt anyone."

It does not seem that there was an increase in the incidence of other types of non-monetary releases as "middle ground" alternatives

between personal recognizance and bail for those defendants with unverified interviews. Unsecured bonds were not popular in general with any of the commissioners. Third party custody to an outside party was considered only if there was some evidence of other community ties and most often only if the would-be custodian was a parent. Third party custody to the Correctional Volunteer Center is generally used only if the CVC has already established and verified community ties, which they had not done with the "overflow" defendants in the experiment.

One commissioner noticed that during the experiment there was an increase in the number of review hearings requested in response to unverified data. A review hearing may be requested by either the commissioner or the defendant. A review hearing affords other parties, i.e., the public defender, county attorney, the CVC or the defendant, an opportunity to secure information for a release review at a later time, usually within the next two days. (During the experiment, the CVC was restricted from securing verified information at any time on "overflow" defendants.) Thus, it is possible that the experiment had an effect not only on release rates but also on the number of release reconsiderations and on the amount of time elapsed between arrest and release.¹

In trying to determine the impact of the CVC recommendation alone on commissioners' decisions, two of the three stated that the contents of the report were more important than the recommendations themselves,

¹This turned out not to be the case. For felony defendants, there were seven bail reconsideration hearings in the experimental group and nine in the control group, or 5% each. Nor was time from arrest to release significantly different for the two groups.

and one stated that he considered both equally. There were no "rubber stamp" approvals based on the CVC recommendations alone, and while the information generally held greater weight than the recommendation, at least one commissioner stated that he felt "more comfortable" when a recommendation was made.

The institution of the "mandatory sentence" statute did not seem to have a great impact on release decisions during the experiment. Often it was not even a possible consideration as the county attorney did not have the information needed to stipulate that he was prosecuting the case in this manner by the time of the Initial Appearance. For several reasons, detailed later, the mandatory sentence statute was not widely utilized during the period of the experiment. One commissioner stated that when stipulated it was one consideration but not of major importance. Only one commissioner stated that with the mandatory sentence in mind he looked at the defendant's criminal record, when available, for prior convictions regardless of the county attorney's stipulation, and in general believed that it had some impact on the defendant's likelihood to flee, resulting in a decrease in the rate of personal recognizance releases and an increase in bail amounts.

C. Special Commissioners

Special Commissioners are responsible for making the release decisions on all felons and city court misdemeanants processed at weekend and holiday Initial Appearances. They are practicing attorneys selected to serve by the Chief Judge. All three special commissioners serving during the experimental study did not serve during the sample period of the cross-sectional study.

The special commissioners expressed recognition of the fact that the experiment was being conducted, but claimed that its existence had no effect on their normal release considerations. All three communicated that their release decisions were based on a "right to release" and a "presumption of personal recognizance."

This attitude was especially true when dealing with misdemeanants, admittedly affected by their simultaneous handling of both felonies and misdemeanors, which made misdemeanors appear inconsequential in comparison. All three claimed a tendency toward releasing most misdemeanants on personal recognizance regardless of the presence of verified information, with the possible exceptions of out-of-State residents and defendants with extensive prior histories of failing-to-appear. In the absence of verified information, questions on community ties, employment, family and prior record were asked of the defendants in court. The commissioners used their personal knowledge of the area in asking certain questions to determine the validity of the information. One commissioner was of the opinion that most misdemeanants do not tend to lie about the information given.

In determining release for felons without verified information, the commissioners similarly asked those questions pertaining to the defendant's background and criminal record, considered the nature of the offense and the circumstances surrounding the arrest and listened to the arguments of the prosecution and defense. One commissioner stated that the verification was not as important as the consideration of the charge and other information regarding the offense. Another commissioner expressed considerable skepticism regarding the recommendation of the county attorneys, claiming that they were geared more toward "persecution" than

"prosecution." Unless the county attorney had hard evidence backing his request for a substantial bail and was known in the past to be legitimate in his requests, this commissioner most often rejected the recommendation. Similar to the other commissioners, Special Commissioners used "gut reactions" in making their decisions and were more likely to release defendants with non-violent felonies on personal recognizance in the absence of verified information.

The "Mandatory Sentence" statute did not seem to affect the felony release considerations of these commissioners during the experiment. One commissioner stated that prior convictions were already one criterion used in determining release and that repeat offenders were often given bonds even before the creation of the Statute. Other positive points, such as the existence of community ties, outweighed consideration of the mandatory sentence. Another commissioner did not believe that the mandatory sentence statute had the effect of increasing the defendants' likelihood to flee, explaining that "most defendants don't realize the seriousness of their situation until they're brought to prison."

The presence of a defendant's relative in court was of moderate importance in the release consideration. One commissioner used the mere presence as a criterion in itself; another considered the person as a potential verifier, and another often granted third party custody releases, especially in the case of younger felons.

The impact of the CVC's recommendation alone on the decision widely varied among the commissioners. One regarded the contents of the report as more important than the recommendation; one considered both equally; and one initially considered all the information, but later merely looked at the recommendation unless the CVC representative was new on the job.

Aside from the already existing liberal release attitudes of these commissioners, one commissioner stated that there was also some communication by the jail administration to the Special Commissioners of a fear of weekend overcrowding, which translated into a "get 'em out" policy; but this held true always, not just during the period of the experiment. During the experiment, the commissioners did not distinguish any change in the procedures of the county attorneys or the public defenders, except that there were more review hearings requested by the public defenders. Two commissioners themselves requested more review hearings, but discontinued this after receiving some criticism from the court over the duplication of proceedings resulting from this.

D. City Magistrates

City Magistrates, or judges, are responsible for making the release determinations for all city court misdemeanants processed at weekday Initial Appearances. Of the six magistrates serving during the experiment, three also served during the sample period of the cross-sectional study.

Release screening by the CVC of misdemeanants was newly instituted for the experimental study and had not been done by the program since June 1977. While the magistrates seemingly did not alter their release considerations merely by identifying the defendants with a particular "group," they reported wide variance in their acceptance and utilization of the program's reports and recommendations. Two judges were quite liberal in their approach to release and readily accepted the program and its recommendations; two were moderate in their approach, somewhat skeptical of the program, and in borderline cases were more likely to

heed the city attorney's release recommendations; two operated relatively independently of the program and tended to be stricter in their release decisions.

The two judges who were more liberal in their approach had a tendency to release most misdemeanants on personal recognizance regardless of the charge or the presence of verified information, though when presented, relied on the CVC's report and recommendation and attested to the program's utility.

The other judges experienced an extended period of adjustment to the initiation of the CVC misdemeanor program. At least two had a propensity toward setting bails rather than releasing defendants on their personal recognizance. In determining release, at least one of these judges considered family connections, community ties, employment and the classification of the misdemeanor offense. The judge asked certain questions thought to make it obvious if the defendant had falsified the information.

This judge felt that due to the time of day that certain arrests were made, the program could not sufficiently verify much of the information. In addition, it was felt that the program placed too much emphasis on certain community ties, looking at the "quantity" rather than the "quality" of the ties, and this judge therefore was in agreement with the program's recommendations only about 60% of the time. The judge believed that the defendant's knowledge of the possibility of serving jail time, even for misdemeanors, loosened minimal ties.

E. Prosecution

1. County Attorney

The county attorneys claimed not to have changed their procedures

in any way during the experiment. In cases where information on a defendant was unverified the standard procedure was to present a stricter release recommendation or to request the setting of bail pending a review hearing. It was impossible to exert any extra effort in attempting to obtain defendant information by the time of the Initial Appearance.

One county attorney believed that the "mandatory sentence" statute resulted in an increase in failures to appear for trial, so prior felony convictions were usually pointed out to the commissioners at Initial Appearance. However, in practice, the statute was not uniformly invoked during the period of the experiment. The existence of a prior conviction noted on a rap sheet is not sufficient to prosecute a case on such grounds. Certifications, copies of the conviction and other documentation must be obtained from the court clerk. This is particularly costly and time consuming if the conviction occurred outside the jurisdiction. The process is additionally hindered by inaccurate record keeping systems.

In addition, many arrests made just after the statute was introduced (the experimental period) were for older warrants or offenses committed prior to the statute's existence, and the statute could not be applied retroactively to these. At the time of the experiment, 60% of the defendants were being charged under the new code and 40% under the old. Even at the time of our followup interviews, 20% were still being charged under the old code.

The statute did produce an inexplicable increase in dismissals (42%) and a substantial decrease in trials (66%). An expected result was an increase in pleas and plea-bargaining, where the mandatory sentence was applied, but the classification of the crime for which the defendant was convicted was reduced, resulting in a lighter mandatory sentence.

2. City Attorney

The city attorneys similarly reported no change in their procedures during the experiment. Their input into release decisions for misdemeanors is not as significant as is the county attorneys' for felonies.

One city attorney expressed the opinion that the major benefit of the CVC misdemeanor program was in the notification of defendants for court dates and not in the release screening. It was believed that most misdemeanants remaining in the system after Initial Appearance and who did not bond out prior to Initial Appearance (about 35%) were released on their personal recognizance by the magistrates anyway.

F. Program Outlook

1. Felony Study

Program personnel stated that the commissioners knew which defendants were in which "groups," due to the presence or absence of the traditional cover sheet on the program's report, which contained the court summary and program recommendation. In addition, it was claimed that due to the public defenders' hostility toward the experiment, they openly informed the commissioners that defendants without verified information were denied consideration by the program because of their random assignment to the "overflow" group. It was also reported that public defenders contacted the program prior to Initial Appearances to try to ascertain which defendants were in the overflow group and then attempted to contact friends or relatives of the defendant to request their presence in court.

Aside from normal consideration of prior record, program representatives did not additionally consider the "mandatory sentence" statute in their recommendations.

2. Misdemeanor Study

Program personnel stated that the grouping of defendants was not so readily distinguishable to the judges in the misdemeanor study because the advent of misdemeanor screening was new and judges were not as familiar with the procedures as were commissioners with the felony program. The younger judges were perceived to have regularly followed program recommendations and to have released most misdemeanants on personal recognizance anyway. Older judges were perceived to have disregarded the program's information and to have used their own questions to determine release.

It was felt by the program director that in both studies, but especially in the felony study, the mere presence of a pretrial release program had a "halo" effect on release decisions. As a result, he thought, releases for defendants in the overflow groups were more lenient during the experimental period than they would have been if the program had not existed at all.

END