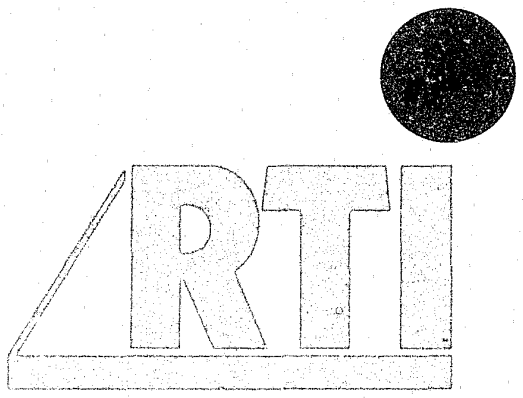

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Research on Alternative Probation Strategies in Maryland

Final Report

by

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ABSTRACT

The research was an attempt to determine whether nonserious offenders sentenced to probation could go unsupervised while on probation without increasing the likelihood of recidivism, thereby permitting more selective use of available probation supervision resources. A field experiment was conducted in Maryland during which individuals who received probation sentences of 12 months or less without special conditions were randomly assigned to regular supervised probation, unsupervised probation, or a community service program. An assessment of social adjustment and recidivism during and after probation for the three groups indicated that the level of supervision did not have a significant effect on outcomes. An exception to this finding was that probationers who had more than five previous arrests and who were not supervised had comparatively high rearrest rates. A process and cost analysis showed that the cost of supervision for those in the supervised probation group was 3.5 times higher than for the unsupervised group. Given these higher costs were not related to more favorable recidivism outcomes when previous arrest history was controlled, it was concluded that supervision resources could be shifted away from some probationer types without increasing the risk to the community.

ACKNOWLEDGMENTS

Two persons were responsible for bringing this project to fruition. Lawrence Greenfeld, formerly with the Corrections Division of the National Institute of Justice (NIJ) and now with the Bureau of Justice Statistics, guided the development of the original research concept and generated support at the federal level for the study. Mickey Allman, formerly Grants Coordinator for the Maryland Division of Parole and Probation (DPP), worked with Mr. Greenfeld in conceptualizing the problem and identifying its implications for state corrections agencies. Mr. Allman's efforts in particular were critical in obtaining the cooperation of District Court judges and DPP officials in the three jurisdictions involved in the project.

The project also benefited from the helpful suggestions of an Advisory Committee: Mr. Stevens Clarke (Esq.) of the Institute of Government, University of North Carolina at Chapel Hill; Dr. John Kramer, Executive Director of the Pennsylvania Commission on Sentencing; and Dr. William Minor, of the Sociology Department of Northern Illinois University.

Ms. Voncile Gowdy, of NIJ's Corrections Division, was involved in many of the ongoing details of the project. She made several suggestions that we believe have improved this report.

During the course of the project, several DPP staff played key roles in seeking to insure the success of the project. They included Mr. Arnold Hopkins, Director of the Division, Mr.William J. DeVance, Mr. Larry Weaver, and, during the latter stages of the project, Mr. Jim Britton. Mr. Joseph Urban and Ms. Delores Beard were helpful in providing secondary data used in the process and cost analysis. Mr. French Mackes, regional DPP director for Baltimore City, lent his support and cooperation at several stages, particularly during the implementation process and the collection of case record data. Ms. Priscilla Griffith, supervisor of the Baltimore City intake unit, was especially cooperative and her staff did an excellent job in implementing the random assignment procedures. Her counterparts in Anne Arundel and Prince George's Counties, including Mr. Robert Dudley and Mr. Jerry Romer, were equally helpful in attempting to implement the project in those jurisdictions. Finally, Mr. Mark Lazarus, who was responsible for placing community service volunteers in Baltimore City, deserves recognition for the work he did under difficult circumstances.

The cooperation of Judge Robert F. Sweeney, Chief District Court Judge of Maryland, and Administrative Judges Edward F. Borderling, Graydon S. McKee, and Thomas J. Curley in the three study sites was critical to the initiation of this project. Their willingness to discuss the project and their cooperation with DPP and the research team were much appreciated.

Members of the Consultants Advisory Board for this project were very helpful to the research team, particularly in the initial stages of work. Mr. Stevens Clarke contributed to our conceptualization of the research during the preparation of the proposal and during the implementation of the study. We wish to make special note of material he contributed to the proposal that is incorporated into the first chapter of this report.

Dr. William Minor also worked closely with us during the implementation phase and accompanied us on site visits involving the planning of the community service intervention. Finally, Dr. John Kramer was very helpful throughout the project and contributed valuable advice on a range of issues that were central to the effort. The RTI project team is grateful to each member of the advisory board for his unique contribution.

The evaluation of this project was a joint effort by James J. Collins, Charles L. Usher, and Jay R. Williams of RTI. Dr. Collins was responsible for the recidivism analyses presented in this report as well as the design of the case record data abstraction activity on which part of the process analysis is based. Dr. Usher conducted the process and cost analyses and monitored the implementation of the assignment process and the three probation alternatives. Dr. Williams had primary responsibility for designing the survey instruments and procedures and, with much assistance from Mr. Allen Duffer and Mr. Dick Waddell, for managing the surveys. He also conducted the assessment of community adjustment presented in this report.

The design of the research was a product of the efforts of all three senior researchers. They were assisted in implementing their research plans by a number of current and former staff persons at RTI. Ms. Supatra Campbell, Ms. Claire Hamilton Usher, Ms. Karen Jones, Ms. Connie Roberson, and Ms. Pat Kerr provided able research assistance during different stages of the study. Ms. Sharon Stucker Weir had responsibility for constructing the complex data base on which the analysis reported here is based. Finally, Ms. Elizabeth Cavanaugh edited the text, and Ms. Lillian Clark has performed admirably in preparing the draft and final versions of this report and other material for this project. Dr's. Collins, Usher, and Williams extend their thanks to all who have contributed to this effort.

EXECUTIVE SUMMARY

A. Introduction

Institutionalization is increasingly recognized as an expensive way of dealing with criminal offenders. Furthermore, the effects of incarceration on future criminal behavior and the readjustment of released prisoners are, at best, uncertain. As a result, there has been increased interest in probation, especially for less serious offenders.

While probation has been perceived to be a relatively cost-effective alternative to incarceration (Gray et al., 1978), increased labor costs have reduced its attractiveness. Data from the Maryland Division of Parole and Probation (DPP) illustrate this point. While the caseload of the Maryland DPP doubled during the past decade, the costs of providing parole and probation services in 1979 were approximately three times those incurred in 1971 (DPP, 1979: III-25). Therefore, in spite of the high costs of prisoner maintenance and capital expenditures in residential corrections, labor-intensive probation services are no longer the bargain we once thought.

The efficiency and effectiveness of probation services are significantly affected by the caseloads of probation agents. While caseloads vary by level of probation--maximum, medium, and minimum--the large number of cases at the minimum and medium probation levels take needed resources from the maximum cases which require closer monitoring. Smaller numbers of minimum and medium level probation cases would make probation services in the more intensive category of maximum level probation more efficient and effective.

The high cost of the more intensive forms of probation supervision and the need to free up resources for use at the more intensive levels of probation spawned a search for acceptable alternatives. The study described here was conducted with the cooperation of the Maryland Division of Parole and Probation and represents an attempt by the National Institute of Justice (NIJ) to assess the cost-effectiveness of three approaches to probation--unsupervised probation, a community-service work orders program, and regular supervised probation.

B. Overview of Evaluation and Design Methodology

The relative cost effectiveness of supervised probation, unsupervised probation, and community service was assessed on the basis of a field experiment conducted in Maryland. Less-serious offenders who received probation sentences of 12 months or less were offered randomly selected assignments to one of the three treatments over a five-month period. Baseline data for probationers in each of the three samples were drawn from an intake form which is routinely completed for DPP cases. An interim assessment of recidivism was made at the mid-point of the intervention for each probationer using information drawn from police records. Six months after his or her probation ended, probationers were interviewed to ascertain changes in their socioeconomic circumstances and to inquire about their probation experience and subsequent involvement with criminal justice agencies. This follow-up survey was supplemented by additional data on arrests and outstanding warrants derived from law enforcement authorities.

Twelve months after probation ended the data collection process was repeated.

During the probation experiment, close attention was paid to the manner in which the three forms of probation were being implemented. At a point at which it was not likely to have affected the outcome of the experiment, evaluation staff conducted process and cost analyses to assess generally the administrative process in each intervention and, specifically, the costs of providing each of the three probation services. The purpose of the evaluation was fulfilled when information derived from the experiment--the impact analysis--was joined with information about program costs--the process and cost analyses--to produce a cost-effectiveness assessment.

Our approach to the evaluation involved four broad tasks--implementation of the experiment; analysis of the process and cost of each probation intervention; measurement of social adjustment; and analysis of each alternative's impact on recidivism. In the sections which follow, each phase of the study and its findings are described.

C. Analysis of Implementation

Several conclusions can be drawn from our review of the implementation of the project and assessment of the integrity of the intake and assignment process. First, many of the probationers involved in this project would be candidates for community service programs in those jurisdictions which have implemented them. In all likelihood, had the city of Baltimore probationers who agreed to participate in this project resided in and been tried and sentenced in one of these counties, the majority would have been offered or ordered into a community service option.

A second conclusion to be drawn is that the probationers involved in this study are not representative of the Maryland's entire DPP caseload, at least in terms of their personal characteristics. They do appear, however, to be representative of District Court probationers who receive sentences of 12 months or less in Baltimore City. The attempt to implement the study in several other counties and thereby make the study sample more representative of the total DPP caseload was prevented by the widespread use of special conditions. Probation sentences are very often accompanied by special conditions such as required treatment for alcohol problems. Special probation conditions usually require supervision, and this precluded placing such individuals in a program in which they may have been assigned to the unsupervised probation or community service conditions. Because almost all of the subjects resided in Baltimore, the findings of this study may be more relevant to criminal justice professionals in other large Eastern cities than they will be to those in rural Maryland.

A third conclusion is that the experimental groups which were generated by the assignment process do not differ significantly with regard to a number of key characteristics, including sex, race, age, marital status, educational achievement, and employment status. In addition, there were no significant differences with regard to these characteristics between probationers who agreed to participate in the project and those who declined participation. However, there was evidence of sampling bias in two areas.

One area involved self-selection in that persons with probationary sentences of less than 12 months were more likely to decline participation (particularly if offered the community service option) than were probationers who faced a full 12-month sentence. The other source of sampling bias seemed to arise from the reluctance of intake staff to assign probationers to an unsupervised status if they seemed unable to pay high fines, court costs, or restitution. Although there were not statistically significant differences in the previous arrest histories of the three groups, there is some evidence that the community service probationers had more serious arrest histories.

In spite of the slight biases which our assessment detected in the assignment process, and in spite of the failure to generate samples in other counties which would be suitable for independent analysis, a general conclusion is that implementation of the project was successful. There are two bases for this judgment. First, important information about the community service program and sentencing practices was obtained from the counties that did not generate sufficient experimental cases because of the use of special conditions. This includes the fact that the program has proven to be very popular among District Court judges, and that it is used in addition to probation and not simply as a substitute sanction. Second, the availability of somewhat limited but important data about the probationers who declined participation enabled us to identify the sampling biases which did arise in the assignment process. Although relatively minor, the fact that they are identifiable will permit the biases to be accounted for in the analysis to follow. This information is not usually available in experimental research and, thus, represents a distinct advantage.

The in-treatment phase of the project was marked by a high rate of noncompliance by the community service volunteers in the project. Lacking any sanctions to apply or incentives to offer to encourage continued participation, it was not possible for DPP to curb this tendency. In addition, the work of some community service probationers was not monitored regularly because the agent assigned to the project was required to attend the training academy during the latter part of the intake and in-treatment phases. In contrast, there was no evidence that the persons assigned to regular supervised status or the unsupervised alternative received treatment that deviated from what had been planned. Therefore, in spite of the problems encountered with the group assigned to the community service option, the field experiment proceeded as planned for the other two groups.

Indicative of the integrity of the supervised and unsupervised treatments was the consistent pattern of agent-client contact and other case-related activity revealed by an analysis of case record data. Supervised probationers were contacted regularly, and unsupervised probationers were not monitored to any great extent during their probationary period. Whereas the supervised group averaged 12.3 contacts during probation, the unsupervised group averaged but 3.5 contacts.

D. Analysis of Recidivism

Our analysis suggests the recidivism of the three experimental groups does not differ. While some differences were observed for officially recorded arrests for probationers initially assigned to community service, when the frequency of previous arrests is controlled the differences are much diminished. The recidivism data do suggest that those who have more than five previous arrests ought to be supervised while on probation. Such individuals who were included in the unsupervised and community service categories in this study had much higher recidivism rates than their counterparts.

E. Social and Community Adjustment

The social and community adjustment factors for both the supervised and unsupervised probationers are very similar. The needs assessment of the Department of Parole and Probation indicated that the most pressing problems for both groups were employment and financial ones. On all other factors of social and community adjustment, both groups were judged to be predominantly at the positive end of the scale.

Probationers in both the supervised and unsupervised groups appear to have made their social and community adjustments reasonably well. Despite the continued criminal behavior of at least half of them, they also sought help to alleviate their financial and employment problems. Whether one views this as making sufficient social and community adjustment or not, the data show that this adjustment process as measured, did not differ for the supervised and the unsupervised probationers.

F. Process and Cost Analysis

Using data obtained from DPP, it was possible to estimate the input, output, and outcome costs of supervising probationers assigned to a medium caseload. An estimate of $237 per case per year was derived from an analysis of ten local offices in Baltimore City. Based on differences in frequency of contact between supervised and unsupervised probationers, it was estimated that the costs to DPP of the supervised group was approximately 3.5 times that of the unsupervised group. In addition, no evidence of increased recidivism was found among those who received less contact or whose cases involved less activity. This suggests that the relatively high costs of supervised probation are not related to lower rates of recidivism.

G. Summary Conclusion

Supervision at this level does not appear to be a critical factor to more effective probation experiences. Based on the data presented in this study, there are clearly subgroups in both the supervised and unsupervised categories that could be unsupervised during their probation with positive results for them and the community. However, others need to be identified, based on their potential for recidivism and a lower level of social and community adjustment, to be assigned to an appropriate level of supervised probation. Clearly, the larger the group that can go unsupervised successfully, the greater the reduction in the Department of Parole and Probation caseloads so that supervised probationers can receive more intensive needs assessment, treatment, and monitoring.

1. INTRODUCTION

Institutionalization is an expensive way of dealing with criminal offenders. Furthermore, the effects of incarceration on future criminal behavior and the successful readjustment of released prisoners are uncertain. As a result, there has been increased interest in probation as an alternative to incarceration, especially for less serious offenders. The interest is justified by the frequency of use of probation. Of all those in jail or prison, or on probation or parole in 1979, 63 percent were on probation (National Council on Crime and Delinquency, 1981).

While probation has been perceived to be a relatively cost-effective alternative to incarceration (Gray et al., 1978), increased labor costs have reduced its attractiveness. Data from the Maryland Division of Parole and Probation (DPP) illustrate this point. While the caseload of the Maryland DPP doubled during the past decade, the costs of providing parole and probation services in 1979 were approximately three times those incurred in 1971 (DPP, 1979: III-25). Therefore, in spite of the high costs of prisoner maintenance and capital expenditures in residential corrections, labor-intensive probation services are no longer the bargain once thought.

The efficiency and effectiveness of probation services are significantly affected by the caseloads of probation agents. While caseloads vary by level of probation supervision--maximum, medium, and minimum--the large number of cases at the minimum and medium probation levels take needed resources from the maximum cases which require closer monitoring. Smaller numbers of minimum and medium level probation cases would make probation services in the more intensive maximum level category more efficient and effective.

The high cost of the more intensive forms of probation supervision and the need to free up resources for use at the more intensive levels of probation spawned a search for acceptable alternatives. The study described here was conducted in Maryland and represents an attempt by the National Institute of Justice (NIJ) to assess the cost-effectiveness of three approaches to probation--unsupervised probation, a community-service work orders program, and regular supervised probation.

A. Research Focus and Issues

The research primarily addresses this question: can lower-cost alternative forms of probation--specifically, unsupervised probation and community service probation--be used without increasing the risk to public safety? But there are two preliminary questions:

1) Why is the research question important?

2) Has the research question been answered adequately by previous research?

1. Importance of Proposed Research to Penal Theory and Policy

Probation, as the term is ordinarily used in American law, is the suspension, subject to certain conditions, of a prison sentence imposed by a court on a convicted criminal offender (Clarke, 1977). Usually, the offender (probationer) is supervised by a probation officer to ensure compliance with the conditions of the suspended sentence during the term of probation. If violations of the conditions are brought to the court's attention, and if certain procedural requirements are satisfied, the court may revoke probation and activate the suspended sentence. It should be emphasized that the administration of probation involves both the court and the probation officer.

The purposes of penal sanctions are retribution, deterrence, restraint, and rehabilitation. Probation supervision is aimed primarily at restraining and rehabilitating the probationer. In theory, the probationer is restrained from committing new crime by (1) contact with the probation officer which makes his activities more visible (or at least makes him feel he is under surveillance), and (2) other restrictions on his activity and privacy that are enforced by the threat of revocation, such as requirements that he submit to searches for contraband, remain employed, and seek permission before changing his place of residence. The probationer is thought to be rehabilitated (i.e., assisted in leading a law-abiding life) by the various kinds of service provided as a part of probation, including counseling, assistance in obtaining employment, and the like (Clarke, 1979). The Maryland probation study seeks to qualify the theory of restraint and rehabilitation through probation supervision by determining whether some offenders do not need (or do not appear to benefit from) the probation service that they now receive.

Achieving the penological goals of restraint and rehabilitation is complicated by the fact that in most states, responsibility for probation is shared between the executive branch of government (the state or local department of correction) and the judicial branch (the criminal court). The relationship between probation officers and the courts has become more formal and visible due to decisions by the U.S. Supreme Court in 1972 and 1973, holding that the revocation of probation requires constitutional due process (Clarke, 1979). But the relationship has long been confused. Judges, prosecutors, and defense attorneys--the "work group" of the criminal court--see probation as the <u>end</u> of a process, unless the probationer comes up on a revocation hearing. Usually, once a probation

sentence is imposed, the "work group" leaves the probation officer the problems of achieving the goals of probation. For the probation officer, the sentence is the beginning of a process of supervision. The officer is trained to supervise but needs to work closely with the court not only in supervision but in imposing the probation sentence. The need for cooperation and good communication between the court and the probation officer has been greater than ever in recent years, because of (1) the new due process restrictions on revocation procedures, and (2) the increasing acceptance of a determinate sentencing philosophy, which calls for (among other things) more deliberate choices of penal sanctions, including probation, and more formal justification of the choices.

Because criminal courts are not as informed as they perhaps should be in the realities of probation supervision, they tend to use probation merely as a token punishment--as a standard sentence when active imprisonment does not seem appropriate. This overloads probation agencies with "token" cases [McCleary (1978) uses the term "paper men"] which probably detracts from agents' ability to supervise effectively the offenders who really need supervision. Considering the problems caused by such overloading brings out the particular value of the research described here.

In most states and especially in the Southeast, prison overcrowding is critical. If they can be used without an increase in risk to the public, community alternatives such as probation are now, more than ever, important alternatives to imprisonment. For probation to be a credible alternative to imprisonment, probation agencies will have to be able to focus their resources on higher-risk offenders--those now being imprisoned who could be put on probation if the courts had more confidence in probation's ability to protect the public. In order to move the threshold for prison, it will

probably be necessary to relieve probation agencies of the burden of the "token punishment," low-risk cases. Although those low-risk probationers receive little supervision at present, there are many of them, and even the paperwork required to maintain their files is a substantial drain on scarce probation resources.

This study addresses the question of whether supervision of low-risk cases can be reduced to a minimum without adding to the community's danger. If it in fact can, then probation resources can be devoted to more selective, deliberate strategies of supervising higher-risk probationers diverted from prison. If probation supervision of low-risk cases is unnecessary, the courts can assist in the reallocation of probation resources by not sentencing the lowest-risk offenders to probation. The research reported here can also promote improved communication between courts and probation officers, which is essential if probation supervision is to become more selective and strategic. By reporting on the results of decisions to sentence low-risk offenders to probation, this study may spur probation officers, judges, prosecutors, and defense attorneys to begin a new dialogue on probation procedures and policies.

2. Previous Research on the Effectiveness of Probation

A review of the literature indicated that the question addressed by the Maryland study had not been adequately answered. Also, authoritative critics support the view that more research of this type is needed (Albanese et al., 1981; Allen et al., 1979; Gottfredson et al., 1977).

The San Francisco Project appears to be the only published systematic study of minimum probation supervision. In minimum supervision, a single officer accepted responsibility for an average of 118 probationers during the random assignment phase of the experiment. The only requirement was

that the probationers submit written monthly reports to the probation officer; other contacts were made only in response to the probationer's request (Robinson et al., 1969). No important differences in recidivism were found between the minimum supervision group and other groups. However, the San Francisco research has been severely criticized:

> ... one's first response to this originally massive n > 2,000) project is a sense of tragedy... the project contributed little to the research question it was designed to answer. After addressing itself to peripheral issues..., suffering from a lack of methodological sophistication... and acute data collection problems, undergoing major alterations in the research design and experiencing a phenomenal attrition rate..., the project provides few bases for significant conclusion (Vetter and Adams, 1971, pp. 336-337).

Fiore, in her review of caseload research in Gottfredson et al. (1977), lists the weaknesses of most previous probation caseload research: (1) lack of controls; (2) basing outcome measurements on subjective information reported by probation officers; and (3) failure to describe exactly how the treatment or service inputs to control and experimental subjects differed. In the Maryland probation study reported here, randomization was used to ensure adequate controls; measures of criminal activity independent of probation officer's records were used (such as state police arrest data); and differences in service inputs were carefully assessed in a process analysis. Gottfredson et al. also point out that increasing caseloads do not necessarily mean fewer contacts between officer and probationer (citing Lohman et al., 1966). Thus, it is important to focus on the actual level of supervision delivered, which this study has done.

A study that is similar to the one described here is that of Star (1979), involving "summary parole" (parole without routine supervisory contacts and without services unless requested by the parolee). Experimentals and controls were selected randomly from a statewide population of

California parolees, of whom 62 percent were excluded (parolees who had been imprisoned for first-degree murder or a sex offense, and parolees subject to special conditions involving psychiatric counseling, abstinence from alcohol, and testing for narcotic use). The experimental ("summary parole") group actually had 50 percent fewer contacts than the controls (an average of five in six months as compared with 10 in six months). No substantial differences in arrests and convictions between the two groups were found in 6- and 12-month follow-ups.

Though the Maryland project did not replicate the California project, Star's results are relevant to the Maryland experiment. The Maryland project involved probation rather than parole; it involved what appears to be a wider selection of offenders than the Star study; it examined the community service option, which the Star study did not consider; and it tested minimal supervision in a different part of the country.

Another study, somewhat similar to the one described here was done by Lichtman and Smock (1981). The Lichtman and Smock study used random assignment to place newly sentenced property offenders into intensive or regular probation. The intensive probation condition involved lower probation officer caseloads, enhanced contact with social services for the probationers, and job placement. Those probationers in the treatment condition, intensive probation, did not show improved recidivism rates over the probationers in regular supervision (the control group). Lichtman and Smock (1981:97) conclude that, "there is no evidence supporting the notion that intensive probation is the key to rehabilitating the young adult property offender."

An experiment in Sweden found that the enhanced resources did not result in improved recidivism, reduction of alcohol misuse, or better

employment performance for probationers and parolees. In spite of increased staff and stepped up psychological and employment services in an experimental probation district, client outcomes were no better than in 13 similar probation districts (Kühlhorn, 1979). Finally, Friday (1979), after an assessment of the evidence from probation research in England and Sweden, concludes that probationers do not need intensive supervision to assure favorable recidivism outcomes. In fact, Friday claims there is evidence that some aspects of probation increase recidivism because those given suspended sentences without supervision sometimes do better than probationers.

The Maryland study, while in part like the Lichtman and Smock effort, takes a broader sampling of offenders and randomly assigns them to regular supervision, to an unsupervised status, and to community service. Here, too, levels of supervision are examined for differences in recidivism rates and other factors such as social or community adjustment. However, in the Maryland study, the comparison is with the everyday standard levels of supervision and an experimental alternative for caseload reduction, unsupervised probation.

B. Report Overview

The remainder of this report addresses the research methodology and findings. Chapter 2 presents the evaluation design and the methodology employed in gathering the data, and reviews the measures employed in the study. Analysis of the processes and costs for the various random assignment groups is done in Chapter 3. Chapter 4 examines the social and community adjustment of the groups. A comparison of the recidivism of the random assignment groups is found in Chapter 5. Chapter 6 summarizes the findings and discusses the implications of this research for planning future probation strategies and applications.

2. EVALUATION DESIGN AND METHODOLOGY

A. Overview

The relative cost effectiveness of supervised probation, unsupervised probation, and community service work orders was assessed in a field experiment conducted in Maryland. Over a five-month period, less-serious offenders who received probation sentences of 12 months or less without special conditions were offered randomly selected assignments to one of the three treatments. Baseline data for probationers in each of the three treatment samples were drawn from intake forms routinely completed for DPP cases. An interim assessment of recidivism was made at the mid-point of the intervention for each probationer using information drawn from police records. Six months after their probation periods ended, experimental subjects were interviewed to ascertain changes in their socioeconomic circumstances and to inquire about their probation experience and subsequent involvement with criminal justice agencies. This follow-up survey was supplemented by additional data on arrests and outstanding warrants derived from law enforcement authorities. Twelve months after probation ended, the data collection process was repeated. Exhibit 1 provides a summary of the data collection activities related to the probationers in this experiment.

During the probation experiment, close attention was paid to the manner in which the three forms of probation were implemented. At a point at which it was not likely to affect the outcome of the experiment, evaluation staff conducted process and cost analyses to assess generally the administrative process in each intervention and, specifically, the costs of providing each of the three probation services. The purpose of the evaluation was fulfilled when information derived from the experiment--the impact

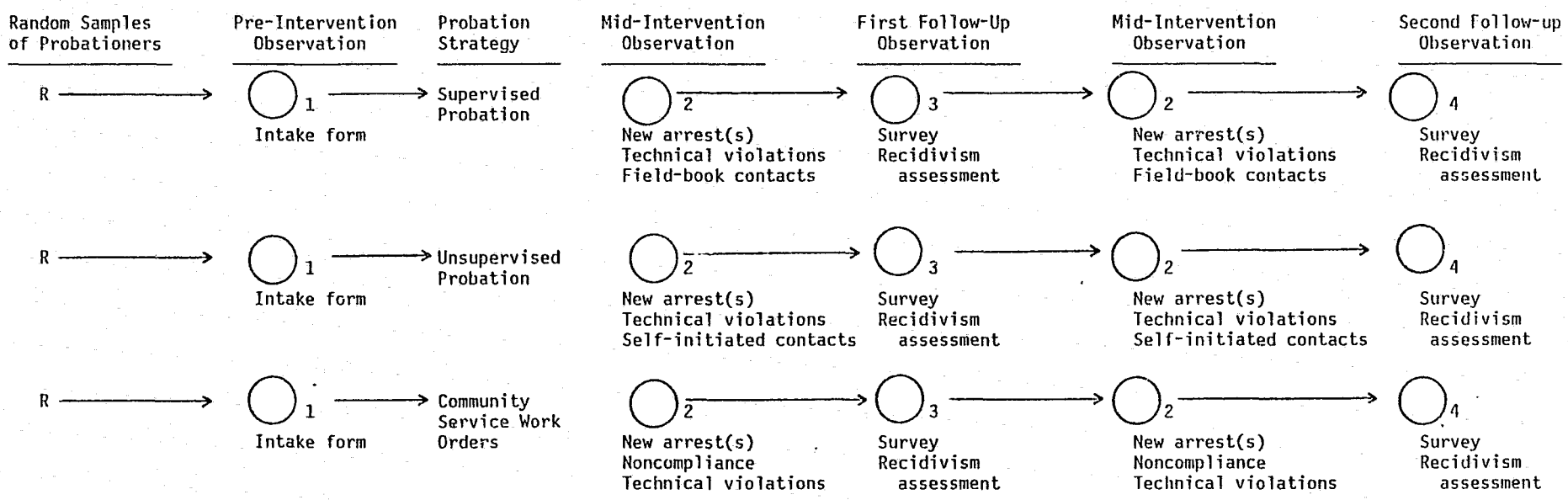


Exhibit 1. Observations of Experimental Subjects

analysis--was joined with information about program costs--the process and cost analyses--to produce a cost-effectiveness assessment.

Our approach to the evaluation involved four broad tasks--implementation of the experiment; analysis of its impact; analysis of the process and cost of each probation intervention; and a determination of the relative cost effectiveness of the interventions. In the sections which follow, each task and its related activities are described.

B. Tasks

1. Task 1: Implementation of the Experiment

Activity 1: Define target population.

Activity 2: Establish and implement procedure for random assignment of subjects to interventions.

Two factors dictated focusing the study on a set of nonserious offenders. First, a major question addressed by the study was whether probation supervision could be reduced or eliminated without adversely affecting outcomes like recidivism and social adjustment. The risk of adverse outcomes was judged to be less for nonserious offenders. Second, it was assumed that judges and other criminal justice personnel would not approve or be comfortable with random assignment of serious offenders to unsupervised probation or community service, and the approval and cooperation of these key individuals was essential.

Accordingly, it was decided to limit the experiment to adult offenders who received a probation sentence of 12 months or less. It was expected that such individuals would be nonserious, first-time offenders. While this was generally true, a few individuals had been convicted of serious felonies and, as will be seen later, a substantial percentage had previous arrest records. Some had many prior arrests.

Successful implementation of the probation experiment was crucial to the evaluation. Activities in the first few weeks of the project had a direct bearing on the validity and ultimate usefulness of the evaluation findings. An important preliminary step was the resolution of several key issues by the judges, DPP staff, and project evaluation staff. The cooperation of judges in each jurisdiction in the experiment was crucial in generating the target population from which assignments to different interventions were made. Perhaps more important was their acceptance of its findings, which could only be expected if they fully understood the effort and agreed with its objectives. Therefore, their opinions were sought concerning the categories of offenders for whom 12 months of probation or less would be an appropriate sentence.

A sample of approximately 1,000 probationers was to be drawn from the target population. Data were requested from each of the target districts to estimate the probable number of eligible probationers that could be assigned to the study within a given time period. Based on the data estimates of probationers who would be receiving a year or less of probation without special conditions, three judicial districts were selected for study.

Our experience in the field did not match the estimated number of probationers eligible for inclusion in the study. The time period for assigning probationers to the study was extended to compensate for the small number of eligibles. Despite these efforts, most of the sample came from Baltimore City, and the total number of probationers included in the study was only 371 or about 40 percent of the intended sample size.

A detailed discussion of the implementation of the experiment is included as Appendix A. The major reason why the pool of eligible probationers was less than expected was due to special conditions given to many probationers--such as a requirement to attend an alcohol treatment program. The statistical data used to estimate the number of eligibles did not provide information about special conditions. It was not possible to include probationers in the experiment who received special conditions. Many of the conditions require supervision--an activity that would have been incompatible with planned random assignment to unsupervised probation and community service.

The approach taken to assignment was to have a DPP intake agents make assignments from the target population to the treatments. This removed this burden from the court and also resulted in a double-blind experiment in which service providers (probation agents) were ignorant of whether or not a probationer was an experimental subject. This approach also did not impose a significant administrative burden on DPP intake staff.

Activity 3: Define interventions, their implementation and administration.

One of the conclusions of a report on criminal justice research by the National Research Council was that--

> ...the interventions that have been tested often seem inappropriate to the task to which they are directed. They appear to be derived primarily from conventional wisdom, scarcely from any careful analysis of the task to be accomplished or from any carefully thought-out theoretical premises regarding crime or rehabilitation (Sechrest et al., 1979: 35).

Furthermore, the inadequacies of program design are complicated by the problems of program implementation or, as it was termed in the National

Academy of Sciences report (Sechrest et al., 1979: 37), "the integrity with which programs have been implemented." In fact, Palumbo and Sharp (1980: 289) argue that the impact of community corrections programs should not be undertaken without an analysis of program implementation, i.e., a process analysis.

This issue was addressed in the Maryland experiment. In spite of the fact that a process analysis was undertaken during the evaluation, a determination of the impact of supervised probation, unsupervised probation, and work orders would be valid only if each intervention had been well-defined conceptually and operationally.

Perhaps the most critical issue to be resolved in this area concerns the definition of an "intervention" or "treatment." For example, the category of supervised probation could have several variants, including probation and a fine, probation and restitution, or probation, fine, and restitution. In addition, the length of probation could be, for example, three months, six months, or 12 months. Obviously, the same sort of variation could occur in the other "treatments."

The only solution to this problem, other than fining every probationer or not fining any, was simply to collect data on fine or restitution requirements and control for this factor through multivariate analysis. The fundamental concern was the likelihood that too few offenders of a given type would receive a particular combination of probation, fine, and restitution to permit reliable analysis of that set of factors.

Another problem which had to be addressed with the help of the Maryland Division of Parole and Probation was the random assignments to the supervised, unsupervised, and community service conditions specified by the research design. At intake, probation agents informed probationers of

their potential role in the study and asked them sign an informed consent form to indicate their willingness to participate. Those assigned to the supervised probation condition were handled in a routine manner. Using the Differentiated Caseload Management System (DCMS), the probationer was classified by type of supervision--maximum, medium, minimum, non-active or delinquent. (If the probationer violates the conditions of his or her probation and a violation warrant is issued, he or she becomes "delinquent.") Since a criterion for inclusion in the study was probation for a year or less with no special conditions, most probationers assigned to supervised probation fell into the medium and minimum classification categories.

Those assigned to unsupervised probation and community service conditions were another matter. The DCMS had no categories for either of these conditions. The non-active probation status was established for those temporarily incarcerated, those in military service, or those hospitalized.

> Although active supervision cannot be rendered in such cases, the agent is still responsible for monitoring these cases monthly to determine when the case is expected to be returned to active supervision (DPP, 1980: 37).

In unsupervised probation, the only contacts the probationer has with a probation agent are those the probationer initiates. The notion of the unsupervised status was explained to each probation office supervisor whose agents would receive cases from the study. It was agreed that these unsupervised cases would be set aside in the files for the duration of their probation only to be dealt with if they initiated a contact or violated the conditions of their probation (such as an arrest brought to the attention of the probation agent). In only a few cases was this unsupervised status violated by probation agents who apparently misunderstood how these cases

were to be treated. Even in these cases, only a few telephone monitoring contacts were made with the probationers during their probationary periods.

Community service, the third random assignment category, was also not within the existing DCMS classification or assignment scheme. Baltimore City, where most of the probationers in the study came from, had little interest in a community service program during the planning stages of the study. The Division of Parole and Probation (DPP) assigned a probation agent to organize a special community service program for the study. Job placements were found throughout the city, and probationers coming under this assignment were directed to various community service activities. Unfortunately, 72 percent of the probationers assigned to community service failed to complete their community service work and were reassigned to some level of supervised probation. For this reason, the community service group was of limited utility as a comparison group in the analysis. This is discussed further in Chapter 3.

Activity 4: Obtain, code, and key data from "Case Record Input-Intake Form" to establish baseline data set.

Data derived from the DPP "Case Record and Input-Intake Form" provided the baseline data for this evaluation. This approach avoided the threat to validity posed by a potential interaction effect of testing; i.e., reaction to a nonroutine interview which intensively explores the background and current circumstances of probationers in the target population (Campbell and Stanley, 1963: 5). It also did not call attention to the fact that a probationer was an experimental subject.

Although it did not provide an optimal variety of data, the intake form did provide data which was crucial in analyzing program impact. This

included information on employment and marital status at the time of intake, the nature of the offense and sentence, and basic demographic data, such as age, sex, and ethnicity.

It is well-known that recidivism varies by demographic factors like age, gender and race (Sellin, 1958; Wilkins, 1969; Wolfgang, Figlio and Sellin, 1972). Employment and family factors have also been shown to influence recividism (Glaser, 1969; Gottfredson et al., 1978). The intake form data permitted control of these factors in the impact analysis. In addition, information on the probationer's nearest relative and employer was invaluable in tracing subjects for the follow-up surveys.

2. Integrity of the Assignment Process

In Appendix A implementation of the random assignment process is discussed in detail; characteristics of the three experimental groups are tested for differences between them; and the characteristics of participants and those who declined participation are compared.

Two factors influenced the random assignment approach. First, it was necessary to exclude from the eligible pool probationers who were given special conditions as part of their sentences. Special conditions usually require supervision by a probation officer so that if a special condition probationer were to be assigned to the unsupervised probation or community service categories, the experimental condition would be contaminated. Many more probationers than expected were given special conditions, and this reduced the pool of participants eligible for the experiment.

Second, the informed consent of each participant in the experiment was required, and participation was voluntary. Comparison of those who participated and those who refused showed no statistically significant differences

between the groups on the basis of sex, race, age, marital status, educational achievement, full-time employment or offense type. There were no statistically significant differences between the three experimental groups on the above dimensions or in arrest history (see Table A-4, Appendix A).

There was some evidence of selection bias in two areas. First, those who had probation sentences of less than 12 months were more likely than those who had a 12-month sentence to refuse participation in the study. This differential refusal rate appears related to receiving the community service option. Those with comparatively short sentences who would have received the community service assignment apparently found the early release incentive insufficient (Appendix A, Table A-5). The second indication of possible participation bias is that nonparticipants were more likely to have to pay a fine, court costs, or restitution than were those who agreed to participate. Conversations with DPP personnel led us to speculate that intake agents discouraged those with financial obligations to the court from participating in a random assignment experiment in which they may have been placed on unsupervised probation or community service.

Table A-4 in Appendix A shows the previous arrest histories of the three experimental groups. Although the chi-square statistic does not reach a significant level and, therefore, suggests the groups are not different, those with six or more previous arrests appear more likely to have participated in the unsupervised and community service groups. Later recidivism analyses (Chapter 5) also suggest this.

In spite of some indication of selection bias, the implementation of random assignment was successful. Most comparisons of respondents and nonrespondents and those in the three assignment groups indicate no difference between the groups. Moreover, the slight biases identified can be compensated for during analyses.

3. Task 2: Impact Analysis

Activity 1: Assess case record.

In order to gather information on the experience of subjects while on probation, a random sample of 75 from each experimental condition (n = 225) was selected for inclusion in a case record analysis. An evaluation of the probation record for these subjects provided information about:

a) initial and subsequent probation officer assessment of the subject;
b) change in supervisory level and other case activities;
c) the number and types of contacts between probation officer and subject and the level of recidivism surveillance,
d) successful completion of probation or failure to complete the probation sentence.

Probation case files contain a great deal of information about the probationary period and the activities of both the probationer and the probation caseworker. Some of this information is used in the recidivism analysis, and some is used to assess other aspects of the probationer's adjustment. Information about the level of supervisory activity for each case is used in the process and cost analysis.

Activity 2: Collect recidivism data

There were two sources of recidivism data used in the study: Maryland state police criminal histories and interviews with the subjects. The interview and survey methodology are described (under Activity 3) below. Maryland State Police criminal histories (rap sheets) were collected at two

points--while most subjects were still on probation and in August, 1983, just prior to the final recidivism analysis--to maximize completeness of the criminal history and recidivism data. Criminal histories are incomplete records of arrest (Hubbard, et al., 1981; Marquis, 1981). Because the Maryland probation subjects were comparatively nonserious offenders, as indicated by their relatively short probation sentences, there was concern that rap sheets would not be available for many subjects. Moreover, non-serious offenses are often formally expunged from records when the offender successfully completes probation. As it developed, the subjects in the experiment had more serious criminal histories than expected. Only 16 percent of those for whom we received rap sheets had no previous arrests, and 23 percent had five or more previous arrests.

The Maryland rap sheets are not likely to provide a complete accounting of out-of-state arrests. It had been expected that rap sheets would also be collected for the subjects from the Federal Bureau of Investigation (FBI) rap sheets. As it turned out, it was not possible to arrange for FBI rap sheets, so the recidivism analysis based on official records is derived from the Maryland rap sheets only.

Rap sheets were received for 322 of 371 subjects. The 49 missing cases are distributed across the three experimental groups such that no statistically significant difference in the rap sheet "response" rate is detectable. Officially recorded police contacts were coded into three categories from the rap sheets: previous arrests, the current arrest, and post-probation arrests. The current arrest is the one that resulted in the probation sentence which caused the individual to be included in the study. Previous arrests are those that occurred before the current arrest, and post-probation arrests are those that occur after the current arrest. Chapter 5 presents the rap sheet recidivism findings.

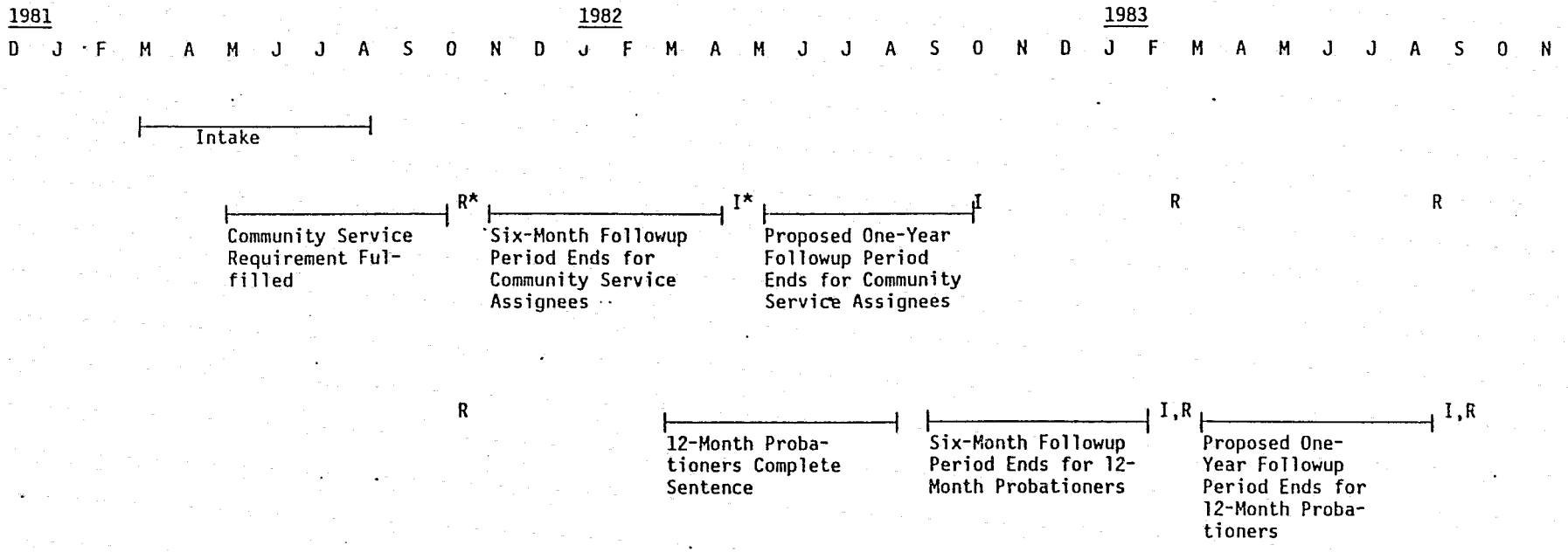
Interviews with subjects provided the second source of recidivism data. Subjects were asked a number of questions about police contacts and involvement in criminal behavior since being put on probation. Some subjects were interviewed twice; 312 subjects were interviewed at least once. The self-reports of these 312 provide the basis for the interview recidivism data. The interview data are described below.

Activity 3: Conduct follow-up surveys

- -- design interview schedule
- -- pretest interview schedule and administration;
- -- telephone vs. personal interviewing and tracing;
- -- code, key, and merge survey data with existing data base.

Each experimental subject was interviewed six months and then 12 months after his or her probation ended. This required four interview periods (see Exhibit 2 for the project schedule). The first group (those with probation sentences of six months or less, mostly including those assigned to community service) was interviewed six months and 12 months after their probation ended. The second group (those with probation periods greater than six months and up to 12 months) were interviewed on the same schedule.

The survey instrument (see Appendix B for the instrument) was designed to obtain information on changes in employment and family/dependents and on contact with public agencies (welfare, education/training, employment service, etc.) during and following probation. In addition, information was requested on contacts with law enforcement officials and new arrests. A pre-test was conducted with a sample of probationers at the DPP Intake office. The pre-test resulted in very few content changes; but specific questions were sharpened, and some format changes were made to simplify administration of this instrument.



*R: Records Check

*I: Interview

Exhibit 2. Project Schedule

Before the interviewing began, a letter was mailed to all potential respondents using address information available from the intake form. The letter informed respondents of the nature of the survey and alerted them that an interviewer would be contacting them by telephone or in person in the near future. Address update information was requested from the Postal Service for those letters that could not be delivered.

The first survey was conducted by telephone and personal interviews. Using the data compiled from the Case Record Input-Intake Form, computer-generated listings which served as interviewing assignment records during data collection were provided to the telephone and field interviewers. The listings included the probationer's name, address, name of nearest relative, employment status (employer), and telephone number. The lapse time from the point of intake into the probation system to discharge and commencement of telephone interviewing varied from 7 to 18 months. In order to reduce the amount of telephone tracing required, the Maryland Division of Parole and Probation was asked to provide discharge summaries/forms or most current address/telephone information in addition to the Case Record Input-Intake Form. These data were keyed and merged with those from the Intake Form for the interviewer assignment listing.

The first followup interviews were completed with those on probation six months or less. Both telephone and personal interviews were used to establish which cost less and which yielded the higher response rate. Subsamples of probationers were randomly assigned to a six-month interview either by telephone or in person. Table 1 shows the results of the interviewing.

Table 1

A Comparison of Telephone Versus Personal Interviews

	Interviews Assigned	Interviews Completed	Cost Per Interview[a]
Telephone	72	39 (54%)	$34.43
Personal	71	53 (75%)	$32.00[b]
Reassigned (from telephone to personal)	17	11 (65%)	-

[a]These costs include interviewer training, supervisory time, interviewer time and expenses, and for the telephone, telephone line charges.

[b]This figure is for personal interviews only and does not include the additional cost for the cases reassigned from telephone to personal interviews.

Attempts to interview a probationer in person were more likely to be successful and cost less than telephone interviews. Missing data, incomplete responses, and refusals did not differ for the two types of interviews (in fact, there were only three refusals in total). The major difference was the telephone interviewers' difficulty in tracing persons who had moved. Field interviewers were far more efficient and effective in tracing probationers and completing assigned interviews (Table 1). They completed 65 percent of the cases which the telephone interviewers could not trace by phone. Given the approximately equal costs of the two types of interviews and the higher yields for the personal interviews, it was decided to do the remainder of the followup interviews as personal interviews.

4. Task 3: Process and Cost Analysis

Activity 1: Collect budget data from DPP, courts, and related agencies.

Activity 2: Interview staff and observe probation process in each intervention.

During the pre-experimental planning and implementation phase of the project, detailed budget data, broken down by units in the local DPP offices, were collected. These data as well as updated information derived from budget allocations which occurred during the experimental period provided the basis for costing various probation activities.

Activity 3: Identify cost centers and determine unit costs of service in each intervention.

Since all probation strategies are highly labor-intensive, the frequency of probationer-staff interaction has the greatest effect on direct costs. Information derived from case records and interviews with DPP staff yielded information about the level of interaction for interventions. This permitted the generation of estimates of the degree of agency-client interaction in each probation modality.

Activity 4: Review survey data regarding contact with probation officer and collateral agencies and refine cost estimates.

It was noted earlier that questions have been raised about the reliability of probationer case record information. The reliability of these estimates was tested using the data from the follow-up surveys. Among the items included were ones which deal with the number and nature of contacts the probationer had with his or her probation officer. For example, were

contacts made by the agent on a monthly, weekly, or daily basis, or not at all? Were any contacts probationer-initiated? Did the agent assist in getting services from other agencies, such as the state employment service or a drug treatment center?

These probationer reports on types and levels of services received also permit estimation of the cost of service to each experimental subject. This approach also avoids the costly process of retrieving each case record and abstracting a service history from it, although this was done with a selected subsample of the probationers. Thus, while the work described in activity 3 was geared primarily to what Gray and his associates (1978) termed "input costs," this analysis yielded "output costs." Cost estimates similar to their "outcome costs" were produced in the cost-effectiveness analysis which comprised the fourth task.

5. Task 4: Cost Effectiveness Analysis

Activity 1: Determine costs of producing observed impact on probationer, family/dependents, the criminal justice system, and society.

Activity 2: Analyze variation in costs of alternatives relative to impact on recidivism and change in socioeconomic function.

The relevance of the Maryland study to probation policy is reflected in the following statement by the National Academy of Science Panel on Research on Rehabilitative Techniques:

> ... the very fact that so many interventions result in equal outcomes... means that different ways of treating criminals may be interchangeable. If that is the case, then, assuming the treatments are equally humane, the less expensive alternative should be chosen (Sechrest et al., 1979: 33).

If, in fact, outcomes are essentially the same for the three approaches to probation, a policy decision should be based solely on cost. Therefore, it is important to determine accurately the costs of producing a given impact.

As indicated previously, because the evaluation was most concerned with two dimensions of impact, it was necessary to measure costs in terms of differences in recidivism rates and in socioeconomic function.

C. Measuring the Impact of Probation

Society's retribution for crimes is usually to imprison the offenders or to otherwise restrict their freedom. In this project, this restriction was probation. During their sentences, probationers are required to follow reporting requirements, and must adhere to other conditions. Probation also ideally expedites the provision of appropriate social services that lead to the offenders' rehabilitation. Probation attempts to fulfill several functions--punishment, deterrence, restraint, and rehabilitation. An evaluation of probation should measure how well it achieves these goals. In the Maryland probation study, two dimensions were studied: recidivism (a failure to deter and restrain) and community adjustment (an indication of rehabilitation).

An important aspect of this research was the measurement of recidivism for the three probation groups subsequent to their assignment to probation. As indicated earlier, probation experiments have shown few differences between experimental and control subjects. Other research also suggests rates of criminal activity are not likely to differ between probation categories when factors like age, gender, race and offender type are controlled. Bailey (1966), Robinson and Smith (1971), Lipton, Martinson, and Wilks (1975) found that correctional interventions of various types tend not to affect the likelihood of recidivism. That is not to say that "nothing works," because clearly some correctional interventions do work for some people some of the time. Adams (1974), for one, emphasizes the value of correctional treatment and has a relatively optimistic view about its

potential. However, the likelihood that recidivism rates would differ significantly for the three probation groups of this research is even less than would normally be expected for two reasons. First, the random assignment of individuals to the three treatments neutralizes the selectivity factor that is often responsible for the observed differences between offenders assigned to different treatments. For example, some or all of the differences in observed recidivism rates for incarcerated and probation samples is undoubtedly explained by the selection of the better risks into probation. Second, recidivism rates were not expected to differ between the probation subgroups because the interventions (regular supervision, no supervision, and voluntary community service) were mild and thus unlikely to affect individual behavior.

1. Recidivism

Because recidivism can be conceived in any number of ways, multiple measures of the phenomenon have been used. Police contacts, arrests, self-reported criminal activity and probation completion/non-completion were the basic measures. Too few subjects were convicted or incarcerated during the study period to permit comparison on those dimensions. Recidivism was measured in a binary (yes/no) fashion and along the time dimension. The groups may differ in the proportions who fail, and they may also differ in the time distribution of their failures. For example, two groups may have the same rate of arrest, but one group may be arrested earlier in the experience period than the other. Chapter 5 includes a discussion of recidivism measures as the well as recidivism analysis.

2. Social and Community Adjustment

Among several reasons Carney (1977:86) cites for probation is that it can "maximize the normalizing (community-based) influences in the

correctional processes." Dressler (1969:16) notes that "probation is a treatment program designed to facilitate the social readjustment of offenders." The emphasis here is on the adjustment to the community and social behavior in the community. For this reason, the probation period is served in the community rather than in a correctional setting. Measures of social and community adjustment and new offenses are indicators of the "success" or "failure" of the probation program (Rumney and Murphy, 1952; Glaser, 1969; Gottfredson et al., 1978; and Albanese et al., 1981).

Rumney and Murphy (1952) note that we should not look for spectacular successes in social and community adjustment from probationers but for the minimum of social adjustment, responsibility, and productivity. The analysis in Chapter 4 will examine the adjustment of probationers from the perspective of the probation officer, from changes in family and employment status, from contacts with community service agencies, and through the assessment of the relationship of services received and agency contacts to recidivism.

3. Cost Analysis

Weimer and Friedman (1979: 262) recommended that cost analysis be a "standard component" in evaluations of correctional programs. This is, of course, essential to cost-effectiveness analysis. However, because most public agencies do not have well-established systems of cost centers, the accuracy of cost estimates is often difficult. Thus, in order to calculate the costs of supervising a probationer, an analyst must know exactly what activities are involved in probation supervision. This requires the identification of discrete probation services and their costs, as well as the measurement of the number and types of services required by different types of cases. This is part of a process known as cost analysis, or cost finding.

The first phase of cost analysis seeks to identify the cost centers in an organization; i.e., those organization units, or individuals, that are engaged in a clearly definable activity, or set of activities, which has a direct bearing on the organization's mission. In this evaluation, we were most concerned with cost centers that delivered three different types of probation services. These are sometimes called "final" cost centers in contrast to "support" cost centers, such as a clerical pool or central administration (Hagedorn et al., 1976: 182). The cost of delivering probation services, therefore, involves the direct costs of the salaries of probation officers, as well as the indirect costs which are incurred when they use the services of other cost centers. Thus, a method of allocating a portion of the costs of support cost centers to the final cost centers must be employed to determine the total costs of a service-producing organizational entity.

In their study of the cost effectiveness of residential community corrections agencies, Gray and his associates identified three dimensions of the total daily cost per client for adult probation services in a metropolitan area. These included average cost per workload unit day (average daily probation agent's salary/50 "work units"),[1] average outside social service agency costs, and average daily indirect costs per case. They estimated the first figure to be $.90, the costs of collateral services to be $2.27, and indirect costs to be $.06, for a total daily cost over the long run of $3.23 per client (Gray et al., 1978: 390). While the process that was used to allocate indirect costs is not entirely clear (Gray et al., 1978: 382-383), this research confirms the need to identify such costs in this type of analysis.

[1]The work units measured in the study were based on different levels of service to different types of probationers. Also, in their computation of average daily costs, Gray et al. fail to show one step (dividing the salaries by 365).

After having identified the services and their composite activities in an agency, cost centers can be identified and a clearer appreciation of exactly what constitutes a "unit of service" can be obtained. In their study Gray et al. (1978) made an important contribution in this regard by distinguishing three types of costs--input, output, and outcome costs. Their typology was actually based on three different levels at which costs could be aggregated. Whereas, input costs measure the cost of a day of program operation, the cost per case describes output costs. The latter is particularly useful when the length of treatment is variable. For example, the length of time that a person in the community service program will be supervised is likely to be substantially less than a person in supervised probation, perhaps three months versus 12 months. As a result, the costs per probationer can be expected to be lower for probationers performing community service, even ignoring the benefit derived from their labor. This also ignores the fact that input costs are likely to differ because different administrative personnel and supervision techniques are used in each alternative.

Outcome costs bear directly on the issue of cost effectiveness, as indicated in the following illustration:

> Suppose a group of delinquents is treated at a cost of $50,000 and has 50 fewer sustained offenses (as a group) than a set of delinquents who did not receive that treatment. Then the cost per reduced sustained offense is $1,000. If an alternative treatment also costs $50,000 but produces 100 fewer sustained offenses than the "untreated" group, then the cost per reduced sustained offense would be only $500. If reduction in the number of sustained offenses is assumed to be an important goal of the corrections subsystem, then the latter treatment is twice as cost-effective as the former treatment program; outcome is doubled without increasing costs (Gray et al., 1978: 379).

This level of understanding can only be obtained from a process analysis which seeks to identify all the activities which eventually produce a service. This analysis ranges from informal discussions with agency

personnel and observation of their activities to carefully designed work measurement techniques (Tripodi et al., 1978: 69-73).

The cost estimates reported in the above research included the costs of outside social services to probationers. Although they represent the largest portion of the costs per day, these "external" costs are usually not borne by the probation agency. These costs were not assessed in the Maryland study because they were borne by the other agencies involved in the community service alternative.

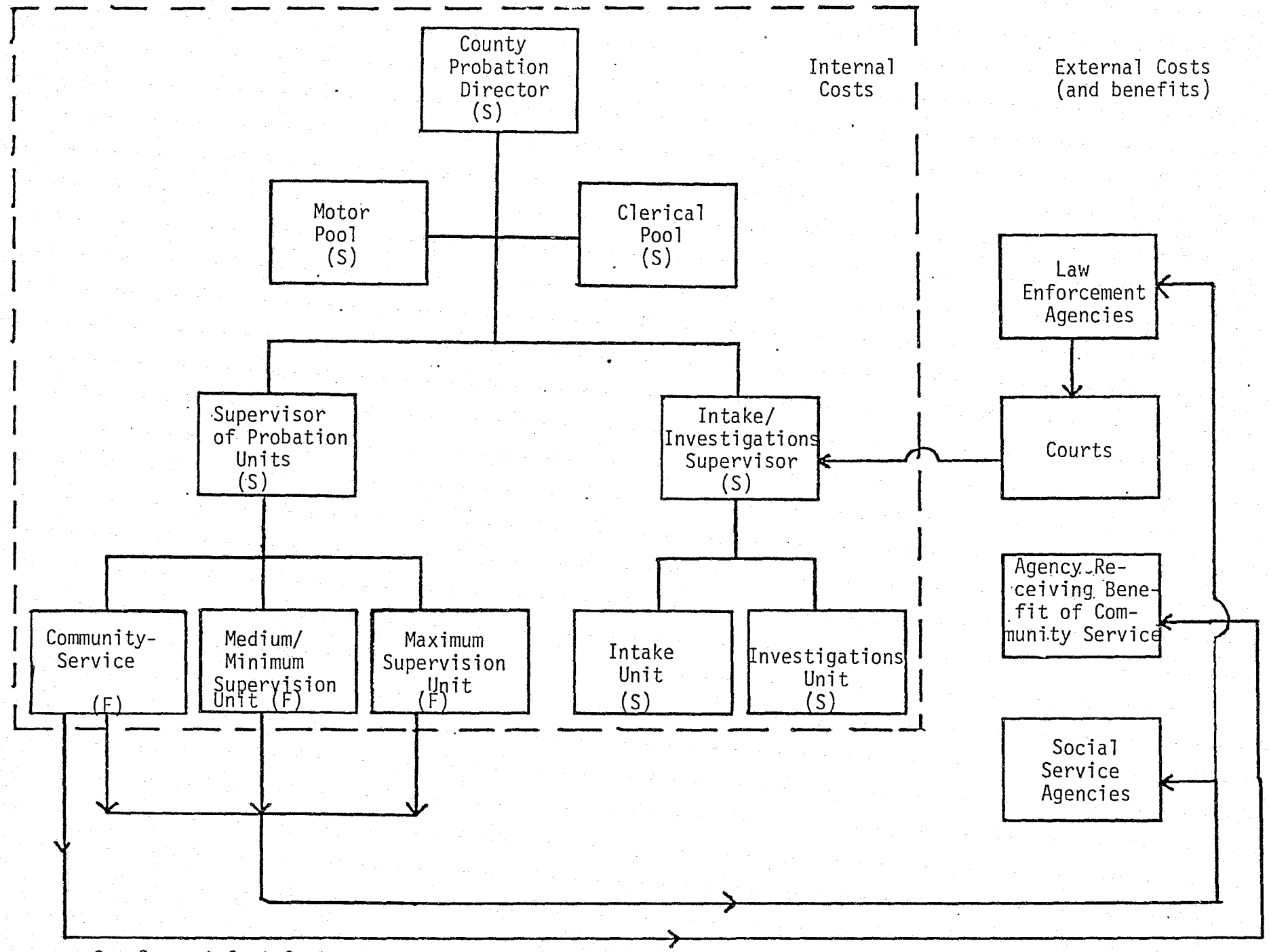
Rehabilitation is the objective of providing social services during probation. This implies that the socioeconomic functioning of persons on probation can be enhanced. Thus, some of the outcome measures involve changes in employment and family/dependents status as well as criminal behavior. Newman, Burwell and Underhill (1978) found in their study of cost effectiveness in community mental health programs that client costs varied widely according to the outcome of their treatment. Their "cost outcome matrix" indicated that the average costs per "improved" client were $265.44, whereas costs of clients who regressed were $202.51. The clients who were maintained at their entry level of function were the least costly to serve ($93.91 per client) (Newman et al., 1978: 25).

These findings suggest that the service costs within a given intervention vary according to the service's impact on the probationer. They also remind us that the impact of each intervention varies, depending on the personal characteristics of the probationer and the way he or she is dealt with by the probation officer. The stereotypical "problem" case, for example, demands the agent's frequent attention, typically requiring warnings and fence mending. The probation officer, on the other hand, may expend above average effort in assisting an unusually responsive probationer. In

a third case, the only interaction between the probationer and the agent may be entirely routine, and the probation may end without a new arrest or a technical violation. The computation of a gross estimate of cost per probationer in a program would conceal variation of this sort.

Exhibit 3 depicts the structure of a hypothetical probation agency and can be used to illustrate some of the concepts of cost analysis (parole functions are omitted). In this agency, we have identified a community service unit (which may or may not be so directly linked to the rest of the organization) and two other supervision units as final cost centers. All other organizational units are seen as supportive of the services being provided by these units, although an argument could be made that the Investigations Unit is actually a final cost center.

The salaries of probation officers in each of the supervision units are the direct costs of these cost centers. Indirect costs are the share of the costs of support cost centers which are attributable to activities which help persons in the final cost centers do their job. For example, in order to allocate a portion of the costs of the clerical pool to the maximum supervision unit, a reliable estimate is needed of the amount of time that clerks and their supervisors spend on work for that unit. If the type of work they do for all cost centers is fairly uniform, we might simply determine the proportion of units of output that was produced for the Maximum Supervision Unit. However, since that is probably not the case, a process analysis would have to be done to identify different types of output, the time it takes to produce a unit of output, and the volume of each type of output produced for a given final cost center.



S: Support Cost Center
F: Final Cost Center

Exhibit 3. Cost Centers in Probation Agencies

Similar allocations, using a variety of cost-finding approaches, can be performed until all the costs of the agency are allocated to these three final cost centers. This yields the input costs for the center. Then, a crude measure of output costs can be determined simply by dividing total costs by the number of probationers who are supervised in a year. Unfortunately, this ignores the length of sentence (and, therefore, service), and more refined measures have to be developed. These would take into account the activities which were involved in delivering a given service and seek to measure the mix of activities that were involved in the delivery of service to each probationer.

Outcome costs are determined, first, by measuring differences in outcome and then, assuming some are found, identifying, for example, the cost per reduction in recidivist incidents of using a more expensive approach. If no differences in impact are detected, then the least expensive alternative is the most cost-effective.

The diagram also points out the domains of internal and external costs. Only activities which take place within the broken line produce costs which must be borne directly by the DPP office. The costs which are imposed on law enforcement agencies and indirectly on the courts due to less effective probation strategies are difficult to measure and external to the probation agency. The same is true of the costs which a social service agency bears when it provides services to a probationer on referral from a probation officer. Again, this evaluation is more concerned with internal costs than those which do not directly affect DPP budgets.

3. PROCESS AND COST ANALYSIS

A. Introduction

As their understanding of the public policy process has become more sophisticated, evaluation researchers have come to recognize the importance of process evaluation. Now, few in the field fail to recognize that, in order to get off the "drawing board" (i.e., legislation, federal regulations), policies and programs must be altered, to a greater or lesser degree, to conform to environmental constraints, the goals and expectations of program administrators, and the characteristics of target populations. Since these policy and program modifications occur so consistently, it is essential that evaluators anticipate them and document their occurrence. In fact, according to Palumbo and Sharp (1980: 289), an assessment of a program's impact should not be undertaken before an analysis is done of how that program was implemented; in other words, before a process evaluation has been done.

The compelling logic behind the argument of Palumbo and Sharp is that process evaluations concern the issue of "goal-directedness" (Franklin and Thrasher, 1976: 151) as opposed to goal achievement, as is the case in impact evaluation. It is of little use, according to their position, to evaluate the impact of a program that is incongruent with the goals and plans on which it is based. Instead, using Suchman's (1967) terminology (Weiss, 1972: 38) notes that such "program failures" must be overcome before an assessment can be made of the underlying theory of change.[1] Thus, a valid

[1]Pressman and Wildavsky (1973: xv) have written, "Policies imply theories. Whether stated explicitly or not, policies point to a chain of causation between initial conditions and future consequences."

and reliable test of a given approach to dealing with a public problem can only be accomplished if the program that is implemented conforms to the program that was designed.

Viewed from the perspective of experimental research, the focus of process evaluation can be described as the "integrity of the treatment." According to Sechrest and his associates, too much attention has been devoted to questions of internal validity, to the detriment of construct validity:

> Treatments as they are delivered in real settings are rarely standardized as they are in the best laboratory experiments. Real treatments are often complex, are sometimes delivered by poorly trained or unmotivated people, and can be totally disrupted by events in the real world. Thus, in many cases, the failure of the actual treatment to produce any significant effect may tell us nothing about the potential effect had the treatment been correctly implemented (Sechrest et al., 1979: 15-16).

In spite of general agreement that process evaluation should be an integral component of almost any evaluation, there is no standard approach for this type of research. Although some efforts have been made in this direction (e.g., Leithwood and Montgomery, 1980), a generic model for process evaluation has not been developed. One reason is the difficulty of operationalizing the concept of goal-directedness. Another is the fact that process evaluations are being done by analysts whose experience and training are derived from three separate and distinct research traditions.[1] They include, first, the fieldwork approach which grew out of the ethnographic research tradition in anthropology and sociology. A second approach

[1]For a review of these three traditions in process evaluation, see Charles L. Usher and Jay R. Williams, "Process Evaluation in Community Corrections," presented at the Annual Meeting of the Southern Sociological Society, Memphis, TN, 1982.

might be termed implementation analysis. Its intellectual roots are in the field of public policy analysis in political science. The third approach evolved from the management sciences, in particular industrial engineering.

Each approach to process evaluation involves a different perspective on the internal operations of public programs, as well as different evaluative techniques.

Each approach also has a particular disciplinary orientation, but each makes a unique contribution to a more complete understanding of the substance and method of process evaluation. Thus, this study adopted a multidisciplinary approach which integrated all three perspectives. This approach linked the evaluation to a conceptual model similar to those employed in political science that incorporate a realistic conceptualization of the public policy process. This integrated approach also involved the application of standardized measurement techniques drawn from industrial engineering, accounting, and other management sciences. Finally, this approach acquired the sensitive insights and color than can be derived only through intensive fieldwork.

B. Phases of the Analysis

The process evaluation for this project was performed during each of four phases of the experiment--design, implementation, in-treatment, and post-treatment. In the design phase, it was necessary to devise a randomization strategy that was acceptable to Maryland's District Court judges. Then, it was necessary to define the "treatments" that would be provided to probationers in the experimental groups. During the implementation phase, the key concern of the research staff was to maintain the integrity of the assignment process as well as the treatments. Once the assignments had

been made, the most important issue became the degree to which the treatments being delivered conformed to these that had been planned for each group. This concern was addressed by thorough documentation of the manner in which persons in the different groups were treated. Finally, following the release of project participants from probation, a cost analysis was performed to develop data necessary for the cost-effectiveness analysis that was the primary focus of the project.

As indicated by this discussion, process evaluation is an ongoing set of activities, but one that is composed of a variety of distinct research efforts with different objectives. In the following sections, each phase of the project and its related process evaluation activities are described.

1. Design Phase

The impetus for the research conducted in Maryland was the rising caseloads and, hence, rising costs of probation in recent years. Many states are using probation as an alternative to incarceration for felons, but other states, such as Maryland, also have experienced growth in their probation caseload from increased numbers of less-serious offenders. As a result, the resources of many probation agencies are being diverted from more-serious cases by the demands of less-serious cases. Due to the lack of reliable data concerning the effectiveness of probation supervision, it has not been possible to argue either that supervised probation was an inefficient means of controlling and rehabilitating various types of offenders, or that it was essential to insure the public's safety and the offender's rehabilitation. Therefore, in light of the potential financial benefits of the two alternatives to supervised probation that were being proposed, it was decided to evaluate their cost-effectiveness in a field experiment.

Naturally, the most significant obstacle to implementing a research strategy that involves random assignment is the cooperation of the judges whose cases are involved. Fortunately, most judges were aware of the problem and were interested in helping to alleviate the burden on the Maryland Division of Parole and Probation (DPP). They also were interested in the potential effectiveness of community service programs that were just being established in Maryland. Meeting each judge and describing the project, nevertheless, was time-consuming.

In spite of their general spirit of cooperativeness, the District Court judges involved in the project did impose some constraints. First, special conditions that they attached to the order to probation (e.g., mandatory alcohol or drug treatment, brief jail sentences) were not to be ignored. These conditions made some offenders ineligible for participation. Second, if probationers failed to comply with the terms of the alternative they had accepted, they were not to be returned to the court for admonishment. Thus, the only sanction that could be used to encourage compliance with the voluntary community service agreement was the threat of being transferred into supervised probation. Finally, the judges did not agree to a waiver of fines, court costs and restitution for community service volunteers, making the experimental community service model nonconforming to the model employed in some Maryland jurisdictions.

A second problem that had to be addressed in the design phase was the need for randomization. The significance of this aspect of the project was underscored by a recommendation from a committee of the National Academy of Sciences.

> [R]andomized experiments should be the design of choice for evaluating every rehabilitation program or intervention and . . . the use of any alternative should require explicit justification subjected to rigorous and searching examination (Sechrest et al., 1979: 60-61).

Such an approach also was encouraged by the success in this type of research conducted at the local level.

During the design phase, the problem was simply who would have responsibility for the assignment process. Basically this involved a choice of the court or the Intake Unit of the local DPP office. Given their interest in and commitment to the project, the DPP seemed a better choice. This inclination also was reinforced by a desire to avoid influencing the judges' sentencing behavior, something that would have occurred if the court had taken responsibility for the assignment process. Therefore, with the approval of the judges to offer these alternatives to eligible probationers, the DPP Intake Units took responsibility for offering a randomly assigned treatment alternative to each probationer who had received a sentence of 12 months or less on supervised probation.

Two alternatives to supervised probation were delineated during the design phase. One was simply unsupervised probation; i.e., the probationer would not be contacted by or be required to report to a probation agent, except to pay fines, court costs or restitution. Nevertheless, persons who solicited help from a probation agent were to be served. The differentiated probation case management system used in Maryland classifies offenders according to risk and need for service before they are assigned to minimum, medium, or maximum supervision levels. However, no guidelines existed prior to this project for placing probationers in an unsupervised status. Therefore, special procedures had to be devised to insure that this initial assessment process was bypassed for persons with this assignment.

The other alternative to supervised probation was community service, an increasingly popular alternative for dealing with less-serious offenders (Cooper et al., 1981). This alternative proved much more complicated to design and implement than unsupervised probation because DPP's role in

community service programs varied among localities that operated such programs. In some counties, a DPP agent managed the program, while in at least one county a CETA worker performed that task under DPP supervision. In addition, it was not clear in some cases whether the community service volunteers had to report to an agent to fulfill their commitment. As a result, it was necessary to establish a basic model of community service that was not affected by the case management system's regular case handling procedures.

In Baltimore City, community service probationers worked for the recreation department cleaning up park facilities and similar maintenance work. This community service program had to be established for District Court probationers with the cooperation of the DPP and agencies of the City. This meant that, unlike some other jurisdictions in Maryland, the community service program being studied had not existed before the initiation of this project.

Another design issue that involved community service was the amount of work required. According to one study of community service restitution (Cooper et al., 1981: 96-99), the hours of service required of misdemeanants might range from 10 to 40 hours, depending on the seriousness of the offense. For example, the Maryland DPP (1980: 12-13) specified three categories of offenders (misdemeanants, minor offenders, and non-violent felony offenders) with maximum service requirements of 64, 120, and 240 hours respectively. Based on our discussions with community service coordinators in other Maryland jurisdictions, a 40-hour requirement to be completed within 60 days appeared to be consistent with prevailing standards (Cooper et al., 1981: 96-99). Therefore, a decision was made that such a requirement would be imposed in each case involving the community service alternative.

Furthermore, consistent with the judges' stipulation, no fines, court costs, or restitution payments could be suspended, and no cases would be closed until all financial obligations were paid.

2. Implementation Phase

Two sets of issues became most prominent in carrying out the experiment. The first set involved the identification and assignment of eligible probationers. The other set of issues pertained to the integrity of the probation alternatives during the in-treatment phase. The first set of issues is discussed below, while the other is discussed in the next section of this chapter.

The validity of the research findings is premised, at least partly, on the assumption that a representative group of less-serious offenders who typically receive probationary sentences of 12 months or less was identified and assigned to the three groups. This required the DPP Intake unit in Baltimore to systematically identify eligible candidates, offer them a particular probation experience based on the assignment sheets provided by the researchers, and make the appropriate referrals for the case. Three problems could have arisen in the assignment process. First, DPP Intake agents could have failed to systematically identify eligible candidates, thereby jeopardizing the representativeness of the pool of eligible candidates. The second potential problem was that self-selection among the eligible probationers in deciding to participate or not to participate could have biased the representativeness of the pool and the individual groups. Finally, in making assignments to the experimental groups, it was tempting for the Intake agents to ignore or override the assignment sheet's designation to the unsupervised category, especially when assigning a person they perceived as "high-risk."

Appendix A includes a detailed comparison of the characteristics of those persons identified at intake as eligible for participation in the project with those of all 1981 Baltimore City District Court intakes. Although, the two samples differed slightly by data collection period and by the eligibility criteria, only slight differences emerge from that comparison. Generally, the profile comparisons for the characteristics of marital status, education, employment, and whether fines, costs, or restitution were levied show a good match between the Baltimore City 1981 intakes and the project intakes. However, the number of cases that were found to be eligible for the project was considerably smaller than DPP officials had projected. The reasons for this are reviewed extensively in Appendix A.

Another problem that could have arisen in the assignment process was the emergence of experimental groups that were systematically different from one another. The issue was also addressed in Appendix A. That comparison of intake data for probationers who agreed to participate in the project permits us to draw some conclusions concerning the equivalence of the experimental samples that were produced by the assignment process. The data presented in Appendix A indicate that participants and nonparticipants were not significantly different in terms of their sex, race, age, marital status, educational achievement, employment status, or criminal offense. Some differences between the groups of participants and non-participants, as well as among experimental groups, were noted, however. For example, it appeared that a larger proportion of the probationers who declined participation had relatively short probationary sentences. Also, it seemed that persons in the community service program tended to have longer sentences

and fines or restitution requirements. This indicates a slight self-selection bias;[1] however, the only suggestion of bias in assignments by the Intake agents was the low proportion of unsupervised probationers who had to pay fines or restitution. Generally, though, it appeared that the experimental groups were equivalent in composition.

In summary, the assignment process proceeded largely according to plan. It appears that a representative pool of eligible probationers was identified and agreed to participate in the project. Furthermore, the assignment of probationers in that pool produced experimental groups that were demographically equivalent although persons who accepted the community service option tended to have somewhat longer sentences (one year versus six months or so) and to have been required to pay fines or restitution.

3. In-Treatment Phase

The first question addressed during the in-treatment phase was the integrity with which the planned probation alternatives were actually delivered. In the case of the group that was to receive regular supervised probation, it was only necessary that their probation officers not be informed of their status as an experimental subject. This was to avoid their receiving more attention or more intensive service than other supervised probationers. Our monitoring of intake forms as information was collected during the experimental intake period indicated that notations were not made on the forms that would identify the probationers as project participants.

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[1]The overall rate of refusals to participate was 30.2 percent; however, the rates of refusals for the three experimental treatments varied substantially. In contrast to the 23.1 percent rate among those offered unsupervised probation, nearly half (48.2 percent) of those probationers who had the option of volunteering for community service refused it. Nevertheless, as the above data indicated, only slight differences, usually statistically insignificant, emerged in the comparison of participants and nonparticipants.

The primary problem with the unsupervised group was that they might have been contacted if they failed to pay fines or restitution. Depending on the frequency and nature of these contacts, this interaction could have amounted to supervised probation. However, the agents assigned to monitor the payment of these fines and restitution had extremely large caseloads composed of low-risk clients, so only minimal contact was expected. In the follow-up survey of the participants in the project, a series of questions was asked to determine the nature and extent of contact between probation officers and members of each group. In addition, the case records of a sample of probationers were examined to identify recorded contacts. This permitted us to measure in two ways the degree to which the unsupervised group was, in fact, not supervised. Results of both types of analysis are discussed below.

One measure of the extent to which probation agents and clients in the various treatment groups interacted was based on the case records of probationers involved in the study. One year after the last subject's case had been opened, subsamples of 75 cases from each of the three groups were selected for analysis. The most important source of data for this analysis was the face sheet (DPP form no. 14) that is a required part of each probationer's case record (Appendix E). This form provides a concise chronological record of contacts and other case-related activities. The use of standardized codes simplified the data collection process and enhanced the reliability of the data (Appendix F).

The data presented in Table 2 describe the frequency of agent-client contact for participants in each of the three study groups. From the distribution shown, it is clear that supervised probationers actually

Table 2. Frequency of Client Contact and Other Case-related Activities[a] Noted in Case Record

	Probation Category		
	Supervised	Unsupervised	Community Service
Client Contacts:			
0	5.3%	54.7%	14.7%
1-5	8.0	29.3	20.0
6-10	16.0	9.3	16.0
11-20	42.7	2.7	23.9
21-30	16.0	--	13.3
>30	12.0	4.0	12.0
	100.0%	100.0%	99.9%[b]
$\chi^2 = 92.68$ $\alpha<.001$			
Other Case-related Activities:			
0	10.7%	13.3%	8.0%
1	14.7	33.3	18.7
2	26.7	29.3	32.0
3	22.7	13.3	18.7
4	12.0	9.3	13.3
>4	13.3	1.3	9.3
	100.1%[b]	99.8%[b]	100.0%
$\chi^2 = 17.23$, n.s.			
(n)	(75)	(75)	(75)

[a]Case-related activities include violation hearings (5.8 percent of the cases in this sample), information gathering (courts and local police--29.8 percent of the sample), motor vehicle record checks (1.8 percent of the sample), state police record checks (79.7 percent of the sample), FBI record checks (4.0 percent of the sample), and other undesignated activities (28.4 percent of the sample).

[b]Column percentages do not sum to 100 percent due to rounding.

received the most supervision. A majority of this group had contact with an agent at least monthly. In fact, the mean number of contacts for supervised probationers was 12.3.

It is also apparent from these data that persons who accepted the unsupervised alternative were not often in contact with DPP during their probationary period. More than half (54.7 percent) of their case records showed no contacts, and another 29.3 percent indicated fewer than six contacts. Overall, probationers in this group averaged 3.5 contacts with a probation agent. Thus, it appears that the case management system was effectively bypassed so that persons assigned to the unsupervised status tended not to come into contact with DPP agents. Other data, however, indicate that a few unsupervised probationers (15.7 percent) had to be changed to an active supervision status (usually minimum supervision). Nevertheless, a very high proportion of this group remained unsupervised throughout their probationary periods.

The frequency of contact for community service volunteers was higher than that of the unsupervised group, but not as high as that of supervised probationers. Probationers in this category averaged 8.8 contacts with an agent during probation. As indicated by Table 2, approximately half (50.7 percent) had ten or fewer contacts, but nearly the same proportion had at least monthly contact with DPP. Additional information about the experience of this group will shed some light on this inconsistent pattern.

Data describing the status of cases at the time of their closing indicate that only 28.2 percent of the persons assigned to community service actually completed their assignment. Of those who failed to complete the community service assignment, 26 percent were reassigned to minimum supervision, 46 percent to medium supervision, and 28 percent to maximum

supervision. Relatively more probationers who did not complete their community service requirement were deemed to need medium or maximum supervision than were probationers originally assigned to supervised probation. The distribution for the group initially assigned to supervised probation was 13.3 percent in minimum supervision, 67.3 percent in medium supervision, and 20.0 percent in maximum supervision.[1] The widely dispersed pattern of agent-client interaction reflected in Table 2 is very important to the issue of the integrity of the community service alternative. That issue is addressed in more detail later in this report.

The most common type of agent-client interaction for probationers in this sample of 225 clients was an office visit by the client. As indicated in Table 3, 60 percent of the study participants whose records were examined had made such a visit. Nearly as many (54.7 percent) had talked with a probation agent on the telephone. In 41.8 percent of the cases, the agent called a third party about the client; however, it appears that these calls usually were not made to employers or those who lived with the client. Collateral contacts were made with employers in only 7.1 percent of the cases and at the home of the client in only 14.2 percent of the cases. The frequency of these types of contact increases only slightly by including negative contacts; i.e., situations in which an intended contact was not made.

The average number of contacts (the second column in Table 3) suggests that most cases showing agent-client contact do not involve frequent interaction. In many cases, the means shown in the table are inflated by a few

[1]It is likely that the experience gained from interaction with the clients during the initial stage of their community service experience provided a clearer basis for assessing their need for supervision when reassignment became necessary.

Table 3. Nature and Extent of Client Contact

Nature of Contact	Cases Involving Contact	Mean Contacts for Cases with Contacts
Positive Contacts with Client:		
Office	60.0%	5.1
Telephone	54.7%	5.1
Client's Home	12.9%	2.9[a]
Miscellaneous	16.4%	2.2[b]
Collateral Contacts:		
Office	6.2%	1.4
Telephone	41.8%	3.1
Place of Employment	7.1%	1.4
Client's Home	14.2%	4.0[c]
Miscellaneous	46.7%	4.2
Negative Contacts:[d]		
Client's Home	9.8%	1.8
Place of Employment	0.4%	1.0[e]
Miscellaneous	0.9%	1.5[f]

[a]Only one home visit was made in 37.9 percent of these cases; another 36.2 percent were visited twice by a probation agent.

[b]Of the cases involving these miscellaneous contacts, 62.6 percent were reported to have had one contact.

[c]Nearly half of these cases (46.9 percent) involved only one collateral contact at the client's home.

[d]When an intended contact is not made, it is "negative".

[e]$n = 1$.

[f]$n = 2$.

extremely large values. Since the vast majority of cases were required to serve a 12-month probationary period, the rates of contact suggest that most were not in contact with DPP on a monthly basis. Furthermore, evidence of written monthly reports from the probationer was found in only 18.2 percent of the cases (data not shown).

Another indicator of the intensity of probation casework is the frequency of other case-related activities. Recorded on the face sheets in the case records of study participants, these activities include attending violation hearings, gathering information about a case from court or police officials, and checking motor vehicle, state police, or FBI records. The system in Maryland also provides for recording judicial conferences and trips involving the transportation of clients, but such activities were not found in our examination of probation case records.

The data concerning other case-related activities in Table 3 have a pattern similar to the client contact pattern, at least for the supervised and unsupervised categories. More activities were recorded for supervised probationers than for unsupervised probationers (means of 2.7 and 1.8, respectively). Also, the level of activity for the community service group (mean = 2.5) falls between the other two groups. In this case, however, the difference between the supervised probationers and those initially assigned to community service is not as great.

The relatively smaller intergroup differences observed in case-related activity levels are related to the nature of the activities. For example, DPP procedures require that a criminal records check be performed for each new case. As a result, intake agents routinely request a state police record check. When the rap sheet is ultimately routed to the case folder, the addition of this information to the record constitutes a "case-related

activity." It is not surprising, therefore, that even cases in the unsupervised category exhibit a minimal level of activity.

In addition to examining case record data to assess the level of interaction between DPP and the study participants, follow-up respondents were asked about their contacts with probation agents while on probation as an additional indicator of agent-client interaction. There are almost always discrepancies between self-reports of behavior and data derived from secondary sources such as case records (e.g., Lichtman and Smock, 1981: 88-89). If important differences appear in the two reports, the conclusions drawn solely on the basis of one data source would be suspect.

Table 4 compares self-reported frequency of telephone contact with the frequencies recorded on the face sheet. In approximately half of the cases (49.7 percent), the survey and case record data are in agreement. In slightly more than a third of the cases (35.3 percent), the case record indicates more frequent contact than the respondent recalled. It is notable that in most of these cases the probationer did not remember receiving more than one call. The remaining 15 percent of the cases involved survey respondents whose recollection suggested more frequent contact than was recorded in the case record.

Table 4 also contains a similar analysis for office visits. Again, in nearly half of the cases the survey data and case record data are roughly the same.[1] In more than 90 percent of these discrepancies, however, the respondent recalled having made more office visits than were recorded on the face sheet in the case record.

[1]The form of the questions in the survey does not permit point estimates of contacts because response categories were in the form of ranges; e.g., two or three times per month versus once a month. The estimates in Table 4 are based on rates estimated from these data as well as information about the length of time the respondent served probation.

Table 4. Frequency of Telephone Contacts and Office Visits According to Survey Report and Case Record

	Probationer's Survey Self- Report				
Case Record	<2	2-5	6-10	>10	Totals
Telephone contacts:					
<2	75	10	4	2	91
2-5	31	4	2	2	39
6-10	20	2	3	5	30
>10	3	3	--	1	7
Total	129	19	9	10	167[a]
Office Visit:					
<2	51	5	10	12	78
2-5	6	6	16	15	43
6-10	1	--	16	21	38
>10	1	--	--	8	9
Total	59	11	42	56	168[a]

[a]This represents the group of cases for which both survey and case-record data are available.

Three problems arose in the implementation of the community service option that threatened its integrity. First, the probation agent who had responsibility for making community service assignments and monitoring compliance had to attend the DPP training academy as soon as the assignments were complete. This meant that some of the probationers in the program were not monitored for several weeks and, therefore, were not pressured to comply with the agreement they had signed. The second problem arose because probationers in the community service program who had large fines still had to pay them upon completion of the community service assignment. Because community service programs often "forgive" fines for those who complete work assignments, and because the Maryland judges would not agree to such a condition, we believe there was a disincentive to complete work assignments.

The third problem was related to the incentive to participate in and to complete work assignments. It was recognized at the outset that many of those who were to receive the community service experimental assignment would perceive the work requirement as a burden, and may thus refuse to participate in the research.[1] As an incentive to participation, community service probationers were advised that they would be recommended for release from probation after they completed 40 hours of work. The work was to be completed within 60 days so that for most of the subjects the probationary period would have been significantly shortened (82 percent had sentences of 12 months duration). DPP and the District Court judges agreed to the early probation release incentive. The incentive proved insufficient, however, as indicated by a comparatively high refusal to participate rate (discussed in Appendix A) and a high rate of failure to complete the 40 hours for those who did agree to participate.

For those three reasons only 28 percent of those assigned to the community service option, completed their 40 hour work obligations. It seems clear from an analysis of the community service option that success of such a program depends on careful screening of participants, matching of the probationer and his or her work assignment, and supervision of work participation. The newness of the Baltimore community service program, the lack of supervision and insufficient incentive all contributed to the low work completion rate. The noncompliance constrains the analysis by inhibiting the ability of this study to draw valid conclusions about community service as a cost-effective alternative to supervised probation.

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[1]Informed consent procedures required that subjects be advised in advance as to which of the experimental conditions they would be assigned to if they agreed to participate.

It is just as clear, however, that the group of offenders assigned to an unsupervised status were not monitored to any real extent during their probationary period. By the same token, those study subjects who underwent regular supervised probation were in regular contact with a DPP agent while on probation. Therefore, it will be possible to compare the experiences of these two groups to determine the effects of supervision on recidivism and community adjustment.

4. In-treatment and Post-treatment Phases

In addition to the follow-up data that were collected from probationers and from probation and police case records, data were collected about the costs of handling probation cases. Weimer and Friedman (1979: 262) recommended that cost analysis be a "standard component" in evaluations of offender-rehabilitation programs. It is, of course, essential in cost-effectiveness analysis; but the fact that most public agencies do not have a well-established system of cost centers often reduces the accuracy of cost estimates and makes this portion of the analysis more difficult.

In order to calculate the costs of supervising a probationer, an analyst must know exactly what activities are involved in probation supervision. The analysis in the previous section identified discrete probation services as well as the number and types of services required by different types of cases. It suggested that only cases in the supervised group actually imposed significant costs on DPP beyond intake.[1] The unsupervised group was largely ignored and, thus, relatively cost-free.

[1]The community service group's failure rate was very high and a large proportion of these probationers were reassigned to supervised probation, making it impossible to calculate costs per case for the community service option.

Gray and his associates (1978) identified three types of costs in their study of residential community corrections. They included input costs, output costs, and outcome costs. The following discussion follows that framework in developing estimates of the costs of serving and supervising probationers in this study.

Table 5 summarizes the input costs associated with probation supervision in local DPP offices in Baltimore City. Whereas the study team led by Gray (1978) measured input costs in terms of the cost of a day of service, we have chosen to measure them in terms of the annual costs associated with an average probation agent. This includes the direct costs of the agent's salary and the indirect costs associated with supervision, clerical support, job-related travel, and office space.

Salaries for probation agents in the ten local offices we studied ranged from an average of $20,125 for Senior Agents to $11,670 for the one Agent I identified in the DPP budget data.[1] Since Senior Agents are unlikely to be responsible for the types of cases under study (recall from the previous section that very few cases assigned to supervised probation are assessed as needing maximum supervision), our focus is more appropriately on the Agents III and II. The weighted average salary for the 81 agents in these positions is $16,838.

Also identified in Table 5 are the indirect costs associated with the support of each probation agent. A comparison of labor costs for three categories of personnel--supervisors, agents, and clerical--permits us to estimate the staff support costs for probation agents in Baltimore City.

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[1]These data apply to 1982, the period during which the study subjects were being supervised; the ten offices are those from which case record data were obtained for the process analysis.

Table 5. Cost Data for Local Division of Parole and Probation Offices

Staff Position	Mean Salary	Baltimore City Local Offices										Totals	Costs
		A	B	C	D	E	F	G	H	I	J		
Field Supervisor II	$23760	1			1		1	1	2	1		6	$142560
Field Supervisor I	$21778	3	1	2	2	1	3	3	8	1	1	23	$500894
Subtotal		4	1	2	3	1	4	4	10	2	1	29	$643454
Senior Agent	$20125	15	6	10	10	7	15	15	35	5	4	113	$2274125
Agent III	$17284	9	3	4	5	2	10	8	24	4	2	65	$1123460
Agent II	$15025	2		3	1		1	3	6		1	16	$240400
Agent I	$11670						1					1	$11670
Subtotal		26	9	17	16	9	27	26	65	9	7	195	$3649655
Office Supervisor	$13822						1	1				2	$27644
Office Secretary II	$13158	1							2			3	$39474
Office Secretary I	$12158		1	1	3	1	1	2	4	1	1	13	$158054
Stenographer/Clerk III	$10416	3		2	3		3	3		1		14	$145824
Stenographer/Clerk II	$9795							1	1		1	2	$19590
Stenographer/Clerk I	$8518	1		2	1		1		4			9	$76662
Typing Clerk IV	$11324		1						1			2	$22648
Typing Clerk III	$10136	1					2		4			7	$70952
Typing Clerk II	$8708	1				2	1			1		4	$34832
Subtotal		7	2	5	7	3	9	7	16	3	2	56	$595680
Supervisor:Agent Ratio (__:1)		.15	.11	.12	.19	.11	.15	.15	.15	.22	.14	.15	$0.18
Clerical:Agent Ratio (__:1)		.27	.22	.29	.44	.33	.33	.27	.25	.33	.29	.29	$0.16
Annual Building Lease		$9750	$5464	$24255	$8580	$12150	$37912	$13640	n/a	$8820	$9000	$111751	
Lease Costs Per Agent		$375	$607	$1426	$536	$1350	$1404	$524	n/a	$980	$1285	$859	
Travel Costs: Personal Autos		$5837	$3945	$3936	$3410	$2358	$9076	$4980	$18660	$3133	$3423	$52202	
Travel Costs: State Autos		$495	$686		$1136		$1664		$5033	$866	$1276	$9014	
Travel Costs: Combined		$6332	$4631	$3936	$4546	$2358	$10740	$4980	$23693	$3999	$4699	$61216	
Travel Costs Per Agent		$243	$514	$231	$284	$262	$397	$191	$364	$444	$671	$313	

It appears, on the basis of the DPP data, that each dollar of an agent's salary is associated with an additional $0.18 for supervision and $0.16 for clerical support. This amounts to a 34-percent overhead rate for administration, effectively increasing the annual cost of an average middle-range agent to $22,563.

Other indirect costs are travel and office space. The use of state automobiles and reimbursement to agents for the use of their personal cars are the two main travel costs. Both types are reported in Table 5 along with the average combined costs per agent. This cost of $313 increases the agent's cost to $22,876 per year. The addition of $859 for leased office space brings the total input costs to $23,735.

As indicated above, the high rate of noncompliance among probationers assigned to community service and the ad hoc nature of the program established to satisfy the study's demands preclude a valid test of program effects. In addition, a detailed assessment of the costs of administering such a program would not be illuminating because the procedures established could not be used as a model for other jurisdictions. It is possible, however, based on DPP's experience in Baltimore City and elsewhere to offer a few observations concerning the costs of community service.

It had been the experience of several jurisdictions in Maryland that community service programs involving hundreds of volunteers can be administered very inexpensively by a single probation agent. For example, based on data provided by the DPP agent who was responsible for the program in Prince George's County at the time this study began, more than 75,000 hours of community service were produced from 2,781 court referrals in 1979. Using prevailing minimum wage rates, this translated into $217,616 in labor for that county. Given that volunteers often could be supervised with

regular work crews, no additional administrative costs were associated with this labor (it was necessary, however, to pay supervisors overtime in order to accommodate volunteers who could only work nights and weekends). It seemed apparent, nevertheless, that this was a great burden to a single agent, and that assistance from community service volunteers was needed to monitor the caseload of court assignees.

The program in Baltimore City was established to follow this model. A probation agent who had formerly worked with a special employment project for DPP was assigned to administer the program prior to assuming responsibility for a regular caseload. It was his responsibility to match community service volunteers with positions that were made available through the Baltimore City Parks and Recreation Department. Through the joint efforts of this agent and the Parks and Recreation Department, a variety of types of work was available on weekdays and weekends, and during daylight and evening hours. Unfortunately, the rate of noncompliance was so high that the program never reached an equilibrium in which the flow of probationers entering and leaving the program was constant. Therefore, it is not possible to estimate costs, particularly output or outcome costs, for this group.

Output costs, as described by Gray et al. (1978), are costs per case. This is a basic measure that does not reflect a level or quality of service, but merely the unit costs of service. In the case of the Maryland DPP, it is possible to estimate such costs on the basis of the the differentiated case management system used in that state. A workload study done by Maryland DPP (1981) at the time the study was implemented indicates that client-to-agent caseload ratios were 45:1 for maximum caseloads, 100:1 for medium caseloads, and 200:1 for minimum caseloads. Based on the fact that more than 80 percent of the subjects entering the supervised-probation assignment were put into medium caseloads, it seems appropriate to use the 100:1 caseload

ratio in estimating output costs. Thus, the cost of supervision for an average probationer in a medium caseload is approximately $237.

It was noted above that supervised probationers averaged 12.3 contacts with a DPP agent, whereas the average for the unsupervised category was 3.5. Since so much of the costs of supervision arise from contacts, it could be estimated that the costs for the supervised group were at least 3.5 times greater than the costs of the unsupervised group (12.3 / 3.5 = 3.5). Thus, the costs imposed by the unsupervised group may be on the order of $68 per subject.

Outcome costs attempt to inject the dimension of effectiveness into cost analysis. Newman et al. (1978), for example, found that the costs of mental health services varied according to the outcome of the treatment a patient received. The costs of treating "improved" clients were found to be more than double those of "maintained" clients, but only about 30 percent higher than the costs of treating clients who regressed. The following series of tables attempts to address this issue for the probationers we studied.

The upper half of Table 6 compares the frequency of agent-client contacts for three groups--probationers who were arrested following the opening of their probation case; probationers who completed at least two years without an arrest following the opening of their probation case; and probationers for whom no record of arrest was available.[1] As indicated by the chi-square statistic, no statistically significant difference is observed in the level of contact for these groups. This suggests, that the costs of supervising recidivists were not significantly different from the costs of supervising persons who completed probation and went another year without being arrested again.

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[1]Chapter 5 addresses the issue of recidivism more intensively and provides a description of data sources and characteristics.

Table 6. Number of DPP Contacts and Police Contacts or Arrests After Beginning of Probationary Period

	Number of DPP Contacts					
	0	1-5	6-10	11-20	21-30	>30
State Police Record of Arrest						
No Record Obtained	14.3%	14.0%	9.7%	19.2%	18.2%	4.8%
No Subsequent Arrest Record	50.0	27.9	48.4	36.5	36.4	23.8
Record of Subsequent Arrest	35.7	58.1	41.9	44.2	45.5	71.4
Totals	100.0%	100.0%	100.0%	99.9%[a]	101.1%[a]	100.0%
$\chi^2 = 13.7$ n.s.						
Probationer's Survey Self-Report of Police Contact						
No Report Obtained	23.2%	11.6%	12.9%	13.5%	9.1%	14.3%
No Contacts Reported	57.1	65.1	67.7	61.5	59.1	47.6
Police Contact Reported	19.6	23.3	19.4	25.0	31.8	38.1
Total	99.9%[a]	100.0%	100.0%	100.0%	100.0%	100.0%
$\chi^2 = 7.61$, n.s.						
(n)	(56)	(43)	(31)	(52)	(22)	(21)

[a]Some column percentages do not sum to 100 percent due to rounding.

This finding is reinforced by the survey data presented in Table 6. Respondents to the follow-up surveys who did not report police contact following the opening of their probation cases had approximately the same level of interaction with DPP as respondents who reported having been stopped by the police. Although the rate of contact for both groups seems somewhat higher than that of nonrespondents, the difference is not statistically significant.

Another indicator of cost is the occurrence of other case-related activities. We noted in Table 2 that the most frequently recorded type of activity was state police record checks. Since this type of activity comprises such a large proportion of total activities reported for the cases we reviewed, we have compared the frequency of this activity with the police record and survey-based indicators of recidivism described above.

Table 7 indicates almost no difference in the frequency of record checks between recidivists and probationers who were not recorded as having a subsequent arrest. The same finding is revealed by responses from probationers who were interviewed six months and/or twelve months following the scheduled expiration of their probationary period. In conjunction with the data reported in the two previous tables, this suggests that the cost of supervision is not systematically related to outcomes.

C. Summary and Conclusions

This chapter described the process and cost analyses undertaken during the four phases of this study. In the design phase, it was important to gain the cooperation of District Court judges in permitting DPP to employ randomization in assigning probationers who received sentences of 12 months or less without special conditions. Although a high degree of cooperation was obtained, probationers who did not comply with a community service assignment could not be brought back to court to gain their cooperation.

Table 7. Number of Record Checks and Police Contacts or Arrests After Beginning of Probationary Period

	Number of Record Checks				
	0	1	2	3	4 or More
State Police Record of Arrest					
No Record Obtained	18.8%	13.4%	11.1%	12.9%	20.0%
No Subsequent Arrest Record	35.4	37.8	38.9	48.4	30.0
Record of Subsequent Arrest	45.8	48.8	50.0	38.7	50.0
Total	100.0%	100.0%	100.0%	100.0%	100.0%
$\chi^2 = 3.11$, n.s.					
Probationer's Survey Self-report of Police Contact					
No Report Obtained	16.7%	13.4%	14.8%	16.1%	20.0%
No Contacts Reported	56.3	61.0	61.1	64.5	60.0
Police Contact Reported	27.1	25.6	24.1	19.4	20.0
Total	100.0%	100.0%	100.0%	100.0%	100.0%
$\chi^2 = 1.24$, n.s.					
(n)	(48)	(82)	(54)	(31)	(10)

A design problem that did not surface until after implementation began was that the only community service program operating in Baltimore City was not able to be used in the study because of constraints on the types of clients that it was able to serve. This required hasty efforts to establish a community service alternative in Baltimore City using the resources of DPP and the Department of Parks and Recreation.

As a result of special conditions attached to probation orders, including drug and alcohol treatment and explicit work orders to community service programs, a large proportion of the potentially eligible population could not participate. The problem was so extreme that almost no participants came from two study sites, and more than 90 percent of the subjects resided in Baltimore City.

In addition to the smaller than expected number of eligible probationers, many probationers refused the community service alternative. Potential subjects were informed of the exact nature of the alternative proposed for them and were required to sign a consent form acknowledging awareness of these special circumstances. Unlike the programs in some jurisdictions, this program did not offer relief from fines or restitution requirements. Both the informed consent requirements and the maintenance of fines and restitution seemed to increase the refusal rate.

The in-treatment phase of the project was marked by a high rate of noncompliance by the community service volunteers. The DPP had no sanction to apply and no incentive to offer to encourage continued participation. In addition, some probationers were not monitored regularly because the agent assigned to the project was required to attend the training academy during the latter part of the intake and in-treatment phases. In contrast, there was no evidence that the persons assigned to regular supervised

status or the unsupervised alternative received treatment that deviated from what had been planned. Therefore, in spite of the problems encountered with the group assigned to the community service option, the field experiment proceeded as planned for the other two groups.

Indicative of the integrity of the supervised and unsupervised treatments was the consistent pattern of agent-client contact and other case-related activity revealed by an analysis of case record data. Supervised probationers were contacted regularly, and unsupervised probationers were not monitored to any great extent during their probationary period. Whereas the supervised group averaged 12.3 contacts during probation, the unsupervised group averaged but 3.5 contacts.

Using data obtained from DPP, it was possible to estimate the input, output, and outcome costs of supervising probationers assigned to a medium caseload. An estimate of $237 per case per year was derived from an analysis of ten local offices in Baltimore City. Based on differences in frequency of contact between supervised and unsupervised probationers, it was estimated that the costs to DPP of the supervised group was approximately 3.5 times that of the unsupervised group. In addition, no evidence of increased recidivism was found among those who received less contact or whose cases involved less activity.

The next chapters delve into the issues of social adjustment and recidivism in greater depth. Although a final conclusion about the cost-effectiveness of supervised probation must await the findings presented there, the data presented here do not suggest that the relatively high costs of supervised probation produce lower rates of recidivism.

4. SOCIAL AND COMMUNITY ADJUSTMENT

Some years ago, a group of students from the United States were visiting a prison in Denmark which was unusual by American standards. The prison was a minimum security facility which allowed prisoners to leave during the day to go to jobs and return at night. Both males and females (some females had small children with them) were housed in the prison and mixed freely. In addition, beer was available to the prisoners. The students noted that while the conditions of the prison were surprisingly liberal, it was still a prison and staffed by correctional personnel. They asked the director of the unit if the inmates had any difficulty adjusting to the prison. His reply was that he hoped they never adjusted to prison. Their goal was to provide the necessary treatment for the inmates so that they could better adjust to Danish society.

In many ways, American probation has similar goals to the Danish prison. As Dressler (1969:16) notes, "probation is a treatment program designed to facilitate the social readjustment of offenders." This treatment program is administered in the community rather than in a prison or correctional facility. The adjudicated offenders do not have to adjust to a prison environment, but they are expected to adjust to the community and society in which they live. The probation period gives them the opportunity to make these adjustments or, at minimum during their "period of testing or furnishing proof of goodness" (Henningsen, 1981:11), at least not to get into further difficulties with the law.

Various levels of probation supervision may be specified for offenders--some with strict treatment and monitoring regimens and others with no supervision but with the opportunity to use community resources identified

by the probation agent. Those probationers needing a low level of monitoring absorb a great deal of the probation agents' time to the apparent detriment of those probationers with greater monitoring and service needs. The purpose of this chapter is to explore the social and community adjustment of the probationers in the sample who were assigned to supervised probation (predominantly medium to minimum level) and those assigned to unsupervised probation.[1] If those who go unsupervised show indicators of adjustment equal to those who are minimally supervised, perhaps selected probationers could go unsupervised, thereby giving probation agents more time to monitor cases requiring intensive supervision. Further, finding no differences in social and community adjustment for the two probationer groups may also suggest that minimal level probation can have positive rehabilitative outcomes for some.

A. Parole and Probation Assessment of Adjustment Factors

The Maryland Division of Parole and Probation (DPP) assesses a variety of adjustment factors for each probationer. The needs of each probationer are assessed, and a treatment plan and a plan to monitor the probationer's progress are developed. Areas specified for the needs assessment are also used to monitor the progress of the probationer. Exhibit 4 shows the Case Assessment, Treatment, and Supervision Plan form and the eleven categories of the Needs Assessment Rating Scale. Scores from one to five are assigned in each of the 11 categories at three points: intake, midpoint of probation, and at case closing. Needs assessment rating instructions are included in Appendix C.

[1]Community service probationers are not included in the analysis because so many did not complete their work obligations. This resulted in a mixing of supervision and no supervision during the study period.

Exhibit 4

CASE ASSESSMENT, TREATMENT AND SUPERVISION PLAN

Case Name _____ Case # _____

USING THE NEEDS ASSESSMENT RATING SCALES,[1] RATE THE CASE AT INTAKE, AT SIX MONTHS (MIDPOINT), AND ONE YEAR (FINAL).

Date Assigned _____

		I	M	F
1.	Relationship w/family	____	____	____
2.	Relationship w/significant other(s)	____	____	____
3.	Living conditions	____	____	____
4.	Physical health	____	____	____
5.	Mental health	____	____	____
6.	Vocational	____	____	____
7.	Employment	____	____	____
8.	Financial situation	____	____	____
9.	Drug abuse	____	____	____
10.	Alcohol abuse	____	____	____
11.	Attitude toward supervision	____	____	____

PROBLEM AREA(S): _____

_____

_____

COMMUNITY RESOURCE NEEDS: _____

_____

_____

SCHEDULE AND GOAL OF PERSONAL CONTACTS: _____

_____

_____

SCHEDULE AND GOAL OF COLLATERAL CONTACTS: _____

_____

_____

SCHEDULE AND GOAL OF EMPLOYMENT CONTACTS: _____

_____

_____

COMMENTS: _____

_____

_____

[1]Needs assessment rating scale instructions for probation officers are included in Appendix C.

The case records of a random sample (n = 225) of the study subjects were reviewed, and selected information on the ratings for supervised cases at intake, at midpoint, and at the close of the cases using needs assessment rating scales was gathered. Unfortunately, only a small number of cases had complete information for the three points in time. Nevertheless, the data in Table 8 indicate probationers' adjustments as judged by their probation agents.

Table 8. Comparisons of Rating Scales Changes from Intake to Case Closing

Factors Considered	Direction of Change		
	Improved	No Change	Got Worse
1. Relationship with Family[a]	2	19 (90%)	--
2. Relationship with Others	3	16 (84%)	--
3. Living Conditions[a]	1	15 (88%)	1
4. Physical Health	--	18 (95%)	1
5. Mental Health	1	18 (95%)	--
6. Vocational Situation	3	15 (79%)	1
7. Employment Situation	4	11 (58%)	4
8. Financial Situation	2	13 (68%)	4
9. Involvement in Drugs	1	16 (84%)	2
10. Involvement in Alcohol	--	18 (95%)	1
11. Attitude toward Supervisor	2	13 (68%)	4

[a]Due to missing data, the numbers do not correspond to the other categories.

Despite the small number of cases, where multiple assessments were made, the pattern is clear--most of the sample represented here were not perceived as having changed from their initial rating. Most adjustment factor assessments show that 80-90 percent of the probationers are in the "no change" category. Moreover, there is no clear pattern of change; some assessment categories move toward improvement, others move toward deterioration.

Family relationships were judged initially to be mostly positive, and this category experienced little change except for two improved relationships. Relationships with others seemed troubled, but the majority of probationers were judged positive on this factor. Again, most made no change in their relationships with others and those that did, improved. Living conditions varied; some were negative or neutral, but most were positive. This proved to be stable at case closing. Physical health was rated more positive for this group than was mental health, although both were rated positive overall. During the period of probation, both physical and mental health remained largely unchanged. Vocational, employment, and financial situations were somewhat negative (especially the vocational rating--that is, the job level). Although these categories remained mostly stable, there were changes in both the improved and worse directions. Drugs and alcohol did not appear to be problems for this group (with the exception of one alcohol problem identified). Over the period of probation, the great majority of this group remained stable in their ratings. Finally, the attitude toward the probation supervisor seemed to slip for some of the group. A smaller portion of them showed improvement, with overall high positive stability on this factor.

B. Family and Employment Status

The first section of Table 9 shows the distribution of marital status for the probationer sample providing data one year after their probation has been completed. Approximately three-quarters of both the supervised and unsupervised probationers were unmarried. These percentages are approximately the same as at the beginning of probation, and the marital status distribution is not different for the supervised and unsupervised probationers. Being assigned to one or the other supervision level does not appear associated with changes in marital status. The distribution of number of dependents was also examined for the supervised and unsupervised probationers (data not shown). No differences were found between the groups for this variable.

Table 9. Marital and Employment Status One Year Following Probation

	Probation Category	
	Supervised	Unsupervised
Marital Status		
Married/Cohabiting	22.3%	21.2%
Unmarried[a]	77.7	78.7
χ^2 = .03, n.s.		
Employment Status		
Full-time	46.8%	38.3%
Part-time	3.2	10.6
Unemployed	50.0	51.1
χ^2 = 4.57, n.s.		
Number of Months Out of Work		
0	29.8%	36.6%
1-4	14.9	15.1
5-8	8.5	10.8
9-12	46.8	37.6
	100.0%	100.1%
χ^2 = 3.62, n.s.		
(n)	(94)	(93)

[a]Includes single, separated, divorced, widowed.

The second section of Table 9 shows that approximately half of those in the two categories were unemployed one year after probation. The chi-square statistic shows that the two groups did not differ significantly from each other on the basis of employment status.

The third section of Table 9 shows the number of months that probationers in the two supervision categories were out of work in the year after probation. Substantial percentages in both categories (46.8 and 37.6 percent) spent nine or more months out of work during that time period. The two groups were not significantly different from each other in their time out of work experiences.

C. Community Agency Contacts

Large communities have a variety of agencies that help citizens with various needs. Probationers can use these services during their probation periods to assist with social and community adjustment. Probationers were asked specifically about the Welfare Department, an Employment Service, the Housing Authority, an Alcohol or Drug Treatment Program, Legal Aid, a Mental Health Center, and a Public Health Center and whether they had contacted agencies other than those specified above. When other agency contact was indicated, it was usually an agency that provided assistance in obtaining work.

Table 10 summarizes whether a contact with a community/agency was made during the respondent's period of probation. Even though the supervised probationers through their required contacts with their probation agent had potentially more opportunity to be referred to and to contact various community agencies, the agency contact roles for the two groups were not significantly different.

The second section of table 10 shows the number of agencies contacted by supervised and unsupervised probationers. While the supervised probationers were somewhat more likely to contact multiple agencies (32 vs. 18 percent contacted two or more agencies), the difference between the two groups was not statistically significant.

The Table 10 data provides some assurance that probationers needing assistance who are not formally supervised on probation will find their way to service agencies. Half the supervised probationers contacted agencies, but so did 39 percent of the unsupervised probationers. Thus, instructing probationers with no obvious service needs to contact their probation officer only when a need arises seems to be a reasonable approach--at least for the kinds of probationers included in this study.

Table 10. Community Agency Contact During Probation

	Probation Category[a]	
	Supervised	Unsupervised
Any Community Agency Contact		
Yes	50.0%	39.4%
No	50.0	60.6
	$\chi^2 = 1.74$, n.s.	
Number of Community Agency Contacts		
0	50.0%	60.6%
1	17.0	21.3
2	19.1	8.5
3 or more	13.8	9.6
	$\chi^2 = 9.49$, n.s.	
(n)	(94)	(94)

[a]Column percentages may not add to 100.0 due to rounding.

The types of agencies contacted are listed in Table 11. Contacts with the welfare department and the employment service in addition to the employment emphasis of the "other agency" category reflect efforts on the part of both the supervised and unsupervised probationers to get help in these areas.

More supervised probationers reported contacting the welfare department and alcohol and drug treatment programs than did unsupervised probationers. It is possible that the supervised probationers were encouraged by their probation officers to seek out these services.

Table 11. Type of Agency Contact During Probation Period

	Supervision Level	
	Supervised	Unsupervised
Agency	Contact	Contact
Welfare Department	28.7%	18.1%
Employment Service	31.9	28.7
Housing Authority	9.6	3.2
Alcohol Treatment Program	12.8	2.1
Drug Treatment Program	5.3	--
Legal Aid	4.3	6.4
Mental Health Center	2.1	1.1
Public Health Clinic	6.4	5.3
Other Agency[a]	11.7	5.3
(n)	(94)	(94)

[a]The most frequently mentioned agencies or programs in this category were Manpower, CETA, Prisoner's Aid, and Social Services.

In summary, a comparison of supervised and unsupervised probationers indicates few differences between the groups. They do not differ in marital and employment status after probation, although the supervised probationers may be more likely than the unsupervised probationers to make use of available public services. Thus, the supervision level provided to these probationers

does not appear significantly related to subsequent adjustment, and unsupervised probationers appear able to find their way to available public services without the active supervision of probation officers. It does appear likely, however, that the unsupervised probationers were less likely to seek alcohol or drug treatment services than if they were included in a supervised caseload.

5. RECIDIVISM

A. Introduction

It is generally accepted that recidivism is the most important outcome indicator for criminal justice system clients. If an individual becomes reinvolved in criminal activity during or after his or her probation and/or is arrested, convicted, or incarcerated again, probation cannot be viewed as a complete "success." It is also true, however, that recidivism and success are not simple concepts. Even if it is accepted that rearrest is the most appropriate indicator of recidivism, the frequency, type and timing of arrests are aspects of recidivism that need to be considered.

Multiple indicators of recidivism will be examined here. These will include:

- noncompletion of probation,
- self-reports of criminal behavior,
- self-reports and official records of arrest,
- time until failure.

The approach will compare these outcomes for the three experimental groups: regular supervised probation, unsupervised probation, and community service. Because of the high noncompletion rate for the community service group described earlier and in Appendix A, the recidivism results should not be attributed to effects of that program.

Before describing the recidivism data, the next two sections will discuss (1) approaches to measuring recidivism, and (2) the relationships of supervision and service levels to recidivism.

B. Approaches to Measuring Recidivism

Recidivism can be measured in any number of ways: rearrest, reconviction or reincarceration. The length of time over which recidivism is measured varies from weeks or months to many years. Clearly, the magnitude of recidivism will be affected by the criterion measure chosen and by the length of the experience period. Hoffman and Stone-Meierhoefer (1980) found that recidivism among a sample of 1,806 prison releases ranged from nine to 60 percent; nine percent were reimprisoned within one year of release, and 60 percent were arrested within six years of release. Waldo and Chiricos (1977) found that the recidivism of a group of work releases ranged from 20-70 percent, depending on which of 18 measures was used.

Estimating recidivism is also not a simple matter in other ways. While success or failure can be conceived as an all or none phenomenon, such an assessment strategy may miss important differences in outcomes. An individual arrested once during an experience period should be distinguished from one who is arrested multiple times; arrest for the offense of robbery has different implications than arrest for a minor offense like drug possession. Moreover, it is important to examine when during an experience period failure occurs. A steady-state probability of failure, that is a risk of failure that does not change during the experience period, should be distinguished from one that varies according to segment of the exposure period.

Recent work in the assessment of recidivism has begun to deal with the complexity of the recidivism issue. Building on the work of Stollmack and Harris (1974), Barton and Turnbull (1981) used a nonparametric regression approach to failure rate analysis. They used time from prison release

until recidivism as the dependent variable, and their approach accommodated the fact that many subjects never recidivate. Schmidt and Witte (1980) used a truncated lognormal regression approach to model time from prison release to reimprisonment for 2,216 prison releases. Thornberry and Jacoby (1979) and Wainer (1981) displayed recidivism data by numerous exposure time categories. This approach gives the number of failures for each time period category so that specific period as well as cumulative failure rates are observable. Harris et al. (1981) examined three analytic techniques frequently used to assess outcomes in correctional research. The authors looked at the assumptions of the techniques and applied them to recidivism data for 257 Illinois parolees. A mixed exponential model provided the best fit to the data, but the authors point out that other analysis situations may be better served by a different technique.

Survival rate analysis has been used in some recent correctional research (Flanagan, 1981; Clarke, Freeman and Koch, 1976; Lichtman and Smock, 1981). This approach has been used for some time in medical research to estimate mortality from disease. Some patients diagnosed as having had a heart attack, for example, will have subsequent heart attacks and some will not, either until their deaths from other causes or until a cutoff point. The cutoff point ends an arbitrary length of time and is referred to as the censor point. Recidivism can be viewed as analogous to disease relapse and analyzed with the survival analysis approach.

Survival analysis evaluates the time period between two events. In the present study, being put on probation is the first event and recidivism or expiration by censor is the second event. Individuals who recidivate will do so at different points in the time period. The goal here will be to determine whether the experimental groups differ from each other in the

timing of their recidivism, that is, whether their survival distributions differ. If the groups differ (for example, if unsupervised probationers failed earlier than regular supervised probationers), this may suggest an initial period of supervision should precede the unsupervised period.

In addition to allowing the evaluation of the time element in recidivism, survival analyses maximize the data available for the assessment of recidivism outcomes. Traditionally, recidivism studies have set a length of time, for example two years, during which subjects are classified as having failed or not failed. The same experience period length is chosen for all subjects so that recidivism rates are comparable. This approach wastes the data for longer periods that may be available for some subjects. Survival analysis makes use of all of the subjects' experiences up to the censor point. The approach does not depend on equal time periods and thus maximizes the analytic potential of data.

C. Probation Services and Recidivism

Over the years there has been considerable interest in the effects of probation or parole supervision on recidivism. The issue has been formed mostly in terms of the effects of caseload size and intensity of supervision. The effects of supervision are important to the purposes of this study because of their implications for the increased use of probation in lieu of incarceration and for the question of how much supervision is necessary to minimize recidivism within a cost effectiveness perspective.

The relationship between supervision and recidivism is not easily summarized. Some of the evidence was reviewed earlier. Methodological problems aside, the preponderance of recent evidence suggests that level of supervision has no effect or only a small effect on the subsequent recidivism of criminal justice clients (Gottfredson, Mitchell-Herzfeld and Flanagan,

1982, Lichtman and Smock, 1981; Star, 1979; Urban and Rural Systems Associates, 1981). If the findings of this previous work are replicated in the Maryland Probation Study, we should find few if any significant differences in the recidivism experience of the supervised and unsupervised probationers and the community service group.

D. Recidivism Data

Recidivism data for the Maryland probationers have been gathered from three sources: client interviews, probation case files, and Maryland State Police criminal histories (rap sheets). During interviews (see Chapter 2 for description of interview schedule and methodology), clients were asked to report whether they had contacts with the police since being put on probation. They were asked to give the date and reason for each contact (if any) and to describe the outcomes of each contact (for example, were you fingerprinted, found guilty). Several interview questions asked the probationers whether and how often they committed eleven kinds of illegal behaviors since being put on probation, regardless of arrest. The illegal activity types were: aggravated assault, robbery, burglary, auto theft, other theft, forgery, dealing in stolen property, gambling, pimping or prostitition, drug sales or manufacturing and driving while intoxicated.

Eighty-four percent (n = 312) of the subjects were interviewed; seventy-five percent (n = 278) were interviewed twice. Recidivism data were taken from only one interview, the later in the case of those interviewed twice. Overall, 36 percent of the interviewed probationers (n = 111) reported having had a contact with the police since being put on probation; 14 percent reported more than one police contact. From zero to four percent reported being involved in at least one of the illegal activities included in the interview schedule. Theft was the most likely offense to be reported;

3.9 percent (n = 12) said they had committed a theft other than an automobile theft.

Random samples of 75 probationers from each of the three experimental groups were selected (n = 225) for inclusion in the probation case record study. Three items of information from the probation case records were of interest for the recidivism aspect of the study: 1) whether there was an indication in the case file that a criminal justice agency had picked up or had contact with the probationer, (2) whether and how often a state police record check was indicated in the file, and (3) whether the case record file showed successful completion of the probation sentence. With regard to number one above, the automated police information system is supposed to tell those who query the system that an individual is currently on probation. The police (or others) are then supposed to notify the Department of Parole and Probation that they have had a contact with the probationer and give the reason for the contact. Thirty percent of the 225 subjects included in the case record analysis had a police or similar (for example, court, jail) notification of contact in their probation case folders. The case records showed that 79 percent of the files included a state police record check (number 2 above).

The case records showed that 73 percent of the 225 subjects included in the case record study successfully completed their probations on time. The remainder either failed to successfully complete their probationary periods or had the period extended. Many subjects who did not complete their sentences on time had their probations extended because they were behind in payment of fines, costs or restitution.

Two separate rap sheet requests were made from the Maryland State Police for all the subjects, yielding rap sheets for 87 percent of the clients (n = 322). We had assumed we would receive rap sheets for close to 100 percent of the subjects because all subjects had at least one arrest and conviction--the one that resulted in their probation. We can only assume that the 49 individuals for whom we did not receive rap sheets did not, for some reason, currently appear in the state police files.

It was originally expected that Federal Bureau of Investigation (FBI) rap sheets would also be collected to improve the scope of coverage for each individual's arrest history. Police records for a particular state may not include information on arrests in other states. However, during the period of the research the FBI suspended its rap sheet services for research purposes for approximately a year, and we were unable to arrange for collection of FBI rap sheets within the study period.

Earlier work has shown that official records of arrest are incomplete (Marquis, 1981; Hubbard et al., 1981). In the Hubbard et al. study, a comparison of self-reports of arrest and official records of arrest showed that individuals typically report more arrests than official records show. This suggests official records are often incomplete--a finding which is consistent with the fact that all of the clients in our study had at least one arrest and conviction and state police records did not exist for 13 percent.

Court orders to withhold an official record or to expunge an existing record may be one reason why no official record of arrest was found for 49 of the clients in our study. For some offenders who did not have a previous arrest, the judge may have ordered the record withheld or expunged if probation was completed satisfactorily. Given that many of the subjects in

the study were comparatively minor offenders as reflected by their sentences (probation of one year or less), this would not be surprising.

The 49 individuals for whom no rap sheets were received are not included in the analysis of recidivism using official records. We have opted not to interpret the failure to receive a rap sheet as indicating no arrests before or after the one that resulted in their inclusion in the study. The 49 subjects with no rap sheets are distributed fairly evenly across the experimental groups (17 supervised probation, 14 unsupervised probation, 18 community service), so there is no evidence of systematic bias in the official criminal history data for study subjects.

E. Recidivism of the Experimental Groups

In the sections below, the recidivism outcomes of the supervised and unsupervised probationers and the community service group are compared. Some of the data come from interviews with the probationers, other outcome data were extracted from probationer's case records, and some data were taken from state police criminal histories. Sample sizes differ because there were different completion rates for the interviews and criminal history checks and because the case record study included only 225 subjects--a random sample of 75 from each of the experimental conditions. Section one below begins the recidivism analysis by comparing the self-reported police contacts of the three groups.

1. Self-reported Police Contacts

Table 12 compares the three experimental groups on whether they reported having one or more police contacts since being put on probation. Thirty-six percent reported at least one such contact. The police contacts need not have been for a criminal offense or resulted in arrest. This variable includes such things as being questioned by police and being

warned by them. The percentages of subjects that report police contacts ranges from 33 to 37 percent in the three experimental groups. Chi-square tests of significance for the overall table and for pairwise comparisons within the table (for example, between supervised and unsupervised probationers) indicate no statistically significant differences between the probation subgroups. Thus, according to their own reports, supervised and unsupervised probationers, and probationers put into the community service program do not differ according to whether they had contacts with the police while on probation.

Table 12. Self-reports of Police Contacts Since Going on Probation

Self-reported Police Contacts	Probation Category			
	Supervised	Unsupervised	Community	Total
None	67.0% (67)	63.7% (65)	62.7% (69)	64.4% (201)
One or more	33.0 (33)	36.3 (37)	37.3 (41)	35.6 (111)
Total (n)	32.1 (100)	32.7 (102)	35.3 (110)	100.0 (312)

$\chi^2 = 0.45$, n.s.

The interviews also asked probationers a series of questions about the outcomes of their police contacts. Were you taken to the police station? Fingerprinted? Found guilty? etc. Comparisons of the experimental groups were based on these variables, and no significant differences were found between the groups. Thus, based on their self-reports of police contacts and on the basis of the outcomes of these contacts, supervised probationers, unsupervised probationers, and those on community services do not differ from each other.

2. Recidivism Surveillance

It is routine for the Maryland Department of Parole and Probation (DPP) to request criminal histories from the Maryland State Police at the beginning of probation and, depending on supervision level, periodically during probation and at the conclusion of the probation period. However, two of the three experimental groups included in the research did not fit into the standard DPP caseload management system. The unsupervised category was created specifically for the research to facilitate examination of the effects of supervision levels. Likewise, the community service group was developed at the outset of the research, and standard routines for managing the community service probationers were not established. The differences between the three probationer categories were largely degree of surveillance, and it is possible that the extent of surveillance partially determined the rates of recidivism.

Table 13 breaks the three groups down into two categories--none or one state police record check indicated in the file and two or more such record checks. Zero and one record check requests are combined because we did not wish to excessively weight the unsupervised category. The instructions to probation officers at the outset of the research were to do nothing with unsupervised cases except to respond to requests for assistance from the probationer or information from other agencies. Therefore, in many cases there was not even a criminal history request from DPP to the state police at the onset of probation. Even with this allowance, the table shows that police record checks were much more likely to be requested for the regular and community service probationers. Overall, 42 percent of the case record files examined showed two or more requests for state police criminal histories.

Table 13. Recidivism Surveillance: Number of State Police Record Checks

Police Record Checks	Probation Category			
	Supervised[a]	Unsupervised[b]	Community Service	Total
None or one	44.0% (33)	70.7% (53)	58.7% (44)	57.8% (130)
Two or more	56.0 (42)	29.3 (22)	41.3 (31)	42.2 (95)
Total	100.0 (75)	100.0 (75)	100.0 (75)	100.0 (225)

$\chi^2 = 10.97$, df 2, prob. .004

a/b $\chi^2 = 10.90$, df 1, prob. .001

Only 29 percent of the case files of those in the unsupervised category indicated two or more requests for criminal histories. Chi-square statistics show that the difference between the supervised and unsupervised probationers is significant below the .01 probability level. Differences between supervised probationers and community service subjects, and between unsupervised probationers and community service subjects do not reach a statistically significant level.

In spite of a clear and expected difference between the supervised and unsupervised probationers in the level of recidivism surveillance through state police record checks, we do not believe this has an effect on recidivism itself as measured by arrests indicated on the record checks. Presumably, the recording of arrests on an individual's criminal history is independent of information requests. More frequent checking of the criminal history increases the likelihood that an arrest will come to the attention

of the probation officer and could, thus, make it more likely that a person will be charged with a probation violation. The recidivism surveillance difference, then, could result in higher rates of probation failure.

3. Probation Success or Failure

Table 14 looks at the probation case closings for the three experimental groups. A probation case was classified as successfully completed if the case was closed at the expiration of the original sentence or if the case was closed early due to satisfactory performance. If the case was still open beyond the original sentence date or if there were a revocation or case closing due to unsatisfactory performance, the probation case was classified as a failure.

Table 14. Success or Failure on Probation

Type of Case Closing	Probation Category			
	Supervised	Unsupervised	Community Service	Total
Success	79.7% (59)	80.0% (60)	78.4% (58)	79.4% (177)
Failure	20.3 (15)	20.0 (15)	21.6 (16)	20.6 (46)
Total	100.0 (74)	100.0 (75)	100.0 (74)	100.0 (223)
Overall $\chi^2 = 0.06$	n.s.			

Table 14 shows very little difference between the experimental groups in the percentages of success and failure. Overall, the successful completion rate was 79 percent and the failure rate was 21 percent. The success rates for the three groups were in the narrow range of 78-80 percent, and

the failure rates ranged between 20 and 22 percent. Clearly, the differences are not statistically significant, so apparently the level of recidivism supervision made no difference to successful completion of probation.

4. Self-reported Criminal Behavior

During the interviews, subjects were asked to report whether and how often they engaged in aggravated assault, robbery, burglary, larceny, auto theft, forgery or embezzlement, selling or receiving stolen goods, gambling, pimping or prostitution, manufacturing or selling illegal drugs and driving while intoxicated. Comparatively few probationers reported involvement in any of these 11 illegal behaviors--from 0.3 percent for several offenses to 3.9 percent for larceny. Compared to the results from other self-report studies of criminal justice clients, these reports are low (Collins et al., 1983; Chaiken and Chaiken, 1981; Peterson and Braiker, 1980). Two factors may explain the comparatively low prevalence of self-reported criminal involvement of the Maryland Probation Study clients: (1) the clients are mostly nonserious offenders as indicated by the fact that they received a probation sentence of 12 months or less, and (2) most subjects were interviewed while still on probation and, even though assured that their responses were confidential, respondents knew they were being interviewed in connection with research on probation. Thus, they may have been reluctant to be forthright in their responses about committing crime; involvement in illegal activity is a violation of probation conditions.

There was little reason to believe that those in the three experimental groups would differ in their willingness to report illegal behavior. The perceived risk of reporting such behavior would seem to be the same for all clients. The comparison of the self-reported illegal behavior of the three

groups is reported in Table 15. Because the prevalence of involvement in any one illegal activity type is low and because a comparison of the experimental groups on a type by type basis showed little difference, the 11 offenses are grouped together, and a single illegal behavior prevalence for any kind of offense is presented in the table.

The first section of Table 15 shows that the experimental groups do not differ significantly from each other. Overall, 14 percent of the subjects reported committing at least one of the eleven illegal acts included in the schedule. The percentages in the three groups reporting one or more of the eleven acts varied little, ranging from 12.7 to 14.7 percent.

5. Official Records of Arrest

As discussed earlier, rap sheets were received for 322 of the 371 clients. These cumulative arrest histories were used to construct official criminal histories for the clients. Most clients had a record of arrest prior to the offense for which they were put on probation. The mean number of recorded arrests was 3.2, and there was considerable variation among the clients. The median and modal numbers of arrests were one, but the mean standard deviation was 4.7, and two clients had 32 and 38 previous arrests, respectively, on their records.

The second section of Table 15 compares the experimental groups on the basis of their previous official arrest history. Clients are grouped according to whether they had no previous arrests, one or two, three to five, or six or more previous arrests. There is some variation between the probation subgroups, although the overall chi-square for the table is not statistically significant. The official records of the community service

Table 15. Reported Offenses and Arrest Recidivism

Illegal Acts/ Arrests	Probation Category: Supervised	Unsupervised	Community Service	Total
Self-reported Illegal Acts				
One or more offenses	13.0% (13)	14.7% (15)	12.7% (14)	13.5% (42)
No offenses reported	87.0 (87)	85.3 (87)	87.3 (96)	86.5 (270)
Total	100.0% (100)	100.0% (102)	100.0% (110)	100.0% (312)
Overall $\chi^2 = 0.21$, n.s.				
Number of Officially Recorded Previous Arrests				
None	16.0% (17)	20.6% (22)	11.0% (12)	15.8% (51)
1-2	56.6 (60)	47.7 (51)	49.5 (54)	51.2 (165)
3-5	16.0 (17)	11.2 (12)	18.4 (20)	15.2 (49)
6 or more	11.3 (12)	20.6 (22)	21.1 (23)	17.7 (57)
Total	99.9% (106)	100.1% (107)	100.0% (109)	99.9% (322)
Overall $\chi^2 = 9.56$, n.s.				
Number of Officially Recorded Arrests Since Going on Probation				
None recorded	54.7% (58)	52.3% (56)	35.8% (39)	47.5% (153)
1	14.2 (15)	24.3 (26)	30.3 (33)	23.0 (74)
2	17.0 (18)	12.2 (13)	12.0 (14)	14.0 (45)
3 or more	14.2 (15)	11.2 (12)	21.1 (23)	15.5 (50)
Total	100.0% (106)	100.0% (107)	100.0% (109)	100.0% (322)
Overall $\chi^2 = 15.67$, df 6, prob. .016				

group suggest more serious criminal histories. Only 11 percent of the community service probationers had no previous arrests, and this percentage was 5-10 percentage points lower than for the other two groups. The difference between the supervised probationers and community service probationers is substantial but not statistically significant. Because past research has clearly shown there is a strong direct relationship between previous arrests and recidivism, and because Table 15 suggests the possibility of subgroup differences, some of the later analyses will control for number of previous arrests in the assessment of recidivism.

The third section of Table 15 shows the percentages of probationers arrested in the three groups according to rap sheets. A majority of all clients (52.5 percent) had an arrest charged to their record subsequent to the offense that resulted in their probation sentence.[1] We will refer to the offense that resulted in probation as the current offense. Sixteen percent of all clients were arrested three or more times since the current offense. The length of time covered differs among the clients because current offense dates differ. This variation should not differ systematically by probation subgroup, however, because subjects were randomly assigned to the experimental conditions. The timing of post-current offense arrests will be the focus of the survival analyses later in the chapter.

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[1]The recidivism experience period is slightly different for self-reported and officially recorded arrests. During interviews, respondents reported about arrests "since going on probation." Rap sheets do not show a probation inception date, and so arrests are classified as the current (probation) offense or as occurring before or after that offense.

The Table 15 data show that the community service probationers were significantly more likely than the supervised and unsupervised groups to be officially charged with an arrest. Only slightly more than a third of community service clients did not have an arrest recorded since the current offense. More than half of the other two probationer groups had no post-current arrests on their records. The community service group was also more likely than the other two groups to have a record of three or more arrests--21 percent compared to 11-14 percent. The official arrest records of the supervised and unsupervised probationers did not differ significantly from each other.

The groups did not differ from each other on the number of arrests since the current arrest. The mean numbers of past-current arrests are 1.2, 1.1 and 1.5 for the supervised, unsupervised, and community service probationers, respectively. T-tests indicate these means are not significantly different from each other, although the community service mean is the highest of the three.

6. Arrest Recidivism by Previous Arrests

Table 16 displays the arrest recidivism for the three experimental groups by their previous arrest histories. Arrest recidivism and previous arrest history are each classified into three categories. The categories are collapsed from the four used in earlier analyses to maximize cell size. As expected, the table shows a strong relationship between previous arrests and recidivism. The first row of the table confirms that most of the variation among the experimental groups is explained by number of previous arrests. The percentages of clients who have no officially recorded arrests after going on probation are almost identical for the

three experimental conditions within previous arrest categories. Sixty-four to sixty-six percent of those with no previous arrest or only one had no arrests after going on probation; 22-26 percent of those with two to five previous arrests had no arrests after going on probation; and 10-13 percent of those with six or more previous arrests had no arrests after going on probation. Among those who were arrested since probation started, the results are less clear and consistent, but the effects of number of previous arrests are still observable. Chi-square statistics are significant below the .05 probability level within the supervised and community service groups but reach only to the .135 probability level among the unsupervised group. The partial gamma for the entire table is a moderately strong, .36.

Table 16. Number of Arrests Since Beginning Probation by Number of Previous Arrests

Number of Arrests Since Beginning Probation	Probation Category: Supervised			Unsupervised			Community Service		
	Number of Previous Arrests: 0-1 (%)	2-5 (%)	6+ (%)	0-1 (%)	2-5 (%)	6+ (%)	0-1 (%)	2-5 (%)	6+ (%)
None	65.5	22.4	12.1	64.3	23.2	12.5	64.1	25.6	10.3
1-2	51.5	45.5	3.0	41.0	28.2	30.8	44.7	31.9	23.4
3 or more	33.3	40.0	26.7	41.7	33.3	25.0	26.1	39.1	34.8

	χ^2 Values	df	Probability
Supervised	11.38	4	.023
Unsupervised	7.02	4	.135
Community Service	9.73	4	.045

Overall--Zero Order Gamma .489 Partial Gamma .360

The results of Table 16 suggest the official record recidivism difference found for the community service group in Table 15 is at least partially explained by arrest history differences.

F. Survival Analysis

The timing of failure is important to the assessment of recidivism. A failure rate that does not differ among groups may be misleading if the failure schedules differ for the groups. Recidivism that occurs soon after the beginning of probation should be distinguished from recidivism that occurs two years later. This issue is addressed by the use of survival analysis techniques. The timing of first failure for the three experimental groups is compared using the SURVIVAL program of the Statistical Package for the Social Science (Hull and Nie, 1981).

Two kinds of failure were analyzed: an arrest as indicated by an entry on a subject's rap sheet and a self-reported police contact from the interview data. Survival analysis requires the dating of events, i.e., specification of a time period, so that the time interval between events can be evaluated. The beginning of the time period for the subjects in the current study is the date each individual's probation case was opened. The ending event for purposes of the analysis is either the date of recidivism (a rap sheet entry or the date of a police contact) or, if an individual did not recidivate, a censor point. The censor point for the rap sheet recidivism data was August 23, 1983 when the rap sheets were generated from the Maryland State Police files. The censor point for the interview recidivism data was an approximation of the interview date. For example, the last series of interviews was conducted between July and early September 1983. Mid-August was the approximate time when half of the interviews were complete. Therefore, the censor point for these interview data was specified as August 15, 1983.

The unit of analysis for each individual is the time period from the start of probation to the date of arrest or police contact or, in the

absence of one of these events, the censor date. Separate survival analyses were carried out with the rap sheet data and the interview data so that distinct time periods were specified for each of these analyses.

The analyses described below assess the time-until-failure distributions for categories of probationers. For our purposes, the most important comparison is that of the supervised, unsupervised, and community service probationers. The statistic used to test whether groups differ in their times until failure is the Lee-Desu statistic (Lee and Desu, 1972). The statistic is asymptomatically distributed as chi-square with g-1 degrees of freedom. The larger the value of Lee-Desu, the more likely it is that the survival distributions of the groups are different from each other. The survival analyses results are reported in life table format and by survival rate plots. Group differences will be assessed by use of the Lee-Desu statistic.

1. Time Until Officially Recorded Arrest

Exhibit 5 shows the cumulative, over time arrest survival distributions for the three probation groups. The exhibit represents graphically how the proportions of individuals that survive, i.e., who do not get arrested, decline over time. The graph also pictures how each of the probationer categories do. The lines for supervised and unsupervised probationers remain fairly close, but the community service curve diverges from the other two. Fewer community service probationers survive without experiencing rearrest. These findings are confirmed in Table 17.

Table 17 provides data on the period between the beginning of probation and the date of the first officially recorded arrest for the three probation groups. This time period is broken down into 90-day segments. The cells give the proportions and numbers of people who failed, that is, who experienced an arrest since the current arrest. The "overall failure rates" for

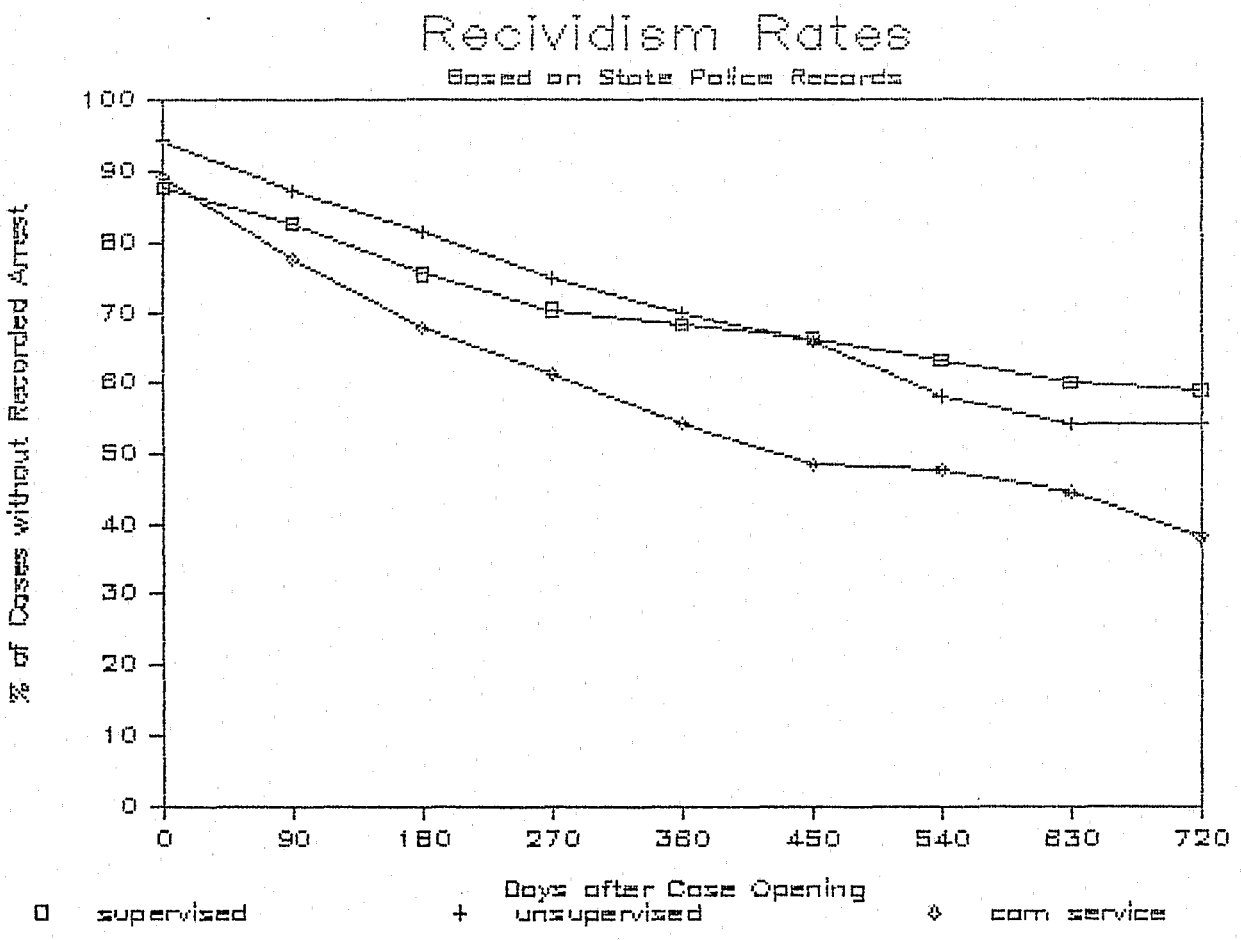


Exhibit 5. Recidivism Survival Plots for Probation Groups (Police Records)

Table 17. Failure Rates for Time Segments by Probation Category

Number of Days from Start of Probation	Probation Category		
	Supervised	Unsupervised	Community Service
<90	.122 (12)	.058 (6)	.107 (11)
90-179	.058 (5)	.072 (7)	.130 (12)
180-269	.086 (7)	.067 (6)	.125 (10)
270-359	.068 (5)	.083 (7)	.100 (7)
360-449	.029 (2)	.065 (5)	.111 (7)
450-539	.030 (2)	.056 (4)	.107 (6)
540-629	.046 (3)	.118 (8)	.020 (1)
630-719	.048 (3)	.067 (4)	.061 (3)
720-809	.019 (1)	.000 (0)	.145 (6)
Overall failure rate	.402	.456	.612

Comparisons	Lee-Desu Statistics	df	Probability
Overall	6.919	2	.031
Supervised/ Unsupervised	.008	1	.931
Supervised/Community Service	4.542	1	.033
Unsupervised/Community Service	5.532	1	.019

Note. Data used to determine failure are from Maryland state police records.

the probation categories differ slightly from those shown earlier because some cases could not be used for the survival analysis because of missing data.

Inspection of the proportions of still-at-risk individuals who were arrested in the time periods suggests two things. First, the community service group has comparatively high failure rates in most time segments. Second, failure rates for all three groups tend to be highest nearer the beginning than the end of the time period. The test statistics indicate the groups' time-until-failure distributions differ, and the community service probationers explain the difference. The time-until-failure distributions for supervised and unsupervised probationers do not differ significantly from each other. Thus, the survival analysis confirms along the time dimension, the findings for the comparison of simple recidivism rates in Table 15. The community service probationers do worse than the other two groups, and the other groups do not differ from each other.

2. Time Until Self-reported Police Contact

A survival analysis was carried out to assess the time until self-reported police contacts for the three experimental groups. The results are difficult to interpret because of a systematic difference in the assessment periods for the three groups. Interviews of the community service probationers were, by design, carried out early in the research. Because the community service group was expected to complete probation earlier than the other two groups, they were targeted for early interview so that they would not be lost to the survey. However, this had the effect of making the exposure period for self-reported police contact shorter than for the other two groups.

Exhibit 6 shows the survival functions for self-reported police contact by probation categories. Although there is divergence of the curves in the early period, the three probation groups do not differ significantly from each other in the timing of recidivism measured by self-reported police contact.

The survival analysis did not indicate statistically significant differences between the three experimental groups in the time-until-failure distributions for self-reported police contact. A comparison of failure proportions for early time segments, however, suggests results consistent with the rap sheet findings. Community service probationers tend to have higher early failure rates for self-reported police contact than do the other two groups. The life table data are not displayed here because of the lack of comparability in the length of exposure periods for the groups.

3. Officially Recorded Arrests and Previous Arrests

It is well established by past research that previous arrest is a strong predictor of subsequent arrest (Chaiken and Chaiken, 1981; Collins, forthcoming; Peterson and Braiker, 1980; Shannon, 1977; Wilkins et al., 1976; Wolfgang, Figlio and Sellin, 1972). Earlier in this chapter it was shown that the experimental groups had somewhat different (though nonsignificant) arrest histories. In this section and again in section five, the relationship of previous arrest to recidivism timing is examined. The survival analysis summarized in Table 18 of this section gives time segment failure rates for three previous arrest categories. Table 19 provides failure rates for the three experimental categories within the three previous arrest categories.

Exhibit 7 shows graphically the divergence of the survival functions according to frequency of previous arrest categories. The lines diverge

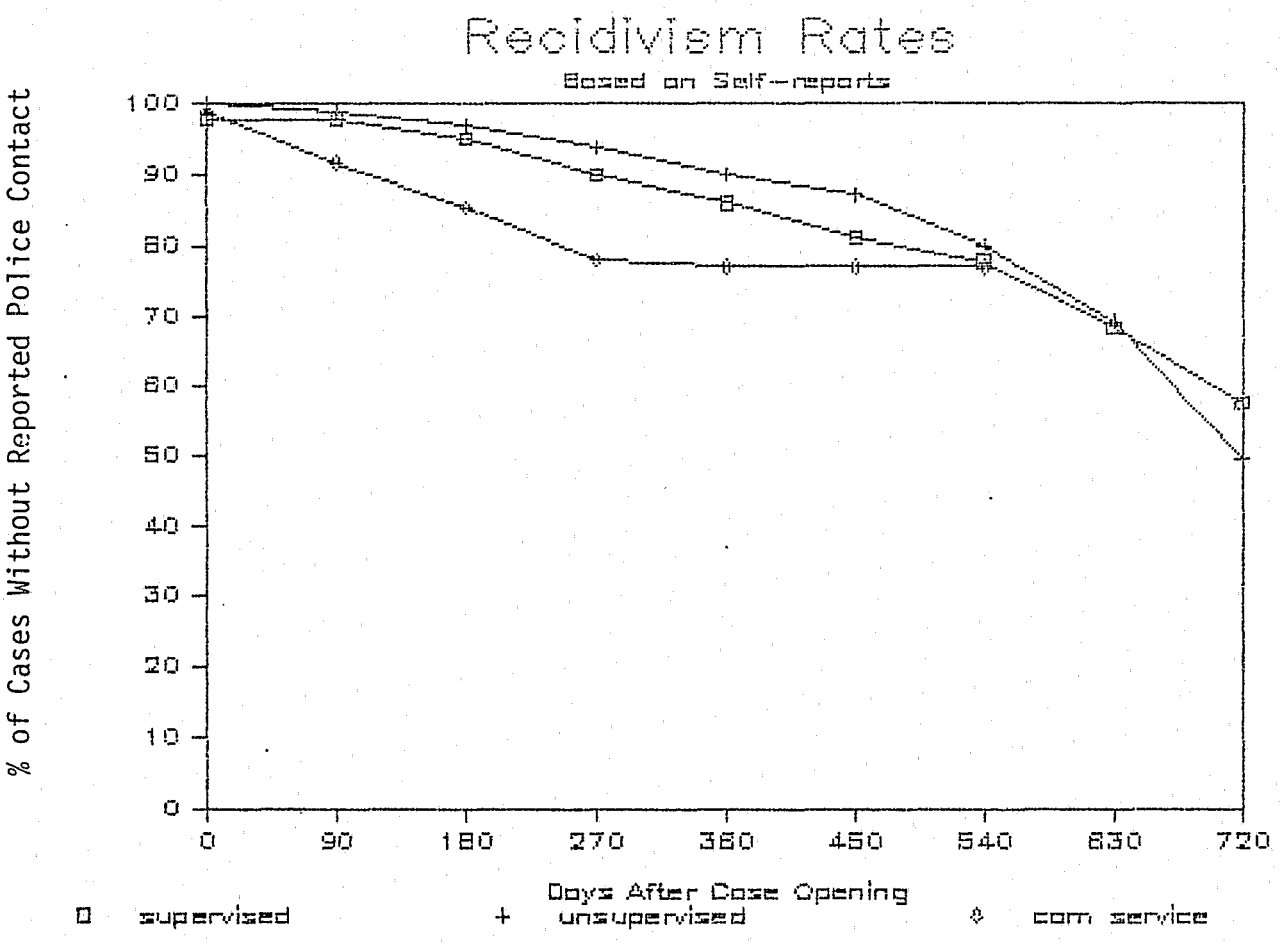


Exhibit 6. Recidivism Survival Plots for Probation Groups (Self-reported Police Contacts)

considerably. Those with fewer previous arrests are more likely to "survive" through the experience period without being arrested.

The Table 18 failure rates, based on the numbers of individuals exposed to risk in each time period, indicate a strong relationship between previous arrests and failure. The overall failure rates increase from 37.6 percent for those who had no previous arrest or only one to 67.9 percent for those with six or more previous arrests. Moreover, those with a higher number of previous arrests were rearrested earlier than those with one previous arrest or no previous arrests. The table shows that the failure rates for the first three time segments range between five and nine percent for the lowest previous arrest category. Failure rates for those with two or more arrests range from 10 to 17 percent for the first three time segments.

The statistical comparison of the three previous arrest categories is also informative. The overall Lee-Desu statistic shows that the time until failure distributions differ for the three categories. Pairwise comparisons locate the significant differences more precisely and indicate the major difference is between the lowest previous arrest category and the other two categories. In spite of the fact that those with six or more previous arrests have an overall failure rate eight points higher than those with two to five previous arrests, these two categories do not differ significantly in the timing of their recidivism. The important difference is that between those who had no previous arrest or one previous arrest and those who had more arrests.

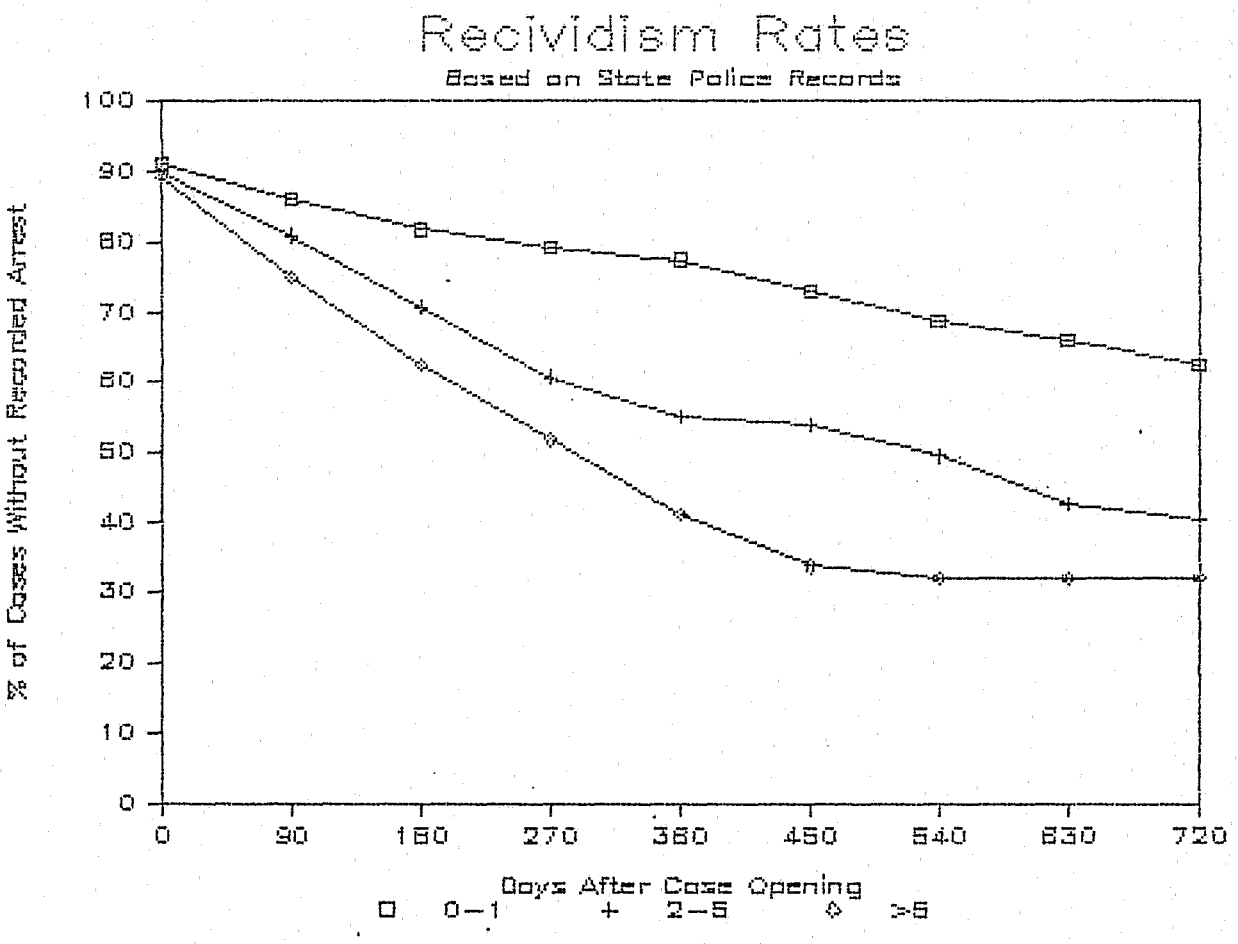
A survival analyses was also carried out using time-until-self-reported police contact as the model variable and number of previous arrests to define the comparison groups. The results were very similar to the results reported above. Those with more previous arrests had earlier police contacts,

Table 18. Failure Rates for Time Segements by Number of Previous Arrests

Number of Days from Start of Probation	Number of Previous Arrests		
	0-1	2-5	6+
<90	.088 (14)	.101 (9)	.107 (6)
90-179	.055 (8)	.100 (8)	.160 (8)
180-269	.051 (7)	.125 (9)	.167 (7)
270-359	.031 (4)	.143 (9)	.171 (6)
360-449	.024 (3)	.093 (5)	.207 (6)
450-539	.057 (7)	.020 (1)	.174 (4)
540-629	.060 (7)	.083 (4)	.053 (1)
630-719	.037 (4)	.136 (6)	.000
720-809	.056 (5)	.057 (2)	.000
Overall failure rate	.376	.597	.679

Comparisons	Lee-Desu Statistic	df	Probability
Overall	21.28	2	.000
0-1/2-5	10.53	1	.001
0-1/6+	17.69	1	.000
2-5/6+	1.98	1	.160

Note. Data used to determine failure are from Maryland state police records.



Number of Previous Arrests

Exhibit 7. Recidivism Survival Plots for Previous Arrest Categories

and the significant differences were between the individuals who had no previous arrest or one previous arrest and those in the two higher previous arrest categories.

The results of these two survival analyses suggest that there is a marked difference between those who have more than one previous arrest and those with one or none. The risk of early recidivism is much higher for the former group.

4. Current Offense Type and Subsequent Arrest

The type of crime one commits has sometimes been found to predict subsequent recidivism. For example, offense seriousness and a history of narcotics use were found to predict recidivism by Wilkins et al. (1976). In order to examine this question for the Maryland Probation Study, the current offense (the offense for which the subject was put on probation and, therefore, became a study subject) was classified into one of four categories, and a survival analysis was done to determine if the failure rate functions differed for the four categories. The current offense was classified as:

- personal injury--assault, robbery, etc.,
- property--larceny, burglary, forgery, etc.
- drug related--narcotics possession, marijuana sales, etc.
- miscellaneous--disorderly conduct, driving under the influence, carrying a concealed weapon, etc.

Table 19 reports the results of a survival analysis to test whether the timing of official arrests after being put on probation differs for the four offense type categories.

Overall, the total failure rates for personal injury and property offenses are very similar (42.8 and 44.6 percent), and the total failure

Table 19. Failure Rates for Time Segments by Type of Current Arrest

Number of Days from Start of Probation	Type of Current Arrest: Personal Injury	Property	Drug-related	Miscellaneous
<90	.100 (4)	.109 (11)	.085 (6)	.087 (8)
90-179	.111 (4)	.100 (9)	.077 (5)	.071 (6)
180-269	.063 (2)	.099 (8)	.100 (6)	.090 (7)
270-359	.067 (2)	.096 (7)	.093 (5)	.070 (5)
360-449	.000	.061 (2)	.041 (6)	.121 (4)
450-539	.000	.032 (2)	.128 (6)	.069 (4)
540-629	.071 (2)	.012 (1)	.098 (4)	.093 (5)
630-719	.077 (2)	.051 (1)	.054 (2)	.061 (3)
720-809	.047 (1)	.000	.092 (3)	.077 (3)
Overall failure rate	.428	.446	.555	.539

Comparisons	Lee-Desu Statistic	df	Probability
Overall	0.752	3	.861

No pairwise comparison approaches statistical significance.

Note. Data used to determine failure are from Maryland state police records. "Current arrest" was the arrest for which the study subject was put on probation.

rates for drug and miscellaneous offenses are similar (55.5 and 53.9 percent). These latter rates are higher than the injury and property failure rates. However, chi-square tests (not shown) indicate the differences in overall failure rates for the offense categories are not statistically significant.

Examination of the failure rates over time for offense types suggests different patterns for the injury and property offenses and the drug and miscellaneous categories. Failure appears more likely to occur earlier rather than later for the injury and property categories. Failure in the drug and miscellaneous categories appears to be more evenly distributed over time or even to increase over time. The Lee-Desu statistics, however, indicate that overall differences and pairwise comparisons do not reach a statistically significant level. Thus, in spite of some apparent regularities in the data that suggest differences, previous arrest type, as categorized here, is not a significant predictor of official recidivism.

5. Rearrest for Probationer Categories by Number of Previous Arrests

Earlier analyses in this chapter showed the community service group had a higher arrest rate since going on probation than the supervised and unsupervised groups. There was also evidence (Table 16) that the previous arrest history of the community service group was at least a partial explanation of the difference. The survival analysis reported in Table 17 above also indicated the community service group differed from the other two groups in their times-until-failure (survival distribution). This section examines the timing of rearrest for the three probation groups within previous offense categories. The analysis will suggest whether previous arrest history explains the difference in timing of recidivism for the groups.

Table 20 shows the findings of a survival analysis of the three experimental conditions while controlling for numbers of previous arrests. Within the 0-1 category, the "overall failure rate" ranges between 31 and 49 percent; failure rates for those with 2-5 previous arrests range between 50 and 68 percent; failure rates for those with six or more previous arrests vary from 36 to 83 percent. The Lee-Desu statistics indicate that within the 0-1 and 2-5 categories, the three probation groups do not differ from each other. For those with six or more arrests, however, there are substantial differences between the experimental groups. While the Lee-Desu statistic does not reach below the .05 level of statistical significance, it does reach below .10, and failure rate differences are substantial. The unsupervised and community service probationers with six or more previous arrests have very high failure rates. The community service probationers, especially, failed early and at a high rate. The numbers of individuals in the six or more previous arrest categories are not high (11, 22, 23), but the unsupervised and community service probationers clearly do not do well. If the findings are valid and generalizable, they suggest that individuals with more than five arrests ought to be supervised on probation. The supervised probationers with six or more previous arrests did not fail at a high rate. The data suggest that those with high numbers of previous arrests are inhibited from rearrest if they are supervised while on probation.

G. Summary

The foregoing analysis suggests the recidivism rates of the three experimental groups do not differ. While some differences were observed for officially recorded arrests for the community service probationers, when the frequency of previous arrests is controlled, the differences are much diminished.

Table 20. Failure Rates for Time Segments by Number of Previous Arrests

Number of Days from Start of Probation	Probation Category: Supervised			Unsupervised			Community Service		
	Number of Previous Arrests								
	0-1	2-5	6+	0-1	2-5	6+	0-1	2-5	6+
<90	.127 (7)	.094 (3)	.182 (2)	.073 (4)	.039 (1)	.046 (1)	.061 (3)	.161 (5)	.130 (3)
90-179	.063 (3)	.069 (2)	.000	.039 (2)	.120 (3)	.095 (2)	.065 (3)	.115 (3)	.300 (6)
180-269	.000 (2)	.222 (2)	.111 (1)	.041 (1)	.046 (3)	.158 (3)	.116 (1)	.087 (4)	.214 (2)
270-359	.044 (2)	.095 (2)	.125 (1)	.021 (1)	.143 (3)	.188 (3)	.026 (1)	.191 (4)	.182 (2)
360-449	.023 (1)	.053 (1)	.000	.022 (1)	.056 (1)	.231 (3)	.027 (1)	.177 (3)	.333 (3)
450-539	.024 (1)	.056 (1)	.000	.044 (2)	.000	.200 (2)	.111 (4)	.000	.333 (2)
540-629	.024 (1)	.118 (2)	.000	.140 (6)	.059 (1)	.125 (1)	.000	.071 (1)	.000
630-719	.050 (2)	.067 (1)	.000	.027 (1)	.188 (3)	.000	.031 (1)	.154 (2)	.000
720-809	.000	.083 (1)	.000	.000	.000	.000	.189 (5)	.091 (1)	.000
Overall failure rate	.309	.599	.364	.346	.500	.682	.487	.677	.826

Comparisons	Lee-Desu Statistic	df	Probability
0-1 Arrests	1.48	2	.476
2-5 Arrests	2.38	2	.305
6+ Arrests	4.82	2	.090

Note. Data used to determine failure are from Maryland state police records.

The difference in recidivism between the community service and the other groups is apparently a function, in part, of the selection of those with more serious records into this experimental group. The recidivism data suggest that those who have more than five previous arrests ought to be supervised while on probation. Such individuals who were included in the unsupervised and community service categories in this study had very high recidivism rates.

6. SUMMARY AND IMPLICATIONS

A. Research Design Integrity and Process Analysis

The random assignment procedures used in the research did not result in the creation of three fully satisfactory experimental conditions. There was some evidence of selection/participation bias with regard to sentence length and previous arrest history and by whether probationers had to pay a fine, court costs or restitution. Persons who had sentences of less than 12 months were more likely to decline participation in the study, especially if they were to be assigned to the community service group. Probationers who had to pay fines, costs or restitution were underrepresented in the unsupervised probation assignment group. Conversations with probation officials led us to hypothesize that because DPP was responsible for collecting payments, probation officers felt they had to supervise those who had to pay fines, costs or restitution needed to be supervised to assure that timely payments were made. Finally, the previous arrest histories of the three experimental groups were not statistically different from each other, but the histories themselves as well as later recidivism analyses suggest those with more serious criminal histories were more willing to participate in the study if they were to be assigned to the unsupervised probation or community service groups.

The design integrity of the community service assignment was also problematic because so many probationers assigned to that experimental group did not complete their 40-hour work assignments. Those who failed to complete their assignments were assigned to regular supervised probation, thus compromising the community service experimental condition. It is thus

not possible to make inferences about the effects of community service in comparison to supervised and unsupervised probation as had been hoped. These results suggest that community service programs should be well organized and their probationers supervised. The laissez-faire approach that characterized the community service program did not work in spite of the fact that community service probationers would have been released early from their probation sentences if they had completed their work assignment.

In most respects, the experimental design was successful. The experimental groups did not differ from each other on the basis of sex, race, age, marital status, education, full-time employment, or type of offense that led to probation. Moreover, the integrity of the supervised and unsupervised conditions was largely maintained. Supervised subjects were dealt with in routine fashion, and the unsupervised probationers were only contacted by probation officers in a few instances and had much lower contact rates than the supervised probationers. The supervised probationers averaged 12.3 contacts during their probationary periods; the unsupervised probationers had an average of 3.5 contacts.

B. Social Adjustment

A comparison of supervised and unsupervised probationers indicated few differences between the groups. They did not differ in employment, marital status, and a number of other factors after going on probation. The supervised probationers appeared more likely than the unsupervised probationers to make use of publicly available services such as those offered by the Welfare Department. Based on probation officers' assessments on factors such as family relationships, mental health, and substance abuse problems, the supervised and unsupervised probationers were viewed as mostly unchanged at the end of probation.

C. Recidivism

The three experimental groups did not differ from each other on several dimensions of recidivism: successful completion of probation, self-reports of arrest and arrest outcomes, and self-reports of involvement in 11 kinds of illegal activity. The recidivism analysis did show the community service probationers were more likely to fail on the basis of state police arrest histories. A significantly higher percentage of community service probationers had arrests entered on their rap sheets after the arrest that resulted in probation. Analysis of this finding showed that previous arrest history probably accounted for the difference. Within number-of-previous-arrests categories, the rap sheet recidivism of the three groups was very similar.

Using self-reports, survival rate analyses of the timing of recidivism showed that the time distributions of rearrest were not different for the three groups. The survival rate findings based on state police data were consistent with the tabular analyses discussed above. The community service probationers were rearrested sooner after going on probation than the other two groups. The survival analyses also showed that rearrest was more likely to occur earlier than later in the experience period for all three probationer groups.

Inspection of rearrest recidivism within experimental and number-of-previous arrests categories showed that unsupervised and community service probationers who had more than five previous arrests had very high failure rates. We interpreted this as an indication that such individuals ought to be supervised on probation. The numbers of individuals in the six or more previous arrests categories are not large, but the officially recorded rearrest rates of the unsupervised and community service probationers are 1.9 to 2.3 times higher than that of the supervised probationers.

D. Cost Analysis

DPP provided data that permitted an assessment of the input, output, and outcome costs of supervising probationers. Using salary and cost data for ten Baltimore offices, it was estimated that the annual cost of supervising a case was $237. Based on differences in the frequency of contact between supervised and unsupervised probationers, the costs to DPP of the supervised group were 3.5 times higher than for the unsupervised group. No evidence of increased recidivism was found among those who received less contact or whose cases involved less activity.

E. Implications

A general conclusion appears warranted on the basis of the research: within this group of probationers who received sentences of 12 months or less without special conditions, the levels and types of supervision and services received appear to be unrelated to social adjustment and recidivism in the period two to two and one half years after the start of probation. An exception may be that those who had more than five or more previous arrests are at increased risk of arrest if they are not formally supervised while on probation. Thus, if the findings are generalizable, there are clearly subgroups that could go unsupervised with positive results for them and the community, and savings for the Department of Parole and Probation. Identifying the group that can go unsupervised successfully can reduce caseloads and permit more intensive needs assessment, treatment, and monitoring of supervised probationers.

Appendix A

An Assessment of the Integrity of the Assignment Process

Appendix A

An Assessment of the Integrity of the Assignment Process

A. Introduction

The purpose of this project was to evaluate the cost-effectiveness of three alternatives for dealing with offenders who normally receive probationary sentences of 12 months or less. In many states, these probationers represent a significant proportion of the probation caseload. Yet, a growing number of professionals in the field are skeptical of the effectiveness of traditional supervised probation as a means of monitoring and rehabilitating such offenders. Many feel that the limited resources of the probation agencies could be better spent handling these cases in other ways.

The three alternatives examined in this project include supervised probation, unsupervised probation, and community service. Under Maryland's differentiated case management system, probationers are normally assigned to one of three levels of supervision--maximum, medium, or minimum. Obviously, the more intensive the level of supervision, the more expensive it is to supervise a given case. As an indication of the level of supervision and service provided to cases of the type involved in this study, 70.7 percent of the probationers assigned to the first experimental category, supervised probation, were designated to receive medium supervision, while 9.8 percent were to receive only minimal supervision (2.4 percent were placed in a "non-active" category). Thus, though their offenses were not very serious, nearly all of the probationers in this study would have normally been regularly contacted by a probation agent.

Some research has suggested that the level of supervision received by certain types of probationers does not have a significant effect on their future criminal behavior (e.g., Lichtman and Smock, 1981). Given the minimal threat to society posed by the type of offenders involved in this research, it was decided to evaluate the effects of not actively supervising a randomly selected group of them. Thus, the probationers in the second experimental group were not to be contacted by a probation agent, although they were given the name of an agent to call if they needed help.

The third experimental group was comprised of offenders who voluntarily agreed to perform 40 hours of community service work (cleaning up parks, painting park benches, etc.). Upon satisfactorily completing this assignment, it was agreed that the Maryland Division of Parole and Probation (DPP) would recommend to the sentencing judge that the remaining probationary period be dismissed. However, the District Court judges involved in the project, did not want noncompliant probationers referred to them. The judges did not stipulate community service in the probation order and, therefore, felt that punishing noncompliance would be inappropriate. In addition, no fines or restitution payments were to be suspended for these volunteers, although this practice is followed in some areas that operate community service programs.

Differences among probationers in the three groups were measured in terms of recidivism and changes in their social and economic circumstances. However, in order to be able to attribute any differences among the three groups to the type of probation experience, we had to demonstrate that the three groups were, in fact, comprised of individuals who were similar in virtually all other respects. Furthermore, if the evaluation is to be helpful in terms of choosing the most cost-effective approach for dealing

with such offenders, it must be shown that the participants in this project were representative of probationers in Maryland who usually receive probationary sentences of 12 months or less with no special conditions.

Our purpose in this appendix is to assess the degree to which the random assignment procedures produced comparable groups. After reviewing these procedures, the efforts to implement the project in three Maryland jurisdictions--Anne Arundel County, Prince George's County, and the City of Baltimore are described. The characteristics of participants in each of the three experimental groups are then compared to those of probationers who declined participation in the project. Finally, an assessment is made of the integrity of the assignment process and of the suitability of the samples for further analysis.

B. Overview of Assignment Process

It is important to understand the focus of this project in terms of the types of probationers who were targeted. Therefore, prior to describing the assignment process and its implementation, we shall discuss how the target group for the study was defined.

1. Definition of the Target Population

Data for fiscal year 1979 indicated that 42.9 percent of cases sentenced to probation by District Courts in Maryland received sentences of 12 months or less (DPP, 1979: III-23). This represented more than one-fourth (27.2 percent) of all the probation cases opened in that year. Assuming, first, that persons receiving such a sentence tend to be first offenders and, second, that their offenses are of a less-serious nature, a substantial proportion of DPP's workload must involve clients who pose little threat to society and need few rehabilitative services.

However, the District Court order requires the expenditure of DPP resources to provide such supervision. If these probationers can be dealt with as well or better in a way using fewer DPP resources, the overall efficiency and effectiveness of DPP operations could be enhanced.

Based on the foregoing assumptions concerning the type of offender who tends to receive a short probationary sentence from a District Court judge, it was decided that 12 months would be the criterion for selecting experimental subjects. Thus, the option of including in the project only persons convicted of particular offenses was rejected in favor of allowing the judge's normal sentencing practices to define the target population; i.e., offenders who receive probationary sentences of 12 months or less. This approach had the advantage of conforming to the judges' beliefs about for whom such a sentence is appropriate. As a result, persons with prior convictions, for example, were not automatically excluded. Therefore, the population of interest in this study was simply the group of offenders who normally received a probationary sentence of 12 months or less in District Court.

DPP felt it had a legal and ethical obligation to support the sentencing judge's assignment of probationers to special programs, so persons who were ordered to attend alcohol or drug treatment programs or who were required to seek mental health services were excluded from participating. In addition, those cases in which a judge stipulated maximum supervision or performance of community service also were diverted from the project. Finally, in order to avoid the potential problem of confounded "treatment" effects, any probationer whose jail sentence was not fully suspended was not offered the option of participating in the project. These strictures drastically narrowed the potential target population. The implications of

delineating the target population in this fashion were discussed in various sections of this report.

2. Assignment Procedures

The maximum 12-month probationary period and the six-month followup period required by the project imposed significant constraints on the selection of participants. Given the schedule of the project, it was necessary to restrict the length of the intake period. Thus, the approach originally proposed to the National Institute of Justice (NIJ) and the DPP was to offer the option to participate to all eligible probationers who went through intake over a three-month period in three jurisdictions in Maryland. The option offered to each probationer was based on a randomly ordered list of assignments, thus insuring randomly selected experimental groups of approximately equal sizes.

The lists of assignments to supervised probation, unsupervised probation, or community service were generated using the PLAN procedure in the Statistical Analysis System software package (SAS, 1979: 339-342). Each list was comprised of randomly ordered blocks containing six randomly ordered slots with two assignment slots for each of the three alternatives (see Exhibit A-1). The assignment procedure simply involved offering the next available slot on an assignment sheet to the next eligible probationer who entered the intake office. If a probationer declined participation, the slot he or she had been offered was offered to the next eligible probationer.

The project was explained to each eligible probationer during the intake interview. This involved telling each probationer how his or her probation experience would be different as a result of participating in the project. Those who had the option of participating in an unsupervised status were told that they would have to report changes of address, were

Exhibit A-1

PROBATIONER ASSIGNMENTS

ANNE ARUNDEL COUNTY ANNAPOLIS

ASSIGNMENT	NAME	CASE NUMBER
SUPERVISED PROBATION	.....	.....
UNSUPERVISED PROBATION	.....	.....
UNSUPERVISED PROBATION	.....	.....
COMMUNITY SERVICE	.....	.....
COMMUNITY SERVICE	.....	.....
SUPERVISED PROBATION	.....	.....
COMMUNITY SERVICE	.....	.....
UNSUPERVISED PROBATION	.....	.....
UNSUPERVISED PROBATION	.....	.....
SUPERVISED PROBATION	.....	.....
SUPERVISED PROBATION	.....	.....
COMMUNITY SERVICE	.....	.....
SUPERVISED PROBATION	.....	.....
COMMUNITY SERVICE	.....	.....
COMMUNITY SERVICE	.....	.....
UNSUPERVISED PROBATION	.....	.....
UNSUPERVISED PROBATION	.....	.....
SUPERVISED PROBATION	.....	.....
UNSUPERVISED PROBATION	.....	.....
UNSUPERVISED PROBATION	.....	.....
SUPERVISED PROBATION	.....	.....
COMMUNITY SERVICE	.....	.....

given a schedule for paying any fines they owed, and were given the name of a probation agent to contact if they needed help while on probation (see Exhibit A-2). Those who accepted the community service option first received a general description of the type of work they would be expected to perform. They also were told that DPP would recommend to the court that their probation be terminated upon satisfactory completion of their assignment and after they had paid any fines, court costs, or restitution they owed. In addition, consistent with the practice in most community service programs (Cooper et al., 1981:100), they had to sign a form which indicated that they voluntarily agreed to participate in the community service program (see Exhibit A-3). Finally, probationers whose experimental assignment was supervised probation were told that a study was being conducted and, although they would not receive any special treatment as a result of their participation, their consent to participate was needed. Thus, each probationer who agreed to participate in the project signed a general consent form (see Exhibit A-4).

Preliminary plans called for the explanation of the project and the actual experimental assignments to be made on a master assignment sheet by the intake unit supervisor in each site. It soon became obvious, however, that it would be necessary to delegate this responsibility to intake agents in each jurisdiction except Anne Arundel County. This was due to the volume of activity in the Baltimore City intake unit and the physical separation of multiple intake offices in Prince George's County. Because each agent then maintained an assignment sheet, there had to be some way to keep from offering the same slot to different probationers interviewed at the same time by different agents.

Exhibit A-2

Referral for Unsupervised Probationers

STATE OF MARYLAND

DIVISION OF PAROLE AND PROBATION

Dear _____:

As a participant in the Maryland probation evaluation project you will not be required to make monthly reports to a probation agent. However, you must notify the Division of Parole and Probation if you are arrested, if you move, or if you change jobs. If necessary, you should report any arrests or changes in address or employment to the following probation agent:

Name: _____

Telephone number: _____.

This person will also be available to help you if you have any employment, family, or health problems. For example, if you have a problem with drugs or alcohol, or if you are having trouble finding a job, this probation agent could help you get assistance from other agencies. After discussing your problem with you, he or she will be able to advise you as to where you might receive help. Therefore, although you are not required to do so, please feel free to call this probation agent if you need help.

Exhibit A-3

COMMUNITY SERVICE PROGRAM - TERMS AND CONDITIONS

1. Within 60 days, I agree to work 40 hours as a community service volunteer.

2. If I have any physical problem which may prevent me from participating in this program, I will bring this to the attention of the probation agent before start_ ing work.

3. I am expected to undertake those tasks which will be assigned by a supervisor of the agency to which I am being assigned. That supervisor will grade the quality of my work and my attitude, and note my attendance. If I do not show for work, or if I leave without being excused, I will receive no credit for that day. The work supervisor is authorized to cancel my participation int he program at any time if my work, attitude or attendance are unsatisfactory. When my service is completed (or when my work is terminated for unsatisfactory conduct), the super_ visor's report will be sent to the Division of Parole and Probation. If the report is satisfactory, the Division will recommend to the judge who sentenced me that I be released from probation, if I have paid all fines and restitution I may owe. Unless told otherwise, I would not be on supervised probation and would not have to report to a probation agent anymore. If my volunteer service is not satisfactory, I will be placed on regular supervised probation.

4. WAIVER AND RELEASE - In consideration of the permission granted to me to participate in this community service program in lieu of being on supervised probation, I hereby, for myself, my heirs, and administrators, release and discharge Baltimore City, Maryland and the State of Maryland, its employees and agents from all claims, demands, actions for injury sustained to my person and/or property during my participation in public work service when such injury is due to negligence or any other fault. I also agree to accept sole responsibility and liability for any injury or damage to a third party resulting from my act(s) or omission(s), and I agree to hold Baltimore City, Maryland and the State of Maryland, its employees and officials harmless from any lawsuit or claim arising therefrom, and I agree to indemnify Baltimore City, Maryland and the State of Maryland, its employees and officials in the full amount of any judgment obtained. I certify that my attendance and participation in this program is wholly voluntary and that I am not, in any way, an employee, servant or agent of Baltimore City, Maryland and the State of Maryland.

I HAVE READ AND UNDERSTAND THE FOREGOING TERMS, CONDITIONS, WAIVER AND RELEASE.

_____

_____

Date Community Service Volunteer

Witness:
_____
(Intake Probation Agent)

Exhibit A-4

Case No. _____

GENERAL INFORMED CONSENT

The purposes and procedures of the Maryland Probation Evaluation Project have been explained to me. I understand that the purpose of the study is to provide information on a large sample of probationers at the time they enter probation, while they are on probation, and six months after their probation has ended. The study will try to find out how people are affected by different types of probation. In reporting information from this study, information about me will always be grouped with information about other probationers so that I cannot be identified. For the purposes of reporting the results of the study, my name will not come to the attention of representatives of the Federal government or the state of Maryland.

This study is being conducted by the Maryland Division of Parole and Probation and the Research Triangle Institute (RTI) under grant number 81-IJ-CX-0005 from the National Institute of Justice. Under Public Law 96-157, "The Justice System Improvement Act of 1979," information which RTI collects about me cannot be subpoenaed or "be admitted as evidence or used for any purpose in any action, suit, or other judicial, legislative, or administrative proceedings."

As part of my participation in the study, I will be asked to respond to a telephone interview which will occur approximately six months after my probation has ended. This interview will require about fifteen minutes of my time and my providing information is strictly voluntary. This option applies to any individual question, as well as to the questionnaire as a whole. I also understand that probation case records and law enforcement records will be checked to obtain information that will not be obtained in the interview. The confidentiality of any information obtained by RTI, as well as any I provide in the telephone interview is guaranteed by the Federal law cited above.

My signature below indicates that the purposes and procedures involved in the Maryland Probation Evaluation Project have been fully explained to me.

Signed this date: _____ by: _____
(Signature of Probationer)

by: _____
(Signature of Intake Probation Officer)

The new procedures were probably more effective than the planned approach. First, the intake agent knew in advance which alternative was to be offered to an eligible probationer. Therefore, the precise conditions of the experimental alternative could be discussed with each candidate, thus avoiding any confusion about how they would be treated as a result of their participation. Second, this approach imposed greater accountability because each agent was responsible for maintaining an assignment sheet. Any eligible probationers who were not invited to participate could be detected by the Intake Supervisor and brought to the agent's attention. Furthermore, rates of refusal were monitored, and significant deviations from the norm were discussed with the relevant agent.

3. Implementation of the Assignment Process

Implementation was delayed by a variety of problems which arose early in the project. First, Anne Arundel County had to be substituted for Montgomery County because an extremely high proportion of District Court probation cases in the latter jurisdiction involved special conditions (particularly alcohol treatment programs), according to DPP officials there. Second, unlike Prince George's and Anne Arundel Counties, Baltimore City did not have a community service program to deal with District Court probationers. As a result, DPP had to establish a program with the assistance of the Mayor's Coordinating Council on Criminal Justice and the City Parks Department. This caused an even further delay in implementation in that jurisdiction.

The project was implemented in Prince George's County and Anne Arundel County at the same time, and final outcomes in the two counties were quite similar. Therefore, implementation in both these jurisdictions is discussed in the next section. Implementation of the project in Baltimore is discussed next.

a. Implementation in Anne Arundel and Prince George's Counties. Intake agents in these two counties began assigning probationers to experimental treatments during the last week in January, 1981. It soon became apparent, however, that very few offenders in these jurisdictions were eligible for the program, either by virtue of having a special condition attached to their orders to probation or by having received probationary sentences of more than 12 months. Therefore, intake probation agents were asked to identify the disqualifying factor in each case where probation was for a year or less.

Approximately one-third of the probationers who received terms of 12 months or less in these counties were required by the sentencing judge to attend alcohol or drug treatment programs (35.1 percent in Prince George's County and 33.4 percent in Anne Arundel County). Because, in these cases, the primary function of the probation agent was to monitor compliance with this stipulation of the probation order, DPP did not have the discretion of determining the appropriate level of supervision. Therefore, in deference to the judges' perception of the need for specific treatment, such cases were diverted from the project.

In spite of some preconceptions among DPP staff, a factor which did not pose a significant problem in either county was a specific requirement by the judge that a case receive "maximum" (i.e., intensive) supervision. None of the probationers in Prince George's County and only two (3.5 percent) in Anne Arundel County were disqualified for this reason.

Another interesting finding was that some persons who received community service sentences from District Court judges also were ordered to be supervised by DPP. Of the cases diverted from the project in Prince George's

County, 38.7 percent fell into this category; 57.9 percent of the disqualified cases in Anne Arundel County had to do community service as well as serve probation. In this context, community service was more a condition of probation than a distinct sentencing alternative. However, given that approximately 300 persons receive community service orders each month in Prince George's County, the 43 persons who received combined sentences in February and March represented a fairly small proportion of that county's total community service work force.

The most significant difference in sentencing patterns between Prince George's County and Anne Arundel County was in the proportion of probationers who received a jail sentence that was only partially suspended. Whereas only 5.3 percent of the offenders in Anne Arundel County had to serve some time in jail in addition to being on probation, more than one-fourth (26.1 percent) of the disqualified cases in Prince George's County involved the combination of a jail term (usually a weekend or a day) and a year or less on probation. One reason for this might have been available space in a fairly new jail facility.

The net effect of the sentencing patterns detected in these two counties was to make the project virtually inappropriate for these sites. Only 14 subjects ultimately were drawn from these jurisdictions before the experimental intake was terminated. However, our experience in attempting to implement the project in Anne Arundel County and Prince George's County did suggest that judges who have the option of placing less-serious offenders in community service prefer that sanction to short probationary sentences. Although a large proportion of Maryland's probation caseload is comprised of persons who received sentences of 12 months or less with no special conditions, it does not appear that those cases originate in jurisdictions which have community service programs.

b. Implementation in Baltimore. After a viable community service alternative was established in Baltimore, it was possible to begin making experimental assignments there late in February, 1981. It soon became apparent that a fairly large number of probationers in that jurisdiction would be eligible for the project. As indicated in Table A-1, the initiation of the project in Baltimore brought about a substantial increase in the number of probationers eligible for the project. For example, during March, the first full month in which assignments were made in Baltimore, 118 probationers were found to be eligible to participate in the project. During April, the number of eligible probationers declined to 109, and the rate of refusals increased from the March rate of 15.3 percent to 22.0 percent. A further decline in the number of eligible probationers during May, as well as a substantially higher refusal rate (40.0 percent), resulted in a total sample of slightly more than 250 probationers, far short of the intake goals originally set for the project. Therefore, the intake period was extended two months.

Table A-1

Number of Eligible Probationers and Rates of Refusal by Month

	February	March	April	May	June	July	Total
Number of Eligible Probationers	11	118	109	100	105	76	530
Rates of Refusal	9.1%	15.3%	22.0%	40.0%	35.2%	46.0%	30.2%

In spite of continuing high refusal rates (35.2 percent in June and 46.0 percent in July), an additional 120 probationers agreed to participate in the program during the final two months of intake. Thus, a total sample of 370 participants was established from a population of 530 eligible probationers, thereby yielding an overall refusal rate of 30.2 percent.

Several possible explanations can be offered for the substantial increase in the rate of refusals as well as the less significant change in the absolute numbers of eligible probationers. First, according to staff in the Baltimore City intake unit, the rate of intakes usually declines during the summer months, primarily due to the scheduling of judges' vacations. Second, although problems with the heating system of the building in which the intake unit is housed forced the closing of the unit a few times during the winter, repeated failure of the air conditioning system required its closing as early as 11:00 a.m. on a number of occasions during the summer. Work proceeded only on a voluntary basis at such times, and a backlog of work naturally developed. This situation probably contributed to a third problem--a possible decline in the aggressiveness of intake agents in encouraging eligible probationers to participate. During the early phases of the project, the novelty of the situation probably inspired agents to spend the necessary time to explain the project fully and to discuss it with each client. It is understandable that their willingness to take this time diminished, especially in light of the necessity of dealing with more clients in a shorter time span and under the less-than-pleasant conditions caused by the summer heat. The basic problem may be that it was much simpler to accept a client's refusal to participate than it was to encourage him or her to participate.

Little could be done by the project team to address the first two problems. Seasonality of intake rates and problems with the physical plant were beyond our purview. However, in seeking to ameliorate the third problem, frequent visits were made to the intake units (in Baltimore as well as Prince George's County), and frequent contacts were made by telephone. Even by the second month of intake, however, the rate of refusals had begun to climb.

One fact which is not reflected in the overall rate of refusals is the significantly higher rate of refusals among those probationers who were offered the community service option. In contrast to the 23.1 percent refusal rate among those assigned to supervised probation and the 2.4 percent refusal rate among those who would not have been supervised, nearly one-half (48.2 percent) of those probationers who had the option of doing 40 hours of community service work refused it. These different rates of refusals clearly reflect the probationers' perception of the benefit the project had for them. First, unsupervised probation appeared to be the most attractive alternative, based on the low rate of refusals among those who were offered this alternative. Second, the lack of a payoff for those receiving regular supervised probation probably caused a number of them to decline participation. Finally, the tradeoff between 40 hours of work in 60 days and 12 months or less on probation was apparently not sufficiently appealing for nearly half of those who received the offer. However, the reasons for the differential rates of refusals and their potential impact on the validity of the study will be explored in greater detail in other sections of this report.

On the basis of records kept by probation agents in the Baltimore intake unit, it appears that no more than 30 percent of the probation

orders issued by District Court judges and involving sentences of 12 months or less contain special conditions. Of those cases which had special conditions attached to them, more than half (57.3 percent) involved required attendance at alcohol treatment programs. Eight percent contained a stipulation that the probationer receive maximum supervision, while 12 percent required the probationer receive mental health services. In four percent of these cases, the probationer was ordered to attend a drug treatment program. Only 2.7 percent of these probationers did not have their jail sentences fully suspended. Finally, in spite of the fact that a community service program was not fully operational, some judges independently ordered 16 percent to perform work such as picking up trash around a police station. Thus, a much smaller proportion of cases in Baltimore involved special conditions than did cases in the other project sites. As a result, slightly more than 96 percent of the probationers involved in the study are from Baltimore.

C. Characteristics of Experimental Subjects

Using the data collected about each probationer during the intake interview and recorded in the "Case Record Input-Intake Form" (DPP form 53) experimental subjects can be profiled. The typical participant in this project was a black man less than 25 years old who was single and had not completed high school. He probably received probation for theft or possession of marijuana, and he had as much a chance of being unemployed as he did of being employed full time. He reported no previous arrests, and although he had to pay court costs, he did not have to pay a fine or restitution. This typical probationer was represented in court by a public defender and pleaded guilty. His jail sentence of six months or less was suspended in favor of his serving probation for 12 months.

This profile is based on summary statistics which describe various social and economic characteristics of probationers who agreed to participate in this study. For example, 78.1 percent of them are males, and 79 percent are black. Their mean age is 28.3 years, although 53 percent are 25 years old or younger. Only 19.3 percent are 35 or older. Nearly two thirds of them (62.5 percent) are single, 18.2 percent are married, and 16.9 percent are divorced or separated. More than half of the subjects (60.9 percent) had completed fewer than 12 years of education, a fact reflected in the mean years of education for the group (10.9). Twenty-nine percent had completed high school, and an additional 10.1 percent pursued education or training beyond high school. While 38.8 percent of the subjects were employed full time, a slightly larger group (39.4 percent) was unemployed. A smaller group (21.8 percent) was employed part time.

More than one third of the sample (34.1 percent) had committed a property crime, usually larceny. The second most common offense was possession of marijuana (18.6 percent of the probationers), whereas the third most common offense was assault (13.2 percent). About as many subjects had been placed on probation for traffic offenses (7.8 percent) as had been convicted of weapons offenses (8.1 percent).

In 82.4 percent of the cases, the probationary sentence was 12 months, while it was six months or less in 14.1 percent of the cases. The use of probation as a sanction for these offenders seems consistent with the fact that most (86 percent) reported no previous arrests, although a check of DPP records at intake revealed that 25 percent had been on probation before. Nevertheless, it appears that the assumption that the project would involve less serious offenders was supported.

Table A-2 presents a comparison of the characteristics of those persons contacted at intake for participation in the project with all 1981 Baltimore City District Court intakes. The two data sets differ slightly by period of data collection and by an eligibility criterion. The project selected probationers during March through July 1981, while the probationer intake data for Baltimore City covered January through early Fall, 1981. These time periods are generally comparable, and we assume this slight difference is insignificant in the comparison of characteristics. The eligibility criteria for the project were that the person be assigned to probation for a period of 12 months or less and that no special conditions be specified. However, the 1981 Baltimore City intake data do not exclude those probationers with special conditions. This difference may explain some of the profile differences for the characteristics sex, race, and age in Table A-2.

The profile comparisons for the characteristics of marital status, education, employment, and whether fines, costs, or restitution were levied show a good match between the Baltimore City 1981 intakes and the project intakes. Slight differences appear in terms of the characteristics of sex, race, and age. For example, the project sample shows a higher proportion of females than occur for the Baltimore City 1981 data. Also, the project sample shows a higher proportion of non-whites than the Baltimore City 1981 data reflect. The sex difference is approximately five percent while the race difference is 10 percent. While these differences cannot be fully explained, it is possible that the court's specification of special conditions may account for part or all of the difference. In this case, more males and whites would have to be receiving special conditions to account for the observed differences.

Table A-2

Characteristics of Eligibles Contacted for the Maryland Probation Evaluation Project and All 1981 Baltimore City District Court Intakes

Characteristics		Eligibles Contacted[a] (n=530)	1981 Intakes[b] (n=1946)
Sex:	Male	79.1%	83.4%
	Female	20.9	16.6
Race:	White	21.8%	31.0%
	Non-White	78.2	69.0
Age:	18-21	27.7%	25.2%
	22-25	22.8	22.3
	26-34	30.2	29.8
	35-49	13.1	16.9
	≥50	6.2	5.8
Marital Status	Married	17.8%	18.1%
	Single	62.2	59.6
	Separated	11.8	12.5
	Divorced	6.3	7.7
	Other	1.9	2.1
Education:	Less than high school	13.0%	13.9%
	9th - 11th grade	48.4	45.9
	12th grade	29.1	30.8
	More than high school	9.5	9.4
Employed Fulltime:	No	58.5%	60.8%
	Yes	41.5	39.2
Fines, Costs or Restitution	No	40.0%	42.4%
	Yes	60.0	57.6

[a]Includes project participants and all persons who declined to participate in the project; that is, all eligibles contacted by the project (n = 530). Most eligibles were from Baltimore City (98%). In some categories there are missing data--for race, 3 cases have missing data; for age, 3 cases have missing data; for marital status, 1 case has missing data; for education, 4 cases have missing data.

[b]These are all probation intakes who received sentences of 12 months or less from January through early Fall, 1981 in Baltimore City District Court. Persons who were assigned special conditions were also included in this group, although probationers with special conditions were not eligible for inclusion in the evaluation project. Data for the evaluation were collected from March through July.

A comparison of the characteristics of this sample with the characteristics of the entire 1979 DPP caseload indicates that the sample is representative of DPP Region II (Baltimore City), although it probably is not representative of the remainder of the state (DPP Annual Report 1979: 17-22). For example, while a majority of the DPP clients in other regions were white, 74.4 percent of those in Region II were not white. Furthermore, a larger proportion of the Region II caseload had not completed high school (65 percent) compared to probationers in other areas (53.6 percent). However, the sample of probationers in this study is similar to the statewide and Region II caseloads in terms of sex and age. Nevertheless, it does not seem appropriate to make inferences to or draw conclusions about any population other than persons in Baltimore City who receive probationary sentences of 12 months or less without special conditions.

D. Assessment of the Integrity of the Assignment Process

Randomization serves two purposes in experimental research. First, random sampling procedures increase the probability that experimental groups (samples) will be representative of the population to which inferences are to be made. Second, random assignment of subjects to experimental and control groups further insures that these samples are equivalent; in other words, that they are groups of people who are similar in all respects other than the treatment their members receive as experimental subjects.

The intake and assignment procedures which were employed in the Maryland probation project appear to have met these requirements in virtually all respects. The population from which assignments were made had been specified precisely, and the identification of potentially eligible

subjects by intake agents was monitored by their supervisors. Furthermore, agents were asked to note the special conditions which required them to exclude from the project any probationer who received a sentence of 12 months or less. Therefore, the population from which assignments have been made seems to conform to the universe of probationers who received such sentences without special conditions.

A potential problem in experimental research involving human subjects is selection bias which results from informed-consent requirements. All potential subjects had to be informed of the purpose of the experiment and the nature of the treatment they were to receive as experimental subjects. Furthermore, intake agents had to obtain the written consent of potential subjects before they could be brought into the experiment. This posed two self-selection problems. First, a high rate of refusals might have threatened to undermine the random selection process by producing experimental groups that were not representative of the population of interest. Second, differential rates of refusal among different experimental groups might have produced non-equivalent experimental groups. In the case of the Maryland project, the required explanation of experimental conditions included a description of the treatment a probationer could expect to receive by participating in a randomly chosen treatment program. A high rate of refusals for a given assignment, such as was experienced in placing probationers in community service, might have produced a selection bias in the sampling process.

An assessment of the integrity of the assignment process that was employed in the Maryland project can be made using data from the intake forms of the 371 probationers who agreed to participate in the project and 160 persons who declined participation. These data permit us to draw some

conclusions concerning the independence of the experimental samples that were produced by the assignment process. If a significant relationship does not exist between the probationers' assignment and the probationers' characteristics, we can be assured of the statistical homogeneity of the samples; in other words, that the samples are comprised of like individuals. We must also be assured that the probationers who have agreed to participate do not differ significantly from those who did not choose to participate.

The data in Table A-3 indicate that participants and nonparticipants are not significantly different in terms of their sex, race, age, marital status, educational achievement, or employment status. This conclusion is based on the chi-square (χ^2) statistics reported in the table. This test of statistical significance measures the degree to which the pattern of data deviates from the pattern expected when there is no relationship between a given characteristic and the probationers' willingness to participate, or their experimental assignment.

Some differences between the groups of participants and nonparticipants, as well as among experimental groups, can be noted, however. For example, it appears that a larger proportion of the probationers who declined participation are employed. Also, it seems that persons in the community service program tend to be older and are more likely to be male than those in the other alternatives. However, in none of these cases is the difference among experimental groups or between participants and nonparticipants statistically significant. This seems to confirm the integrity of the assignment process.

Table A-3

Personal Characteristics of Eligibles Contacted

Characteristics	Probation Category			All Participants (n=370)	Persons Declining Participation (n=160)
	Supervised Probation (n=122)	Unsupervised Probation (n=121)	Community Service (n=127)		
Sex: Male	75.4%	76.0%	82.7%	78.1	81.3%
Female	24.6	24.0	17.3	21.9	18.8
	χ^2 = 2.38, n.s.[a]			χ^2 = .67, n.s	
Race: White	21.3	18.2%	20.2%	19.9%	26.3%
Non-white	78.7	81.8	79.8	80.1	73.8
	χ^2 = .38, n.s.			χ^2 = 2.64, n.s	
Age: 18-21	32.2	32.5	21.3	28.5	25.8
22-25	24.8	15.8	32.3	24.5	18.9
26-34	23.1	30.0	29.9	27.7	35.9
35-49	14.9	15.0	10.2	13.3	12.6
≥50	5.0%	6.7%	6.3%	6.0%	6.9%
	χ^2 = 13.50, n.s.			χ^2 = 4.46, n.s	
Marital Status:					
Married	19.0%	18.2%	17.5%	18.2%	16.9%
Single	60.3	60.3	66.7	62.5	61.3
Separated	9.9	13.2	8.7	10.6	14.4
Divorced	7.4	5.0	6.4	6.3	6.3
Other	3.3	3.3	.8	2.5	.6
	χ^2 = 4.60, n.s.			χ^2 = 5.75, n.s	
Education:					
Less than high school	15.1%	9.1%	12.7%	12.3%	14.4%
9th-11th grade	49.6	46.3	50.8	48.6	46.9
12th grade	25.2	35.5	26.2	29.0	29.4
More than high school	10.1	9.1	10.3	10.1	8.8
	χ^2 = 4.90, n.s.			χ^2 = 2.95, n.s.	
Employed Fulltime:					
No	53.3%	65.3%	64.6%	61.0%	52.5%
Yes	46.7	34.7	35.4	39.0	47.5
	χ^2 = 4.68, n.s.			χ^2 = 3.39, n.s.	

[a]Differences among the groups are not statistically significant ("n.s.") at the .05 level of significance.

Table A-4 compares the experimental groups and the participants and nonparticipants for type of current offense and number of previous arrests. According to data reported there, nearly half (46.2 percent) of the probationers who agreed to participate in this project had been convicted of either larceny or the possession of marijuana. However, the other half of the participants had been convicted of a variety of offenses, ranging from assault to invasion of privacy. Although such a wide range of offenses precludes a test of the statistical significance of differences among the groups, there does appear to be a difference between the community service group and the other experimental groups in terms of the proportion of larceny convictions. Regrouping the offenses and testing for differences (see middle of table A-4) does not reveal significant differences among experimental groups or between participants and nonparticipants.

Table A-4 also compares the experimental groups on the basis of number of previous arrests shown on their official criminal histories (rap sheets). The groups do not differ significantly from each other, although in the six or more previous arrests row, it can be seen that the unsupervised probationers and community service subjects appear to have more serious criminal histories than the subjects assigned to supervised probation.

The data in Table A-5 suggest that persons with probationary sentences of less than 12 months were more likely to decline participation. Furthermore, those with shorter sentences were less likely to accept the community service alternative or, to a lesser degree, be willing to serve as an experimental subject while on supervised probation. This may explain why the community service category includes more persons convicted of larceny, and the unsupervised category includes more convicted of possession of marijuana. This is because only two-thirds (67.5 percent) of the persons

Table A-4

Current Offense and Previous Arrests of Eligibles Contacted

	Probation Category				Persons Declining Participation
	Supervised Probation (n=123)	Unsupervised Probation (n=121)	Community Service (n=127)	All Participants (n=371)	(n=160)
Offense Category					
Larceny	28.7%	21.5%	32.3%	27.6%	18.1%
Drugs (marijuana)	18.9	20.6	16.5	18.6	23.1
Assault	13.9	13.2	12.6	13.2	11.9
Traffic	9.0	10.2	4.7	8.1	10.0
Weapons	8.2	7.4	8.7	8.1	6.3
Drugs (not marijuana)	1.6	4.2	5.5	3.8	5.6
Public Peace	3.3	7.4	3.9	4.9	3.1
Fraud	4.1	5.8	.8	3.5	7.5
Other	12.3	9.2	15.0	12.2	14.4
Personal	13.9%	13.2%	13.4%	13.5%	11.9%
Property	36.9	28.1	37.0	34.1	26.9
Drugs	20.5	24.8	22.0	22.4	28.1
Miscellaneous	28.7	33.9	27.6	30.0	33.1
	χ^2 = 3.34, n.s.			χ^2 = 3.89, n.s.	
Number of Previous Arrests[a]					
None	16.0	20.6	11.0	15.8	NA[a]
1-2	56.6	47.7	49.5	51.2	NA[a]
3-5	16.0	11.2	18.4	15.2	NA[a]
6 or more	11.3	20.6	21.1	17.7	NA[a]
	χ^2 = 9.56, n.s.				χ^2 NA

Note. The current offense is the offense for which the person was then on probation and, hence, an invited participant in this research.

[a]Previous arrest information is not available for those who declined participation in the research.

Table A-5

Length of Probationary Sentences for Eligibles Contacted

Length of Probationary Sentence	Probation Category			All Participants	Persons Declining Participation
	Supervised Probation (n=122)	Unsupervised Probation (n=121)	Community Service (n=127)	(n=370)	(n=160)
Less than 12 months	18.9%	25.6%	8.7%	17.6%	27.5%
12 months	81.2	74.4	91.3	82.4	72.5
	$\chi^2 = 12.51$ ($\alpha < .05$)			$\chi^2 = 6.75$ ($\alpha < .05$)	

convicted of drug-related offenses received a full 12 months' probationary sentence, whereas 86.8 percent of the probationers convicted of other offenses (including larceny) received a sentence of that length. In spite of the relationship between the length of the sentence and the type of offense, it seems that the reduced stakes involved for persons with shorter sentences was a more plausible determinant of their tendency not to participate than was their status as a larcenist or a possessor of drugs.

The only other possible source of sampling bias is examined in Table A-6. It is clear that a significantly higher proportion of the persons who declined participation had to pay a fine, court costs, or restitution. Furthermore, it can be seen that a relatively lower proportion of persons assigned to unsupervised probation had to pay a fine, court costs, or restitution. This same tendency is revealed in the more detailed analysis of fines and court costs, although the differences indicated in those analyses were not statistically significant.[1] An implication that could be drawn from this is that intake agents discouraged unsupervised probation for persons who were required to make these payments. In fact, the intake agent's judgment of a probationer's ability to pay was a critical factor, especially for probationers who had to pay large fines or who incurred substantial court costs. Thus, DPP's responsibility for overseeing the payment of fines and court costs overrode the rules of the assignment process in a few cases.

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[1]No analysis was conducted regarding restitution since only 11.4 percent of the cases involved restitution.

Table A-6

Fines, Court Costs, or Restitution Requirements of Eligibles Contacted

Court Ordered Condition	Probation Cagetory			All Participants	Persons Declining Participation
	Supervised Probation (n=122)	Unsupervised Probation (n=121)	Community Service (n=127)	(n=370)	(n=160)
Fines, costs, or restitution:					
No	40.2%	53.7%	35.4%	43.0%	33.1%
Yes	59.8%	46.3%	64.6%	57.0%	66.9%
	$\chi^2 = 9.04$ ($\alpha < .05$)			$\chi^2 = 4.51$ ($\alpha < .05$)	
Fine amount:					
$0	62.3%	73.6%	55.1%	63.5%	NA[a]
$1 - 50	12.3	10.7	20.5	14.6	NA[a]
$51 - 100	13.1	9.9	10.2	11.1	NA[a]
$101 - 249	5.7	4.1	6.3	5.4	NA[a]
$250 - 500	6.6	1.7	7.9	5.4	NA[a]
	$\chi^2 = 14.8$, n.s.				
Amount of court costs:					
$0	43.4%	58.7%	42.5%	48.1%	NA[a]
$1 - 15	44.3	36.4	46.5	42.4	NA[a]
$16 - 30	9.0	3.3	7.9	6.8	NA[a]
$35 - 120	3.3	1.7	3.2	2.7	NA[a]
	$\chi^2 = 9.84$, n.s.				

[a]Amounts of fines and court costs are not available for those who declined participation in the research.

E. Conclusions

Several conclusions can be drawn from this review of the implementation of the project and assessment of the integrity of the intake and assignment process. First, many of the probationers involved in this project are good candidates for community service programs. This conclusion is based on the fact that almost no offenders receive probationary sentences of 12 months or less in Prince George's and Anne Arundel Counties without special conditions, such as required treatment for alcohol abuse. In all likelihood, had the probationers who agreed to participate in this project resided in and been tried and sentenced in one of these counties, the majority would have been offered a community service option.

A second conclusion is that the probationers involved in this study are not representative of the Maryland's entire DPP caseload, at least in terms of their personal characteristics. They do appear, however, to be representative of District Court probationers who receive sentences of 12 months or less in Baltimore City. Since almost all of the subjects reside in Baltimore, the findings of this study may be more relevant to criminal justice professionals in other large Eastern cities than they are to those in rural Maryland.

A third conclusion is that the experimental groups which were generated by the assignment process do not differ significantly with regard to a number of key characteristics, including sex, race, age, marital status, educational achievement, employment status, offense type, or previous arrest history. In addition, there were no significant differences with regard to these characteristics between probationers who agreed to participate in the project and those who refused to participate. However, there

was evidence of sampling bias in two areas. One area involved self-selection. Persons with probationary sentences of less than 12 months were more likely to decline participation (particularly if offered the community service option), than were probationers who faced a full 12-month sentence. The other source of sampling bias seemed to arise from the reluctance of intake staff to assign probationers to an unsupervised status if they seemed unable to pay mandated fines, court costs, or restitution.

In spite of the slight biases which our assessment detected in the assignment process, and in spite of the failure to generate samples in two counties which would be suitable for independent analysis, a general conclusion is that implementation of the project was successful. There are two bases for this judgment. First, in spite of not being able to implement the project in Anne Arundel and Prince George's Counties, important information about the community service program and sentencing practices was obtained. This includes the fact that the program has proven to be very popular among District Court judges, and that it is used in addition to probation and not simply as a substitute sanction. Second, the availability of somewhat limited but important data about the probationers who declined participation enabled us to identify the sampling biases which arose in the assignment process. Although relatively minor, the fact that they are identifiable permits compensation for them. This information is not usually available in experimental research and, thus, represents a distinct advantage.

Appendix B

Interview Schedule

PROJECT 2122 I.D. # _____

FOLLOWUP INTERVIEW SCHEDULE

Hello, I'm calling from the Research Triangle Institute in North Carolina. Recently we sent you a letter to remind you that we would be getting back to you for some additional information. If you recall, when you went on probation in _____ of 1981 you agreed to participate in a
(MONTH)
research project being done by us in cooperation with the Maryland Division of Parole and Probation. At that time you were told that we would call you for a brief interview.

We are interested in finding out what has happened to you since you went on probation. Knowing this may help people who have to serve probation in the future.

I want to remind you again that your participation in this study is strictly voluntary and that Federal law guarantees that any information you give us will remain confidential (PUBLIC LAW 96-157, THE JUSTICE IMPROVEMENT ACT OF 1979). We want to assure you that any information you give us that would identify you will not be seen by the Maryland Division of Parole and Probation, the courts, or any law enforcement agency.

Unless you have any questions, let's begin.

1. Are you married and living with your (wife/husband)?	1	Yes . . . . . 01 (2) No 02 (3)
2. How long have you been married?	2	Years married . . ____ (6)
3. Are you ... [READ RESPONSE LIST] ... ?	3	Separated, ... 01 (4) Divorced, ... 02 (4) Widowed, ... 03 (4) Living as married (that is, living with someone but not married), or. . 04 (5) (are you) Single and never been married . . . . . 05 (6) OTHER [SPECIFY AT LEFT] 06 (5)
4. How long have you been (separated/divorced/widowed)?	4	Years in this marital status: ____ (6)
5. How long have you had this arrangement?	5	Years in this arrangement . . ____
6. How many persons are dependent on you for financial support--like a spouse, children, parents, or other family members?	6	# dependents . . ____
7. Are you employed full-time, part-time, or are you unemployed?	7	Employed fulltime . . 01 (8) Employed parttime . . 02 (8) Unemployed 03 (9)
8. How long have you had this job?	8	Days on this job ___1 (19) Weeks held job ___2 (19) Months held job ___3 (19) Years held job ___4 (19)
9. Are you on welfare?	9	Yes . . . . . 01 (10) No 02 (11)
10. How long have you been on welfare?	10	Days on welfare ___1 Weeks on welfare ___2 Months on welfare ___3 Years on welfare ___4

11. Are you disabled?	11	Yes . . . . . 01 (12) No 02 (13)
12. How long have you been disabled?	12	Days disabled ,,,1, Weeks disabled ,,,2, Months disabled ,,,3, Years disabled ,,,4,
13. Are you a student?	13	Yes . . . . . 01 (14) No 02 (15)
14. How long have you been a student?	14	Weeks as a student . . . . . ,,,2, Months as a student . . . . . ,,,3, Years as a student . . . . . ,,,4,
15. Are you retired?	15	Yes . . . . . 01 (16) No 02 (17)
16. How long have you been retired?	16	Weeks retired ,,,2, Months retired ,,,3, Years retired ,,,4,
17. Are you keeping house?	17	Yes . . . . . 01 (18) No 02 (19)
18. How long have you been keeping house?	18	Weeks as a housekeeper . . . ,,,2, Months as a housekeeper . . . ,,,3, Years as a housekeeper . . . ,,,4,
19. How many different jobs have you had in the last 12 months?	19	# different jobs last 12 months: ,,,
20. How many weeks or months have you been out of work that is, not had a job in the last year?	20	Weeks out of work . . . . . ,,,2, Months out of work . . . . . ,,,3, No time out of work 000

Now we need to know something about your contacts with public agencies. I have a list of agencies here and I have to read the entire list whether you have been in touch with any of them or not.

21. While you were on probation, did you contact any of the following agencies for help? [READ DOWN LIST AND CIRCLE ONE CODE FOR EACH AGENCY]

Card 2

	21. CONTACT AGENCY?		22. Was the service you received from (AGENCY NAME) satisfactory? SATISFACTORY SERVICE?	
	YES	NO	YES	NO
a. Welfare Department	01	02	01	02
b. Employment Service	01	02	01	02
c. Housing Authority	01	02	01	02
d. Alcohol Treatment Program	01	02	01	02
e. Drug Treatment Program	01	02	01	02
f. Legal Aid	01	02	01	02
g. Mental Health Center	01	02	01	02
h. Public Health Clinic	01	02	01	02
i. Any other Public Agency (SPECIFY_____)	01	02	01	02
	IF ANY "YES," COMPLETE Q. 22 FOR THAT AGENCY	IF ALL "NO," GO TO Q. 23		

23. Did a probation officer suggest that you contact any of these agencies? Yes . . . . 01 (24) No. 02 (25)	24. Which ones? [READ LIST. CIRCLE CODE FOR AGENCIES MENTIONED] SUGGESTED BY P.O.		25. Did a probation officer help make arrangements for you to get help from any of these agencies? Yes . . . . 01 (26) No 02 (27)	26. Which ones? [READ LIST. CIRCLE CODE FOR AGENCIES MENTIONED] HELP FROM P.O.	
	YES	NO		Yes	No
a. Welfare Department	01	02		01	02
b. Employment Service	01	02		01	02
c. Housing Authority	01	02		01	02
d. Alcohol Treatment Program	01	02		01	02
e. Drug Treatment Program	01	02		01	02
f. Legal Aid	01	02		01	02
g. Mental Health Center	01	02		01	02
h. Public Health Clinic	01	02		01	02
i. Any other Public Agency	01	02		01	02

27. While you were on probation, how often did a probation officer come to your home? [PROBE: Would you say daily, weekly, monthly, yearly, or what ?] [PROBE: How many times a (week/month/year)?]	27	Daily . . . . . 01 Weekly: 4 to 6 times a week . . 02; 2 or 3 times a week . . 03; 1 time a week . . . . . 04 Monthly: 2 or 3 times a month . 05; 1 time a month . . . . 06 Yearly: 4 to 11 times a year. . 07; 2 or 3 times a year . . 08; 1 time a year or less . 09 Never 10
28. How often did a probation officer come to the place where you were working? [PROBE: How many times a (week/month/year)?]	28	Daily . . . . . 01 Weekly: 4 to 6 times a week . . 02; 2 or 3 times a week . . 03; 1 time a week . . . . . 04 Monthly: 2 or 3 times a month. . 05; 1 time a month . . . . 06 Yearly: 4 to 11 times a year. . 07; 2 or 3 times a year . . 08; 1 time a year or less . 09 Never 10
29. How often did you have to go to the probation office? [PROBE: How many times a (week/month/year)?]	29	Daily . . . . . 01 Weekly: 4 to 6 times a week . . 02; 2 or 3 times a week . . 03; 1 time a week . . . . . 04 Monthly: 2 or 3 times a month. . 05; 1 time a month . . . . 06 Yearly: 4 to 11 times a year. . 07; 2 or 3 times a year . . 08; 1 time a year or less . 09 Never 10

30. How often did a probation officer call you on the telephone? [PROBE: How many times a (week/month/year)?]	30	Daily . . . . . 01 Weekly: 4 to 6 times a week . . 02; 2 or 3 times a week . . 03; 1 time a week . . . . . 04 Monthly: 2 or 3 times a month. . 05; 1 time a month . . . . 06 Yearly: 4 to 11 times a year. . 07; 2 or 3 times a year . . 08; 1 time a year or less . 09 Never 10
31. Other than making payments on fines and the like, how often did you have to mail in a postcard, letter, or some type of form to a probation officer? [PROBE: How many times a (week/month/year)?]	31	Daily . . . . . 01 Weekly: 4 to 6 times a week . . 02; 2 or 3 times a week . . 03; 1 time a week . . . . . 04 Monthly: 2 or 3 times a month. . 05; 1 time a month . . . . 06 Yearly: 4 to 11 times a year. . 07; 2 or 3 times a year . . 08; 1 time a year or less . 09 Never 10
32. How often did you contact a probation officer to try to get help with a problem? [PROBE: How many times a (week/month/year)?]	32	Daily . . . . . 01 Weekly: 4 to 6 times a week . . 02; 2 or 3 times a week . . 03; 1 time a week . . . . . 04 Monthly: 2 or 3 times a month. . 05; 1 time a month . . . . 06 Yearly: 4 to 11 times a year. . 07; 2 or 3 times a year . . 08; 1 time a year or less . 09 Never 10

Card 3

33. Have you had any contacts with the police from the time you went on probation in _____ (MONTH) of 1981 until now?	33	Yes . . . . . 01 (34) No 02 (59)
34. How many?	34	# police contacts ___

FOR EACH CONTACT MENTIONED, ASK POLICE CONTACT SERIES (Q. 35 - Q. 58). THE FIRST CONTACT TO BE REPORTED IS THE MOST RECENT. THEN THE NEXT MOST RECENT CONTACT AND SO ON SHOULD BE RECORDED, WORKING BACKWARDS IN TIME TO THE TIME THE RESPONDENT WENT ON PROBATION.

POLICE CONTACT SERIES

35. What was the date of your most recent contact with the police?	35	Month: _____ ☐☐ 198 ☐
36. Where did this police contact take place--what city or town?	36	City/Town: _____ State: _____ ☐☐
37. Did the police take you to the police station at anytime?	37	Yes . . . . . 01 (38) No 02 (57)
38. What happened then--were you only questioned and then released?	38	Yes . . . . . 01 No 02
39. Were you fingerprinted?	39	Yes . . . . . 01 No 02
40. Were you photographed?	40	Yes . . . . . 01 No 02
41. Were you charged with a crime?	41	Yes . . . . . 01 (42) No 02 (43)
42. What were you charged with? [DESCRIBE THE OFFENSE(S) BRIEFLY BUT COMPLETELY.]	42	Offense: _____ ☐☐☐☐ _____
43. Were you locked up?	43	Yes . . . . . 01 (44) No 02 (45)
44. How long were you locked up? [SPECIFY WHETHER HOURS, DAYS, WEEKS, OR MONTHS.]	44	Hours locked up ___1 Days locked up ___2 Weeks locked up ___3 Months locked up ___4
45. Did you have to post bail?	45	Yes . . . . . 01 (46) No 02 (47)
46. How much was the bail that the court set?	46	Bail set by court . . $ ___,___

47. Has this case gone to court or will it go to court?	47	Yes, did or will . . 01 (48) No, did/will NOT go to court . . . 02 [Box A]	
48. Has the court finished with your case or is it still deciding?	48	Case has not gone to court yet . . . 01 [Box A] Court still deciding (case pending) . . 02 [Box A] Court finished case (case disposed of). 03 (49)	
49. Did the court find you guilty?	49	Yes . . . . . 01 No 02	
50. (Did the court) make you pay a fine	50	Yes . . . . . 01 No 02	
51. (Did the court) make you pay court costs	51	Yes . . . . . 01 No 02	
52. (Did the court) make you pay restitution?	52	Yes . . . . . 01 No 02	
53. (Did the court) make you go to jail or prison?	53	Yes . . . . . 01 No 02	
54. (Did the court) give you a suspended sentence?	54	Yes . . . . . 01 No 02	
55. (Did the court) put you on probation?	55	Yes . . . . . 01 No 02	
56. Did the judge make you do anything else?	56	Yes [SPECIFY BELOW] . 01 [Box A] _____ No . . . . . 02 [Box A]	

57. What happened? What was the contact with the police about? What did the police do--tell me about it. [GET A DESCRIPTION OF THE CONTACT.]

_____

_____ ☐☐

_____

58. Did you get a ticket? Yes . . . . . 01 No 02

[PROBE FOR TYPE OF TICKET: RECORD ABOVE.]

X REPEAT POLICE CONTACT SERIES (Q. 35 - Q. 58) FOR NEXT MOST RECENT POLICE CONTACT
-OR-
GO TO Q. 59.

Card 4

59. From the time you went on probation in (MONTH): _____ of 1981 until now, how many days did you spend in detention, jail or prison?	59	Days in prison ⌊_⌋⌊_⌋⌊_⌋

Whether or not you had any contacts with the police since you went on probation, I want to ask you about some activities that you may have been involved in during the period from (MONTH): _____ of 1981 until now. We don't want to know details about these activities, just the number of different times that you might have done them.

Remember, the information you give us will be handled in the strictest confidence. No information you give us will be seen by the Maryland Division of Parole and Probation, the courts, or any law enforcement agency.

I have a list of eleven illegal activities here. I have to read the entire list, whether you did any of these or not. If you have any questions about what anything on the list means, please stop me and I will explain the term to you. OK, let's begin.

60. From (MONTH) of 1981 until now, did you do any of the following? [READ DOWN LIST AND CIRCLE ONE CODE FOR EACH ACTIVITY.]

	60. DID ACTIVITY? YES	NO
a. Assault	01	02
b. Robbery (like a stick-up or mugging)	01	02
c. Burglary (breaking or entering to commit a crime)	01	02
d. Auto theft	01	02
e. Other theft (like shoplifting or stealing)	01	02
f. Forgery or embezzlement	01	02
g. Fencing stolen property	01	02
h. Gambling (like bookmaking or running numbers)	01	02
i. Pimping or prostitution	01	02
j. Illegal drugs - selling or making them	01	02
k. Driving while intoxicated	01	02
	IF ANY "YES" COMPLETE Q.61-Q.63 FOR EACH "YES" ACTIVITY.	IF ALL "NO" GO TO BOX A, PAGE 12.

Card 5

	61. Since (MONTH) of 1981, for (ACTIVITY), how many times did you do this? NUMBER OF TIMES DONE?	62. Were you caught by the police for this? CAUGHT BY POLICE? YES	NO	63. Did you usually know the person you did this to? KNEW VICTIM? YES	NO
a.	____ (62)	01	02	01	02
b.	____ (62)	01	02	01	02
c.	____ (62)	01	02	01	02
d.	____ (62)	01	02	01	02
e.	____ (62)	01	02	01	02
f.	____ (62)	01	02	01	02
g.	____ (62)	01	02	MAKE CERTAIN THAT ALL Q.'S ON THIS PAGE HAVE BEEN COMPLETED FOR EVERY "YES" ACTIVITY.	
h.	____ (62)	01	02		
i.	____ (62)	01	02		
j.	____ (62)	01	02		
k.	____ (62)	01	02		

BOX	
A	REFER TO INFORMATION/TRACING SHEET. IF PROBATIONER WAS ASSIGNED TO COMMUNITY SERVICE, COMPLETE THE COMMUNITY SERVICE SUPPLEMENT SERIES (Q. 64 - Q. 69) BELOW. OTHERWISE, GO TO Q. 70, NEXT PAGE

COMMUNITY SERVICE SUPPLEMENT

64. When you began your probation period in (MONTH): _____ of 1981, you were assigned to carry out Community Service as a condition of your probation. The following are questions about your Community Service experience. What kind of work were you assigned to do? [GET DESCRIPTION OF WORK ASSIGNMENT(S).]	64	DESCRIPTION OF WORK ASSIGNED: _____ _____ ☐☐ _____ _____ ☐☐ _____ _____ ☐☐
65. Did you complete your full 40 hours of Community Service?	65	Yes . . . . . 01 (66) No 02 (68)
66. Have you had any contact with the Division of Parole and Probation since the completion of your Community Service work assignment?	66	Yes . . . . . 01 (67) No 02 (70)
67. What kind of contact did you have? [PROBE FOR DETAILS]	67	TYPE CONTACT W/DIV. OF PROBATION: _____ _____ _____ _____ (70) ☐☐
68. How many hours did you work?	68	# hours worked. . . ☐☐
69. Why didn't you work the full 40 hours?	69	REASON(S) DID NOT WORK 40 HRS.: _____ ☐☐ _____ ☐☐ _____ ☐☐ _____

REFER TO INFORMATION/TRACING SHEET.

70.	Finally, I need to update some information about your address and employment. Is your address still ... [READ FROM INFORMATION/TRACING SHEET] ...?	70	Yes . . . . . 01 (72) No 02 (71)
71.	What is your home address now?	71	RECORD NEW ADDRESS ON INFORMATION/TRACING SHEET.
72.	Is your telephone number still ... [READ FROM INFORMATION/TRACING SHEET] ...?	72	Yes . . . . . 01 (74) No . . . . . 02 (73) No Phone. 03 (74)
73.	What is your phone number now?	73	RECORD NEW PHONE NUMBER OF INFORMATION/TRACING SHEET.
74.	Do you still work for ... [READ FROM INFORMATION/TRACING SHEET] ...?	74	Yes . . . . . 01 (END) No . . . . . 02 (75) Not Working . . . . . 03 (END)
75.	What is your current employer's name and address?	75	RECORD NEW EMPLOYER'S NAME AND ADDRESS ON INFORMATION/TRACING SHEET

END That's all we need to know. My supervisor may call you either to verify that I spoke with you or to get additional information about something that may not be clear. Thank you very much for your time and cooperation.

Appendix C

Needs Assessment Rating Scale

NEEDS ASSESSMENT RATING SCALE

I. Relationship w/family

1. Contact with family always hostile, directly related to current or previous arrests.

2. Contact with family sometimes hostile or separated from family with no communication or support.

3. Unremarked relationship or separated geographically, family not a significant influence.

4. Friendly relationship, but no emotional or financial support.

5. Positive supportive relationship.

II. Relationship w/significant other(s)
(Wife, husband, girlfriend, boyfriend, close friend, peer group)

1. Usually hostile relationship or high degree of negative reinforcement.

2. Sometimes hostile relationship or some degree of negative reinforcement.

3. Unremarked relationship or no significant other.

4. Friendly, peaceful relationship; little or no support.

5. Positive supportive relationship.

III. Living Conditions

1. Transient unstable situation (rooming house, halfway house, etc.)

2. Living in totally dependent relationship with parents, relatives, or friend.

3. Living in semi-dependent relationship with parents, relative or friend.

4. Maintaining residence in below average environment (high crime, deteriorating buildings, rural poverty, etc.)

5. Maintains residence in average or above average environment.

IV. Physical Health

1. Current health problem or permanent disability which precludes employment and requires regular medical care.

2. Current health problem or permanent disability which limits employment and requires medical care.

3. General poor health which limits employment--no regular medical care required.

4. Minor health problems which periodically affect employment situation.

5. No health problems.

VI. Mental Health

1. Medically diagnosed mental health problem, treatment recommended but not currently being provided.

2. Medically diagnosed mental health problem, treatment currently being provided.

3. Indications of some degree of undifferentiated mental health problem, treatment a possibility.

4. No indication of current mental health problems.

5. Seemingly well adjusted in most elements of life style.

VI. Vocational

1. Unskilled.

2. Semi-skilled.

3. Skilled.

4. "White Collar" (non-professional)

5. "White Collar" (professional)

VII. Employment

1. Very poor employment history, erratic short term jobs.

2. Irregular employment.

3. Employed now but was unemployed or laid off during past year.

4. Currently employed and has been for last year, not enthusiastic about job situation.

5. Steady employment record, satisfied with job situation.

VIII. Financial Situation

1. Completely dependent on state and/or social service agencies for support.

2. Completely dependent on family and/or spouse for support.

3. Supported by job income plus government supplements (food stamps, medicaid, AFDC, etc.)

4. Supported by job income--significant amount of indebtedness.

5. No financial problem.

IX. Drug Abuse

1. Verification of strong indication of use of addictive drugs, not currently in treatment.

2. Verification of strong indication of use of addictive drugs, currently in treatment.

3. Regular "recreational" use of drugs, no significant impact on family, job, etc.

4. Past history of experimentation with drugs or past history of serious drug problem.

5. No apparent drug abuse problem.

X. Alcohol Abuse

1. Verification or strong indications of serious alcohol problem, not currently in treatment.

2. Verification of strong indication of serious alcohol problem, currently in treatment.

3. Some indications of alcohol problem.

4. Past history of alcohol abuse.

5. No apparent problems with alcohol.

XI. Attitude Toward Supervision

1. Hostile, antagonistic, aggressive.
2. Arrogant, condescending, egotistical.
3. Unconcerned, apathetic, disinterested.
4. Casual, nonchalant, superficial.
5. Cooperative, open, concerned.

Appendix D

Case Record Data Abstraction Form

OFFICE: AGENT: DPPCASE #:

NAME: RTIID:

ADDRESS: TEL:

CASE OPEN: 04 21 81 EXPIRATION: 04 20 82 OFFENSE CODE: P083

PAYMENT PLAN: $000 99

FINES-F: $0000 COST-C: $0000 REST-1: $0000

ASSIGNMENT: COMMUNITY SERVICE

INITIAL SUPERVISION LEVEL: MINIMUM(1) MEDIUM(2) INTENSIVE(3)

SUPERVISION LEVEL CHANGE: NO CHANGE(00) LOWER(-1) HIGHER(01)

COMMUNITY RESOURCE NEEDS: _____ # INDICATED

PERSONAL CONTACTS SCHEDULE: <MONTHLY(1) MONTHLY(2) WEEKLY(3) >WEEKLY(4)

COLLATERAL CONTACTS SCHEDULE: <MONTHLY(1) MONTHLY(2) WEEKLY(3) >WEEKLY(4)

EMPLOYMENT CONTACTS SCHEDULE: <MONTHLY(1) MONTHLY(2) WEEKLY(3) >WEEKLY(4)

CASE PLAN	SCORES I	M.	F	! TYPE & NUMBER OF CONTACTS	
RELATIONSHIP WITH FAMILY:	___	___	___	! OP: ____	TP: ____
RELATIONSHIP WITH OTHERS:	___	___	___	! OC: ____	TC: ____
LIVING CONDITIONS:	___	___	___	! HP: ____	CONF: ____
PHYSICAL HEALTH:	___	___	___	! HC: ____	HEARING: ____
MENTAL HEALTH:	___	___	___	! HN: ____	INFO: ____
VOCATIONAL:	___	___	___	! EP: ____	TRANSP: ____
EMPLOYMENT:	___	___	___	! EC: ____	MVR: ____
FINANCIAL SITUATION:	___	___	___	! EN: ____	STATE POL: ____
DRUG ABUSE:	___	___	___	! MP: ____	FBI: ____
ALCOHOL ABUSE:	___	___	___	! MC: ____	OTHER: ____
ATTITUDE TOWARD SUPERVISION:	___	___	___	! MN: ____	

DATE CASE CLOSED: _____ TYPE CLOSE(0-9):___

Appendix E

DPP-Supp-14 Form

DPP-Sup-14

STATE OF MARYLAND
DIVISION OF PAROLE AND PROBATION

Name				Alias	
Institution				No.	
Sex	Race	Hgt.	Wgt.	Eyes	Hair
Marital Status			Occupation		
Birthdate			Religion		
			F.B.I. #		
Offense					
Court			Judge		
Sentence			Sentenced		
Paroled			Expiration		
Residence					
Employment					
Intensive			Supervisor		
Standby			Supervisor		
Honor			Supervisor		
Inactive:			Supervisor		
Delinquent:			Supervisor		
Telephone:					
Residence—					
Employment—					

	19	R	S	19	R	S	19	R	S
JAN.									
FEB.									
MAR.									
APR.									
MAY									
JUN.									
JULY									
AUG.									
SEPT.									
OCT.									
NOV.									
DEC.									

Special conditions of parole

Other

Appendix F

Client Contact Codes and Descriptions

Codes and Descriptions

Code	Description
Home Positive (HP)	On site contact with the client in his home.
Home Negative (HN)	A home visit in which no contact was made.
Home Collateral (HC)	On site contact in the home with someone other than the client
Employment Positive (EP)	On site contact with the client at his place of employment
Employment Collateral (EC)	Contact with the employer of the client or some other employee working with the client. This contact need not occur on the actual employment site. This contact also includes any written verification of employment (i.e., pay stub, letters from employers).
Employment Negative (EN)	An employment visit in which no contact is made.
Office Positive (OP)	Contact in the office with the client.
Office Collateral (OC)	Contact in the office with someone other than the client.
Failure to Report (FTR)	Failure of the client to appear for a scheduled office appointment to be noted in bold red print.
Special Condition Verification (SCV)	Contact which verifies special conditions.
Telephone Positive (TP)	Contact with the client by telephone.
Telephone Collateral (TC)	Contact about the client by telephone or when the client telephones and leaves a message for the agent
Miscellaneous (MP or MC)	Explain type of contact in field notes. This category covers any type of contact not specifically listed above and may be positive or collateral.

SOURCE: Maryland DPP, Community Supervision Manual, p. 52.

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